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Easing Renegotiation Rules in Public Procurement: Evidence from a Policy Reform

Abstract

Public procurement contracts require frequent renegotiation. We exploit the Czech implementation of an EU policy as a natural experiment to investigate the effect of eased renegotiation rules. We document that the eased renegotiation rules decreased winning bids but did not change the final price: the extra renegotiated price compensates for the lower winning bids. We next develop a stylized theoretical model to explain these results. Finally, we provide suggestive evidence of changes in contract allocation, but find no evidence of any reduction in the quality of the delivered objects.

JEL-Codes: H570, D440, D730.

Keywords: public procurement, renegotiation, contracts.

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1 Introduction

Public procurement accounts for about 12% of GDP and roughly 25% of general government spending in OECD countries (OECD, 2016, 2019). Uncertainty about the scope of work and final costs makes public procurement contracts incomplete and justifies allowing contracts to be renegotiated (Bajari et al., 2014). Indeed, renegotiation is a legitimate and widespread phenomenon in many jurisdictions. In the US, for instance, about 10.5% of federal public procurement contracts are renegotiated ex-post (Brogaard et al., 2021). Renegotiation is likely to change the prospects of winning the procurement competition and, thus, the bidding strategies of firms and the outcomes of the procurement markets. Allowing renegotiation may introduce inefficiencies, as the most cost-efficient supplier is no longer guaranteed to win. Suppliers anticipating high enough gains from renegotiation might strategically underbid more efficient competitors.

In this paper, we study how allowing renegotiation of awarded contracts affects the winning bid and the final price. To do so, we exploit a policy reform in the Czech Republic that, for the purpose of harmonizing with an EU Directive,¹ eased the rules for renegotiation in the event of cost overruns. Before the reform, if cost overruns occurred, renegotiation was only allowed in exceptional circumstances and was rarely initiated. After the reform, eased renegotiation rules applied to all contracts, including existing contracts awarded no more than three years before October 2016. Due to their susceptibility to cost overruns, construction contracts have been exposed to the reform disproportionately more than contracts in other sectors.

To empirically study the effect of this reform, we use a universe of Czech procurement contracts above circa EUR 80,000 for supplies of goods and services and above EUR 240,000 for construction works from 2014 to 2017. We first show that the reform increased the share of renegotiated contracts, especially in the construction sector. About 40% of construction contracts awarded a year after the reform were renegotiated, while among contracts in other sectors, the rate of renegotiated contracts remains negligible even after the reform. We then estimate several time series analyses, studying the impact of the reform on the evolution of the winning bids and final prices of construction contracts over time. Furthermore, we use the construction contracts as the treatment group and extend the empirical work by several difference-in-differences specifications using the exposure to the reform as a source of identifying variation.

We show that the possibility of renegotiation decreases the winning bid by more than 3% of the estimated value of the contract compared to the pre-reform period. The

 $^{^{1}}$ Article 72 of the 2014/24/EU Directive prescribes when renegotiation of a public procurement contract is permitted. This directive had to be transposed into national laws in all member states by April 2016.

decrease is robust to various specifications, including using Slovak construction contracts as a control group. To estimate the effect on the final price, we distinguish between the effects on contracts awarded before the reform but exposed to the eased renegotiation rules due to the retroactivity of the reform (so that bidders could not anticipate the possibility of renegotiation at the time of bidding); and contracts where bids were placed after the reform (so that bidders could anticipate the possibility of renegotiation). In the former, the final price increases by about 2% of the estimated value. However, this increase seems temporary, as we find no effect on the final price for contracts awarded under the eased renegotiation rules. The final price of contracts with the possibility of renegotiation anticipated is thus the same as for contracts in the pre-reform period, in which renegotiation was impossible. However, when we weight contracts by their estimated value to estimate fiscal expenditures for the procuring authorities, the final price for contracts with anticipated renegotiation increases by 2.4% of the estimated value. Importantly, we show that this increase is driven by a few of the largest contracts, for which renegotiation represents a substantial financial burden for the procurer authorities.

We next construct a stylized theoretical model to explain these results. In this model, bids are higher before the reform because firms, unable to renegotiate, do not factor in any extra price from renegotiating when bidding. If bidders are allowed to renegotiate but do not yet anticipate this in their bids, they continue to bid in the same manner, with the increase in the final price explained by winning bidders engaging in renegotiation. Conversely, if bidders anticipate being allowed to renegotiate at the time of bidding, they bid more aggressively, factoring in the extra price they expect to obtain from renegotiation. These lower bids are offset by the extra price obtained, explaining why the final price does not change after the reform.

Finally, we provide suggestive evidence of changes in contract allocation but find no evidence of any reduction in the quality of the delivered objects. We show only a slight and, at best, marginally statistically significant shift in the allocation of contracts towards less productive firms and firms with more experience with renegotiation. Furthermore, the quality of construction contracts—measured as an inverse value of the number of repair projects—does not seem to be affected by the reform.

To the best of our knowledge, this is the first paper that empirically evaluates the impact of the 2014^2 reform of the EU procurement market on the bidding strategies and the final price of public procurement contracts. The Czech procurement market is a suitable setting for studying the reform for two reasons. First, data on contracts below the European *de-minimis* thresholds and on renegotiation before and after the reform are available. Second, similar to other EU member countries such as Bulgaria, Poland,

 $^{^{2}}$ The EU directive is from 2014 and the reforms in member states usually took effect in 2016.

and Romania, in Czechia, the rules before the reform essentially prevented renegotiation except for rare circumstances, such as natural disasters. Such rigid rules allow us to estimate near to the full effect of the eased renegotiation rules.³

Our paper is part of an empirical literature investigating the effect of post-bidding alterations to procurement contracts on the procurement outcomes. Findings in this literature point in different directions. Decarolis (2014) studies the introduction of first-price auctions for Italian public works and observes lower bids but also lower performance, measured by longer delays. The author explains this as the adverse selection of firms with higher benefits of changing the contract, which can then recuperate their lower bids through alterations.⁴ Similarly, Ryan (2020), studying Indian power contracts, finds that politically better-connected firms lower their bids, anticipating higher benefits when renegotiating; moreover, such firms increase the probability of renegotiation by not indexing their bids to future energy prices.⁵ However, in a study on Californian highway pavement contracts, Bajari et al. (2014) observe that when bidders can expect contract alterations for which they receive positive compensations, bids go up rather than down. The authors attribute this to adaptation costs firms incur when making alterations to the scope of the contract (i.e. "disruptions to the normal flow of work" (p.1289), and "happing, dispute resolution, and opportunistic behavior" (p.1289) induced by renegotiation). While the positive compensation would as such lead bidders to decrease their bids, this is more than compensated by large adaptation costs.⁶

These papers also differ in the method used to measure the effect of alterations. Bajari et al. (2014) and Jung (2016) compare contracts according to their levels of alterations, with the challenge of controlling for contract characteristics. Ryan (2020) uses a structural model and counterfactual simulation to estimate the effect of strict contract enforcement. Our paper is in line with literature that looks at the effect of institutional

 $^{^{3}}$ For example, in the Netherlands, public contract modifications were possible, but there was no legislative framework for contract modification. The admissibility of modifications was determined based on the basis of case law, introducing uncertainty in the Dutch legal system (Hebly and van den Boogaart, 2017).

⁴Cameron (2000) provides a similar explanation for results obtained by comparing US states with rigid and flexible systems for power purchase contracting. She observes that in rigid systems, bids are lower, but the probability of a contract breach is higher. The author explains that rigid systems ironically cause more alterations in selecting firms that are more likely to default.

⁵Baltrunaite (2020), Baranek and Titl (2024), Schoenherr (2019) and Titl and Geys (2019) show that firms with political connections enjoy various forms of preferential treatment. Renegotiation of a procurement contract can be one of the ways for politically connected firms to extract additional rents. Indeed Brogaard et al. (2021) find that, in the U.S., firms with connections bid low initially and then renegotiate the contract conditions such as prices, deadlines etc. They find that connected firms were three times more likely to successfully renegotiate procurement contracts ex post.

⁶Using a similar approach, Jung (2016) confirms this result for Vermont road construction works, though lower adaptation costs are estimated. Jung et al. (2019) find in the same setting that bidders who anticipate a change in input quantities skew their bids.

changes (Cameron, 2000; Decarolis, 2014), and is unique in studying a natural experiment with a switch from not allowing renegotiation of cost overruns to allowing it.^{7,8} The experience with this natural experiment is useful for policymakers in any country considering reforms that ease or tighten renegotiation rules in public procurement. This is particularly relevant for other European Union member states with comparable legal frameworks based on the same EU Directive.

We structure the remainder of the paper as follows. Section 2 describes the institutional design of the procurement market in Czechia and the studied reform. In Section 3, we present the empirical examination of the changes in the winning bid and the final price. In Section 4, we develop a stylized theoretical model that explains our empirical findings. We next study the allocation of contracts and the quality of construction projects in Section 5. We end with a discussion of our results in Section 6.

2 Institutional Design

Since its transition from a communist centrally-planned economy to a market-based economy in the early 1990s, the Czech Republic has been one of the most prosperous countries in the Central and Eastern Europe region. The current GDP *per capita* in PPP is comparable to Spain and Italy. Nevertheless, in 2010, the National Economic Council of the Czech Government identified weak institutions as one of the crucial factors hindering economic growth. The lack of a strong and stable institutional framework also applies to the public procurement law.

We study a reform that was adopted in April 2016 and took effect from October 1st, 2016 and introduced a clear and simple renegotiation procedure for public procurement contracts.⁹ The reform was adopted as Act No. 134/2016 Coll. on public procurement. Before the reform, if a cost overrun occurred and the winning bidder could not deliver the object at the increased costs, the procuring authority had to launch a new competition open to all firms or a less transparent procedure called "negotiated procedure without publication". The latter was possible only if additional public works or supplies and services were necessary due to unexpected circumstances, and a new contract was tech-

⁷De Silva et al. (2017) study the effect of a reduction in procurement agencies' budgets for supporting contract changes on highway construction costs. They find that the costs decreased and that projects were more often finished on time and within the budget.

⁸An alternative empirical strategy for studying contract renegotiation could be the instrumental variable approach used by, for example, Carril and Duggan (2020). They find that increased industry concentration led to more frequent price renegotiation of public procurement contracts.

⁹There were several other changes that the reform brought. One of them is relevant to our identification. Specifically, it is the change in the threshold for the simplified below-threshold regime, which only affected the treatment group. In Section O.A5 of the Online Appendix, we show that all our findings are robust to excluding the contracts affected by this change.

nically or economically inseparable from the initial contract. Under these conditions, the contract could be awarded to the same firm. The additional expenses could not exceed 30% of the price of the initial contract.¹⁰ In practice, it was difficult to assess whether the circumstances for "negotiated procedure without publication" were satisfied. Procurement officers therefore tended to apply this procedure only in emergency situations such as natural disasters.

The 2016 reform transposed European directives¹¹ into the Czech legislative system and enabled adjustments to the awarded contracts without launching a new competition. According to the explanatory memorandum, the law aims to set new clear rules to prevent corruption and to ensure effective use of public funds and to make finalization of public procurement faster, while keeping the transparency and fairness of the competition high. Under the new rules, a modification of the contract is allowed as long as it is not substantial, i.e. the modification does not change the nature of the contract. The modification is considered substantial if, for example, it changes which firms can participate in the competition and it leads to a substantial extension of the range of performance. Furthermore, the value of modification must be lower than the minimum of a) the financial limit for an above-threshold public contract and b) 10% (15%) of the initial price of the contract for services and supplies (for public works). In the event of unforeseen circumstances, the value of modifications can be up to 50% of the original price of the contract as long as the nature of the contract does not change. However, after the deduction of the price for public works, services, and supplies that have not been performed, the total price increase must not exceed 30% of the original price of the contract. Importantly for our research design, the new renegotiation rules applied also to ongoing public procurement contracts initiated no more than 3 years before October 1st, 2016.

3 Empirical Evidence

We exploit the institutional setting and reform implementation to answer two research questions. First, how did the eased renegotiation rules change the winning bids? Second, how did the reform impact the final price? The latter question breaks down into two subquestions. How did the unanticipated possibility of renegotiation affect the final price in the period immediately after the reform? How did the reform change the final price of

¹⁰Furthermore, the procurer had to provide reasons why an open competition could not be launched (such as too little time available).

¹¹Directives 2014/24/EU, 2014/25/EU, and 2014/23/EU. Note that even though the European directives were adopted in 2014, it was likely very difficult for Czech firms to create informed anticipation of the reform. This is because the transposition of European directives is often cumbersome; there is only a deadline by which the transposition to national law should be implemented, but not a preset date. The transposition frequently occurs after the official deadline.

contracts once the bidders could anticipate the possibility of renegotiation? We address these questions in this section.

3.1 Data

We use the universe of Czech public procurement contracts with an estimated value above the thresholds of approximately EUR 240,000 for construction sector contracts and EUR 80,000 for other contracts. By law, procurement authorities must disclose detailed information about all contracts with an estimated value above the aforementioned thresholds in an online system called the Gazette of Public Procurement.¹² We use a hand-cleaned version of the dataset processed by a private company called Datlab s.r.o. Furthermore, many procurers choose to report contracts below these thresholds voluntarily. Overall, our initial dataset contains 18,874 contracts that add up to CZK 517 billion (EUR 22 billion) in total estimated value. The dataset covers detailed information about each contract, including project industry classifications (CPV codes), engineering estimates of value, initial prices, final prices, numbers of competitors, and identities of contractors and procuring authorities. The descriptive statistics regarding the data are provided in Table B1 in Appendix B.

Figure 1a shows shares of renegotiated contracts on contracts awarded between January 2014 and December 2017 in construction (our treatment group, as we explain in detail later) and in four similar industries—industrial machinery, technical services, transportation, and energy—that will form a baseline control group. The share of renegotiated contracts in construction gradually increases over time, and at the end of the observed period, i.e., two years after the reform, fluctuates around 40%. We note that our dataset contains all renegotiations that happened until December 2019.¹³ The information about price renegotiation comes from so-called "announcements about a change in the contract" published in the Gazette of Public Procurement and was collected by the same company as the main procurement dataset. The share of renegotiated contracts among the nonconstruction sectors remains around 5% on average throughout the studied period. The gradually increasing trend observed even before the eased renegotiation rules came into effect is caused by the fact that the eased rules applied also to existing contracts awarded no more than 3 years before the reform. These contracts were awarded before the rules changed but were renegotiated subsequently. Presumably, the share of contracts that could have been renegotiated is thus increasing in the time before the reform, reaching 100% in October 2016.

¹²https://vvz.nipez.cz

¹³According to the available delivery data (covering 45% of the contracts), 75% of all construction contracts are expected to be delivered within one year, and 95% within two years.

Figure 1: Trends in Renegotiation



Notes: The left panel shows shares of renegotiated contracts by industry by the date when the contract was awarded. The baseline control group consists of contracts in *industrial machinery, technical services, transportation,* and *energy.* Note that the figure does not represent the date when the contract was renegotiated, but awarded. Since the eased renegotiation rules applied also to existing contracts awarded no more than 3 years before the reform, even contracted awarded before the reform could be renegotiated. That resulted in the gradual increase of renegotiated contracts awarded before the reform. The right panel shows the frequency of renegotiated amounts among contracts that have been renegotiated by the industry between January 2014 and December 2017 by the date when the contract was awarded. The mass of contracts at zero means that many contracts have been renegotiated by only a small amount, counting less than 1% of the winning bid. The gray dashed vertical line indicates the threshold of 30% that stands for the legally binding restriction in most cases.

We observe 937 renegotiated contracts in either construction (885) or baseline control group (52) contracts.¹⁴ Among the 937 renegotiated contracts, the average change in the price was a 7.9% increase compared to the winning bid. The average renegotiation is slightly higher among the baseline control group (9.2%), but the difference is not statistically significant. Figure 1b shows a histogram of relative price increases compared to the winning bids. Roughly 18% of renegotiation leads to a lower final price than the initial winning bids.¹⁵ In most cases, renegotiation leads to an increase between 0% and 30%.

3.2 Empirical Strategy

A standard problem with procurement data is that every contract is awarded only once, and therefore, it is impossible to observe the same contract under different policy settings. To ensure comparability of observations, we define our main outcome variables as ratios

¹⁴There are additionally 66 renegotiated contracts in *other* industries.

¹⁵Based on the review of documentation for a small sample of randomly chosen renegotiated contracts, a renegotiation resulting in a lower final price is a contract modification that usually also reduces the scope of contractual performance, subsequently leading to a reduction in the final price.

of the winning bid to the estimated value (winning bid ratio) and of the final price to the estimated value (final price ratio). Each procurement competition announcement contains an estimated contract value, which is determined by professionals and is used to define the applicable public procurement procedure.¹⁶ Using the estimated value as a benchmark for contract price has been repeatedly done in the literature (see e.g. Bajari et al., 2014; Baranek and Titl, 2024).

To define the treatment group, we rely on the argument that contracts in the construction sector are more predisposed to cost overruns. While the reform did not explicitly target construction contracts, it aimed to enable renegotiation in situations where cost overruns occurred. These are more likely to happen in construction, where a lack of information at the time of bidding is omnipresent, for instance concerning soil quality. Similarly, neither public authorities nor suppliers can take into account unpredictable future events related to, for instance, weather. Furthermore, construction contracts tend to last longer and are thus more prone to exogenous shocks. The problem tends to be even more prominent in repair and renovation contracts when the tender documentation is based on old, often imprecise records. The exposure to the reform is thus disproportionately larger among construction contracts than in other sectors. The identifying variation is induced by the different exposure to the reform and different prevalence of the need for renegotiation among the construction contracts and contracts in the control group. This is consistent with the divergence in the shares of renegotiated contracts between construction and non-construction contracts in Figure 1b.

We start the empirical analysis by studying the time evolution of each outcome variable around the time of the policy reform. We run the following regression on a sample of all construction contracts in the studied period:

$$y = \alpha + \sum_{t} \beta_{t} Quarter_{t} + \gamma X + \varepsilon, \qquad (1)$$

where y represents the outcome variable (either the winning bid ratio or the final price ratio), X controls for selection methods and public procurement procedure used to award the contract¹⁷, and $Quarter_t$ stands for quarterly dummy variables. While the time

¹⁶The estimated value is determined, and the tender documentation is prepared by experts (IT department, civil engineers) and then forwarded to the department or personnel responsible for the tender.

¹⁷The control variables are operationalized as a set of dummy variables for each type of selection method and each public procurement procedure, respectively. The selection methods are two: lowest price contracts and scoring auctions known as "Most Economically Advantageous Tenders" (MEAT). The procurement procedures include mainly open procedure (56% of the contracts in our data), negotiated procedures (21%), approaching bidders (18%), restricted procedure (4%), and others such as competitive dialog or innovation partnership that account for less than 1% of all contracts (see Directive 2004/18/EC for details about these procedures). In Appendix B, we show that our results are robust to omitting the

analysis is potentially informative about the immediate effect of the reform, the mediumterm evolution of bidding strategy and final prices may be influenced by other factors, such as economic conditions, EU funding cycles, shocks in material prices, and long-term trends of decreasing competition in public procurement in the EU (The European Court of Auditors, 2023).

To control for the confounding factors, we extend the analysis by implementing the difference-in-differences specification, thereby increasing the robustness and credibility of our results. Contracts in the construction sector do not have a natural control group. We therefore rely on three control groups: a) a baseline control group, which consists of contracts from four industries—industrial machinery, technical services, transportation, and energy—which resemble the construction industry, for example, in the requirement of detailed and individualized specifications, b) a broader control group that consists of all contracts and c) Slovak construction contracts. Unfortunately, due to a lack of detailed information on contract renegotiation on Slovak contracts, we use the Slovak data to estimate the effect on the winning bids but not the final prices. Using diverse control groups addresses various sources of confounding factors. Control groups based on Czech contracts account for Czech-specific influences, such as business and political cycles, while Slovak construction contracts control for factors relevant to the construction sector in the region of Central Europe, including, for example, the cost of construction materials.

We run the following difference-in-differences specification:

$$y = \delta Post + \beta Post * Construction + \gamma X + \omega_{cpv} + \varepsilon,$$
⁽²⁾

where y represents the outcome variable (either the winning bid ratio or the final price ratio), Construction and Post are indicator variables that equal 1 for construction contract and for contract awarded in the post-reform period, respectively, ω_{cpv} stands for industry 4-digit (6-digit) CPV code fixed effects, and X controls for selection methods and public procurement procedure used to award the contract. The coefficient of our interest is β . To interpret β as a causal average treatment effect on the treated (ATT), we assume that the so-called parallel trend assumption holds, i.e., in the absence of the policy change, the average winning bid and the average final price of construction contracts would evolve the same as non-construction contracts. We provide evidence supporting the parallel trend assumption by estimating regression (2) with a set of quarterly dummies for the whole period instead of *Post*, and testing the difference in the treatment and baseline control

controls for the type of selection method and public procurement procedure.

group in the pre-treatment period.

3.3 Winning Bid

We start by showing that the eased renegotiation rules decreased the winning bids. We first plot coefficients from the time series analysis and then run the difference-indifferences to estimate the ATT effect. We set the time of the reform to April 2016, when the policy change was adopted by parliament and could evoke an anticipation effect.

Figure 2a plots β_t from regression (1) using a ratio of the winning bid and the estimated value of the contract. It shows an increasing trend before the eased renegotiation rules were implemented, a substantial drop at the time of the reform followed by a period of stability, and then a return to the increasing trend in the last two quarters. Evidence from a simple time series analysis thus suggests that the eased renegotiation rules interrupted the increasing trend and reduced the winning bid.





Notes: The left panel shows the evolution of the winning bid ratio (winning bid over the estimated value of the contract) for construction contracts over time by plotting coefficient β^t from regression (1). The right panel plots estimated quarterly differences in winning bid ratio between construction and baseline control contracts over time. In both figures, the effect is normalized to 0 in Q1 2016. In the dynamic difference-in-differences approach, we control for 4-level CPV codes industry fixed effects, selection methods, and public procurement procedure. See regression (2).

Figure 2b shows the coefficients from the dynamic difference-in-differences using the winning bid ratio as the outcome variable. It provides evidence supporting the parallel trend assumption, as none of the quarterly differences between construction and non-construction contracts from the pre-treatment period are statistically significant. It further shows a visible downward shift after the reform, providing evidence of the fall of the winning bid. A comparison of Figures 2a and 2b further suggests that controlling for non-construction contracts suppresses the increasing trend apparent in the time series analysis and thus provides credible evidence on the causal medium-term impact of easing the renegotiation rules.

Table 1 shows the effect of eased renegotiation rules on the winning bid ratio estimated using the difference-in-differences specification as in regression (2). The effect is negative and statistically significant: eased renegotiation rules led to a decrease in the winning bid by around 3% of the estimated value.

	Winning Bid Ratio	Winning Bid Ratio
Post Treatment \times Construction	-0.031*** (0.006)	-0.032^{***} (0.006)
Post Treatment	$0.007 \\ (0.005)$	0.010^{**} (0.005)
Industry FE N	4-digit CPV 13,502	6-digit CPV 13,263

	Table 1:	Effect	of Eased	Renegotiation	Rules or	1 Winning	Bid
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Standard errors in parentheses

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

Notes: The table shows results from two difference-in-differences specifications, using the winning bid ratio (winning bid over the estimated value of the contract) as the outcome variable. The two columns differ in the granularity of industry fixed effects. Post Treatment is a dummy variable that equals 1 for contracts procured after the reform and 0 otherwise. Construction is a dummy variable that equals 1 for construction contracts and 0 otherwise. We control for the type of procurement procedure, the evaluation criteria, and a set of industry fixed effects.

We complement this analysis with robustness exercises presented in Appendix B.1. These robustness checks contain various alternative specifications, including using: (i) the logarithm of the winning bid as the outcome variable and controlling for a logarithm of the estimated value, (ii) all non-construction contracts as the control group, (iii) the Slovak construction sector as the control group, (iv) a sample restricted to contracts with non-negative renegotiation, and (v) a specification without controlling for the type of procurement procedure and the type of evaluation criteria. Overall, the point estimates remain stable across all these specifications; the ATT effect is consistently around 3% of the estimated value. All difference-in-differences specifications thus provide robust evidence that easing renegotiation rules decreased the winning bid among construction contracts.

A potential concern of our empirical specification is that a part of the control group in the main specification was exposed to the legal possibility of renegotiation as well. In particular, Figure 4 shows that some contracts in the control group (less than 5%) awarded after the reform have been renegotiated. This mechanism works against our estimates as it tends to attenuate the true effect. Our estimates thus likely constitute a lower bound on the ATT causal effect on the winning bids.

3.4 Final Price

We next analyze the effect of the reform on the final prices in the intermediate and postreform periods. For clarity of the comparison, we define three settings. Setting I—a baseline setting—contains contracts that were awarded and terminated before the reform was implemented, i.e., the reform could not impact the final price, either through bidding or renegotiation. Contracts in setting II—an intermediate setting—were ongoing when the reform was implemented. Due to the retroactivity of the reform, these contracts were unexpectedly exposed to the eased renegotiation rules. The final prices in setting II thus correspond to the final prices of contracts that were awarded according to pre-reform rules but could be renegotiated under the eased rules. Finally, contracts in setting III were awarded under the new renegotiation rules. Bidders, therefore, knew the winning bidder could renegotiate if cost overruns occurred.

Because our dataset lacks information about when the contracts were renegotiated and terminated, we approximate and classify contracts into setting II if they were awarded within a year before the reform, i.e., between April 2015 and April 2016, and are thus likely to last after the reform was implemented. Contracts awarded before April 2015 are then classified as setting I.¹⁸ We cannot rule out that some contracts in settings I and II are misclassified and are, in fact, subject to different renegotiation rules. To provide more robust evidence, we replicate the results using different dates to assign contracts into settings I and II in Appendix B.2. Contracts awarded after April 2016 certainly belong to setting III. Figure 3 shows the preferred classification of contracts into the three settings.

Figure 4 shows that the share of renegotiated contracts and the value of renegotiation vary across settings and between construction and non-construction sectors. Both the share and the value of renegotiated contracts confirm that renegotiation is prevalent in setting III among construction contracts, as almost 40% of contracts were renegotiated, and the average renegotiated amount among renegotiated contracts reached 6% of the estimated value. The second highest share of renegotiated contracts is among construction contracts in setting II, followed by non-construction contracts in setting III. The remaining combinations of settings and industries show rather negligible values. In Table O.A.1

¹⁸Note that contacts classified as setting I are awarded within the 3-year period before the reform; however, they are likely to be terminated before it was implemented, and therefore not affected by the reform.

Setting I		Setti	Setting II Settin		ng III	
•		•	•	•	•	
2014m1		2015m4	2016m4	2016m10	2017m12	

Figure 3: Contracts Classification into Three Settings

Notes: The figure shows the definitions of our three settings that differ by the renegotiation rules. Setting I—a baseline setting—consists of contracts awarded between January 2014 and April 2015. Setting II ("contracts awarded following pre-reform rules but renegotiated under eased rules") consists of contracts awarded between April 2015 and April 2016. Setting III ("contracts awarded and renegotiated under eased rules") consists of contracts awarded between October 2016 and December 2017.

in the Online Appendix, we provide regression results confirming that construction contracts in setting III are renegotiated substantially more often and to a larger extent than others.



Figure 4: Share and Value of Renegotiation

Notes: The left panel shows the share of contracts that were renegotiated, while the right panel shows the relative value of contracts that were renegotiated (ratio of the amount renegotiated in all contracts to the value of all contracts). Both panels document that renegotiation in setting III among construction contracts was significantly more pronounced than in other settings and in non-construction contracts.

3.4.1 Final Price between Settings I and II

We start by estimating the effect on the final prices of contracts between settings I and II, i.e., the effect on the final price in the intermediate period. The share of renegotiation contracts increased from 1% for construction and 0.2% for non-construction contracts in setting I to 6.5% for construction and 0.5% for non-construction contracts in setting II. We use the final price ratio to estimate (i) the evolution of the ratio over time and (ii) dynamic difference-in-differences. The primary specifications correspond to regression (2) using 4-digit and 6-digit CPV code industry fixed effects.

Figure 5a shows the evolution of the final price ratio for contracts in settings I and II over time. It provides evidence of a price increase after the easing of the renegotiation rules. Figure 5b then provides more convincing evidence of the increase in final price in the intermediate setting by plotting the quarterly treatment effects from the dynamic difference-in-differences specification. Furthermore, Figure 5b shows evidence supporting the parallel trends in the pre-treatment period.

In the first two columns in Table 2, we present the results from the primary differencein-differences specification. The key finding is that the unanticipated possibility of renegotiation increased the final price among construction contracts by 2% of the estimated value of the contract, i.e., the final price of construction contracts in setting II is higher than those in setting I.

Figure 5: Final Price: Effect on Unanticipated Possibility of Renegotiation Change from Setting I to II



Notes: The left panel plots quarterly averages of the final price ratio estimated from regression (1) on a sample of all construction contracts in settings I and II. The right panel shows ATT estimates on the construction contracts from a dynamic difference-in-differences with the final price ratio as the outcome variable in settings I and II.

We complement this finding with a set of robustness checks (see Appendix B.2). These robustness checks contain various alternative specifications, including using: (i) the logarithm of the final price as the outcome variable and controlling for a logarithm of the estimated value, (ii) all non-construction contracts as the control group, (iii) a sample restricted to contracts with non-negative renegotiation, (iv) the specification without controlling for the type of procurement procedure and the type of evaluation criteria, and (v) alternative split dates to differentiate contracts into settings I and II. Overall, these exercises show the robustness of the main finding that the unanticipated possibility of renegotiation led to an economically meaningful and statistically significant increase

in the final price of procurement contracts.

3.4.2 Final Price between Settings II and III

Next, we compare the final prices in settings II and III. The difference between these two settings captures how the final prices change once bidders adjust their behavior to the eased renegotiation rules after the intermediate period. Figure 6a shows the estimates from the time series analysis before and after the reform using the final price ratio. It does not show a clear pattern. Figure 6b plots the treatment coefficients from the dynamic difference-in-differences. Once we control for other contracts, a slight decrease in the final price ratio appears after the reform.

Figure 6: Final Price: Possibility of Renegotiation is Anticipated Change from Setting II to III



Notes: The left panel plots quarterly averages of the final price ratio estimated from regression (1) on a sample of all construction contracts in settings II and III. The right panel shows ATT estimates on the construction contracts from a dynamic difference-in-differences with the final price ratio as the outcome variable in settings II and III.

The point estimates from regression (2) are indeed negative. The third and fourth columns in Table 2 show that the final price decreased by almost 2% of the expected value on average, and the effect is marginally statistically significant in both specifications.

A battery of the same robustness exercises as in the previous subsection is presented in Appendix B.2 and yields a negative effect of a similar magnitude; however, some results are not statistically significant. Overall, we find evidence of a slight decrease in the final price between settings II and III. While point estimates in all specifications are negative, some are not statistically distinguishable from a null effect. Note that the comparison between settings II and III is based on fewer observations than the other between-setting comparisons of the final price. This may limit our statistical power to reject the null effect.

3.4.3 Final Price between Settings I and III

Finally, we compare the final prices in settings I and III, thereby comparing the final prices in the baseline setting with those that emerged from the post-reform behavior. We find no difference between the final prices. However, we find that the reform led to increased aggregated fiscal expenditures of procurers, as larger contracts are more likely to be renegotiated.

Figure 7a plots a time evolution of the final price ratio covering the two settings. Except for the last quarter (2017 Q4) in setting III, all point estimates are within 5% of the estimated value from the normalized value. Most of them are statistically insignificant. Adding a control group strengthens the evidence of the null effect, as shown in Figure 7b. Estimates from the dynamic difference-in-differences show no pre-trend and no visible effect on the final price ratio after the reform.





Notes: The left panel plots quarterly averages of the final price ratio estimated from regression (1) on a sample of all construction contracts in settings I and III. The right panel shows ATT estimates on the construction contracts from a dynamic difference-in-differences with the final price ratio as the outcome variable in settings I and III.

We present the results from difference-in-differences in the last two columns in Table 2—both specifications report negligible and statistically insignificant null effects. We report results from the same battery of robustness exercises conducted in preceding comparisons in Appendix B.2. All of them consistently yield a null effect. Overall, we provide robust evidence of a null effect on the final price between settings I and III. As a back-of-the-envelope verification of our results, note that the increase in final price from setting I to setting II is about 2% of the estimated value, and then the final price decreased by almost 2% between settings II and III. This simple arithmetic implies that the final price between setting I and setting III should not change, confirming our results.

	Final Price Ratio		Final Price Ratio		Final Price Ratio	
	Setting I and II		Setting II and III		Setting I and III	
Post \times Constr.	0.022^{***}	0.017^{**}	-0.019^{**}	-0.019^{*}	-0.002	-0.007
	(0.007)	(0.007)	(0.010)	(0.010)	(0.010)	(0.010)
Post	0.010^{*}	0.011^{*}	0.005	0.010	0.019^{***}	0.024^{***}
	(0.006)	(0.006)	(0.007)	(0.008)	(0.007)	(0.008)
Industry FE (CPV)	4-digit	6-digit	4-digit	6-digit	4-digit	6-digit
N	9,118	8,881	5,144	4,960	6,933	6,723

Table 2: Final Price: Difference-in-Differences Results

Standard errors in parentheses

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

Notes: This table summarizes the effect of the reform on the final price ratio. For each comparison of the final price in two settings, we report results from two specifications that differ in the granularity of the CPV. The first two columns show the difference between settings I and II and thus capture the effect of the unanticipated possibility of renegotiation on the final price. The third and fourth columns show the difference between settings II and III. The last two columns show the differences between settings I and III. The effect thus represents the difference in final prices between the pre-reform and post-reform periods. Each regression is estimated following regression (2).

We further assess the fiscal consequences induced by the reform for the procurement authorities. Despite the null effect on the final price on average, the reform could still increase overall fiscal costs if renegotiation was concentrated among larger contracts. The share of renegotiated contracts indeed increases with the estimated value of the contracts, and the positive gradient is visible for each setting. However, the effect is most pronounced in setting III, where small contracts are rarely renegotiated, while among the top 10% of largest contracts, approximately a third of them undergo renegotiation. For more details, see Figure B1 in Appendix B.

We re-estimate the effect of the reform on the final prices between settings I and III, weighting the regression by the estimated value of the contracts. The first two columns of Table 3 present our estimates.¹⁹ In contrast to the baseline results, the weighted specification yields a positive and statistically significant effect on the final price. The eased renegotiation rules increased the final price by 2.5% of the estimated value. The effect is driven by a limited number of the largest contracts, which we show in the remaining two columns of Table 3. In these two columns, we re-estimate the regressions using contracts below CZK 1 billion (EUR 40 million), which effectively reduces the sample size by 20 (21) contracts, and the increase in the final price disappears.

	Price Ratio	Price Ratio	Price Ratio	Price Ratio
Set. III \times Constr.	0.024**	0.024^{**}	I ^{**} 0.003 -0.000	
	(0.011)	(0.012)	(0.011)	(0.011)
Setting III	0.044^{***}	0.053^{***}	0.038^{***}	0.049^{***}
	(0.009)	(0.011)	(0.009)	(0.010)
Industry FE	4-digit CPV	6-digit CPV	4-digit CPV	6-digit CPV
Ν	6,933	6,723	6,912	6,703
Sample	Weighted	Weighted	Weighted	Weighted
Contracts	All	All	Below CZK 1 bil.	Below CZK 1 bil.

Table 3:	Final Price	ce: Fiscal	Consequen	ces of the	Reform
	Contracts	Weighted	by Expected	Value of Co	ontracts

Standard errors in parentheses

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

Notes: The table shows the results from four difference-in-differences specifications using the final price ratio as the outcome variable. Observations are weighted by the expected value of contracts. Columns 1 and 2 present results from the full sample of all contracts. Columns 3 and 4 present results from a restricted sample of contracts with an expected value below CZK 1 billion. Columns differ in the granularity of industry fixed effects. Setting III is a dummy variable that takes the value of 1 for contracts from setting III (renegotiation is possible, and firms know this when submitting their bids) and 0 for contracts from setting I (renegotiation is impossible, and firms know this when submitting their bids). Constr. is a dummy variable that equals 1 for construction contracts and 0 otherwise. In each specification, we control for the type of procurement procedure and the type of evaluation criteria.

4 Theory

We now construct a stylized model with the purpose of explaining the obtained results in Section 3. We start by explaining how our modeling decisions are driven by these results, and then set up the model.

¹⁹In Table O.A.5 in the Online Appendix, we present the findings from an alternative specification using the logarithm of the outcome variables while controlling for the logarithm of the contracts' estimated value. The findings are similar in magnitude and statistical significance to the results presented here.

4.1 Preliminaries

Our starting point is that a procurement process takes the form of a sealed-bid firstprice auction. In such auctions, the individual bidder maximizes profits by placing a bid equal to the expected cost of the bidder with the second-lowest cost (supposing the focal bidder has the lowest costs). An added feature specific to our context is that the costs of delivering may change after the winning bidder has been determined, which, due to the reform we study, may lead to renegotiation. As Figure 1b shows, after the reform, the renegotiated amount is positive in the majority of cases where renegotiation took place. For this reason, our model limits itself to cost overruns. When they are liable for cost overruns, rational bidders should factor these into their expected costs, resulting in higher bids. When they are allowed to renegotiate after cost overruns occur, however, rational bidders should correct their bids downwards to adjust for any expected extra price obtained from renegotiating. Key to the specification of the model is, therefore, insight into how winning bidders and procurers address the problem of cost overruns before and after the reform.

Consider first cost overruns *after* the reform. At that point of time, we observe a substantial share of renegotiated contracts (see Figure 4a), and a positive average value of renegotiation (see Figure 4b). We conclude that the procurers' typical response to a cost overrun was to renegotiate with the winning bidder, rather than to start up a new procurement process.²⁰

Consider next cost overruns *before* the reform, and distinguish between two potential scenarios. In scenario 1, when a cost overrun occurs, the procurer starts up a new procurement process. In this case, in the initial procurement process, bidders bid based on the costs of delivering the object, excluding expected cost overruns, as they will not incur them. Alternatively, in scenario 2, a new procurement process is not started, and the winning bidder is made liable for cost overruns and needs to incur them without renegotiation taking place (as renegotiation is not allowed). Therefore, in scenario 2, bidders bid based on expected costs, including expected cost overruns. Scenario 2, bidders bid based on expected costs, including expected cost overruns. Scenario 2 is the more plausible one when starting a new procurement process is relatively expensive, and when bidders are involved in many projects. Bidders are then able to compensate for any losses made in one procurement process on a cost overrun, by profits made in other projects.

Suppose scenario 1 were true. Then the final price would increase after the reform.

²⁰This follows the line of research that sees contracts as incomplete (e.g. Laffont and Tirole, 1990), making renegotiation plausible after the reform. Alternatively, when after the reform, the price changes, this is because the procurer and winning bidder settled ex-ante on a complete contract specifying who pays for what part of the cost overrun (cf. e.g. Aghion et al., 1994). This alternative modeling assumption is addressed in Appendix A.3, where we conclude that it does not align with our results.

This is because before the reform, the final price is based on costs excluding any cost overrun, while after the reform, the final price includes the cost overrun. Though after the reform, bids are lowered downwards to factor in any extra price obtained from renegotiating, this is compensated in the final price by the winning bidder obtaining the extra price with positive probability. Yet, an increase in the final price is not what we observe. We therefore dismiss scenario 1 and base our model on scenario 2, where winning bidders are liable for cost overruns before the reform.

4.2 Model

Having explained our modeling decisions, we now proceed with our stylized model (for a complete analysis, see Appendix A.1). A procurer (he) wants to buy an object with value v to him and faces n risk-neutral bidders, with $n \geq 2$. The winning bidder (she) is determined through a sealed-bid first-price auction, in which each bidder i simultaneously with all other bidders submits a bid, and where the bidder with the lowest bid delivers the object to the procurer. The individual bidder's net expected cost of delivering the object, denoted c_x , is independently and identically distributed over the range $[c_x^{\min}, c_x^{\max}]$, according to the cumulative distribution function $F_x(c_x)$, with corresponding density function $f_x(c_x)$ (where x refers to settings I, II or III). Bidders know their own net expected cost and how the net expected costs of other bidders are distributed. The procurer initially knows only the distribution of bidders' net expected costs. Depending on the setting, net expected cost takes into account the expected extra price obtained from renegotiating if a cost overrun occurs.

Before bidding, each bidder observes her cost c^A in the absence of a cost overrun. Before delivering the object, the winning bidder with probability π finds out that she can only deliver the object when incurring a cost overrun c^B (this could be, e.g., due to a change in the state of nature). After the winning bidder is determined but before the object is delivered, the procurer observes the cost c^A of the winning bidder, whether she faces a cost overrun, and when she does, the size c^B of the cost overrun.²¹

In setting I (a pre-reform baseline setting; see Figure 3 for the three settings considered), when the winning bidder faces a cost overrun, she cannot renegotiate and needs to deliver the object (which is only possible when incurring the cost overrun) at her winning bid $p_{\rm I}^A$. This is why the individual risk-neutral bidder assesses her net expected cost to equal $c_{\rm I} = c^A + \pi c^B$ at the time of placing her bid. Each bidder places a bid equal to what

²¹This may be because the procurer can infer information about the bidder's type from the bid that she placed (Shachat and Tan, 2015), because the preparations that the winning bidder makes for delivering the object reveal her type (e.g., Herweg and Schwarz, 2018; Chang, 2019), or because the fact that the winning bidder initiates renegotiation induces the procurer to scrutinize this bidder (Wang, 2000).

she expects the net expected cost of the second-lowest-cost bidder to be, conditional on her winning (see Appendix A.1). This is also the price at which the object is delivered, whether there is a cost overrun or not.

In setting II ("contracts awarded following pre-reform rules but renegotiated under eased rules"), both the procurer and the bidders expect that renegotiation is not possible. Bidders therefore submit the same bids as in setting I, and the same bidders continue to win. However, let the winning bidder have been determined and let a cost overrun occur. Then unexpectedly, winning bidders are allowed to renegotiate, and the winning bidder renegotiates an extra price. The final price now differs from setting I in that an expected extra price is added to the same winning bid as in setting I, explaining why we observe the final price to increase from setting I to setting II (see Section 3.4.3).

In setting III ("contracts awarded and renegotiated under eased rules"), renegotiation is also possible, but contrary to setting II, bidders and procurer correctly anticipate that a winning bidder can renegotiate when a cost overrun occurs. This is why the individual risk-neutral bidder assesses her net expected cost to equal $c_{\text{III}} = c^A + \pi c^B - \pi E$ at the time of placing her bid, where E is the extra price obtained from renegotiating, and πE is the expected extra price obtained from renegotiating. Each bidder again bids what she expects the second-lowest net expected cost to be, conditional on her having the lowest net expected cost. The fact that the net expected cost corrects for the expected extra price obtained from renegotiating in case of a cost overrun explains why we observe the winning bid to be lower after the reform (see Section 3.3). Intuitively, bidders bid more aggressively after the reform because they, in part, bid for the right to obtain an extra price in case of a cost overrun.

The final price in setting III consists of the winning bid, which incorporates the expected extra price obtained from renegotiating for the bidder with the *second-lowest* net expected cost, plus the expected extra price charged by the *winning bidder*. Compared to setting II, the expected extra price the winning bidder obtains from renegotiating, is offset by lower bids. Indeed, we observe the final price to be lower in setting III than in setting II (see Section 3.4.2). The intuition is then that overshooting of the final price took place in setting II, because bids in setting II could not yet factor in the expected extra price obtained from renegotiating. This overshooting is then corrected in setting III, which explains why we observe the final price to decrease from setting II to setting III. The observation of an equal final price across settings I and III (see Section 3.4.3) is then interpreted as the expected extra price obtained from renegotiating being competed away in the bidding process.

In Appendix A.1, both for settings II and III, we formally model the renegotiation process by means of a bargaining model, to explain why the average renegotiated value is larger in the latter setting. Also, we argue in Appendix A.3 that the predictions of our stylized model are robust when deviating in several ways from its simplifying assumptions, including scoring auctions, winner's curse, bidder moral hazard, procurer moral hazard, adverse selection, bidder risk aversion, and bidder entry.

While before the reform the most cost-efficient bidders win, after the reform costinefficient bidders can also win if they expect the extra price from renegotiation to be sufficiently high, causing them to have the lowest net expected cost. If cost-inefficient firms can win after the reform, one would at first sight expect a higher final price after the reform. However, we show in Appendix A.2 that inefficiency is still compatible with the unchanged final price from setting I to setting III we obtain in Section 3.4. Intuitively, suppose the most efficient bidder continues to win after the reform; then she may do this at a lower final price because she is at less of an advantage than before the reform when the second-most efficient firm has larger bargaining power. This same effect can depress prices after the reform even when cost-inefficient firms win. It is thus worth investigating separately how the reform affected the allocation of contracts, as we do in the next section. We additionally investigate in the next section how the reform affected quality.

5 Contract Allocation and Quality of Procurement Projects

The reform did not have any long-term effect on the final price. It could, however, affect other policy-relevant outcomes, such as contract allocation and the quality of procurement projects. The former possibility is of particular interest, as easing the renegotiation rules may lead to less efficient firms with a higher expected gain from renegotiation winning over more efficient ones.

In this section, we provide suggestive evidence indicating that after the reform, the allocation of contacts changed towards firms with more previous experience with renegotiation and towards less efficient firms. The effect, however, is small and, at best, only marginally statistically significant. More importantly, the shift in contract allocation does not seem to impact the quality of the procurement projects. The quality of construction contracts—measured as the inverse of the number of ex-post repairs required—continued to follow the same growth trend with no apparent slowdown after the reform.

Contract Allocation Firms in our dataset differ in the frequency of contract renegotiation. We use the variation to categorize firms according to their previous—prereform—experience with renegotiation. In particular, we define a firm as experienced with renegotiation if it renegotiated at least one of its contracts awarded before the reform, i.e., contracts awarded under baseline rules but renegotiated due to the retroactivity of the eased rules of renegotiation. We restrict the sample to firms awarded at least 3 contracts in the pre-reform period.²² Only 10% of all firms awarded at least 3 contracts renegotiated at least one contract before the reform.

In the difference-in-differences setting, we estimate whether, after the reform, the share of contracts awarded to firms experienced with renegotiation changed. The empirical specification resembles the regression equation (2) with the outcome variable being a dummy equal to 1 if the winner is an experienced firm, and 0 otherwise. The results presented in columns (1) and (2) of Table 4 show a positive effect between 2.2 and 3.6 percentage points, suggesting that after the reform, more contracts were awarded to firms more experienced with renegotiation. Only one of the two estimates, however, is marginally significant.²³

Our definition of firms experienced with renegotiation implies that these firms must have been competitive and won several contracts (just) before the reform. Consequently, they may have been more likely to win contracts in the post-reform period regardless of the reform. The effect is further amplified by the unknown control group of firms—those that won several contracts but never renegotiated them—that may no longer compete in the public procurement market after the reform. This tends to increase the estimated parameter. On the contrary, the estimated effect is possibly attenuated by new firms that are not classified as firms experienced with renegotiation because they started competing after the reform but could be successful in renegotiation.

²²In Appendix B, we replicate the same exercise with firms that were awarded at least 4 contracts.

 $^{^{23}}$ We use firms' experience purely descriptively and do not take a position on whether their marginal advantage in the post-reform period is due to firms' unobserved characteristics (such as characteristics of the legal department) or their learning from previous renegotiations.

	Exper. w/ Re	enegotiation $(= 1)$	Productivity		
Post \times Constr.	0.036* 0.022		-0.036	-0.047	
	(0.021)	(0.022)	(0.096)	(0.094)	
Post	0.009	0.017	0.018	0.009	
	(0.017)	(0.019)	(0.083)	(0.083)	
Industry FE	4-digit CPV	6-digit CPV	4-digit CPV	6-digit CPV	
# contracts awarded	At least 3	At least 3	NA	NA	
Ν	10,264	10,096	2,380	2,280	

Table 4: Change in Allocation of Contracts and Winning Firms' Productivity

Standard errors in parentheses

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

Notes: This table shows the results from four difference-in-differences specifications using the dummy variable experience with renegotiation as the outcome variable in the first two columns and the standardized (using industry-level pre-treatment productivity) log productivity in the last two columns. The parameters of the production function used to obtain the productivity were estimated using the Levinsohn-Petrin method on a sample of firms with more than 100 employees. Columns (1) and (2) and columns (3) and (4), respectively, differ in the granularity of industry-fixed effects. Post is a dummy variable that equals 1 for contracts procured after the reform (renegotiation is possible, and firms know this when submitting their bids) and 0 otherwise. Constr. is a dummy variable that equals 1 for construction contracts and 0 otherwise.

We complement the analysis of contract allocation changes with a study of the average productivity of winning firms. We first estimate the productivity, representing the total factor productivity, of Czech firms (for more details, please see the Online Appendix, where we also present robustness results) and run a difference-in-differences specification using a normalized measure of the productivity of the winning firm as the outcome variable. The results in the last two columns of Table 4 show small negative and statistically insignificant effects. We interpret the results as a lack of evidence for a substantial change in the productivity of winning firms in the construction sector.

Quality of the Project Delivered We further assess the effect of the reform on the quality of the procurement project delivered. The quality of a delivered project is inherently difficult to measure, as very little to no post-award data is usually collected and reported systematically. The previous literature often used indirect measures such as the delays in delivery and duration of the contract execution (see e.g. Coviello et al., 2018; Decarolis, 2014; Decarolis et al., 2020). Instead, we rely on a novel measure of quality proposed by Baranek (2020) and Baranek and Titl (2024). This measure is specific to the construction sector and based on the assumption that a higher-quality project needs fewer

Figure 8: Quality of Public Procurement Projects over Time



Notes: The figure shows the change in the quality of public procurement projects in the construction sector. The coefficients come from a regression identical to the regression (1), with the dependent variable being the negative value of the total lifetime cost of construction projects divided by the estimated cost. The effect is normalized to 0 in Q1 2016. In the dynamic difference-in-differences approach, we control for 4-level CPV codes industry fixed effects, selection methods, and public procurement procedures.

ex-post repairs. In our context, repairs of the initial procurement projects are awarded through new procurement contracts. This means that we can observe the repairs in our data. However, we do not observe repairs directly (e.g., using a unique identifier). Therefore, we use text algorithms to match repairs to the initial contracts. A manual validation shows that approximately 93% of the matches are correct.²⁴

To obtain the measure of quality, we calculate the expected sum of repairs of a project, divide it by the cost estimate of the original project, and multiply it by minus one. In Figure 8, we conduct the same time series analysis as in Figure 2a, but the outcome variable is the quality measure. The figure shows a steady increase in quality. There seem to be no changes in the quality of projects at the time of the changes in the renegotiation rules.

Despite some caveats, we conclude that we found suggestive evidence of a small shift in contract allocation but did not find evidence for economically and statistically significant effects on the quality of procurement projects.

²⁴In Appendix C, we provide details on the matching procedure, the calculation, and the validation.

6 Discussion and Concluding Remarks

Public procurement contracts are necessarily incomplete and frequently require renegotiation. The scope for renegotiation of public procurement contracts is often strictly regulated by public procurement laws. This paper studies the consequences of easing renegotiation rules on procurement outcomes. To do this, we use exogenous variation induced by a Czech reform that allowed winning firms to renegotiate.

We show that this reform substantially increased the share of renegotiated contracts, showing that winning firms find it profitable to renegotiate. However, at the same time, we observe that the winning bids decreased after the reform. Interestingly, the reform had no long-run effect on the final price of contracts. After a temporal increase in the final price caused by the retroactivity of the law, which enabled contracts awarded before the reform to be renegotiated, the final price returned to its pre-reform level. Nevertheless, when weighing observations by the estimated value of contracts, the long-term final price increases by 2.4% of the estimated value. This increase is driven by a few of the largest contracts, resulting in adverse fiscal consequences of easing renegotiation rules.

We next construct a stylized model that explains these observations. In this model, lower winning bids after the reform result from less precautionary bidding in anticipation of an expected extra price obtained from renegotiating in case of a cost overrun. This more aggressive bidding dissipates this expected extra price, explaining why bidders did not pay more after the reform.

From a welfare perspective, despite the final price not changing after the reform, inefficiency could still be obtained because firms could win due to having higher expected gains from renegotiation rather than being the most cost-efficient. As our model shows, the fact that the final price does not increase after the reform does not exclude that there is inefficiency. Indeed, we provide suggestive evidence that the allocation of contracts shifted towards firms more experienced with renegotiation. Moreover, the fact that the final price remains unchanged after the reform does not exclude that winning bidders provided lower quality after the reform. To examine the changes in quality, we employ a novel method that measures quality as minus the number of ex-post repairs required. We do not find any evidence of changes in the quality of delivered projects around the reform.

Welfare effects that we do not consider in the paper are, first, that the reform shifted the risk from the bidders to the procurer, as bidders could renegotiate compensation for the cost overrun, and the procurer faced a more risky final price. To the extent that the procurer is arguably less risk averse than the bidding firms (which may have owners that cannot diversify), the shift in risk because of the reform is welfare improving. Second, we only partially consider the transaction costs caused by allowing for renegotiation. Our model can accommodate the transaction costs of the bidders in the form of a cost of renegotiation incurred by them. The fact that bidders renegotiate often after the reform and that the final price does not go up suggests that this part of the transaction costs is not that large. This does not exclude, however, that the procurer faces large transaction costs of eased renegotiation rules, which we have not considered, and which could potentially decrease welfare.

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A Modeling Details, and Robustness Analysis

A.1 Modeling details and results

We consecutively determine the equilibrium bid $b_x(t)$ of the bidder of type t, the average bid $E[b_x]$, and the average winning bid in the auction $E[p_x^A]$.

$$b_x(c_x) = \frac{1}{[1 - F_x(c_x)]^{n-1}} \int_{c_x}^{c_x^{max}} y(n-1)[1 - F_x(y)]^{n-2} f_x(y) \,\mathrm{d}y \tag{1}$$

$$E[b_x] = \int_{c_x^{min}}^{c_x^{max}} \left[\frac{1}{[1 - F_x(z)]^{n-1}} \int_z^{c_x^{max}} y(n-1)[1 - F_x(y)]^{n-2} f_x(y) \, \mathrm{d}y \right] f_x(z) \, \mathrm{d}z \qquad (2)$$

$$E[p_x^A] = \int_{c_x^{min}}^{c_x^{max}} \left[\int_z^{c_x^{max}} y(n-1) [1 - F_x(y)]^{n-2} f_x(y) \, \mathrm{d}y \right] n f_x(z) \, \mathrm{d}z \tag{3}$$

The net expected cost c_x includes the expected cost of a cost overrun, and when renegotiation is possible and is anticipated, is corrected for the expected profit from renegotiation. As no renegotiation is expected in setting II, the average winning bid is the same as in setting I. However, the average final price, which is the sum of the winning bid and the expected extra price from renegotiation, is larger in setting II than in setting I, because of the possibility of renegotiation. Note that we make the difference here between expected extra price from renegotiation, and expected profit from renegotiation, because the latter includes both the expected extra price from renegotiation and the cost the winning bidder may face from initiating renegotiation.

We next show that the average winning bid is lower in setting III than in setting I. In each setting, average bids in the case of perfect information between bidders taken over all possible realizations of the net expected costs, are identical to average bids in the case of imperfect information. This is because with imperfect information, a risk neutral bidder, across all realizations of the net expected costs where she has the lowest net expected cost, bids the average net expected cost of the bidder with the secondlowest net expected cost. For this reason, without loss of generality, we can compare the average bids across the settings in the case of perfect information. Consider a particular realization of parameters, and let these parameters be such that in setting I it is the case that $c_{I,1} < c_{I,2} < ...$, but in setting III it is the case that $c_{III,j} < c_{III,i} < ...$ (where bidders may be ordered differently in setting III because their net expected costs are corrected for their expected profit from renegotiation). It follows that in setting I the winning bid is $c_{I,2}$, and in setting III it is $c_{III,i}$. If i = 2, then the winning bid is lower in setting III. If $i, j \neq 2$, then given that $c_{III,i} < c_{III,2}$ and $c_{III,2} \leq c_{I,2}$, it follows that the winning
bid is again lower in setting III. If j = 2, i = 1, then $c_{\text{III},2} < c_{\text{III},1} < c_{\text{I},1} < c_{\text{I},2}$, and the winning bid is once more lower in setting III. Finally, if j = 2 and $i \neq 1$, then $c_{\text{III},2} < c_{\text{III},i} < c_{\text{I},1} < c_{\text{I},2}$, and the winning bid is again lower in setting III.

In order to explain why the probability of renegotiation (see Figure 4a) and the value of renegotiation (see Figure 4b) is larger in setting III than in setting II, we adopt a standard bargaining model. In such a model, the extra price is a weighted sum of the maximum willingness-to-pay of the procurer for the winning bidder incurring the cost overrun, denoted v^B , and the minimum extra price at which the procurer is willing to incur the cost overrun (alternatively, the procurer's willingness-to-pay is determined by the ease of starting up a new procurement process, which is not changed by the reform). The weight α given to v^B now reflects the bidder's bargaining power, where $\alpha = 1$ means that the bidder possesses maximal bargaining power, as she can charge an extra price equal to the procurer's willingness to pay for the cost overrun; $\alpha = 0$ means that the bidder has minimal bargaining power, as the extra price then reflects the minimum price at which the winning bidder is willing to incur the cost overrun. In this way, in setting III, the extra price equals $\alpha v^B + (1 - \alpha)c^B$: given that the initial bid does not commit the bidder to incurring the cost overrun, at a minimum the winning bidder requires to be compensated for the cost overrun. We surmise that in setting II, the extra price equals instead $\alpha v^B + (1 - \alpha) \cdot 0$: as in this setting the procurer and bidders assumed that the winning bidder was liable for any cost overrun, a winning bidder with minimal bargaining power is not able to negotiate any extra price, as the winning bid was already supposed to compensate for any cost overrun. We assume that a winning bidder who renegotiates incurs a cost d and therefore renegotiates only if the extra price exceeds this cost.

Given this bargaining model, the probability of renegotiation, and the expected extra price, are both weakly higher in setting III than in setting II. First, note that in setting III, each bidder type conditional on winning is weakly more likely to renegotiate, and when renegotiating to renegotiate a higher extra price. The only way for the probability of renegotiation and the expected extra price to be lower in setting III is that bidders who do not renegotiate in setting III (and who therefore also do not renegotiate in setting II) become more likely to win in setting III than in settings I or II. But this is not possible, as it is the bidders who renegotiate who become more likely to win in setting III.

A.2 Example

In order to see that the fact that the average final price does not change from setting II to setting III does not exclude that there is inefficiency in setting III, consider the following simple example. There are only two bidders, 1 and 2. The type of bidder 1 is common knowledge among the two bidders, and bidder 2 knows her own type. However, bidder

1 does not know the type of bidder 2. In particular, while bidder 1 always has lower bargaining power than bidder 2, bidder 1 does not know bidder 2's bargaining power. It is common knowledge that with probability q_W , bidder 2 is of the winner type (state W) and has large bargaining power; with the complementary probability $1 - q_W$, bidder 2 is of the loser type (state L) and has lower bargaining power. While bidder 2 has higher bargaining power than bidder 1, only with probability q_W does this suffice for bidder 2 to have lower net expected cost than bidder 1; denoting by $\alpha_{2,s}$ bidder 2's bargaining power in state s, with s = W, L, it is the case that $\alpha_1 < \alpha_{2,L} < \alpha_{2,W}$. Bidder 1 now bids (a tiny bit below) the net expected cost of bidder 2, conditional on bidder 2 having the largest net expected cost, or $c_{I,2} - \pi(\alpha_{2,L}v^B + (1 - \alpha_{2,L})c^B - d)$. Bidder 2 in state W in turn bids a tiny bit below this bid. Bidder 2 wins in state W, and bidder 1 in state L. The average final price therefore approaches $c_{I,2} - \pi (\alpha_{2,L} v^B + (1 - \alpha_{2,L}) c^B - (1$ $d) + \pi [q_W(\alpha_{2,W}v^B + (1 - \alpha_{2,W})c^B) + (1 - q_W)(\alpha_1v^B + (1 - \alpha_1)c^B)].$ Inefficiency is thus obtained with positive probability (as long as q_W is positive); moreover, as long as d is not too large, a range of small but positive q_W exists such that the average final price is lower than or equal to the final price $c_{I,2}$ in setting I.

A.3 Robustness

We next reflect on the extent to which the theoretical explanations of the empirical findings presented in Section 4.2 depend on the assumptions we make.

Scoring auctions. In contrast to our stylized model, the auction may take the form of a scoring auction, where the procurer determines a vector of quantities that the winning bidder needs to deliver, and where a bid consists of a vector of prices at which a bidder wants to deliver these quantities (e.g., Herweg and Schwarz, 2018; Ryan, 2020), such as in the Most Economically Advantageous Tender. In this case, bidders can be considered as having a pseudo-type, calculated as a weighted sum of their cost of delivering the quantities (Asker and Cantillon, 2008). Our model can be reinterpreted in these terms.

Complete contracts. An alternative scenario for setting III to the scenarios presented in Section 4.1 is that the observed price changes are not the result of renegotiation, but of a complete contract that, at the time when the winning bidder is determined, fixes ex ante to what extent bidder or procurer pay for any possible cost overrun. If the procurer pays part of the cost overrun, contrary to scenario 2, it is not the case that the winning bidder carries all the risk. However, when a bidder considers her net expected cost, she then corrects this downwards for any extra price that was agreed in the contract for her incurring a cost overrun. We note that in setting II, the final price increases compared to setting I (section 3.4.1). Since bidders in setting II did not anticipate that renegotiation would become possible, this higher final price cannot be the result of a scenario where a complete contract specifies how one may deviate from the winning bid in case of a cost overrun. The only alternative is that genuine renegotiation is taking place in this setting. This strengthens the argument that renegotiation also cannot be excluded in setting III. In fact, we argue that this is the more plausible scenario. An argument for the complete-contract scenario is that setting III offers an advantage to the procurer compared to setting II: the fact that one can deviate from the winning bid is no longer unexpected, and because of this competition could force bidders into clear terms about what happens in case of a cost overrun, limiting the winning bidder's ability to still negotiate a higher price. Yet, quite to the contrary, our observations show that the value of renegotiation is higher in setting III than in setting II (see Figure 4b). For this reason, we focus on a model where in setting III the contract is incomplete, and where renegotiation takes place in case of a cost overrun.

Winning bid affecting bargaining position. We have assumed that the winning bid does not affect the bidder's bargaining position. In the institutional setting that we study, the maximal extra price the winning bidder can renegotiate is set at a fraction of the winning bid, namely 30 %. Our model implicitly assumes that this constraint is not binding because the value to the procurer of the bidder incurring the cost overrun does not exceed this fraction. However, if the constraint is binding, then bidders have an incentive to bid more to raise the maximum extra price that they can bargain for. But, as Figure 1b shows, few renegotiated contracts hit the constraint.

Bargaining in setting II. The winning bidder who faces a cost overrun may be seen as bargaining with the procurer for providing an extra service with value v^B to her, and cost c^B to the procurer (for procurement with renegotiation modeled in this way, see Fugger et al., 2019). In this sense, one could argue that the minimal extra price that the winning bidder can obtain from renegotiating is c^B , both in settings II and III.²⁵ If this is the case, provided the change from setting II to setting III does not change how bidders are ordered according to their net expected costs, the probability of renegotiation should be identical in settings II and III, as well as the expected extra price; the final price is then lower in setting III than in setting II. Yet, this is not what we observe. Intuitively, the extra price that the winning bidder can bargain for in setting II is lower, given that the winning bid was already supposed to cover for the possibility of a cost overrun. We reflect this in the assumptions by assuming a lower minimal extra price for the winning

²⁵This is equivalent to the generalized Nash bargaining solution, with zero disagreement payoff of the winning bidder, and disagreement payoff $v - v^B$ for the procurer. The generalized Nash bargaining solution can be justified as being the result of a sequential bargaining process, with the parameter α reflecting the bidder's discount factor, determining her patience during the bargaining. For a similar argument in the context of procurement and renegotiation, see Herweg and Schwarz (2018). Alternatively, following Waehrer (1995), α is interpreted as the probability that the bidder can make a take-it-or-leave-it offer, where with the complementary probability the procurement can make a take-it-or-leave-it offer.

bidder. The predictions remain the same when reflecting this intuition by assuming that each bidder type has lower bargaining power, or a higher cost of renegotiation, in setting II than in setting III.

Winner's curse. We have assumed that bidders' costs are privately and independently drawn from the same distribution (cf. Wang, 2000; Shachat and Tan, 2015; Herweg and Schwarz, 2018). While this is compatible with the event of a cost overrun rather than no cost overrun being common to all bidders, bidders' costs without a cost overrun may be correlated, and they may only receive noisy signals about their own costs. In such a model, bidders have an incentive to adjust their bids upward to avoid the winner's curse, where a bidder wins the auction because of having received a biased low-cost signal. Allowing bidders to renegotiate then makes the consequences of being subject to the winner's curse less severe, and makes bidders bid more aggressively (see Waehrer, 1995; Harstad and Rothkopf, 1995; Roelofs, 2002). But bids are then lowered to anticipate the profits from renegotiation, which is the same mechanism as we describe in the case of independently drawn costs.

Negative profits from renegotiation. We have focused on the positive profit that bidders may obtain from the possibility of a cost overrun. However, one could also conceive of cost overruns leading to more costs than benefits to the winning bidder due to adaptation costs. In this case, in the logic of our model, bidders would in setting III adjust their bids upwards rather than downwards (for a model along these lines, and empirical evidence in the context of Californian highway contracts, see Bajari et al., 2014). Yet, the fact that in setting IIIwinning bids are lower, suggests that positive profits from renegotiation dominate in our data.

Bidder moral hazard. Assume that bidders make an additional decision of how much to invest in cost reduction. Given our conclusion that our observations are explained by a pre-reform situation where bidders typically carried the risk of any cost overrun, and a post-reform situation where bidders could in part recuperate the cost overrun by renegotiating, the possibility of moral hazard arises, where post-reform bidders invest less in cost reduction (Cox et al., 1996). Yet, one would expect this to result in an increase in final price, which we do not observe. For this reason, we do not include such bidder moral hazard in our model.

Procurer moral hazard. When in setting I the winning bidder carries the risk of cost overruns, one may expect procurer moral hazard in the sense that the procurer lacks incentives to invest in finding out his needs. Failing to make such investments has the additional advantage to the procurer that bidders are made more homogeneous, leading to more competition and lower bids (Ganuza, 2007). Yet, because in setting III renegotiation makes the procurer carry part of a cost overrun, such moral hazard would

be reduced, which would have an increasing effect on bids. As we observe the opposite, we do no include such procurer moral hazard in the model.

Adverse selection. We have implicitly assumed that bidders all provide the same quality. If this is not the case, the sealed-bid first-price auction could lead to adverse selection of bidders who have the lowest cost because they also provide the lowest quality (Lopomo et al., 2023). Yet, in as far as such adverse selection occurs, it is not affected by the reform.

Bidder risk aversion and endogenous entry. We have assumed risk-neutral bidders. Yet, as is well-known from auction theory, risk-averse bidders in first-price auctions bid more aggressively when facing risk to insure themselves against the risk of losing (Krishna, 2009), meaning in procurement auctions that they bid lower than the expected net cost of the second-lowest-cost bidder. With heterogeneously risk-averse bidders, this extends to the more risk-averse bidders bidding more aggressively (Cox et al., 1982). If bidders face less risk in setting III, from this perspective it suggests that bidders bid higher in setting III than in setting I, which is the opposite of what we observe. At the same time, if bidders face a cost of participating in individual procurement processes, there is a self-selection effect against risk-averse bidders would then enter, who bid more aggressively, which aligns with what we observe. At the same time, ceteris paribus entry of additional bidders means a lower winning bid. Overall, allowing for risk-averse bidders.

B Additional Robustness Checks

	Settin	g I	Setting	; II	Setting	III
	Non-Constr.	Constr.	Non-Constr.	Constr.	Non-Constr.	Constr.
Number of Contracts	1882	3653	1455	2291	943	906
Estimated Cost (median, CZK mil.)	5	4.9	4.7	13	7	19.7
Selection Method (%)						
Lowest Price	76.9	86.7	79.8	87.5	92	90.1
MEAT	22.7	13.2	19.9	12.4	6.6	8.2
Procurer Type (%)						
National Agency	4.7	8.7	5.5	19.2	8.6	15.7
National Authority	7.7	5.6	10.5	4.8	15.1	7.9
Public Body	26.7	13.4	27.8	18.1	24.7	15.9
Regional Agency	5.0	7.0	6.4	8.9	3.9	6.3
Regional Authority	26.1	55.1	23.5	43.1	29.9	46.7
Other	29.9	10.2	26.2	5.9	17.8	7.4
Procedure Type (%)						
Approaching Bidders	24.3	17.7	28.0	21.0	1.8	3.2
Competitive Dialog	0.0	0.0	0.0	0.0	0.2	0.2
Negotiated	5.3	2.9	4.3	3.2	0.0	0.2
Negotiated W/O Publication	9.6	4.8	9.1	11.7	9.8	4.1
Negotiated W/ Publication	0.0	0.0	0.0	0.0	1.5	1.6
Open	57.4	66.2	57.4	57.2	84.2	78.1
Outright Award	2.0	0.3	1.0	0.6	0.0	0.0
Restricted	1.4	8.0	0.3	6.2	2.5	12.6
Size						
Above Threshold	29.5	4.9	25.6	7.4	63.0	27.9
Below Threshold	70.5	95.1	74.4	92.6	37.0	72.1
EU Funding $(\%)$	56.9	60.1	47.4	32.3	39.0	40.1
Czech firm awarded $(\%)$	94.7	99.2	95.0	99.5	95.5	99.0
Average Number of Bidders if Open Procedure	3.1	6.4	2.5	7.0	2.1	5.6

Table B1: Descriptive Statistics

Notes: This table shows descriptive statistics for contracts awarded to construction and non-construction sectors and in the three settings we study.

B.1 Winning Bid

In the main specification presented in Section 3.3, we show the eased renegotiation rules decreased the winning bids (see Table 1). Below, we present five robustness exercises confirming the results.

First, we use the logarithm of the winning bid as the outcome variable and extend regression (2) by controlling for the logarithm of the estimated value of the contract (for a similar approach, see, e.g., Lewis and Bajari, 2011). The estimates are reported in Table B2 and show that the reform led to a 4% decrease in the winning bid. Note that since the estimated values of contracts tend to be larger than the winning bid, a 4% decrease in the winning bid corresponds well to a decrease by 3.1% of the estimated value documented in Section 3.3. This exercise shows that our results are robust to different specifications of the outcome variable. For the remaining robustness exercises, we report both the winning bid ratio and the logarithm of the winning bid as the outcome variables in the same table.

The next two robustness exercises address potential concerns regarding the choice of the control groups. Second, we extend the control group by including all non-construction contracts as the control group. The findings are reported in Table B3 and show that using all non-construction contracts as the control group does not change the results. The point estimates barely change and remain around -3% of the estimated value and -4% of the winning bid.

Third, we replicate the exercise using Slovak construction contracts as the control group to provide additional evidence that our choice of the control group does not drive the results. It is possible that the construction sector follows a different business cycle than the other sectors and faces different productivity and cost shocks. While our pre-trend analysis and using different sets of industries in the control group alleviate these concerns, we still add an additional exercise to rule out construction-specific factors as drivers of our results. To do so, we collect data on winning bids from Slovakia, arguably the most similar economy with close historical ties to Czechia and significant language proximity that allows for more straightforward cross-border trade.

Slovakia, as an EU member state, also had to transpose the EU Directives 2014/24/EU, 2014/25/EU, and 2014/23/EU into its law.²⁶ Luckily, the Slovak implementation did not change renegotiation rules, and thus, we can use the Slovak construction contracts as a valid control group. The findings, presented in Table B4, seem to closely align with the findings from our main specifications. Compared to the Slovak contracts, the winning bids in the Czech construction contracts declined by between 2.8% and 3.6%. The results thus closely resemble the effect estimated in the main specifications.

Fourth, to ensure the results are robust to negative renegotiation, we replicate the main specifications on a restricted sample of contracts with non-negative renegotiation. Keeping only contracts with non-negative renegotiation does not change the results either. The point estimates imply a decrease of 3.3% of the estimated value and a 4% decrease in the winning bid (see Table B5).

Fifth, we omit selection methods and the type of public procurement procedures from

 $^{^{26}{\}rm It}$ has been done through a new Act on Public Procurement (the "Act") adopted on 18 November 2015 and effective as of 18 April 2016.

the regressions to ensure our estimates are not affected by so-called bad controls. In a specification without controlling for the type of procurement procedure and the type of evaluation criteria, the point estimates remain at -3% for the winning bid ratio and just below -4% for the logarithms of the winning bid (see Table B6).

Overall, the results across all specifications remain stable, and all difference-in-differences specifications provide robust evidence that easing renegotiation rules decreased the winning bid among construction contracts. The ATT effect is consistently around 3% of the estimated value and around 4% of the winning bids.

	Winning Bid (log)	Winning Bid (log)
Post Treatment \times Construction	-0.039***	-0.040***
	(0.007)	(0.008)
Post Treatment	0.008	0.013**
	(0.006)	(0.006)
Estimated Value (log)	1.011***	1.003***
	(0.002)	(0.002)
Industry FE	4-digit CPV	6-digit CPV
N	13,502	13,263

Table B2: Effect of Eased Renegotiation Rules on Winning Bid Specification with Log Winning Bid

Standard errors in parentheses

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

Notes: This table shows results from two difference-in-differences specifications, using the logarithm of the winning bid as the outcome variable. The two columns differ in the granularity of the industry fixed effects. Post Treatment is a dummy variable that equals to 1 for contracts procured after the reform and 0 otherwise. Construction is a dummy variable that equals 1 for construction contracts and 0 otherwise. We control for the type of procurement procedure, the evaluation criteria, and a set of industrial fixed effects.

	Winning Bid Ratio		Winning	Bid (log)
Post \times Constr.	-0.031***	-0.029***	-0.039***	-0.038***
	(0.005)	(0.005)	(0.006)	(0.006)
Post	0.007^{*}	0.008**	0.008*	0.010**
	(0.004)	(0.004)	(0.004)	(0.005)
Estim. Value (log)			1.009***	1.002***
			(0.002)	(0.002)
Industry FE	4-digit CPV	4-digit CPV	4-digit CPV	6-digit CPV
Ν	18,095	17,661	18,095	$17,\!661$

 Table B3: Effect of Eased Renegotiation Rules on Winning Bid

 All Contracts in the Control Group

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

Notes: The table shows the results from four difference-in-differences specifications. In columns 1 and 2, the outcome variable is the winning bid ratio; in columns 3 and 4, the outcome variable is the logarithm of the winning bid. Columns differ in the granularity of industry fixed effects. Post is a dummy variable that equals 1 for contracts procured after the reform (renegotiation is possible, and firms know this when submitting their bids) and 0 otherwise. Constr. is a dummy variable that equals 1 for contracts. The control group contains all non-construction contracts. In each specification, we control for the type of procurement procedure and evaluation criteria.

	Winning Bid Ratio	Winning Bid Ratio
Post Treatment \times Czechia	-0.036***	-0.028***
	(0.008)	(0.008)
Post Treatment	0.010	0.007
	(0.006)	(0.006)
Industry FE	4-digit CPV	6-digit CPV
Ν	$14,\!359$	14,297

 Table B4: Effect of Eased Renegotiation Rules on Winning Bid

 Slovak Construction Sector as a Control Group

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

Notes: This table shows results from two difference-in-differences specifications using the winning bid ratio as the outcome variable and Slovak construction contracts as the control group. Using Slovak construction contracts ensures that the estimated effect in the primary specification is not an artifact of the control group.

	Winning Bid Ratio		Winning	Bid (log)
Post \times Constr.	-0.033***	-0.033***	-0.041***	-0.042***
	(0.006)	(0.006)	(0.007)	(0.008)
Post	0.007	0.010**	0.008	0.013**
	(0.005)	(0.005)	(0.006)	(0.006)
Estim. Value (log)			1.011***	1.002***
			(0.002)	(0.002)
Industry FE	4-digit CPV	6-digit CPV	4-digit CPV	6-digit CPV
N	13,355	13,116	13,355	13,116

 Table B5: Effect of Eased Renegotiation Rules on Winning Bid

 Contracts with Non-negative Renegotiation

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

Notes: The table shows the results from four difference-in-differences specifications. In columns 1 and 2, the outcome variable is the winning bid ratio; in columns 3 and 4, the outcome variable is the logarithm of the winning bid. Columns differ in the granularity of industry fixed effects. Post is a dummy variable that equals 1 for contracts procured after the reform (renegotiation is possible, and firms know this when submitting their bids) and 0 otherwise. Constr. is a dummy variable that equals 1 for construction contracts and 0 otherwise. The regressions are run on a restricted sample of contracts with non-negative renegotiation. In each specification, we control for the type of procurement procedure and the type of evaluation criteria.

	Winning	Bid Ratio	Winning Bid (log)		
Post \times Constr.	-0.031***	-0.029***	-0.038***	-0.036***	
	(0.006)	(0.006)	(0.007)	(0.008)	
Post	0.007	0.008	0.004	0.009	
	(0.005)	(0.005)	(0.006)	(0.006)	
Estim. Value (log)			1.015***	1.006***	
			(0.002)	(0.002)	
Industry FE	4-digit CPV	6-digit CPV	4-digit CPV	6-digit CPV	
N	13,989	13,744	13,989	13,744	

 Table B6: Effect of Eased Renegotiation Rules on Winning Bid

 No Control Variables

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

Notes: The table shows the results from four difference-in-differences specifications. In columns 1 and 2, the outcome variable is the winning bid ratio; in columns 3 and 4, the outcome variable is the logarithm of the winning bid. Columns differ in the granularity of industry fixed effects. Post is a dummy variable that equals 1 for contracts procured after the reform (renegotiation is possible, and firms know this when submitting their bids) and 0 otherwise. Constr. is a dummy variable that equals 1 for construction contracts and 0 otherwise. We do not control for the type of procurement procedure and the type of evaluation criteria.

B.2 Final Price

Below, we discuss several alternative specifications used to estimate the effects of the reform on the final prices in the intermediate and post-reform periods. Section 3.4 in the paper presents the main specification to estimate these effects.

In this section of the appendix, we show that findings presented in Section 3.4.1, 3.4.2, and 3.4.3 are robust to various alternative specifications. First, in Table B7, we show the estimated effects from a specification that uses the logarithm of the final price as the outcome variable and controls for a logarithm of the estimated value of the contracts. The final price between settings I and II increased by 2.2% and 2.9%, depending on the granularity of the CPV codes used as fixed effects (see Columns 1 and 2). The final price between settings II and III decreased by 2.6% (see Columns 3 and 4). The final price between settings I and III did not significantly change.

For the sake of completeness, in each of the remaining four sets of robustness exercises, we report the results using the final price ratio and the logarithm of the final price as the outcome variables. In the latter specification, we control for a logarithm of the estimated value of the contract (as in the first set of robustness checks presented above).

Second, we report the results using an alternative control group. In this set of robustness exercises, the control group consists of all non-construction contracts. The effect on the final price between settings I and II is statistically significant, with the point estimates around 1.5% of the estimated value and above 2% of the final price (see Table B8). The effect on the final price between settings II and III is statistically insignificant, with point estimates of -0.7 and -1. Alternative specifications using the logarithm of the final price show a 1.1% and 1.6% decrease in the final price (see Table B12). The latter estimate is marginally statistically significant. The results of the estimation of the effect of the reform on the final price between settings I and III are presented in Table B16. The point estimates are even smaller than in the main specification and statistically insignificant.

Third, we present a set of robustness exercises with the sample restricted to contracts with non-negative renegotiation. The effect of the reform on the final price between settings I and II remains positive, statistically significant, and of a comparable magnitude as in the main specification (see Table B9). The effect of the reform on the final price between settings II and III results are presented in Table B13. The estimates are statistically significant, negative, and of similar magnitude to the main specification. Also, in this specification, the estimation of the effect of the reform on the final price between setting I and III yields a null effect in each specification (see Table B17).

Fourth, we present the results from the specifications without controlling for the type of procurement procedure and the type of evaluation criteria. The effect of the reform on the final price between settings I and II remains positive, statistically significant, and of a comparable magnitude as in the main specification (see Table B10. Using this specification, the effect on the final price between settings II and III also yields negative effects, of which two are marginally statistically significant (see Table B14). The findings in Table B18 show that even in a specification that does not control for selection methods and procedure of the procurement auction, the effect of the reform on the final price between settings I and III remains statistically indistinguishable from zero.

Fifth, we present the results using alternative split dates to differentiate contracts into settings I and II. Also these results align with the main findings. In Table B11, we present the estimation of the effect on the final price between settings I and II, and the effect actually intensifies as the split date shifts closer to a more recent date (2015 m7 instead of 2015 m4), reducing the share of contracts that were terminated before the implementation of the reform and thereby increasing the proportion of contracts in setting II that were actually exposed to the reform. Conversely, moving the split date back in time (2015 m1, instead of 2015 m4) reduces the observed effect. In Table B19, we present the findings from the estimation of the effect of the reform on the final price between periods II and III. The magnitude of the point estimates is similar to the findings in the main specification (a decrease between 1.5% and 2.8%). Two of the four point estimates are statistically significant. The effect of the reform on the final price between settings I and III, as in other specifications, remains negligible and statistically insignificant in all three specifications (see Table B19).

Overall, these robustness checks reassure us about our main findings. First, we find robust evidence that the unanticipated possibility of renegotiation led to an economically meaningful and statistically significant increase in the final price of procurement contracts. Second, we find evidence of a slight decrease in the final price between settings II and III. While point estimates in all specifications are negative, some are not statistically distinguishable from a null effect. Note that the comparison between settings II and III is based on fewer observations than the other between-setting comparisons of the final price. This may limit our statistical power to reject the null effect. Third, we provide robust evidence of a null effect on the final price between settings I and III.

	Final Price (log)		Final Pr	Final Price (log)		Final Price (log)	
	Setting I and II		Setting I	Setting II and III		Setting I and III	
Post \times Constr.	0.029***	0.022^{**}	-0.026^{**}	-0.026^{**}	-0.002	-0.005	
	(0.008)	(0.009)	(0.011)	(0.012)	(0.012)	(0.012)	
Post	0.012^{*}	0.013^{*}	0.001	0.008	0.020^{**}	0.026^{***}	
	(0.007)	(0.007)	(0.009)	(0.009)	(0.009)	(0.010)	
Estim. Value (log)	1.009^{***}	1.001^{***}	1.016^{***}	1.010^{***}	1.008^{***}	1.001^{***}	
	(0.002)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	
Industry FE (CPV)	4-digit	6-digit	4-digit	6-digit	4-digit	6-digit	
N	9,118	8,881	5,144	4,960	6,933	6,723	

Table B7: Final Price: Difference-in-Differences Results

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

Notes: This table summarizes the effect of the reform on the logarithm of the final price. For each comparison of the final price in two settings, we report results from two specifications that differ in the granularity of the CPV. The first two columns show the difference between settings I and II and thus capture the effect of the unanticipated possibility of renegotiation on the final price. The third and fourth columns show the difference between settings II and III. The last two columns show the differences between settings I and III. The effect thus represents the difference in final prices between the pre-reform and post-reform periods. We further control for the logarithm of the estimated value of the contract.

	Final Price Ratio		Final Pr	rice (log)
Set. II \times Constr.	0.018***	0.014^{**}	0.025^{***}	0.020***
	(0.006)	(0.006)	(0.007)	(0.007)
Setting II	0.014***	0.014***	0.016***	0.016***
	(0.004)	(0.004)	(0.005)	(0.005)
Estim. Value (log)			1.005***	0.999***
			(0.002)	(0.002)
Industry FE	4-digit CPV	6-digit CPV	4-digit CPV	6-digit CPV
N	12,041	11,635	12,041	11,635

Table B8: Final Price: Change from Setting I to IIAll Contracts in the Control Group

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

Notes: The table shows the results from four difference-in-differences specifications. In columns 1 and 2, the outcome variable is the final price ratio; in columns 3 and 4, the outcome variable is the logarithm of the final price. Columns differ in the granularity of industry fixed effects. Setting II is a dummy variable that takes the value of 1 for contracts from setting II and 0 for contracts from setting I (renegotiation is impossible, and firms know this when submitting their bids). Constr. is a dummy variable that equals 1 for contracts and 0 otherwise. The control group contains all non-construction contracts. In each specification, we control for the type of procurement procedure and the type of evaluation criteria.

	Final Pr	ice Ratio	Final Price (log)		
Set. II \times Constr.	0.022***	0.017^{**}	0.029***	0.023***	
	(0.007)	(0.007)	(0.008)	(0.009)	
Setting II	0.010*	0.011^{*}	0.012^{*}	0.013^{*}	
	(0.006)	(0.006)	(0.007)	(0.007)	
Estim. Value (log)			1.009***	1.001***	
			(0.002)	(0.003)	
Industry FE	4-digit CPV	6-digit CPV	4-digit CPV	6-digit CPV	
N	9,091	8,854	9,091	8,854	

 Table B9: Final Price: Change from Settings I to II

 Contracts with Non-negative Renegotiation

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

Notes: The table shows the results from four difference-in-differences specifications. In columns 1 and 2, the outcome variable is the final price ratio; in columns 3 and 4, the outcome variable is the logarithm of the final price. Columns differ in the granularity of industry fixed effects. Setting II is a dummy variable that takes the value of 1 for contracts from setting II and 0 for contracts from setting I (renegotiation is impossible and firms know this when submitting their bids). Constr. is a dummy variable that equals to 1 for contracts and 0 otherwise. The regressions are run on a restricted sample of contracts with non-negative renegotiation. In each specification, we control for the type of procurement procedure and the type of evaluation criteria.

	Final Pri	ice Ratio	Final Price (log)		
Set. II \times Constr.	0.028***	0.022***	0.037***	0.029***	
	(0.007)	(0.007)	(0.009)	(0.009)	
Setting II	0.007	0.008	0.009	0.010	
	(0.006)	(0.006)	(0.007)	(0.007)	
Estim. Value (log)			1.015***	1.006***	
			(0.002)	(0.002)	
Industry FE	4-digit CPV	6-digit CPV	4-digit CPV	6-digit CPV	
Ν	9,204	8,969	9,204	8,969	

Table B10: Final Price: Change from Setting I to IINo Control Variables

Standard errors in parentheses

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

Notes: The table shows the results from four difference-in-differences specifications. In columns 1 and 2, the outcome variable is the final price ratio; in columns 3 and 4, the outcome variable is the logarithm of the final price. Columns differ in the granularity of industry fixed effects. Setting II is a dummy variable that takes the value of 1 for contracts from setting II and 0 for contracts from setting I (renegotiation is impossible and firms know this when submitting their bids). Constr. is a dummy variable that equals 1 for contracts and 0 otherwise. We do not control for the type of procurement procedure and the type of evaluation criteria.

	Price	Ratio	Price	Ratio	Price	Ratio
Set. II \times Constr.	0.016**	0.010	0.025***	0.021***	0.020***	0.015^{**}
	(0.007)	(0.007)	(0.007)	(0.008)	(0.007)	(0.007)
Setting II	0.012^{**}	0.014^{**}	0.009	0.008	0.009	0.008
	(0.005)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)
Industry FE (CPV)	4-digit	6-digit	4-digit	6-digit	4-digit	6-digit
Ν	$9,\!688$	$9,\!450$	$9,\!688$	$9,\!450$	$9,\!688$	$9,\!450$
Cutoff Date	2015	5 m1	2015	$5 \mathrm{m7}$	2015 m	n1/m7

Table B11: Final Price: Change from Setting I to IIAlternative Cutoff Dates for Setting Classification

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

Notes: This table replicates the main specifications using alternative cutoff dates for classification contracts into settings I and II. Columns 1 and 2 use 2015 m1, columns 3 and 4 use 2015 m7, and finally, columns 5 and 6 use 2015 m1 for construction contracts and 2015 m7 for non-construction as cutoff dates for classifying contracts into settings I and II. The last definition of cutoff dates is motivated by potentially varying lengths of contracts in construction and non-construction sectors. The outcome variable in all regressions is the final price ratio.

	Final Price Ratio		Final Pr	ice (log)
Set. III \times Constr.	-0.010	-0.007	-0.016*	-0.011
	(0.009)	(0.009)	(0.010)	(0.010)
Setting III	-0.003	-0.001	-0.006	-0.003
	(0.006)	(0.006)	(0.006)	(0.007)
Estim. Value (log)			1.011***	1.006***
			(0.002)	(0.003)
Industry FE	4-digit CPV	6-digit CPV	4-digit CPV	6-digit CPV
Ν	7,209	6,846	7,209	6,846

Table B12: Final Price: Change from Setting II to IIIAll Contracts in the Control Group

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

Notes: The table shows the results from four difference-in-differences specifications. In columns 1 and 2, the outcome variable is the final price ratio; in columns 3 and 4, the outcome variable is the logarithm of the final price. Columns differ in the granularity of industry fixed effects. Setting III is a dummy variable that takes the value of 1 for contracts from setting III (renegotiation is possible, and firms know this when submitting their bids) and 0 for contracts from setting II (renegotiation is possible, and firms didn't know this when submitting their bids). Constr. is a dummy variable that equals 1 for construction contracts and 0 otherwise. The control group contains all non-construction contracts. In each specification, we control for the type of procurement procedure and the type of evaluation criteria.

	Final Pr	ice Ratio	Final Price (log)	
Set. III \times Constr.	-0.019*	-0.019*	-0.026**	-0.025**
	(0.010)	(0.010)	(0.011)	(0.012)
Setting III	0.005	0.009	0.000	0.007
	(0.007)	(0.008)	(0.009)	(0.009)
Estim. Value (log)			1.016***	1.010***
			(0.003)	(0.003)
Industry FE	4-digit CPV	6-digit CPV	4-digit CPV	6-digit CPV
N	5,075	4,890	5,075	4,890

Table B13: Final Price: Change from Setting II to IIIContracts with Non-negative Renegotiation

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

Notes: The table shows the results from four difference-in-differences specifications. In columns 1 and 2, the outcome variable is the final price ratio; in columns 3 and 4, the outcome variable is the logarithm of the final price. Columns differ in the granularity of industry fixed effects. Setting III is a dummy variable that takes the value of 1 for contracts from setting III (renegotiation is possible, and firms know this when submitting their bids) and 0 for contracts from setting II (renegotiation is possible, and firms didn't know this when submitting their bids). Constr. is a dummy variable that equals 1 for construction contracts and 0 otherwise. The regressions are run on a restricted sample of contracts with non-negative renegotiation. In each specification, we control for the type of procurement procedure and the type of evaluation criteria.

	Final Pri	ice Ratio	Final Price (log)	
Set III × Constr	-0.017*	-0.012	-0.023**	-0.018
	(0.009)	(0.012)	(0.011)	(0.012)
	()		()	()
Setting III	0.010	0.011	-0.000	0.005
	(0.007)	(0.008)	(0.008)	(0.009)
Estim. Value (log)			1.021***	1.014***
			(0.003)	(0.003)
Industry FE	4-digit CPV	6-digit CPV	4-digit CPV	6-digit CPV
N	5,511	5,320	5,511	5,320

Table B14: Final Price: Change from Setting II to III No Control Variables

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

Notes: The table shows the results from four difference-in-differences specifications. In columns 1 and 2, the outcome variable is the final price ratio; in columns 3 and 4, the outcome variable is the logarithm of the final price. Columns differ in the granularity of industry fixed effects. Setting III is a dummy variable that takes the value of 1 for contracts from setting III (renegotiation is possible and firms know this when submitting their bids) and 0 for contracts from setting II (renegotiation is possible and firms didn't know this when submitting their bids). Constr. is a dummy variable that equals to 1 for construction contracts and 0 otherwise. We do not control for the type of procurement procedure and the type of evaluation criteria.

	Price	Ratio	Price	Ratio	Price	Ratio
Set. III \times Constr.	-0.017*	-0.014	-0.026**	-0.028***	-0.015	-0.016
	(0.009)	(0.010)	(0.010)	(0.011)	(0.010)	(0.010)
Setting III	0.007	0.010	0.005	0.009	0.007	0.012
	(0.007)	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)
Industry FE (CPV)	4-digit	6-digit	4-digit	6-digit	4-digit	6-digit
Ν	6,339	$6,\!150$	4,391	4,207	5,738	$5,\!550$
Cutoff Date	2015	5 m1	201	5 m7	2015 r	m1/m7

Table B15: Final Price: Change from Setting II to IIIAlternative Cutoff Dates for Setting Classification

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

Notes: This table replicates the main specifications using alternative cutoff dates for classification contracts into settings II and III. Columns 1 and 2 use 2015 m1, columns 3 and 4 use 2015 m7, and finally, columns 5 and 6 use 2015 m1 for construction contracts and 2015 m7 for non-construction as cutoff dates for classifying contracts into setting II. The last definition of cutoff dates is motivated by potentially varying lengths of contracts in construction and non-construction sectors. The outcome variable in all regressions is the final price ratio.

	Final Pri	ice Ratio	Final Price (log)	
Set. III \times Constr.	0.002	-0.001	0.002	0.001
	(0.009)	(0.009)	(0.010)	(0.010)
Setting III	0.015***	0.018***	0.016**	0.019***
	(0.006)	(0.006)	(0.007)	(0.007)
Estim. Value (log)			1.006***	1.001***
			(0.002)	(0.002)
Industry FE	4-digit CPV	6-digit CPV	4-digit CPV	6-digit CPV
Ν	9,226	8,848	9,226	8,848

Table B16: Final Price: Change from Setting I to IIIAll Contracts in the Control Group

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

Notes: The table shows the results from four difference-in-differences specifications. In columns 1 and 2, the outcome variable is the final price ratio; in columns 3 and 4, the outcome variable is the logarithm of the final price. Columns differ in the granularity of industry fixed effects. Setting III is a dummy variable that takes the value of 1 for contracts from setting III (renegotiation is possible and firms know this when submitting their bids) and 0 for contracts from setting I (renegotiation is impossible and firms know this when submitting their bids). Constr. is a dummy variable that equals to 1 for construction contracts and 0 otherwise. The control group contains all non-construction contracts. In each specification, we control for the type of procurement procedure and the type of evaluation criteria.

	Final Pr	ice Ratio	Final Price (log)	
Set. III \times Constr.	-0.001	-0.006	-0.001	-0.004
	(0.010)	(0.011)	(0.012)	(0.013)
Setting III	0.019**	0.023***	0.020**	0.025**
	(0.007)	(0.008)	(0.009)	(0.010)
Estim. Value (log)			1.008***	1.002***
			(0.003)	(0.003)
Industry FE	4-digit CPV	6-digit CPV	4-digit CPV	6-digit CPV
N	6,873	6,665	6,873	6,665

Table B17: Final Price: Change from Settings I to IIIContracts with Non-negative Renegotiation

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

Notes: The table shows the results from four difference-in-differences specifications. In columns 1 and 2, the outcome variable is the final price ratio; in columns 3 and 4, the outcome variable is the logarithm of the final price. Columns differ in the granularity of industry fixed effects. Setting III is a dummy variable that takes the value of 1 for contracts from setting III (renegotiation is possible and firms know this when submitting their bids) and 0 for contracts from setting I (renegotiation is impossible and firms know this when submitting their bids). Constr. is a dummy variable that equals 1 for construction contracts and 0 otherwise. The regressions are run on a restricted sample of contracts with non-negative renegotiation. In each specification, we control for the type of procurement procedure and the type of evaluation criteria.

	Final Pri	ice Ratio	Final Price (log)	
Set. III \times Constr.	0.007	0.006	0.009	0.009
	(0.009)	(0.010)	(0.011)	(0.012)
Setting III	0.016**	0.018**	0.013	0.017^{*}
	(0.007)	(0.008)	(0.008)	(0.009)
Estim. Value (log)			1.012***	1.006***
			(0.002)	(0.003)
Industry FE	4-digit CPV	6-digit CPV	4-digit CPV	6-digit CPV
Ν	7,310	7,098	7,310	7,098

Table B18: Final Price: Change from Settings I to IIINo Control Variables

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

Notes: The table shows the results from four difference-in-differences specifications. In columns 1 and 2, the outcome variable is the final price ratio; in columns 3 and 4, the outcome variable is the logarithm of the final price. Columns differ in the granularity of industry fixed effects. Setting III is a dummy variable that takes the value of 1 for contracts from setting III (renegotiation is possible and firms know this when submitting their bids) and 0 for contracts from setting I (renegotiation is impossible and firms know this when submitting their bids). Constr. is a dummy variable that equals to 1 for construction contracts and 0 otherwise. We do not control for the type of procurement procedure and the type of evaluation criteria.

	Price Ratio		Price Ratio		Price Ratio	
Set. III \times Constr.	-0.006	-0.014	-0.005	-0.010	-0.002	-0.007
	(0.010)	(0.011)	(0.009)	(0.010)	(0.010)	(0.010)
Setting III	0.026^{***} (0.008)	0.032^{***} (0.008)	0.021^{***} (0.007)	0.026^{***} (0.008)	0.021^{***} (0.007)	0.025^{***} (0.008)
Industry FE (CPV)	4-digit	6-digit	4-digit	6-digit	4-digit	6-digit
Ν	6,314	$6,\!108$	8,258	$8,\!053$	$6,\!910$	6,705
Cutoff Date	2015	$5 \mathrm{m1}$	$2015~\mathrm{m7}$		$2015~\mathrm{m1/m7}$	

 Table B19: Final Price: Change from Setting I to III

 Alternative Cutoff Dates for Setting Classification

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

Notes: This table replicates the main specifications using alternative cutoff dates for classification contracts into settings I and III. Columns 1 and 2 use 2015 m1, columns 3 and 4 use 2015 m7, and finally, columns 5 and 6 use 2015 m1 for construction contracts and 2015 m7 for non-construction as cutoff dates for classifying contracts into setting I. The last definition of cutoff dates is motivated by potentially varying lengths of contracts in construction and non-construction sectors. The outcome variable in all regressions is the final price ratio.





Notes: The figure shows the share of renegotiated contracts in each decile by the estimated value of contracts and by setting. It combines both construction and non-construction contracts.

C Quality of Procurement Projects

Since the public procurement projects are not marked as repairs, we need to identify them based on our assessment. This is impossible to do manually, as the number of potential combinations of contracts in our dataset exceeds hundreds of millions. For example, possible combinations of all contracts for the biggest construction procurer in the country—the Road and Motorway Directorate—accounts for almost 10 million combinations. We automate the process following Baranek (2020) and Baranek and Titl (2024).

To make the problem solvable, the following restrictions are imposed. To consider a procurement project a repair, the same buyer has to procure the original project and the repair project. The original project has to awarded before the repair tender. For a procurement project to be considered a repair, one of the keywords such as *repair*, *maintenance*, *revision*, *follow-up*, *reconstruction*, ... has to be in the project description. We use the same library of 50 keywords as in Baranek and Titl (2024).

Next, we need to identify the follow up projects. We use the similarity of the text descriptions of the original project and the possible repair project to do so. The similarity is obtained from the tri-gram algorithm (for details, see Manning and Schutze, 1999). This algorithm converts a high-dimensional object—in our case the text description of a procurement project—into a vector of all consecutive triplets of characters within the text. The similarity of the texts is then determined by calculating the proportion of overlapping triplets.

It appears important to adjust for the similarity with all other procurement projects. Following Baranek and Titl (2024), we calculate the final probability $(p_{i,j})$ that procurement project j is a repair for a project i:

$$p_{i,j} = \frac{s_{i,j}}{\sum_{i' \in I} s_{i',j}}$$
(1)

where $s_{i,j}$ is the similarity of *i* and *j* (calculated using the tri-gram algorithm) and *I* is the set of all projects.

The verification of this algorithm has been done in Baranek and Titl (2024). A manual check of all public procurement contracts of a randomly chosen middle-size procuring authority – the city of Most – shows that the accuracy is about 93%. The sample of public procurement contracts used in Baranek and Titl (2024) covers the period 2006–2018. I.e., it covers a longer period and the dataset used in this paper overlaps with the one from Baranek and Titl (2024). This suggests that the accuracy would be very similar on the period covered in this paper (2014–2017). For more details on this verification, see also Baranek (2020).

O.A Online Appendix

O.A1 Frequency and Value of Renegotiation by Settings

	Re	enegotiati	ion	Renegotiation				
	Extensive Margin			Inte	Intensive Margin			
Setting I	0.007**	0.003	-0.014**	0.001	0.001	-0.001		
	(0.003)	(0.007)	(0.006)	(0.000)	(0.001)	(0.001)		
Setting II	0.034***	0.006	0.033***	0.003***	0.001	0.002***		
	(0.003)	(0.006)	(0.006)	(0.000)	(0.001)	(0.001)		
Setting III	0.132***	0.025***	0.156***	0.008***	0.001^{*}	0.009***		
	(0.003)	(0.006)	(0.006)	(0.000)	(0.001)	(0.001)		
Setting I x Constr.		-0.011			-0.001			
		(0.010)			(0.001)			
Setting II x Constr.		0.044***			0.002			
		(0.010)			(0.001)			
Setting III x Constr.		0.229***			0.014***			
		(0.009)			(0.001)			
Construction		0.017***			0.001^{*}			
		(0.006)			(0.001)			
Set of FEs	No	No	Yes	No	No	Yes		
Ν	$19,\!154$	14,058	$13,\!572$	19,154	14,058	$13,\!572$		

Table O.A.1: First Stage: Frequency and Value of Renegotiation

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

Notes: This table shows results from a series of regressions using extensive and intensive margins of renegotiation as the outcome variable and dummies for each setting as the explanatory variables. It shows that setting III is more prone to renegotiation measured both by the extensive margin (dummy that a renegotiation happens) and the intensive margin (value of renegotiation as a share of estimated value). In contrast, in setting I, renegotiation barely occurs.

O.A2 Contracts Weighted by Expected Value of Contracts

To better understand the fiscal consequences induced by the reform for the procurement authorities, we re-estimate the main regressions weighting the observations by the contracts' estimated value. The effect of the reform on the final price between settings II and III from the weighted regression is reported in the main text Section 3.4 Table 3. Below, we report the results from the weighted regression for other main specifications.

First, we re-estimate the weighted version of the regressions reported in Table 1 in the main text. The findings are substantively similar with somewhat larger points estimates. The findings are shown in Table O.A.2.

	Winning	Bid Ratio	Winning Bid (log)	
Post \times Constr.	-0.044***	-0.039***	-0.043***	-0.039***
	(0.007)	(0.008)	(0.009)	(0.009)
Post	0.024***	0.030***	0.023***	0.037***
	(0.007)	(0.007)	(0.008)	(0.009)
Estim. Value (log)			1.001***	0.979***
			(0.001)	(0.001)
Industry FE	4-digit CPV	6-digit CPV	4-digit CPV	6-digit CPV
N	13,502	13,263	$13,\!502$	$13,\!263$

 Table O.A.2: Effect of Eased Renegotiation Rules on Winning Bid

 Contracts Weighted by Expected Value of Contracts

Standard errors in parentheses

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

Notes: The table shows the results from four difference-in-differences specifications. In columns 1 and 2, the outcome variable is the winning bid ratio; in columns 3 and 4, the outcome variable is the logarithm of the winning bid. Observations are weighted by the expected value of contracts. Columns differ in the granularity of industry-fixed effects. Setting III is a dummy variable that takes the value of 1 for contracts from setting III (renegotiation is possible, and firms know this when submitting their bids) and 0 for contracts from setting I (renegotiation is impossible, and firms know this when submitting their bids). Constr. is a dummy variable that equals to 1 for construction contracts and 0 otherwise. In each specification, we control for the type of procurement procedure and the type of evaluation criteria.

Second, we report the results from the weighted regressions estimating the effect of the eased renegotiation on the final price between settings I and II (see Table O.A.3), and on the final price between settings II and III (see Table O.A.4). The corresponding non-weighted estimates are presented in Table 2. The effect on the final price between settings I and II remain positive and significant but again the point estimates are larger. The effect on the final price between settings II and III remain negative, insignificant, and as before the point estimates are larger in absolute values.

	Final Pri	ice Ratio	Final Price (log)	
Set. II \times Constr.	0.053***	0.059***	0.065***	0.076***
	(0.010)	(0.010)	(0.012)	(0.012)
Setting II	0.010	-0.000	0.007	-0.010
	(0.009)	(0.009)	(0.011)	(0.011)
Estim. Value (log)			1.001***	0.977***
			(0.002)	(0.002)
Industry FE	4-digit CPV	6-digit CPV	4-digit CPV	6-digit CPV
Ν	$9,\!118$	8,881	$9,\!118$	8,881

Table O.A.3: Final Price: Change From Setting I to II Contracts Weighted by Expected Value of Contracts

Standard errors in parentheses

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

Notes: The table shows the results from four difference-in-differences specifications. In columns 1 and 2, the outcome variable is the final price ratio; in columns 3 and 4, the outcome variable is the logarithm of the final price. Observations are weighted by the expected value of contracts. Columns differ in the granularity of industry-fixed effects. Setting II is a dummy variable that takes the value of 1 for contracts from setting II (renegotiation is possible, and firms didn't know this when submitting their bids) and 0 for contracts from setting I (renegotiation is impossible, and firms know this when submitting their bids). Constr. is a dummy variable that equals 1 for contracts and 0 otherwise. In each specification, we control for the type of procurement procedure and the type of evaluation criteria.

	Final Price Ratio		Final Price (log)	
Set.III \times Constr.	-0.020	-0.016	-0.015	-0.014
	(0.013)	(0.013)	(0.014)	(0.015)
Setting III	0.019*	0.021*	0.011	0.030**
	(0.011)	(0.012)	(0.013)	(0.014)
Estim. Value (log)			1.014***	0.991***
			(0.002)	(0.002)
Industry FE	4-digit CPV	6-digit CPV	4-digit CPV	6-digit CPV
Ν	$5,\!144$	4,960	$5,\!144$	4,960

Table O.A.4: Final Price: Change From Setting II to III Contracts Weighted by Expected Value of Contracts

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

Notes: The table shows the results from four difference-in-differences specifications. In columns 1 and 2, the outcome variable is the final price ratio; in columns 3 and 4, the outcome variable is the logarithm of the final price. Observations are weighted by the expected value of contracts. Columns differ in the granularity of industry-fixed effects. Setting III is a dummy variable that takes the value of 1 for contracts from setting III (renegotiation is possible and firms know this when submitting their bids) and 0 for contracts from setting II (renegotiation is possible and firms didn't know this when submitting their bids). Constr. is a dummy variable that equals to 1 for contracts and 0 otherwise. In each specification, we control for the type of procurement procedure and the type of evaluation criteria.

Finally, we re-estimates the effect of the reform on the final price between periods I and III using an alternative specification with the logarithm of the final price as the outcome variable (in the main text, we report the findings from the specification with the ratio of the final price over the estimated value as the outcome variable; see Table 3). The results from this alternative specification are reported in Table O.A.5. The weighted effect is between a 3 and 4% increase in the final price on a full sample. Reducing the sample to contracts below CZK 1 billion makes the effect negligible and statistically insignificant. It confirms that the largest contracts drive the increase in the weighted final price and the increase in fiscal costs for procurers.

	Final Price (log)	Final Price (log)	Final Price (log)	Final Price (log)
Set. III \times Constr.	0.029**	0.041***	-0.000	0.004
	(0.012)	(0.014)	(0.013)	(0.014)
Setting III	0.039***	0.052***	0.033***	0.047***
	(0.011)	(0.012)	(0.011)	(0.012)
Estim. Value (log)	1.015^{***}	0.995^{***}	1.023***	1.005^{***}
	(0.002)	(0.002)	(0.002)	(0.002)
Industry FE	4-digit CPV	6-digit CPV	4-digit CPV	6-digit CPV
Ν	6,933	6,723	6,912	6,703
Sample	Weighted	Weighted	Weighted	Weighted
Contracts	All	All	Below CZK 1 bil.	Below CZK 1 bil.

Table O.A.5: Final Price: Change From Setting I to IIIContracts Weighted by Expected Value of Contracts

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

Notes: The table shows the results from four difference-in-differences specifications using the logarithm of the final price as the outcome variable. Observations are weighted by the expected value of contracts. Columns 1 and 2 present results from the full sample of all contracts. Columns 3 and 4 present results from a restricted sample of contracts with an expected value below CZK 1 billion. Columns differ in the granularity of industry-fixed effects. Setting III is a dummy variable that takes the value of 1 for contracts from setting III (renegotiation is possible and firms know this when submitting their bids) and 0 for contracts from setting I (renegotiation is impossible and firms know this when submitting their bids). Constr. is a dummy variable that equals to 1 for construction contracts and 0 otherwise. In each specification, we control for the type of procurement procedure and the type of evaluation criteria.

O.A3 The Estimation of Productivity

We complement the analysis of contract allocation with a study of the productivity of awarded firms. The starting point is a log version of a production function represented as a sum of a log of total factor productivity, log of labor, and log of capital. We first estimate the sector-specific coefficient of labor and capital share in the production function and then retrieve each firm's productivity log as a residual from the production function. We implement it by running the *prodest* command in STATA using the Wooldridge method and the Levinsohn-Petrin method (Wooldridge, 2009; Levinsohn and Petrin, 2003). We restrict the sample to mid and large-sized firms to ensure comparability and similarity to firms participating in procurement competitions. This approach allows us to estimate firm-year-specific productivity on a subset of firms that are comparable to each other. Depending on the specifications, we exclude self-employed individuals and small-sized firms with less than 20, 50, and 100 employees. To estimate firms' productivity, we utilize Czech firm data from the Orbis database and deflator data from Horvát and Webb (2020). In the process of estimating the productivity of firms, we followed Gal (2013). We estimate industry-specific production functions at the NACE 2 level using the control function approach. Due to data availability considerations, we combine several NACE 2 industrial levels when necessary. When estimating the productivity function, we use the adjusted operating revenue turnover, taking into account the production deflator, as the outcome variable. Unfortunately, the number of employees of the Czech firms in the Orbis data is reported as a categorical variable. Instead, we use the cost of employees adjusted by the consumer price index deflator as the labor input. For the capital input, we use tangible fixed assets adjusted by gross fixed capital formation deflators, calculated using the perpetual inventory method. To approximate intermediate inputs, we rely on a proxy measure. For a comprehensive examination of the coverage and representativeness of the Orbis data, please refer to Bajgar et al. (2020).

Having estimated the parameters of production functions, we compute the predicted log of total factor productivity at the firm-year level and normalize it by using the mean and standard deviation of firms in the corresponding industry during the pre-treatment period.

O.A4 The Effect on Winning Firms' Productivity

We use the same main specifications as in the main text, but now the productivity of the winning firms is the dependent variable. The findings are presented in Tables O.A.6 (for the Wooldridge method) and O.A.7 (for the Levinsohn-Petrin method). In columns 1 and 2, the production function parameters were estimated using a sample of firms with more than 100 employees. In columns 3 and 4, the parameters of the production function were estimated on a sample of firms with more than 50 employees.

The effect on the productivity of the winning firm tends to be negative but negligible and statistically insignificant. In Table O.A.6, all point estimates are negative, but none is statistically significant. Similarly, the estimates of the effect of the reform on the average productivity in Table O.A.7 are negative and vary between -0.059 to -0.015 standard deviations of pre-reform log productivity and insignificant.
	Productivity	Productivity
Post \times Constr.	-0.036	-0.047
	(0.096)	(0.094)
Post	0.018	0.009
	(0.083)	(0.083)
Industry FE	4-digit CPV	6-digit CPV
Ν	2,380	2,280

Table O.A.6: The Effect of Renegotiation on the Productivity of Winning Firms

Standard errors in parentheses

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

Notes: The table shows the results from four difference-in-differences specifications using the standardized (using industry-level pre-treatment productivity) log productivity as the outcome variable. In columns 1 and 2, the production function parameters were estimated using the Wooldridge method on a sample of firms with more than 50 employees. Columns differ in the granularity of industry-fixed effects. Post is a dummy variable that equals 1 for contracts procured after the reform (renegotiation is possible, and firms know this when submitting their bids) and 0 otherwise. Constr. is a dummy variable that equals 1 for construction contracts and 0 otherwise.

In Table O.A.7, we present additional results using a productivity measure estimated on a sample of firms with more than 50 employees and employing the Levinsohn-Petrin method (Levinsohn and Petrin, 2003).

	Productivity	Productivity	Productivity	Productivity
Post \times Constr.	-0.059	-0.015	-0.048	-0.049
	(0.095)	(0.094)	(0.085)	(0.083)
Post	0.014	-0.030	0.051	0.055
	(0.082)	(0.083)	(0.074)	(0.073)
Industry FE	4-digit CPV	6-digit CPV	4-digit CPV	6-digit CPV
Ν	2,380	2,280	$3,\!207$	3,091

Table O.A.7: The Effect of Renegotiation on the Productivity of Winning Firms

Standard errors in parentheses

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

Notes: The table shows the results from four difference-in-differences specifications using the standardized (using industry-level pre-treatment productivity) log productivity as the outcome variable. In columns 1 and 2, the production function parameters were estimated using the Levinsohn-Petrin method on a sample of firms with more than 100 employees. In columns 3 and 4, the production function parameters were estimated using the Levinsohn-Petrin method on a sample of firms with more than 50 employees. Columns differ in the granularity of industry-fixed effects. Post is a dummy variable that equals to 1 for contracts procured after the reform (renegotiation is possible and firms know this when submitting their bids) and 0 otherwise. Constr. is a dummy variable that equals to 1 for construction contracts and 0 otherwise.

O.A5 Excluding Contracts with the Estimated Value between 10M and 50M CZK

The reform of the Czech public procurement law introduced in 2016 led to, among others, a change in the rule for the simplified below-threshold regime contracts. This rule only affects the construction sector contracts but not the control group contracts and that is why it could potential threat our identification. Specifically, the threshold for the simplified below-threshold regime (which is only used in the construction sector) was increased to CZK 50 mil (after the reform) from previous CZK 10 mil (before). The simplified below-threshold procedure allows procuring authorities to directly contact at least 5 potential suppliers instead of announcing the contract publicly and making it open to all bidders that qualify, i.e., procuring authorities can pre-select bidders and limit competition by using this procedure. Note that 5 bidders is around the average number of bidders in procurement.

To be sure that this does not drive our results, we replicate the findings from Tables 1 and 2 excluding the contracts with the estimated value in the interval between CZK 10 mil. and CZK 50 mil. Note that this is a rather extreme version of a robustness exercise

that disregards many observations in the treatment group.

Table O.A.8 replicates Table 1 in the main text. The findings in the table show that the decrease in the winning bid ratio remains substantively the same. If anything, the effect is even more pronounced.

	Winning Bid Ratio	Winning Bid Ratio
Post Treatment \times Construction	-0.037***	-0.036***
	(0.007)	(0.007)
Post Treatment	0.005	0.008
	(0.005)	(0.006)
Industry FE	4-digit CPV	6-digit CPV
Ν	8,683	8,442

Table O.A.8: Effect of Eased Renegotiation Rules on Winning Bid

Standard errors in parentheses

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

Notes: The table shows results from two difference-in-differences specifications, using the winning bid ratio (winning bid over the estimated value of the contract) as the outcome variable. Compared to Table 1 in the main text, all contracts with the estimated value between 10M and 50M CZK were removed from the sample. The two columns differ in the granularity of industry fixed effects. Post Treatment is a dummy variable that equals 1 for contracts procured after the reform and 0 otherwise. Construction is a dummy variable that equals 1 for construction contracts and 0 otherwise. We control for the type of procurement procedure, the evaluation criteria, and a set of industry fixed effects.

Table O.A.9 replicates Table 2 in the main text on a sample without contracts between CZK 10 and 50 million of the estimated value. The estimated effects resemble the ones obtained in the main specification in the main text. In particular, the effect on the final price between settings I and II is slightly higher (instead of 2.2%, it is 2.3% for 4-digit CPV specification, and instead of 1.7%, it is 2% for the 6-digit CVP specifications). For the effect between settings II and III, the effect is again negative but marginally attenuated. Instead of -1.9% it is -1.1% and -1.4%, respectively. Finally, for the long-term effect, the point estimates point to a statistically insignificant effect.

Overall, the findings from these exercises show that the change of the rule for the simplified below-threshold regime is unlikely drive our main findings.

	Final Price Ratio		Final Pr	Final Price Ratio		Final Price Ratio	
	Setting I and II		Setting I	Setting II and III		Setting I and III	
Post \times Constr.	0.023^{***} (0.008)	0.020^{**} (0.008)	-0.011 (0.012)	-0.014 (0.013)	0.008 (0.012)	0.006 (0.013)	
Post	0.006	0.007	0.003	0.010	0.017^{**}	0.026^{***}	
	(0.006)	(0.006)	(0.008)	(0.009)	(0.008)	(0.009)	
Industry FE	4-digit CPV	6-digit CPV	4-digit CPV	6-digit CPV	4-digit CPV	6-digit CPV	
N	5,977	5,755	3,494	3,314	4,462	4,266	

Table O.A.9: Final Price: Difference-in-Differences Results

Standard errors in parentheses

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

Notes: This table summarizes the effect of the reform on the final price ratio. Compared to Table 2 in the main text, all contracts with the estimated value between 10M and 50M CZK were removed from the sample. For each comparison of the final price in two settings, we report results from two specifications that differ in the granularity of the CPV. The first two columns show the difference between settings I and II and thus capture the effect of the unanticipated possibility of renegotiation on the final price. The third and fourth columns show the difference between settings II and III. The last two columns show the difference in final prices between the pre-reform and post-reform periods. Each regression is estimated following regression (2).