Contract Splitting in Public Procurement†

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Abstract

This paper studies contract splitting - the act of splitting contracts into multiple smaller ones - as a mechanism of manipulation in public procurement. Leveraging the procurement administrative registry in Portugal and exploiting a reform that lowered discretion thresholds, we find that contract splitting is the main mechanism of manipulation. Buyers split to circumvent competitive requirements, more so for goods and services than for less divisible construction works. We discuss the implications of contract splitting for commonly used bunching estimators, documenting the existence of a splitting-induced bias. Discretion-seeking manipulation is motivated by favoritism rather than efficiency promotion.

Keywords: Contract Splitting, Manipulation, Bunching, Public Procurement

JEL Classification: D73; H57; H72; P16

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1 Introduction

Public procurement bridges public funds with private activity, and is therefore highly regulated. Most rules are based on contract prices, and thresholds govern the trade-off between discretion and competition. If a contract's price exceeds the threshold, competitive procedures are implemented (*auctions*). Otherwise, the contract can be awarded at discretion (*direct awards*). Such rules do not always bind *de facto*, as buyers manipulate the price of contracts to exercise discretion. Manipulation can be achieved by reducing a contract's price (*contract shifting*), or by splitting large contracts into multiple smaller ones (*contract splitting*). While manipulation through contract shifting is widely recognized, little is known about contract splitting and its implications.

In this paper, we study the role of contract splitting in public procurement. We start by quantifying manipulation relying on commonly used bunching estimators. We then investigate whether contract splitting is an important mechanism of manipulation. We study how splitting affects the core assumptions underlying those estimators, and document the existence of a splitting-induced bias. Finally, we study the consequences of manipulation for the procurement process in a setting where splitting dominates, focusing particularly on *seller* selection and post-award contract performance. Discretion-seeking splitting behavior carries different welfare implications depending on whether it is driven by efficiency-promoting motivations, which improve the use of public funds and the provision of public goods, or favoritism, contributing to resource misallocation and erosion of public trust.

The laboratory is the Portuguese 2017 Procurement Reform. Portugal shares a regulatory framework with other European countries, but its public procurement system is characterized by substandard practices, low transparency, widespread favoritism, and corruption (EU Commission, 2019, 2020). The reform significantly reduced the thresholds for direct awards, thereby limiting buyers' discretion to a narrower range of contract prices. We combine comprehensive procurement contract data with sellers' firm-level information, elected politicians' identities, and independently-provided buyers' transparency scores. The richness and granularity of the data allow us to study price and awarding procedure choices, post-award procurement outcomes, and relevant seller-level characteristics, such as repeated procurement relationships, local preferences, and political connections.

We begin by providing evidence of strong manipulation across different procurement sectors (McCrary (2008) tests). We find a sharp post-reform increase in the number of contracts below the new discretion-compatible threshold in goods, services, and construction. We formally quantify the extent of manipulation resorting to two widely used bunching estimators. Both compare the observed price distribution with an alternative counterfactual. The first counterfactual is built by interpolating a fitted contract price distribution over the manipulation region around the threshold (Saez, 2010; Chetty, Friedman, Olsen, & Pistaferri, 2011; Kleven & Waseem, 2013). The second approach uses the pre-reform distribution as counterfactual (Palguta & Pertold, 2017). Bunching is sizable and statistically significant. The excess mass of contracts in the bunching region is equal to 6.9 (resp., 9.6) times the average counterfactual density for goods and services (resp., construction) in the first post-reform year.

Our empirical strategy to identify splitting leverages the quasi-experimental setup provided by the reform. We define *Procurement Needs* as buyer-product-sector triplets, and employ the within estimator to investigate how the composition of contracts changed following the reform. The metrics of interest include total value purchased, number of contracts, and number and share of contracts close to, but below, the new threshold. Large increases in the number of contracts for the same total value procured are consistent with splitting, while a constant number of contracts and a lower total value are consistent with shifting. We complement this approach with a distributional analysis relying on the pre-reform distribution as a counterfactual. Splitting involves manipulation within a larger window around the discretion threshold than shifting. Shifting high-price contracts is unlikely, as it would entail significant price or quantity reductions. In contrast, contract splitting allows contracts, even large ones, to be distributed across multiple smaller ones. We investigate how far from the threshold the reform reshaped the contract price distribution, and develop measures comparing missing and excess masses.

The main finding is that contract splitting is the dominant mode of procurement manipulation in our setting. Buyers manipulate by purchasing a constant total value per product through a higher number of contracts (17% increase relative to the pre-reform mean). We zoom in on specific sectors and find that splitting dominates for goods and services, where projects and quantities are easily divisible, but is far less relevant for construction works, characterized by indivisible projects (Coviello, Guglielmo, & Spagnolo, 2017). The distributional analysis corroborates these findings. In goods and services, the reform induces changes over a wide price range, and excess mass below the threshold largely exceeds the missing mass above. In construction works, distributional changes concentrate around the threshold, and the difference between excess and missing masses is significantly smaller. The fact that contract splitting is a dominant mode of manipulation is striking, since it is strictly forbidden by law. However, we observe that buyers diversify suppliers (around 18% more sellers relative to the baseline mean), which may reflect an effort to stay under the radar of oversight authorities.

We then turn to the methodological implications of contract splitting. We show that splitting introduces bias in bunching estimates, as it implies a failure of the bounded manipulation assumption underlying widely used interpolated counterfactual estimators (Chetty et al., 2011). Split contracts do not always result in partitions that fall close to the threshold, implying changes over a wide range of the price distribution. The interpolated counterfactual approach relies on post-reform manipulated data and may fail to account for these broader changes. We use the pre-reform price distribution as a reference for the true counterfactual, and compare bunching estimates from the interpolated and the pre-reform counterfactuals. Differences between the two are interpreted as evidence of bias. Building on the previous result that splitting dominates in services, but not in indivisible construction works, we interpret a larger bias in services as splitting-induced.

We find that contract splitting can significantly bias bunching estimates based on interpolated counterfactuals: the constructed counterfactual is overestimated, biasing bunching estimates downwards. In services, where splitting is prevalent, the interpolated counterfactual approach underestimates bunching by 24.3%, compared to that obtained from the pre-reform counterfactual. In construction, where splitting is less relevant, this difference is much smaller (7.4%).

Finally, we characterize the manipulation induced by the reform and its consequences for procurement in a context where contract splitting dominates. We test competing theories on the role of discretion, contrasting efficiency-promoting motivations to favoritism. Our findings highlight favoritism as the primary driver of contract splitting. First, manipulation also occurs in standardized procurement projects (Bandiera, Prat, & Valletti, 2009; Brugués, Brugués, & Giambra, 2024), where the contractibility of quality decreases the need to foster it through relational advantages (Calzolari & Spagnolo, 2020; Kang & Miller, 2022). Second, manipulated contracts are more often awarded to politically connected and local firms, typically associated with favoritism and corruption (Baltrunaite, 2020; Akcigit, Baslandze, & Lotti, 2023; Colonnelli & Prem, 2022). Third, buyers who rank low in transparency manipulate more. Finally, manipulation entails worsened post-award performance: all else constant, split contracts have longer expected duration that does not translate into fewer delays, and benefit from fewer price discounts.

Related Literature We contribute to the recent literature on manipulation in public procurement, as well as its consequences. Palguta and Pertold (2017) first show that discretionary thresholds induce manipulation in public procurement of goods, services, and construction, exploiting a threshold-introducing reform in Hungary. Szucs (2023) analyzes service contracts in Hungary and Carril (2021) focuses on goods and services in U.S. federal contracts to assess the effect of discretion on procurement outcomes while accounting for manipulation. In an important related contribution, Coviello, Guglielmo, Lotti, and Spagnolo (2022) focus on construction works in Italy to study the effect of manipulation on outcomes. We are the first to document strong evidence of contract splitting as the main mechanism of manipulation. Contract splitting has received limited attention because it is illegal in most countries, and in many cases constrained by project indivisibility. Consistently, we show that splitting is dominant in goods and services contracts, but not in construction works, where most projects are arguably indivisible (Coviello et al., 2017).

Our analysis complements the only two papers that, to the best of our knowledge, address contract splitting. Carril (2021) examines the trade-off between discretion and regulation, studying large contracts ($100 000 threshold) and threshold increases. Manipulation is shown to arise from *contract shifting*. Splitting is ruled out as a significant mechanism, for instance, by showing that manipulation occurs in buyer-seller pairs with a single yearly transaction. We show that, in our setting, splitting occurs at the buyer-product level and buyers often split contracts among multiple sellers. We study low-value contracts (€20 000 threshold) and exploit a threshold-reducing reform, the ideal quasi-experimental setup to examine splitting behavior by tracking buyers' purchasing pattern when reducing contract values is needed to retain discretion. In a paper subsequent to ours, Ivars and Cruz (2024) use similar conceptual arguments and find that shifting is the main form of manipulation. They develop a theoretical model to analyze buyers' optimal choice of the manipulation mechanism. In their setting, thresholds are unrelated to discretion, and instead distinguish auction formats. In ours, the threshold divides discretionary from competitive procedures.

We also contribute to the methodological literature of bunching estimators to quantify manipulation, by analyzing the implications of contract splitting for the relative performance of existing counterfactual estimation methods. First developed to study income tax responses (Chetty et al., 2011; Chetty, Friedman, & Saez, 2013; Kleven & Waseem, 2013;

Kleven, 2016), bunching methods compare the observed distribution to a counterfactual built by interpolation to areas around the thresholds, where manipulation is observationally evident. Palguta and Pertold (2017) adapted these methods to public procurement, proposing an estimator that relies on pre-reform unmanipulated distributions as counterfactuals, accounting for concentration of contracts at round values. We show that splitting may imply a failure of the bounded manipulation assumption, biasing bunching estimates based on interpolated counterfactuals. Our results highlight the importance of testing for contract splitting and recommend using non-manipulated data to build counterfactuals.

We contribute in three additional ways. We add to the scarce literature on who manipulates. Coviello et al. (2022) show that appointed officials manipulate while elected ones do not. We show that manipulation is associated with less transparent buyers. We show that manipulation is selective, favoring politically connected and local firms, adding to the existing evidence on the procurement-related advantages for such firms (Fisman, 2001; Khwaja & Mian, 2005; Goldman, Rocholl, & So, 2009, 2013; Titl, De Witte, & Geys, 2021; Brogaard, Denes, & Duchin, 2020; Baltrunaite, 2020). Finally, we speak to the broad literature on the links between agency and regulation in public organizations, particularly those related to discretion, manipulation, and performance in public procurement.[1] Bandiera et al. (2009), Coviello et al. (2017), Fazio (2022), and Decarolis, Fisman, Pinotti, and Vannutelli (2025) show that discretion can enhance procurement performance and product quality. Palguta and Pertold (2017), Baltrunaite, Giorgiantonio, Mocetti, and Orlando (2021), and Szucs (2023) find that it increases favoritism and awards to less productive firms. Our results highlight the negative effects of discretion-seeking manipulation on procurement quality. Theories tying the benefits of discretion to institutional quality (Bosio, Djankov, Glaeser, & Shleifer, 2022; Carril, 2021) can help explain different findings across contexts.

Roadmap The rest of this paper is organized as follows. Section 2 describes the institutional setting, the procurement reform, and the data. Section 3 quantifies manipulation. In Section 4, we document that contract splitting is a relevant mechanism of manipulation. Section 5 discusses the implications of splitting for bunching estimation. In Section 6, we address potential motivations underlying contract splitting, and investigate the consequences for the procurement process and quality. Section 7 concludes.

[1] Examples include different types of auctions (Decarolis, 2014, 2018), characteristics of bureaucrats (Coviello & Gagliarducci, 2017; Decarolis, Giuffrida, Iossa, Mollisi, & Spagnolo, 2020), centralized purchase agreements (Bandiera et al., 2009), publicity requirements (Coviello & Mariniello, 2014; Carril, Gonzalez-Lira, & Walker, 2022), or audits (Gerardino, Litschig, & Pomeranz, 2024).

2 Institutional Setting and Data

The Public Procurement Code *(Código dos Contratos Públicos*, PPC) stipulates the eligible awarding procedures based on the anticipated price, typically mentioned in the public notice, and defined as the maximum price that the procuring entity (*buyer*) is *ex-ante* willing to pay the supplier (*seller*) for the project. The PPC stipulates two other price concepts: the contract price (henceforth, *price*) results from the bidding process and is the price agreed upon contract; the final price is the realized total cost, including discounts or cost overruns, reported after the project's conclusion.[2]

Until 2018, two awarding procedures were available. Up to a maximum anticipated price of €75 000 for goods and services and €150 000 for construction projects and related services (henceforth, construction), the buyer has full discretion to invite a seller to submit a bid – *direct awards*.[3] The procedure is simple, speedy, and requires little bureaucracy. When the anticipated price exceeds the threshold, the contract must be awarded through *open auctions*, increasing both the transparency and the bureaucratic burden. They require a formal publication in the Government Gazette or the Journal of the European Union, involve preliminary and final evaluation reports by a pre-determined jury, hearings, and sometimes negotiations. Open auctions are competitive – any interested firm can submit a bid – and can be used for projects of any price.

2.1 The Public Procurement Reform

In a stated effort to reduce the bureaucratic burden and promote higher transparency and sound use of public funds, the Portuguese Parliament approved a procurement reform in August 2017, effective from January 2018. The legislation significantly reduced the thresholds for direct awards, thereby limiting buyers' full discretion to a narrower range of contract values. The thresholds decreased from €75 000 to €20 000 for purchases of goods and services, and from €150 000 to €30 000 in construction.

For projects with anticipated price between the old and new thresholds, the reform introduced the *restricted auction* awarding procedure. In restricted auctions, buyers are required to invite at least three firms to submit bids, which are subject to a formal evaluation

[2] We observe the final and contract prices. By definition, the latter is lower than the anticipated price, which allow us to infer the eligible procedures.

[3] A buyer cannot select a seller to whom it has awarded contracts in cumulative value exceeding the threshold over the previous two years.

by an independent jury.[4] Thus, these entail lower (resp., higher) discretion and higher (resp., lower) transaction costs than direct awards (resp., open auctions). Table A.1 summarizes the regulatory setting and changes introduced by the reform.

2.2 Data

We collected data on all electronically registered public procurement contracts in Portugal by web-scraping *Portal Base*, the e-procurement platform maintained by the Institute of Public Markets, Real Estate and Construction.[5] These data are available and regularly updated since 2009, following the mandate to electronically register all procurement contracts exceeding €5 000. We focus on the period around the procurement reform, specifically between 2015 and 2019.

The data encompass granular information on the project's characteristics – the procured item's complete Common Procurement Vocabulary (CPV) code, location, and the name and tax identifier of the buyer. Moreover, it includes contract information, namely, the awarding procedure, signing date, contract price, expected duration, and seller identity. When the seller is a consortium, we observe the identities of all its members. Lastly, we observe the final price, adjustments to the deadline (a justification may be stated), and the actual duration of the contract, which allow us to compute post-award performance indicators such as price discounts, cost overruns, and delays.[6]

We merge the procurement data with three exhaustive datasets. We link sellers to Bureau van Dijk's Orbis through the unique tax identifiers. Orbis includes corporate information such as the sector, headquarters' location, financial fundamentals, and managers' names of the near universe of Portuguese firms (around 800 000).[7] We match the managers to elected politicians at the local executive and legislative bodies, publicly available from the National Election Commission (*CNE*). We label a firm as politically connected if a manager is an elected official at the time of the contract award, or was in the preceding electoral cycle (Khwaja & Mian, 2005; Faccio, 2006). Throughout our period of analysis, elected officials could hold corporate positions while serving office, and those firms were

[4]Under certain conditions (supplier exclusivity, auctions with no bids, and auctions where other bids have been excluded), buyers can use direct awards in the same price region as before.

[5]The universe of reported contracts is available at `www.base.gov.pt`.

[6]We use the textual justifications to refine our measures of incomplete projects and late contracts. A list of expressions is shown in Online Appendix Table B.3.

[7]We downloaded the data in 2019, at the end of our sample period.

allowed to participate in public tenders.[8] Finally, we integrate the Municipal Transparency Index (MTI) from Transparency International (an anti-corruption NGO) for all buyer municipalities. The MTI evaluates municipal transparency across more than 75 internationally comparable criteria, aggregated into seven dimensions, including public procurement.[9]

2.3 Sample and Descriptive Statistics

Our sample comprises all publicly available public procurement contracts, with prices below the open auction threshold. We exclude contracts awarded by entities that do not directly provide public goods or services.[10] We exclude contracts with particular procurement frameworks largely negotiated at the central government level. We also remove small contracts with expected duration below 20 days and price below €5 000 (resp., €10 000) for goods and services (resp., construction), which are waived from the publicity obligation.

Table 1 presents the main descriptive statistics. Goods and services account for at least 80% (resp., 65%) of the number (resp., value) of public contracts each year. Before the reform, around 61% of contracts for goods and services (29% of total value) were awarded for less than €20 000 and 38% of construction for less than €30 000, reflecting the relatively small procurement values in our analysis. Consistently, 94% of the contracts were awarded through direct awards. Municipalities are the largest buyer category, representing close to half of the transactions. In line with the reform's goal of increasing transparency while limiting the bureaucratic burden, around 47% of the procurement value shifted from direct awards to restricted auctions following the reform.

3 Manipulation Evidence

In this section, we present visual evidence of bunching below the direct awards threshold and quantify the extent of manipulation, resorting to two commonly used bunching estimators.

We plot McCrary (2008) discontinuity tests for goods and services, and construction in Figure 1. The null hypothesis of continuity of the density at the threshold is not rejected pre-reform (blue), as shown by overlapping confidence intervals on both sides. However, in the

[8] Insofar as the elected official did not own more than 10% of the firm.

[9] 1) Organization, Social Composition, and Municipality Functioning; 2) Plans and Reports; 3) Taxes, Tariffs, and Regulations; 4) Relations with Society; 5) Public Procurement; 6) Financial and Economic Transparency; 7) Transparency in Urbanism. To know more about the index, visit `transparencia.pt`.

[10] These include public-private partnerships, associations, foundations, the Central Bank, and other residual categories.

post-reform period (red), we observe a surge in the number of contracts with prices just below the new threshold (henceforth, *bunching region*). This finding reveals strong manipulation. The yearly binned distributions (Figure A.1) provide evidence that the reform was not anticipated, that bunching is not driven by rounding at reference values and, reassuringly, bunching does not disappear at the (unchanged) open auction thresholds.

3.1 Interpolated Counterfactual

The first approach to quantify manipulation follows Saez (2010), Chetty et al. (2011), and Kleven and Waseem (2013). This method constructs a counterfactual under two assumptions. The first is that, absent the threshold, the price distribution would be smooth. The second is the bounded manipulation assumption, which states that manipulation occurs within a bounded neighborhood of the threshold (*manipulation region*). The approach proceeds by fitting a flexible polynomial to the contract price distribution excluding that neighborhood. The counterfactual is then built by interpolating over the manipulation region – *interpolated counterfactual*.

To estimate the counterfactual, we split the price distribution in bins of €250 and normalize the threshold to 0. We define the manipulation region within the bins $R_{left} = 6$ and $R_{right} = 16$ following visual inspection of Figure A.1. We fit a polynomial of order 12 (chosen through a five-fold cross-validation procedure) to the distributions, excluding the manipulation region:[11]

$$C_j = \alpha + \sum_{k=0}^{12} \beta_k \cdot (Z_j)^k + \sum_{i=-R_{left}}^{R_{right}} \gamma_i \cdot \mathbb{1}\left[Z_j = i\right] + \varepsilon_j \tag{1}$$

where C_j is the number of contracts in bin j, $Z_j = \{..., -1, 0, 1, ...\}$ is the running bin variable, measured in bin distance to the threshold, and $\mathbb{1}\left[Z_j = i\right]$ are dummies for the bins within the manipulation region. The counterfactual density is obtained using the predicted values of equation 1, setting all γ_i to zero:

$$\widehat{C}_j = \hat{\alpha} + \sum_{k=0}^{12} \widehat{\beta}_k \cdot (Z_j)^k \tag{2}$$

We compute the excess number of contracts ($\hat{B}$) and the excess mass ($\hat{b}$) in the bunching region $[-R_{left}, 0]$. The latter is defined as the excess number of contracts relative to the

[11]Details and results for the five-fold cross-validation are presented in the Appendix Table B.1.

average counterfactual density, respectively as:

$$\widehat{B} = \sum_{j=-R_{left}}^{0} C_j - \widehat{C}_j = \sum_{i=-R_{left}}^{0} \widehat{\gamma}_i, \qquad \hat{b} = \frac{\widehat{B}}{\sum_{j=-R_{left}}^{0} \widehat{C}_j / R_{left}} \tag{3}$$

The reform induced bunching around the threshold, large in magnitude and pervasive across sectors. Table A.2 presents the results by year. The excess mass of contracts in the bunching region, $\hat{b}$, is equal to 6.9 (resp., 9.6) times the average counterfactual density for goods and services (resp., construction) in the first post-reform year. Bunching is even higher two years after. We find some positive but small excess mass in construction in the years before the reform, possibly due to rounding.

3.2 Pre-Reform Counterfactual

The alternative approach follows Palguta and Pertold (2017). Unlike the interpolated counterfactual, it relies on the data's panel dimension, using the pre-reform price distribution as counterfactual – *pre-reform counterfactual*. The assumption is that, absent the reform, the price distribution would remained unaltered.

We estimate the following regression:

$$C_{jt} = \alpha_j + \alpha_t + \sum_{i=-R_{left}}^{0} \gamma_i \cdot \mathbb{1}\left[Z_j = i\right] \cdot Post_t + \varepsilon_{jt} \tag{4}$$

where C_{jt} is the number of contracts in bin j and year t, α_j and α_t are bin and year fixed-effects, and $Post_t$ is a post-reform binary indicator. The model is estimated by Poisson conditional fixed-effects quasi-maximum-likelihood, most suitable for count data.

Using the pre-reform counterfactual, Table A.3 shows an average 489% (resp., 378%) increase in the density of contracts in goods and services (resp., construction) in the first bin below the threshold. Excess mass is large and statistically significant, but decreasing with distance to the threshold. Thus, when manipulation occurs, contract prices tend to concentrate in the largest values that enable discretion.

The two estimators differ in two relevant dimensions. First, while the excess mass of the interpolated counterfactual estimator does not have an intuitive economic interpretation, the appropriate transformation of the coefficients $\widehat{\gamma}_i$ in equation 4 does.[12] Second, the pre-reform counterfactual estimator does not require smoothness around the threshold, and is

[12] Indeed, $[exp(\widehat{\gamma}_i) - 1) \times 100]$ is the percentage change, on average, with respect to the pre-reform period.

flexible enough to account for price rounding.[13] In Section 5, we show that the pre-reform counterfactual approach can be superior for a third reason: the bunching estimates derived from the interpolated counterfactual approach are biased under contract splitting. In the next section, we document that contract splitting is a relevant mechanism of manipulation.

4 Contract Splitting

4.1 Splitting vs. Shifting

Manipulation can be achieved in two ways. Buyers can reduce the anticipated price of a given project just enough to make it compliant with discretion – *contract shifting*, or they can divide large price contracts into multiple discretion-eligible ones – *contract splitting*. Shifting and splitting have distinct implications for the distribution of contract prices.

First, splitting involves manipulation within a larger window around the discretion threshold than shifting. Shifting high-price contracts is unlikely, as it would entail significant price or quantity reductions. Therefore, shifting concerns contracts with prices not too far above the threshold, which are manipulated to prices just below it. In contrast, contract splitting allows even large price contracts to be distributed across multiple smaller ones. Those partitions may fall within or outside the bunching region. As a result, contract splitting can lead to contract prices farther to the left of the threshold as well. Nevertheless, devising and awarding contracts is costly, creating an incentive to minimize their number, and possibly award them for the highest discretion-compatible prices.

Second, there are implications for the number of contracts around the threshold. With contract shifting, each missing contract above the threshold corresponds to a single contract in the bunching region. Conversely, contract splitting entails additional contracts in the bunching region without a corresponding reduction to the right of the threshold. Therefore, larger differences between excess mass in the bunching region and missing mass above the threshold suggest a greater importance of contract splitting.

The degree of divisibility of each procurement project is likely to affect the prevalence of splitting. Projects in the construction sector are less divisible than goods and services contracts (Coviello et al., 2017). We exploit this difference in some of our analyses.

[13] Figure A.1 shows the existence of rounding at the new thresholds even before the reform, although not different in magnitude compared to other round values.

4.2 Implications for the Price Distribution

We employ the bunching estimator proposed by Palguta and Pertold (2017) to assess the extent to which the reform induced changes in the contract price distribution. We regress the bin-level number of contracts C_{jt} in year t, on year (α_t) and bin fixed effects (α_j), and the interactions of a post-reform indicator ($Post_t$) with a wide set of bin dummies to each side of the threshold:

$$C_{jt} = \alpha_j + \alpha_t + \sum_{i=-25}^{0} \gamma_i^L \cdot \mathbb{1}\left[Z_j = i\right] \cdot Post_t + \sum_{i=1}^{45} \delta_i^R \cdot \mathbb{1}\left[Z_j = i\right] \cdot Post_t + \varepsilon_{jt}, \qquad (5)$$

The estimator relies on bin-level comparisons with the pre-reform period. Significant interaction terms away from the threshold allow us to infer how far in the distribution the manipulation-related changes materialize. Changes over wider price ranges are consistent with contract splitting, while local changes around the threshold are suggestive of contract shifting. We estimate equation 5 separately for goods and services, and construction.

Figure 2 plots the $\widehat{\gamma}^L$ and $\widehat{\delta}^R$ coefficients, with the statistically significant ones in blue, and the non-significant in red unfilled bars. The results support contract splitting as a dominant mechanism for procurement of goods and services. Coefficients are negative and significant over a wide price range to the right, suggesting that the partition of large contracts occurs. The significant excess mass far to the left indicates partitions with prices below the bunching region. Manipulation is significant and decaying throughout the whole range. In the construction sector, in which there is a lower degree of divisibility, we find large and significant manipulation only in close proximity to the threshold. These local changes are more consistent with contract shifting.

A complementary approach relates missing and excess masses around the threshold. The *absolute mass difference* ($\hat{D}$) is the difference between excess mass in the bunching region and missing mass above the threshold, each computed relative to the no-manipulation pre-reform counterfactual. The *relative mass difference* ($\hat{d}$) is equal to the ratio between $\hat{D}$ and the average mass in the whole manipulation range before the reform.[14] Larger values of $\hat{D}$ and $\hat{d}$ reveal a higher degree of contract splitting. The excess contract mass to the left of the threshold, $\hat{E}$, and the missing contract mass to its right, $\hat{M}$, are computed as follows:

[14] Cengiz, Dube, Lindner, and Zipperer (2019) employ a conceptually similar procedure to study the evolution of missing and excess jobs around the minimum wage.

$$\hat{E} = \sum_{i=-R_{left}}^{0} \hat{\gamma}_i^L \times C_{i,2017}, \qquad \text{and} \qquad \hat{M} = \sum_{i=1}^{R_{right}} \hat{\delta}_i^R \times C_{i,2017}$$

where $C_{i,2017}$ is the number of contracts in bin i at baseline (last pre-reform year), and $\hat{\gamma}$ and $\hat{\delta}$ are obtained by estimating equation 5 with the bunching estimation parameters used in Section 3. The absolute and relative mass difference measures, $\hat{D}$ and $\hat{d}$, are defined as:

$$\hat{D} = \hat{E} - \hat{M} \qquad \textit{and} \qquad \hat{d} = \frac{\hat{D}}{(1/3) \times \sum_{t \leq T} \mathbb{1}\{year = t\} \times \sum_{i=-R_{left}}^{R_{right}} C_{it}}$$

Table 2 reports the estimates of $\hat{D}$ and $\hat{d}$, separately for goods and services, and construction. The excess mass of contracts to the left of the threshold, net of the missing mass to its right, is as large as 52% of the pre-reform density in goods and services, and drops to 39% in the construction sector, suggesting a less relevant role for splitting in construction, consistently with the distributional changes shown in Figure 2.

4.3 Implications for Procurement Composition

We now turn to explicit evidence of contract splitting. We define a *Procurement Need* (PN) as a product-buyer-sector triplet, where the product is identified by its full CPV code. This way, we isolate the product-specific needs of each procuring entity, and evaluate how the composition of purchases changes as a response to the reform. We compute six annual measures for each PN: total value purchased (TV), total number of contracts (NC), average contract value (AV), and number of sellers (NS). Additionally, we compute the number (NBR) and share (SBR) of contracts in the bunching region.

As we are interested in identifying the most important mechanism of bunching, we restrict our analysis to PNs with at least one contract in the bunching region post-reform.[15] We estimate the following equation:

$$y_{it} = \delta_i + \gamma Post_t + \epsilon_{it} \tag{6}$$

where y_{it} denotes each of the outcomes for PN i in year t, δ_i are PN fixed-effects, and $Post_t$ is the post-reform indicator. By applying the within-estimator, we isolate the compositional responses to the reform for each PN, captured by coefficients $\hat{\gamma}$. Standard

[15] Table A.4 compares the descriptive statistics of full and restricted samples, showing that the latter contains more recurrent procurement needs and is tilted towards professional services.

errors are clustered at the CPV and buyer level.

A negative $\hat{\gamma}$ for the yearly total purchased value and a null $\hat{\gamma}$ for the number of contracts supports shifting as dominant mechanism of manipulation, as does a small negative $\hat{\gamma}$ for the average contract value. By contrast, a non-negative $\hat{\gamma}$ for total purchases, a positive one for the number of contracts, and a large negative coefficient for the average contract value support splitting.[16] Table A.5 summarizes the hypotheses. By validating the hypotheses associated with one of the mechanisms, we shed light on the dominant one, although we cannot rule out the presence of the other.

Regarding the additional outcomes, a zero $\hat{\gamma}$ coefficient for the number of sellers indicates that split contracts are awarded to a constant set of repeated sellers. Positive $\hat{\gamma}$ for the number and share of contracts in the bunching region indicate that buyers manipulate strategically into price ranges close but below the threshold, in order to achieve discretion while minimizing transaction costs.

The main results are presented in Panel A of Table 3. They reveal a statistically significant increase in the average number of contracts per PN of 0.7 following the reform, corresponding to a 17% increase relative to the pre-reform mean. We find a much smaller increase in total value purchased (7%), statistically indistinguishable from zero. Together, they imply a very large and significant decrease of €4 075 (13.9% of the pre-reform mean) in the average contract value. Buyers do not alter the total value of purchases for each product, but significantly increased the number of contracts through which they procure them. The findings support contract splitting as the dominant mechanism of manipulation.

Additionally, we estimate a more than five fold increase in the number of contracts in the bunching region, and a nine fold increase in their share. These results provide evidence that, following the reform, buyer strategically split large contracts into multiple ones, with prices below but close to the new discretion threshold. This behavior may stem from buyers' efforts to minimize administrative and financial transaction costs while maintaining full discretion.

In Panels B to E of Table 3, we show sector-specific results. Consistent with Figure 2, splitting dominates for goods and specially for services. The increase in the number of service contracts is significant at the 1% level, and outmatches the rise in total value purchased, so that average contract prices decrease by 13%. The patterns for goods are also consistent with contract splitting, and despite the sizable increase in number of contracts, statistical

[16] Any non-negative coefficient for total purchases is compatible with contract splitting; a zero estimate is consistent with adjustments only through the number of contracts.

insignificance does not allow us to confidently reject other explanations. The splitting behavior is not present for construction, as (non-significant) decreases in the number of contracts exclude splitting as the dominant mechanism. We further exclude construction-related services and consider the more indivisible set of construction works. The patterns remain consistent with shifting, as total value procured decreases, without any significant change in the number of contracts. The findings suggest that contracts in construction are either shifted or not procured. In any case, the patterns are inconsistent with splitting.

The fact that contract splitting is a dominant mode of manipulation is striking, since it is strictly forbidden by law.[17] However, we observe that buyers diversify suppliers (around 18% more sellers relative to the baseline mean), which may reflect an effort to stay under the radar of oversight authorities.

The main conclusions are robust to variations in the analysis. We restrict the sample to PNs for which purchases are made every year (recurring needs). The results in Panel A of Table 4 corroborate splitting evidence: a stable total value of purchases per PN is carried out through a significantly higher number of contracts. The growth in the number of contracts (11% of pre-reform mean) is more than four times higher than the (non-significant) growth in total purchase value (2.5%). In Panel B, we restrict the sample to procurement contracts with relatively short expected duration (up to 1 year) to exclude large multi-year contracts that could be split into yearly partitions. We find a statistically insignificant 5% increase in the total value of purchases accompanied by an 18% increase in the number of contracts, consistent with splitting. Finally, we analyze the sensitivity of our results to different bunching regions in Table A.6. Splitting patterns are robust to any of the considered ranges, with estimated coefficients within the baseline confidence intervals.

Contract splitting has relevant implications for the methods employed to estimate manipulation in the public procurement literature. Under contract splitting, manipulation can occur over wider ranges of the contract price distribution than those affected by contract shifting. This threatens the validity of key assumptions underlying widely used bunching estimators, such as bounded manipulation and correspondence between missing and excess mass assumptions. We now turn to the discussion of the explicit implications of contract splitting for such estimators.

[17] In Portugal, the law states *"a contract's value cannot be partitioned as to exclude it from legal requirements"* (nr.8, Article17 of the PPC). Roads and buildings are common examples of indivisible projects.

5 Splitting-Induced Bias in Bunching Estimation

In this section, we show that contract splitting has implications for the performance of the bunching estimator that relies on the interpolated counterfactual distribution. We document the existence of bias and offer insights into its magnitude.

Contract splitting entails manipulation on a wide price interval, over which the interpolated counterfactual approach fits a polynomial (Section 3.1).[18] This violation of the bounded manipulation assumption may bias the interpolated counterfactual densities.

We use the pre-reform price distribution as a reference for the true counterfactual (Section 3.2). Absent anticipation, this distribution is unaffected by the threshold-changing reform. We compare bunching estimates from the interpolated and the pre-reform counterfactuals. Differences between the two are interpreted as evidence of bias. Building on the previous result that splitting dominates in services, but not in indivisible construction works, we compare the bias in both sectors. We interpret a larger bias in services as splitting-induced.

5.1 Empirical Framework

We proceed in four steps. First, we split the price distribution in €250 bins and define c_{jt} as the share of contracts in bin j and year t. We compute the bin-specific shares separately for services and construction works.

Second, we construct the counterfactual densities. The interpolated counterfactual density is based only on post-reform data and is estimated as in Section 3.1. For each post-reform year, 2018 and 2019, the *interpolated counterfactual* distribution is given by the estimated coefficients of equation 1, setting γ_i to zero. The *pre-reform counterfactual* is given by the pre-reform densities, to which we apply the same smoothing procedure. Since there is no pre-reform bunching, we do not include bin dummies.

In the third step, we compare the observed distributions with each counterfactual. We apply the estimator proposed by Palguta and Pertold (2017). For the interpolated counterfactual, the specification takes the form:

$$c_{jtD} = \alpha_j + \alpha_t + \sum_{i=-R_{left}}^{0} \delta_i^{inter} \cdot \mathbb{1}[Z_j = i] \cdot \mathbb{1}[D = 1] + \varepsilon_{jtD}, \tag{7}$$

[18] For example, a contract of €36 000 could be split in two of €20 000 and €16 000, implying changes from considerably above to considerably below the threshold.

where $D = 1$ denotes the observed distributions and $D = 0$ are the estimated counterfactuals. For the pre-reform counterfactual, the specification reads:

$$c_{jt} = \alpha_j + \alpha_t + \sum_{i=-R_{left}}^{0} \delta_i^{pre} \cdot \mathbb{1}[Z_j = i] \cdot Post_t + \varepsilon_{jt}, \tag{8}$$

where $Post_t$ denotes the post-reform indicator and i indexes the bunching bins. The coefficients of interest δ_i measure bunching, i.e., the average excess density of contracts in bin i in the observed distribution compared to the respective counterfactual.

Finally, we compute the bias. For each bin j, $bias_j$ compares bunching estimates from the interpolated counterfactual with those from the pre-reform one, our reference for the true counterfactual. The overall $Bias$ is the average of $bias_j$ over the bunching region:

$$bias_j = \frac{\hat{\delta}_j^{\,inter}}{\hat{\delta}_j^{\,pre}} - 1 \qquad Bias = \frac{1}{R_{left}} \sum_{j=-R_{left}}^{0} bias_j \tag{9}$$

5.2 Results

In Figure 3, we plot the counterfactual densities around the threshold under each approach and by sector. The interpolated counterfactual is similar to the pre-reform one for construction works, but not in the splitting-prone services. The interpolated counterfactual is above the pre-reform one from around bin -25 onwards, and it is clearly overestimated around the threshold, i.e., within the gray-shaded excluded region.[19] Therefore, when splitting is a dominant mechanism of manipulation, the interpolated counterfactual is biased upwards.

Table 5 displays the $\hat{\delta}_i$ estimates from regressions 7 and 8, as well as the estimates for the bin-specific and overall bias (equation 9). The bunching underestimation may be as large as 24% in the splitting-intensive services sector, but much smaller (around 7%) in construction works, where splitting is less relevant.[20] This suggests that, when splitting is the dominant mode of manipulation, the interpolated counterfactual's under-performance is substantial.

The evidence is robust to changes in key parameters. In Panel A of Table A.7, we show that our conclusions are invariant under different polynomial orders. In Panel B, we show that the bias does not result from a particular choice of excluded bins to the left of the threshold. Excluding a higher number of bins to the left reduces bias, as the influence of

[19] The two counterfactuals become observationally similar farther away from the excluded region, where split contracts are less likely to occur.

[20] Contract splitting may still arise in this sector, as discussed in Section 4.2.

splitting outside the excluded region decreases. Panel C shows that the results exhibit low sensitivity to changes in the number of excluded bins to the right of the threshold.

6 What Motivates Manipulation?

Manipulation is explained by the buyers' preference to award procurement contracts at discretion. We show this by estimating an event study to compare the probability of direct award procedures in *vs* outside the bunching region, before and after the reform:

$$DA_{it} = \alpha_t + \eta BR_{it} + \sum_{t \neq 2017} \beta_t \cdot BR_{it} + \mu_b + \eta_{cpv} + \lambda_m + \delta_c + X'_{it}\Gamma + \varepsilon_{it} \tag{10}$$

where $DA_{it} = 1$ if contract i was directly awarded in year t, as opposed to through an auction. BR is a indicator for a contract price in the bunching region, and β_t are year-specific coefficients measuring the difference with respect to the pre-reform year. $\mu_b, \eta_{cpv}, \lambda_m, \delta_c$, and θ_s are buyer type, CPV code, month, municipality, and sector (goods, services, or construction) fixed effects. The vector of controls X_{it} includes price and expected duration of the project. Standard errors are clustered at the CPV code level. We consider two samples: either contracts with price below the new threshold or all contracts.

The estimates of β_t are plotted in Figure 4. They confirm the absence of pre-trends, and show that the use of direct awards surges for contracts in the bunching region, when compared to the remaining discretion-eligible contracts (red), indicating that manipulation of procurement contracts is discretion-seeking. The conclusion remains unchanged considering all contracts as a control group (blue).[21]

Discretion-seeking manipulation carries different welfare implications depending on whether it is driven by efficiency-promoting motivations, which improve the use of public funds and the provision of public goods, or favoritism, which contributes to resource misallocation and erosion of public trust. This section provides an exploratory analysis of the normative implications of our findings.

[21]The magnitudes are larger because the range of prices for which direct awards are allowed decreases following the reform.

6.1 The Competing Hypotheses

Discretion plays a pivotal role in fostering procurement quality in contexts where it is non-contractible (Manelli & Vincent, 1995; Albano, Calzolari, Dini, Iossa, & Spagnolo, 2006; Calzolari & Spagnolo, 2020). Contract splitting allows the buyers to protect sellers from competition and increase the relational value through a repeated-game that incentivizes sellers to maintain the desired quality. Moreover, buyers can restrict their interactions to a smaller number of sellers, from which they can extract valuable informational rents. In such contexts, long-term relationships are bound to arise between buyers and sellers, with positive impact on procurement quality (Spagnolo, 2012; Kang & Miller, 2022). Discretion is also effective at circumventing lengthy, often inefficient, bureaucratic procedures (Bandiera, Bosio, & Spagnolo, 2021; Szucs, 2023). Bandiera et al. (2009) show that excessive payments in procurement are driven by inefficiencies rather than corruption. The elapsed time between tender and contract signature is typically significantly higher for competitive than discretionary contracts (IMPIC, 2019). Additionally, discretion allows buyers to promote bureaucrats' initiative and engagement, capitalizing on their expertise to better screen sellers. Although the use of such knowledge cannot be easily embedded into regulations, it creates positive value on procurement outcomes with limited impact on quality and corruption, including reduced prices or fewer project delays (Kelman, 1990, 2005; Bandiera, Bosio, & Spagnolo, 2021; Coviello et al., 2017; Decarolis et al., 2025; Bandiera, Best, Khan, & Prat, 2021; Bosio et al., 2022).

Efficiency-promoting motives deliver three testable implications. First, if contract splitting aims to enhance the relational advantages that ensure non-contractible quality, one would expect little or no splitting when quality is contractible, as in procurement of standardized or homogeneous products. Second, we expect buyers to restrict their procurement relationships to a small set of frequent, loyal suppliers. Finally, efficiency-promoting motives should be reflected in post-award performance (frequent discounts, fewer delays, and lower cost overruns).

In contrast to efficiency-promoting motivations, manipulation may be driven by *favoritism*, i.e., procuring entities exerting discretionary power to pursue private interests or protect their favored sellers from competition (Banfield, 1975; Palguta & Pertold, 2017; Bosio et al., 2022).

We test this possibility by analyzing whether the reform-induced split contracts are awarded to special interest groups (SG). First, local firms, which directly contribute to the local economies and can return political dividends to the procuring entities, e.g., because they employ voters, can be favored over non-local ones (Branco, 1994; Coviello et al., 2017; Coviello & Gagliarducci, 2017). Second, we consider politically connected firms. Politicians' connections to firms have been shown to influence procurement activity, with benefits often reciprocated through campaign contributions, political donations, or corruption. (Goldman et al., 2013; Brogaard et al., 2020; Szucs, 2023; Baltrunaite, 2020; Titl et al., 2021). Businesses leverage their executives' positions of power to build advantageous networks that transcend party affiliation and endure over time (Faccio, 2006; Khwaja & Mian, 2005; Colonnelli & Prem, 2022). We examine how buyers' transparency correlate with the extent of the manipulation. Stronger manipulation by less transparent buyers supports favoritism rather than efficiency motives. Finally, we expect favoritism to correlate with weaker post-award performance, contrary to efficiency-enhancing discretion. We test this hypothesis using the available procurement outcomes: expected duration, delays, and renegotiations. Table A.8 summarizes the testable implications.

6.2 Empirical Tests

6.2.1 Standardized Procurement

We investigate contract splitting in standardized procurement, i.e., the purchase of homogeneous or generic goods or services by a large set of public entities, for which quality is largely contractible (Bandiera et al., 2009; Brugués et al., 2024). We follow Bandiera et al. (2009) that define standardized procurement according to *(i)* homogeneity, i.e., comparable products whose price is a direct function of observable characteristics (contractible quality), *(ii)* diffusion, i.e., significant share of public authorities procuring the product, and *(iii)* relevance, i.e., whether the product takes up a sizable share of the public budgets. We match all standard goods or services identified by Bandiera et al. (2009) with the corresponding CPV codes.[22]

We investigate contract splitting in the subsample of standardized contracts by estimating equation 6. Furthermore, we augment the equation with an interaction term and run it on the full sample to compare the extent of contract splitting in standardized vis-à-vis

[22]For example, a "Lunch Voucher" corresponds to CPV code 30199770-8 "Luncheon voucher". The detailed correspondence is shown in Table B.2.

non-standardized procurement:

$$y_{it} = \delta_i + \beta_0 Post_t + \gamma Post_t \times SP_i + \epsilon_{it} \tag{11}$$

Table 6 presents the results for the standardized subsample in odd columns, and the augmented version in the whole sample in even columns. We find patterns consistent with contract splitting in standardized procurement. Following the reform, there is no significant change in the total amount spent by buyers (3.6%), but a large significant increase (13.4%) in the number of contracts. The number and share of contracts awarded in the bunching region increase significantly, both economically and statistically. The interaction terms reveal no statistically significant difference in splitting behavior in standardized compared to non-standardized procurement.

6.2.2 Seller Selection

The second set of testable implications relates to the type of sellers to whom potentially split contracts are awarded. We investigate selective manipulation in favor of local firms, politically connected firms, and repeated sellers.

We define a firm as local if it is established in the same municipality where projects are conducted. Thus, a firm is local for some contracts and non-local for the remaining. A firm is politically connected if one of its managers is an elected politician, or has been in the previous electoral term. Until 2020, such firms were allowed to participate in procurement tenders. Although accurately observed in our setting, this is an extreme form of political connection, and thus we expect it to underestimate the full extent of political linkages. Finally, we define repeated sellers in a given year if they were awarded at least one public procurement contract in the previous two years. Panel A of Table A.9 shows the percentage of contracts awarded to the three seller groups, before and after the reform. Before the reform, 29% of the contracts are executed by local firms, around 9% by politically connected ones, and over 80% by repeated sellers. These figures remain virtually unchanged after the reform.

We estimate difference-in-differences regressions to study whether a firm f supplying contract i in year t and that belongs to one of the special interest groups SG_{ift} ($Local_{ift}$, PC_{ft}, and $Repeated_{ft}$) is more likely to be awarded a contract with price in the bunching region ($BR_{i,t}$) after the reform:

$$\begin{aligned} BR_{it} = \alpha_0 + \alpha_t + \xi SG_{ift} + \boldsymbol{\beta} Post_t \times SG_{ift} \\ + \mu_b + \eta_{CPV} + \lambda_m + \delta_c + \theta_s + \tau_p + \epsilon_{ift} \end{aligned} \tag{12}$$

The specification encompasses a high-dimensional fixed-effect structure including year (α_t), buyer type (μ_b), CPV(η_{cpv}), month(λ_m), municipality(δ_c), sector(θ_s), and awarding procedure (τ_p). We control for expected project duration and cluster the standard errors at the CPV code and municipality level. We run equation 12 on both the full sample and on the subsample of direct awards.

A positive β coefficient indicates that the probability that a contract is awarded in the bunching region after the reform increases significantly more for contracts allocated to special interest groups. Positive estimates for local and politically connected firms are consistent with favoritism. Repeating sellers can be consistent with both types of motives. However, a non-positive coefficient is unlikely due to efficiency-promoting motivations.

The evidence in Table 7 is consistent with favoritism-driven selection. Both local and politically connected firms are more likely to receive potentially split contracts at least at the 10% significance level.[23] The results are stronger for direct awards. Unlike previous evidence for Italy (Coviello et al., 2022), we find that repeated sellers are relatively less likely to be awarded contracts in the bunching region, post-reform. The fact that split contracts are more likely awarded to local firms, but not to repeated sellers, suggests that the preference to procure locally is not driven by existing procurement relationships.

The fact that the overall share of contracts received by SGs is unchanged following the reform (see Panel A of Table A.9) suggests a role for targeting of procurement contracts: local and politically connected firms were significantly more likely to get contracts specifically for prices just below the threshold.

6.2.3 Buyers' Transparency

We now zoom in on municipalities, for which we observe transparency measures. Municipalities are the largest group of buyers, representing around half of total procurement both in terms of number and value of contracts. We consider three measures based on the Municipal Transparency Index: the overall index (MTI), its public procurement score (MTI Procure-

[23]Table A.10 shows that these results are robust to alternative definitions of bunching region.

ment), and an indicator for the top 10% best performers in procurement transparency for 2017, the last pre-reform year.

First, in a contract-level approach, we restrict the sample to contracts i purchased by municipalities m and estimate versions of equation 12 interacting the post indicator with the transparency measure of interest ($Transp$). We replace municipality with district fixed-effects (δ_d), and cluster the standard errors at the CPV and district levels. We are thus exploiting cross-sectional variation within regions, accounting for shared time-invariant characteristics. The specification reads:

$$\begin{aligned} BR_{imt} = \alpha_0 + \alpha_t + \xi Transp_m + \beta Post_t \times Transp_m \\ + \eta_{CPV} + \lambda_m + \delta_d + \theta_s + \tau_p + \varepsilon_{imt} \end{aligned} \tag{13}$$

Second, for each municipality and year, we compute the share of contracts in the bunching region. Then, we regress it on the transparency measures interacted with a post-reform indicator (municipality-level approach):

$$ShareBR_{mt} = \alpha_0 + \alpha_t + \xi Transp_m + \beta Post_t \times Transp_m + \epsilon_{mt} \tag{14}$$

where $ShareBR_{m,t}$ is the share of contracts in the bunching region awarded by municipality m in year t, and λ_t are year fixed effects. In either case, negative $\hat{\beta}$ coefficients indicate a negative correlation between transparency and splitting behavior.

Table 8 shows that transparent municipalities manipulate significantly less. Higher MTI buyers are associated with less contracts in the bunching region after the reform. Focusing on the contract-level approach (Contr columns), we find that a standard deviation increase in the MTI (Column (1)) is associated with a $(0.16 \times 0.02) = 0.33\%$ reduction in the probability of awarding a contract in the bunching region, around $(0.33 \div 7.80) = 4.2\%$ of the post-reform outcome mean. The magnitude is slightly larger when focusing on direct awards (Column (2)). Focusing on the procurement component, we also find negative coefficients, but statistically insignificant at the mean. However, there is significant correlation with transparency at the top of the distribution. Municipalities with top 10% procurement transparency scores are 1.18% less likely to award a contract in the bunching region (2% when considering direct awards).

These results are largely corroborated by the municipal approach (Mun) shown in Columns (3), (6), and (9). A standard deviation increase in MTI (transparency score) is associated with a reduction in manipulation of $((0.0017 \times 0.03) \div 0.074) = 7\%$ of the post-reform outcome mean (11%, resp.). Municipalities in the top decile of procurement transparency scores manipulate 20% less than the average post-reform outcome mean. Taken together, our results support the hypotheses of favoritism rather than efficiency-promoting motivations underlying contract splitting.

6.2.4 Post-Award Performance

Finally, we test whether manipulation carries implications for contract performance. We consider four outcomes: expected duration, delays, price renegotiations, and discounts.

Ex-ante *expected duration* is measured in days and is a contractible dimension. All else equal, higher expected duration is associated with lower efficiency and greater leniency of buyers, supporting the notion that a principal can mitigate a reduction in quality by shortening contract duration (Calzolari & Spagnolo, 2020).

The three remaining outcomes reflect post-award performance. We consider delays, classifying a project as *late* if the conclusion date is after the contracted deadline, or if the justification for deadline change explicitly mentions delays.[24] More delays indicate worse procurement performance. Then, we study price changes through an indicator for contract and final price mismatches. Price adjustments are typically due to cost overruns, or early contract terminations that result in partial or no delivery of the procured item and lead to a significantly lower final price. Both renegotiation types reflect anomalies in the regular procurement process (Decarolis, 2014; Decarolis et al., 2020; Carril, 2021). Price changes can also be beneficial when the final price is lower than the contract price. It may happen due to efficiency gains in project execution or initial cost overestimation. We refer to these cases as price discounts. More and greater discounts imply more efficient procurement.

Thus, we classify price changes into three categories: a *cost overrun* indicator, capturing cost-inflating price revisions, which takes the value of 1 when the final price exceeds the anticipated price by 15% or more; an *incomplete project* indicator, capturing early terminations and incomplete projects, equal to 1 if the justification explicitly mentions "contract termination" or "fewer works than procured" or if the final price is lower than the contract price by more than 25%; and a *discount* indicator, capturing relatively small price differences arising from seller efficiency or cost overestimation, equal to 1 if the final price is between 90% and 100% of the contract price.

[24] To avoid classifying marginal deviations as late contracts, we classify a project as late only if the date of conclusion is beyond the agreed deadline by at least 10% of the expected duration. The list of expressions used for this classification can be found in Table B.3.

Panel B of Table A.9 shows the aggregate descriptive statistics for the contract performance measures, before and after the reform. Following the reform, the statistics point to an aggregate deterioration of contract execution. The share of late contracts increases significantly, as well as expected contract duration. A small decrease in price changes is driven by a sizable reduction in discounts. Post-reform reductions in incomplete projects and cost overruns are less pronounced. Cost overruns are generally rare.

We are interested in comparing the outcomes of contracts in the bunching region with those of remaining contracts. We regress the outcome of interest, y_{it}, on the bunching region BR_{it} indicator, year fixed-effects λ_t, and an interaction term between BR_{it} and the post-reform indicator, $Post_t$. The effect of manipulation (bunching region) on the evolution of outcomes must be isolated from the effects of discretion (any price below the threshold). To that end, we include an indicator and interaction term for contracts below the new direct award thresholds ($Discretion = 1$). Both groups of contracts are compared to the competitive restricted auctions, i.e., contracts with price above the thresholds. The specification is presented in equation 15, and it includes fixed effects for year (α_t) CPV code (η_{CPV}), buyer (μ_b), month (λ_m), municipality (δ_c), sector (θ_s), and awarding procedure (τ_p). We control for project's price for precision and cluster the standard errors at CPV code and buyer type level:

$$\begin{aligned} y_{it} = \alpha_0 + \alpha_t + \beta_0 Post_t + \beta_1 BR_{it} + \beta_2 BR_{it} \times Post_t + \beta_3 Discretion_{it} \\ + \beta_4 Discretion_{it} \times Post_t + \eta_{cpv} + \mu_b + \lambda_m + \theta_s + \tau_p + \varepsilon_{it} \end{aligned} \tag{15}$$

The coefficient β_0 measures the aggregate evolution in the performance measure over time. The coefficient β_4 measures the evolution in outcomes of contracts eligible for discretion but not in the bunching region, relative to competitive contracts. Finally, β_2 is the differential effect for contracts in the bunching region. The quality of contract execution in the bunching region is informative about the effects of manipulation.

The first row of Table 9 documents some deterioration in contract quality. Not only contracts become longer, but also delays happen more frequently. Additionally, buyers are paying the full contract price more often (fewer discounts). The second row shows how contracts eligible for discretion evolve relative to projects awarded through restricted auctions. There is suggestive evidence that discretion may be beneficial: the increase in expected duration of projects is significantly lower for contracts awarded at discretion by 73% ($-13.24/18.09$). The increase in delays is reduced by 14% ($0.010/0.073$) and the reduction in discounts by

39% (0.007/0.018), although non statistically significant at conventional levels. Contracts in the bunching region become longer relative to the remaining discretion-eligible contracts, and this does not translate into fewer delays. Manipulation into the bunching region decreases the duration-associated benefit of discretion by around 67% ($\frac{(-13.24+8.82)/18.09}{(-13.24/18.09)} - 1$). The reduction in discounts in the bunching region is amplified by 37% ($-0.007/0.019$).

Altogether, the evidence from the different testable hypotheses supports favoritism over efficiency-promoting motives for manipulation, in a context where contract splitting is its dominant mechanism. We find that contract splitting is present in standardized procurement, where non-contractible quality is a less relevant concern. We also find no evidence of manipulated contracts being especially awarded to a small set of repeated sellers, as implied by relational quality goals. In turn, we find that manipulation allows seller selection, as contracts are more likely awarded to local and politically connected businesses, associated with favoritism. Consistently, less transparent buyers manipulate more. Finally, we present indicative evidence that manipulation is associated with worse post-award performance.

7 Conclusion

This paper provides strong evidence of the widespread role of contract splitting in public procurement. This finding challenges existing perspectives on manipulation in public procurement and offers crucial insights for both academic research and economic policy. Understanding the prevalence of contract splitting is essential for designing more effective procurement systems that curb manipulation and enhance transparency.

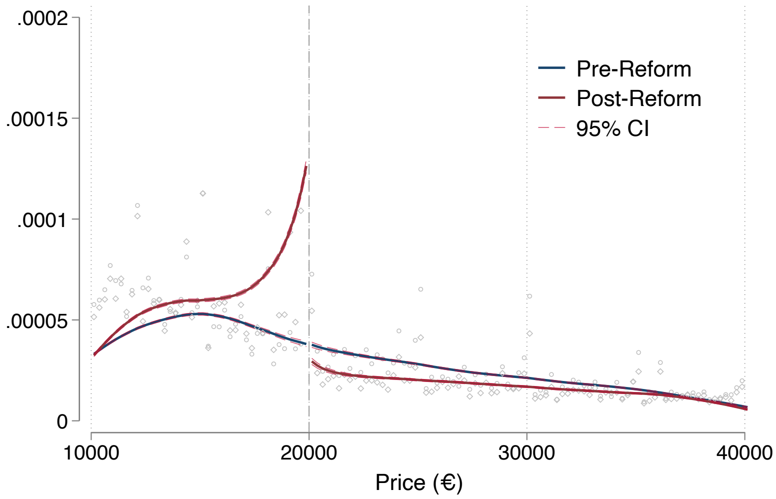
Exploiting a reform that significantly reduced the discretion threshold for procurement contracts in Portugal, we find that contract splitting is a dominant mechanism of manipulation, particularly for the procurement of divisible goods and services. Following the reduction in discretion limits, buyers procure the same value through a significantly larger number of contracts. Many of these contracts' prices fall in the left vicinity of the threshold, and bunching arises. We show that contract splitting arises due to a public entities' preference to retain and use discretionary power.

We find that splitting is not aimed at building efficiency-improving relationships with sellers or enhancing non-contractible quality. Additionally, buyers with lower transparency scores manipulate more. Buyers' manipulation into discretion is used to select sellers, which in turn influences procurement outcomes. Split contracts are more likely awarded to local and politically connected firms, and exhibit worse procurement performance. Altogether, our evidence supports favoritism over efficiency-promoting motives for contract splitting.

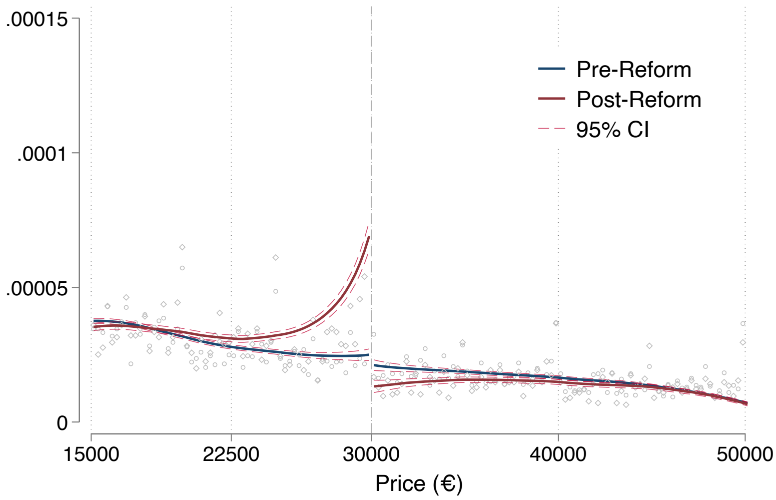
Contract splitting carries relevant methodological implications for the measurement of manipulation that have been overlooked in the literature. We show that splitting implies a failure of the bounded manipulation assumption, introducing bias in bunching estimators based on interpolated counterfactuals. Our results highlight the importance of testing for splitting when quantifying manipulation, and recommend using non-manipulated data to build counterfactuals. Adapting interpolated counterfactual estimators to account for splitting is an important future research avenue. These conclusions are extremely relevant in the context of public procurement, but extend to other settings where splitting in the running variable is plausible.

8 Figures

Figure 1: McCrary (2006) Tests



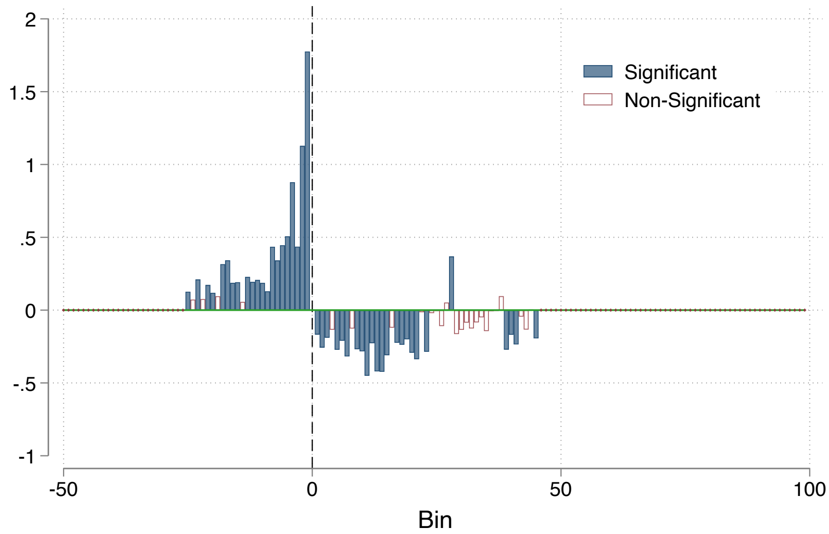
(a) Goods and Services



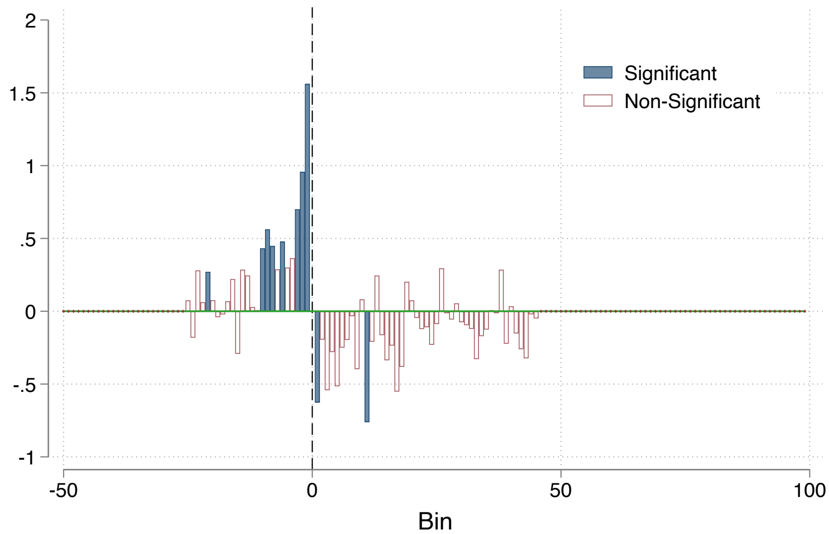
(b) Construction

Notes: This figure shows graphical evidence of McCrary (2008) tests around the new discretion thresholds before (2015–2017, in blue) and after (2018–2019, in red) the reform. The dots represent binned densities (width of €250). The solid lines are kernel estimates and the dashed lines are the respective 95% confidence intervals. The null of continuity of the density around the threshold is rejected if confidence intervals on both sides of the threshold do not overlap.

Figure 2: Distributional Changes



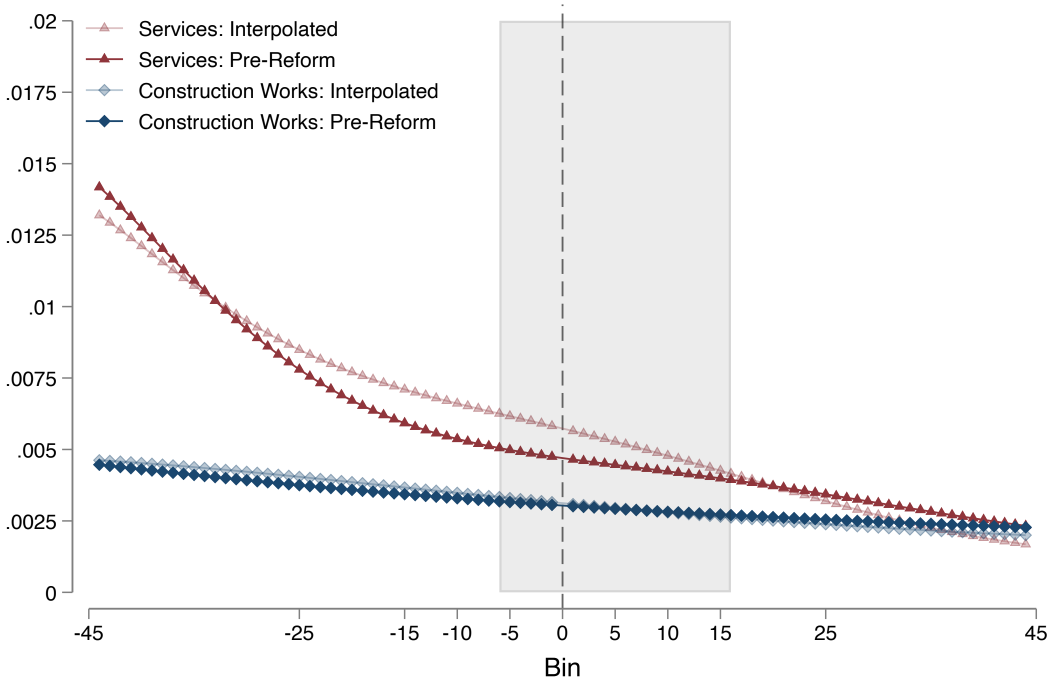
(a) Goods and Services



(b) Construction

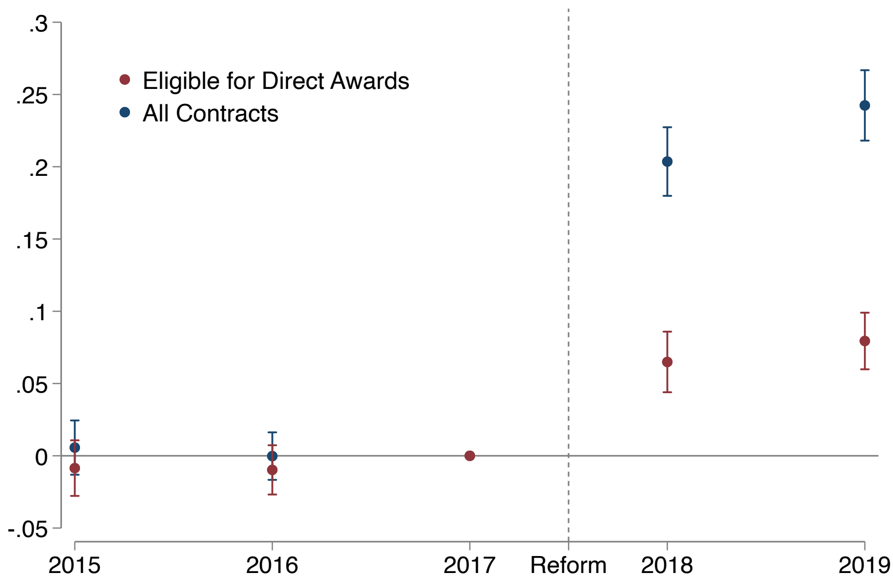
Notes: This figure shows graphical evidence of reform-induced distributional changes. The bars correspond to the coefficients $\widehat{\gamma}^L$ and $\widehat{\delta}^R$ in equation 5. The blue bars represent significant differences in the corresponding bin density, relative to the pre-reform counterfactual. The unfilled red bars represent coefficients statistically indistinguishable from 0 at the 5% significance level.

Figure 3: Interpolated and Pre-Reform Counterfactuals by Sector



Notes: This figure shows densities for services and construction works under the interpolated (transparent line) and pre-reform (solid line) counterfactuals. Services (red) is the splitting intensive sector. Construction works (blue) is the sector where splitting is negligible. The excluded region in the interpolation exercise is shown in shaded gray.

Figure 4: Use of Direct Awards in the Bunching Region



Notes: This figure plots yearly estimates $\hat{\beta}$ from equation 10 and the corresponding 95% confidence intervals. We consider two samples: either contracts eligible for direct awards (below the new threshold) or all contracts.

9 Tables

Table 1: Descriptive Statistics

	Pre-Reform				Post-Reform			
	Nr	%	Value	%	Nr	%	Value	%
Sector								
Goods	13 144	31.8%	294.9	24.4%	14 657	32.4%	337.2	27.3%
Services	21 093	51.0%	497.2	46.8%	25 167	55.6%	577.7	52.6%
Construction	7 134	17.2%	416.3	34.5%	5 472	13.1%	318.7	25.8%
Below New Thresholds								
Goods and Services	20736	60.6%	226.8	28.6%	25862	64.9%	307.5	33.6%
Construction	2679	37.6%	50	12.0%	2205	40.3 %	44.7	14.0%
Awarding Procedure								
Direct Award	38 809	93.8%	1100.8	91.1%	26 170	57.8%	498.3	40.4%
Restricted Auction	n/a	-	n/a	-	15 231	33.6%	580.1	47.0%
Open Auction	2 562	6.2%	107.5	8.9%	3 895	8.6%	155.2	12.6%
Buyer Type								
Municipality	20 151	48.7%	663.2	54.9%	23 534	52.0%	686.8	55.7%
Parish	2 285	5.5%	53.1	4.4%	2 095	4.6%	44.0	3.6%
Hospital	5 890	14.2%	128.9	10.7%	5 602	12.4%	129.1	10.5%
Other State Entities	3 444	8.3%	94.1	7.8%	3 996	8.8%	103.0	8.4%
Remaining	9 602	23.2%	269.1	22.3%	10 070	22.2%	270.6	21.9%

Notes: This table shows yearly average descriptive statistics for the pre- (2015-2017) and post-reform (2018-2019) periods. The sample includes all procurement contracts with price larger than €5 000 for goods and services and €10 000 for construction, and lower than the open auction thresholds. The values are in million €. New direct awards thresholds are €20 000 for goods and services, and 30 000 for construction. Percentages in this category are shares of respective sector. Remaining buyer types include "Other Firms", "Municipality Associations", "Higher Education", "State", "Justice", "Military", and "Professional Licensing Bodies".

Table 2: Mass Differences

	Goods and Services	Construction
Absolute Mass Difference ($\hat{D}$)	1659.78	186.38
	[91.61]	[34.49]
Relative Mass Difference ($\hat{d}$)	0.516	0.388
	[0.032]	[0.079]

Notes: This table presents estimates of absolute and relative mass difference. Absolute mass difference is defined as the difference between the estimated excess number of contracts in the bunching region and the estimated number of missing contracts in the manipulation region to the right of the threshold. The relative mass difference measures absolute difference as a share of baseline pre-reform number of contracts over the whole manipulation region. Bootstrapped standard errors in parentheses.

Table 3: Procurement Composition

Panel A: All Contracts

	Total Value	Nr Conts	Avg Value	Nr Sellers	Nr Conts BR	% Conts BR
	(1)	(2)	(3)	(4)	(5)	(6)
Post	8 275	0.694***	−4 075***	0.614***	0.789***	0.401***
	[5 494]	[0.141]	[643]	[0.101]	[0.025]	[0.015]
Pre-Reform Mean	113 008	4.069	29 356	3.342	0.142	0.046
Nr PNs	3 140	3 140	3 140	3 140	3 140	3 140
Observations	9 801	9 801	9 801	9 801	9 801	9 801
Panel B: Goods						
Post	10657	0.684	-2871***	0.543**	0.735***	0.429***
	[8 800]	[0.433]	[853]	[0.229]	[0.033]	[0.040]
Pre-Reform Mean	110324	4.939	25255	3.418	0.180	0.044
Nr PNs	706	706	706	706	706	706
Observations	2 118	2 118	2 118	2 118	2 118	2 118
Panel C: Services						
Post	12 258***	0.750**	−3 369***	−0,704***	0.818***	0.399***
	[2 953]	[0.106]	[429]	[0.097]	[0.036]	[0.017]
Pre-Reform Mean	88716	3.701	25982	3.287	0.141	0.051
Nr PNs	2 113	2 113	2 113	2 113	2 113	2 113
Observations	6 617	6 617	6 617	6 617	6 617	6 617
Panel D: Construction						
Post	-21240	-0.347	−10 820***	0.171	0.724***	0.369***
	[31 089]	[0.620]	[1126]	[0.294]	[0.024]	[0.045]
Pre-Reform Mean	261949	4.682	56815	3.557	0.083	0.019
Nr PNs	306	306	306	306	306	306
Observations	1 027	1 027	1 027	1 027	1 027	1 027
Panel E: Construction Works						
Post	-44600***	-0.258	−11 460***	-0.174	0.723***	0.428***
	[11 547]	[0.153]	[1192]	[0.100]	[0.036]	[0.017]
Pre-Reform Mean	197423	3.125	58502	2.574	0.057	0.019
Nr PNs	212	212	212	212	212	212
Observations	687	687	687	687	687	687

Notes: This table presents the results for effect of the reform on the composition of Procurement Needs (PN) (within-PN effects). PN defined as a buyer-product-sector combination. Unit of observation is a PN - year. Sample includes PNs with at least one contract in the bunching region (BR), after the reform. BR defined as the interval between the post-reform thresholds and 1500€ below. Panel A presents the results for all PNs. Panel B, C, and D show the results for PNs in goods, services, and construction, respectively. Panel E isolates construction works (i.e., excluding construction related services), the less divisible group. Standard errors clustered at the buyer and and CPV group level in parentheses.
Significance levels: 0.1* 0.05** 0.01***

Table 4: Procurement Composition: Robustness

Panel A: Recurring Procurement Needs

	Total Value	Nr Conts	Avg Value	Nr Sellers	Nr Conts BR	% Conts BR
	(1)	(2)	(3)	(4)	(5)	(6)
Post	4 695	0.740**	−2 038***	0.759***	0.714***	0.195***
	[14 493]	[0.364]	[667]	[0.279]	[0.058]	[0.011]
Pre-Reform Mean	202938	7.263	29070	5.817	0.238	0.037
PN Fixed Effects	✓	✓	✓	✓	✓	✓
Nr PNs	547	547	547	547	547	547
Observations	2 735	2 735	2 735	2 735	2 735	2 735

Panel B: Short Duration Contracts

	Total Value	Nr Conts	Avg Value	Nr Sellers	Nr Conts BR	% Conts BR
	(1)	(2)	(3)	(4)	(5)	(6)
Post	6 556	0.718**	−4 326***	0.565***	0.791***	0.400***
	[7 698]	[0.189]	[927]	[0.110]	[0.027]	[0.017]
Pre-Reform Mean	117790	3.978	30174	3.145	0.117	0.039
PN Fixed Effects	✓	✓	✓	✓	✓	✓
Nr PNs	2 003	2 003	2 003	2 003	2 003	2 003
Observations	6 149	6 149	6 149	6 149	6 149	6 149

Notes: This table presents the results for effect of the reform on the composition of Procurement Needs (PN) (within-PN effects). PN defined as a buyer-product-sector combination. Unit of observation is a PN - year. Sample includes PNs with at least one contract in the bunching region (BR), after the reform. BR defined as the interval between the post-reform thresholds and 1500€ below. Panel A includes PNs with at least one purchase every year. Panel B includes contracts with duration of at most one year. Standard errors clustered at the buyer and and CPV group level in parentheses.
Significance levels: 0.1* 0.05** 0.01***

Table 5: Bunching Bias

	Services			Construction Works		
	$\hat{\delta}^{pre}$	$\hat{\delta}^{inter}$	$bias$	$\hat{\delta}^{pre}$	$\hat{\delta}^{inter}$	$bias$
Bin_{-1}	9.48	7.22	-0.238	9.50	8.86	-0.067
Bin_{-2}	3.62	2.74	-0.241	2.64	2.45	-0.070
Bin_{-3}	1.43	1.08	-0.243	1.89	1.75	-0.073
Bin_{-4}	3.29	2.49	-0.245	1.46	1.34	-0.076
Bin_{-5}	1.47	1.11	-0.246	2.03	1.87	-0.079
Bin_{-6}	1.52	1.14	-0.246	1.26	1.16	-0.081
Bias		-0.243			-0.074	
CI 95%		[-0.221, -0.267]			[-0.015, -0.127]	

Notes: This table presents estimates of bin-specific and overall bias. Coefficients on bin-specific bunching are estimated from equations 7 and 8. The left-side panel shows results for services, where splitting dominates, and the right-side one for construction works, where splitting is negligible. Bin-specific *bias* is computed as $\frac{\hat{\delta}^{inter}}{\hat{\delta}^{pre}} - 1$. *Bias* is given by the average of bin-specific bias. Bootstrapped 95% confidence intervals in parentheses.

Table 6: Procurement Composition: Standardized Procurement

	Total Value		Nr Conts		Avg Val		Nr Sellers		Nr Conts BR		% Conts BR	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Post	2 392	8 489	0.464**	0.703***	-6 738***	-3 973***	0.384**	0.623**	0.704***	0.793***	0.492***	0.398***
	[6 498]	[5 749]	[0.207]	[0.148]	[2 396]	[659]	[0.183]	[0.107]	[0.045]	[0.025]	[0.058]	[0.015]
Post×SP		-5 287		-0.226		-2 653		-0.223		-0.093**		0.087
		[9 102]		[0.272]		[2 310]		[0.228]		[0.047]		[0.055]
Pre-Reform Mean	46527	113008	1.582	4.069	30519	29356	1.418	3.342	0.103	0.142	0.074	0.046
PN Fixed Effects	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Nr PNs	155	3 140	155	3 140	155	3 140	155	3 140	155	3 140	155	3 140
Observations	404	9 801	404	9 801	404	9 801	404	9 801	404	9 801	404	9 801

Notes: This table presents the results for effect of the reform on the composition of Procurement Needs (PN) (within-PN effects) for standardized procurement. PN defined as a buyer-product-sector combination. Unit of observation is a PN - year. Sample includes PNs with at least one contract in the bunching region (BR), after the reform. BR defined as the interval between the post-reform thresholds and 1500€ below. standardized procurements are standard and homogeneous products, defined according to (Bandiera et al., 2009). Odd columns include only standardized product PNs. Even columns include all PNs, and study differential post-reform effects for standardized PNs. Standard errors clustered at the buyer and and CPV group level in parentheses.
Significance levels: 0.1* 0.05** 0.01***

Table 7: Selective Manipulation

	Local		Politically Connected		Repeated Sellers	
	(1)	(2)	(3)	(4)	(5)	(6)
SG	-0.0042**	-0.0045*	-0.0038*	-0.0038*	0.0040**	0.0037*
	[0.0018]	[0.0019]	[0.0018]	[0.0019]	[0.0020]	[0.0021]
Post × SG	0.0149***	0.0206***	0.0111**	0.0145*	-0.0189***	-0.0239***
	[0.0034]	[0.0047]	[0.0055]	[0.0081]	[0.0053]	[0.0068]
Sample	All	DA	All	DA	All	DA
Observations	163 037	129 913	198 722	162 345	198 694	162 331

Notes: This table presents results on post-reform change in contracts awarded to special groups (SG) for prices in the bunching region (BR). Outcome in all regressions is indicator for contract in the BR, defined as the interval between the post-reform threshold and 1500€ below. SG is the special interest group under consideration, and is indicated on top of each column. Sample indicates whether all contracts were used, or only direct awards (DA). Regressions include year, buyer type, CPV code, awarding procedure and execution municipality fixed-effects. Standard errors clustered at the CPV code and execution district in parentheses. Standard errors clustered at the CPV code and execution municipality in parentheses.
Significance levels: 0.1* 0.05** 0.01***

Table 8: Buyers' Transparency

	MTI			Procurement			Top 10% Procurement		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Transparency	-0.00	-0.01	0.01	-0.00	-0.00	0.00	0.19	0.01	0.35
	[0.00]	[0.01]	[0.01]	[0.01]	[0.00]	[0.00]	[0.29]	[0.00]	[0.25]
Post $\times$ Transparency	-0.02**	-0.03*	-0.03**	-0.01	-0.01	-0.02**	-1.18**	-2.04**	-1.55*
	[0.01]	[0.02]	[0.01]	[0.00]	[0.01]	[0.00]	[0.48]	[0.91]	[0.67]
Approach	Contr	Contr	Mun	Contr	Contr	Mun	Contr	Contr	Mun
Sample	All	DA	All	All	DA	All	All	DA	All
Outcome Mean (Post)	0.078	0.111	0.074	0.078	0.111	0.074	0.078	0.111	0.074
Transparency SD	0.16	0.16	0.17	0.32	0.32	0.40			
Observations	101 649	81 491	1 539	101 649	81 491	1 539	101 649	81 491	1 539

Notes: This table presents results on post-reform change in contracts awarded for prices in the bunching region (BR) by baseline Transparency. Outcome in all regressions is indicator for contract in the BR, defined as the interval between the post-reform threshold and 1500€ below. The transparency measure under consideration in the regression is indicated on top of each column. MTI is the municipal transparency index. Procurement is the procurement component of the MTI. Top 10% Procurement is an indicator for municipalities in the top decile of procurement transparency. Approach indicates the exercise between contract-level approach (Contr) and municipality approach (Mun) Sample indicates whether all contracts were used, or only direct awards (DA). Regressions include year, month, CPV code, awarding procedure and execution district fixed-effects. Standard errors clustered at the CPV code and execution district in parentheses.
Significance levels: 0.1* 0.05** 0.01***

Table 9: Post-Award Performance

	Expected Duration		Late		Price Changes		Cost Overruns		Discounts		Incomplete Projects	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Post	18.09***	-	0.073***	-	-0.024*	-	-0.003	-	-0.018***	-	-0.003	-
	[4.08]		[0.009]		[0.013]		[0.002]		[0.005]		[0.006]	
Post × Discretion	-13.24***	-13.29***	-0.010	-0.010	-0.002	-0.002	-0.000	-0.000	0.007*	0.007*	-0.003	-0.003
	[3.73]	[3.74]	[0.017]	[0.018]	[0.009]	[0.009]	[0.000]	[0.000]	[0.004]	[0.004]	[0.004]	[0.004]
Post × BR	8.82**	8.91**	0.015	0.016	-0.016***	-0.016***	0.002	0.002	-0.008***	-0.008***	-0.002	-0.002
	[3.92]	[3.88]	[0.013]	[0.013]	[0.003]	[0.003]	[0.001]	[0.001]	[0.002]	[0.002]	[0.003]	[0.003]
Year FE	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
N	198 153		60 594		198 153		198 153		198 153		198 153	

Notes: This table shows results on relative evolution of post-award performance, following the reform. Uneven columns include year fixed-effects, while even columns include the *Post* dummy. Outcome under consideration is indicated on top of each column. Post indicates periods after the reform. Discretion indicates contracts for values below the new discretion threshold (i.e., less than €20 000 for goods and services and less than €30 000 for construction). Bunching region (BR) defined as the interval between the post-reform threshold and 1500€ below. Regressions include year, month, CPV code, buyer, sector, awarding procedure and execution municipality fixed-effects. Standard errors clustered at the CPV and buyer type level in parentheses. Significance levels: 0.1* 0.05** 0.01***

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Contract Splitting in Public Procurement

Online Appendix

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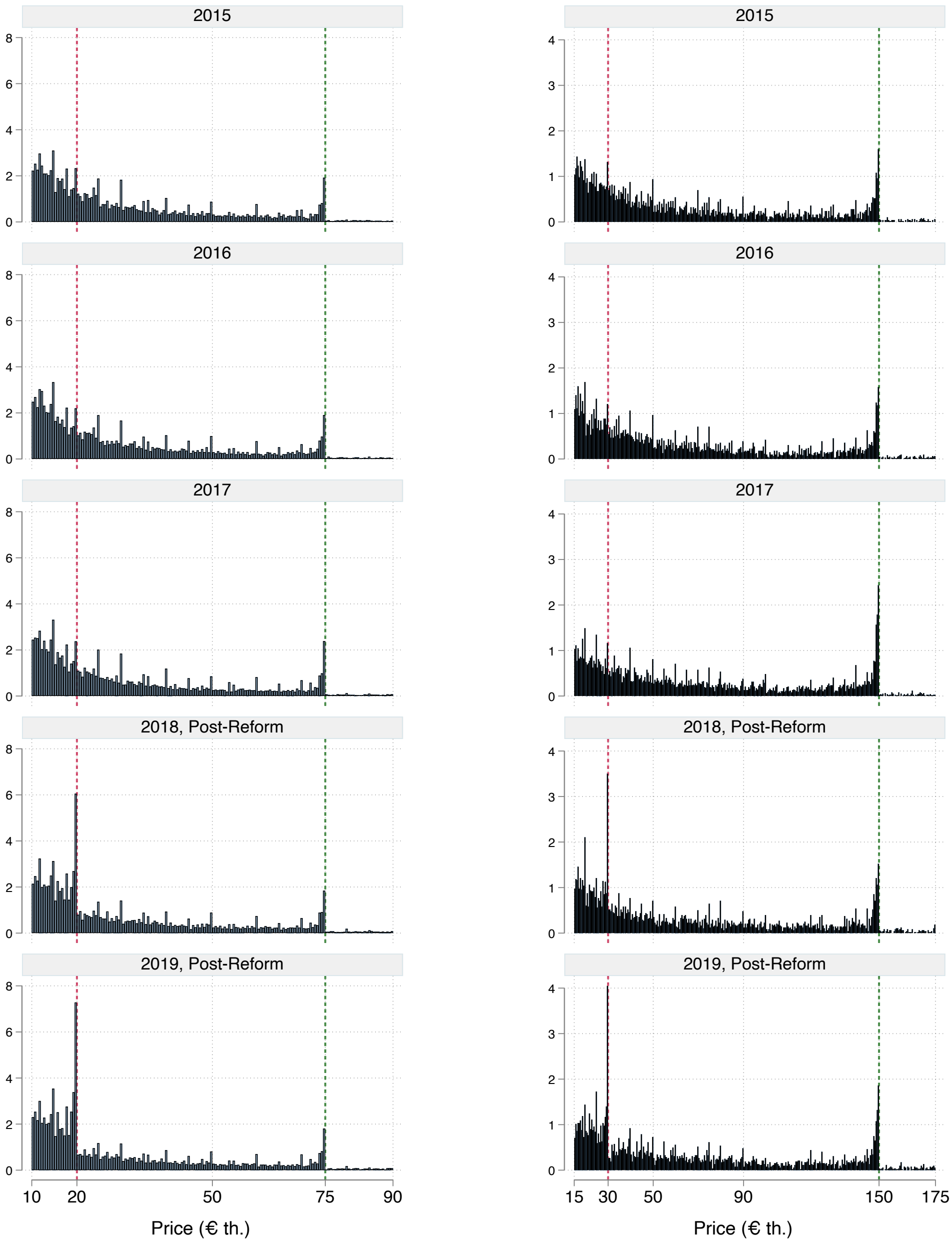
Appendix A Figures and Tables

Figure A.1: Contract Price Yearly Distribution



(a) Goods and Services (b) Construction

Notes: This figure shows yearly contract price distributions. The bars represent binned densities (width of €250). Dashed red and green vertical lines represent the pre- and post-reform direct awards thresholds, respectively: €75 000 (pre) and €20 000 (post) for goods and services, and €150 000 (pre) and €30 000 (post) for construction.

Table A.1: Change in Thresholds and Awarding Procedures

	Direct Award	Restricted Auction	Open Auction
Goods and Services			
Pre-Reform	[€0, €75 000[	-	[€75 000, ∞[
Post-Reform	[€0, €20 000[	[€20 000, €75 000[	
Construction			
Pre-Reform	[€0, €150 000[	-	[€150 000, ∞[
Post-Reform	[€0, €30 000[	[€30 000, €150 000[	

Notes: This table shows the anticipated price ranges in which the respective procedures apply, before and after the reform. The reform was introduced by the Decree-Law nº 111-B/2017 of August 31st, effective from January1st, 2018.

Table A.2: Bunching Estimation: Interpolated Counterfactual Approach

	Goods and Services			Goods		Services		Construction		
	$\widehat{b}$	se	$\widehat{B}$	$\widehat{b}$	se	$\widehat{b}$	se	$\widehat{b}$	se	$\widehat{B}$
Pre-Reform										
2015	-0.20	(0.22)	-32	-0.02	(0.36)	-0.31	(0.28)	1.42**	(0.73)	25
2016	-0.10	(0.23)	-17	0.07	(0.40)	-0.19	(0.27)	1.64***	(0.66)	36
2017	0.19	(0.21)	37	1.13***	(0.41)	-0.37	(0.25)	1.22**	(0.57)	30
Post-Reform										
2018	6.91***	(0.33)	1567	4.92***	(0.50)	7.80***	(0.42)	9.64***	(1.22)	160
2019	10.01***	(0.39)	2258	7.42***	(0.61)	11.11***	(0.49)	11.29***	(1.17)	224

Notes: This table shows the buncihng estimates from the interpolated counterfactual approach. First column $\hat{b}$ of each sector shows the estimates of excess mass in the bunching over the average counterfactual mass in the same range, as defined in equation 3. The third column $\hat{B}$ shows the estimate for the excess number of contracts in the bunching region below the threshold, as defined in equation 3. Bootstrapped standard errors in parentheses.
Significance levels: 0.1* 0.05** 0.01***

Table A.3: Bunching Estimation: Pre-Reform Counterfactual

	Bin_{-1}	Bin_{-2}	Bin_{-3}	Bin_{-4}	Bin_{-5}	Bin_{-6}
Goods and Services	4.89***	2.08***	0.54***	1.40***	0.66***	0.56***
	(0.07)	(0.04)	(0.02)	(0.03)	(0.02)	(0.02)
Goods	2.85***	0.99**	0.30***	0.63***	0.54***	0.44***
	(0.04)	(0.02)	(0.01)	(0.02)	(0.02)	(0.02)
Services	5.44***	1.27***	1.19***	1.52***	2.48***	-0.05
	(0.11)	(0.26)	(0.09)	(0.10)	(0.16)	(0.03)
Construction	3.78***	1.61***	1.02***	0.44***	0.35***	0.62***
	(0.07)	(0.04)	(0.03)	(0.02)	(0.02)	(0.02)

Notes: This table shows the bunching estimation results from the pre-reform approach, as in Palguta and Pertold (2017). Coefficients are shown as $exp(\widehat{\gamma}_i) - 1$ and are interpreted as average post-reform $\times 100\%$ increase in number of contracts in bin i (. bins width of €250). All specifications include year and bin fixed effects. Standard errors clustered at the bin level in parentheses.
Significance levels: 0.1* 0.05** 0.01***

Table A.4: PN Descriptive Statistics

	Full Sample		Restricted Sample	
	Nr	%	Nr	%
Total	83532		5217	
Goods and Services	70771	84.7	4727	90.6
Construction	12761	15.3	490	9.4
Every Year	1814	2.2	504	9.7
Number Contracts	1.65		3.44	
Number Sellers	1.50		2.89	
Largest CPV Divisions:				
45 - Construction Work	12121	14.5	467	9.0
79 - Law and Business Consultancies	8420	10.1	872	16.7
71 - Architecture, Engeneering	6368	7.6	632	12.1
72 - Information Technologies	4982	6.0	345	6.6
50 - Repair and Maintenance	4547	5.4	258	4.9

Notes: This table shows the descriptive statistics for Procurement Needs (PN) with at least one contract in the bunching region following the reform, and all PNs. PN defined as a buyer-product-sector combination. The restricted sample includes only those PNs with at least one contract in the BR following the reform. BR defined as the price interval between the new threshold - €20 000 for Goods and Services and €30 000 for Construction - and €1 500 less. Number of contracts and number of sellers indicate the average number of contracts and sellers per PN. Every Year indicates PNs that occur every year. We show the most prevalent CPV divisions, defined by the first two digits in the CPV code.

Table A.5: Implications Under Contract Shifting and Splitting

	Contract Shifting	Contract Splitting
Total Value Purchased ($\hat{\gamma}^{TV}$)	< 0	≥ 0
Number of Contracts ($\hat{\gamma}^{NC}$)	$= 0$	> 0
Average Contract Value ($\hat{\gamma}^{AV}$)	< 0, small	< 0, large

Notes: This table summarized the hypothesis for the empirical contract splitting tests. $\hat{\gamma}$ are the estimated coefficients from regressions of the respective outcome on a post-reform indicator and Procurement Need fixed effects, according to equation 6 for the respective outcome. PN defined as a buyer-product-sector combination.

Table A.6: Contract Splitting: Bunching Region Sensitivity

	Total Value	Nr Conts	Avg Value	Nr Sellers	Nr Conts BR	% Conts BR
	(1)	(2)	(3)	(4)	(5)	(6)
$[-750, 0[$	10 685	0.826***	−4 264***	0.725***	0.792***	0.400***
$[-1000, 0[$	9 796	0.760***	−4 122***	0.693***	0.796***	0.402***
$[-1250, 0[$	8 619	0.716***	−4 012***	0.631***	60.792***	0.403***
$\mathbf{[-1500, 0[}$	**8 275**	**0.694*****	**−4 075*****	**0.789*****	**0.614*****	**0.401*****
baseline	**[−2 556, 19 106]**	**[0.42, 0.97]**	**[−5 342, −2 809]**	**[0.41, 0.81]**	**[0.74, 0.84]**	**[0.37, 0.43]**
$[-1750, 0[$	7 026	0.641***	−3 997***	0.569***	0.788***	0.402***
$[-2000, 0[$	6 543	0.601***	−4 039***	0.547***	0.793***	0.402***
$[-2250, 0[$	6 136	0.556***	−3 904***	0.524***	0.786***	0.399***

Notes: This table shows the sensitivity of the procurement composition results to different definitions of bunching region (BR). Baseline BR in bold. The first row shows the definition of the BR considered in the regressions. Outcome of each regression indicated on top of each column. Unit of observation is a Procurement Need (PN) - year. PN defined as a buyer-product-sector combination. PNs with at least one contract in the bunching region after the reform are included. Standard errors clustered at the buyer and CPV group level in parentheses.
Significance levels: 0.1* 0.05** 0.01***

Table A.7: Bunching Bias: Robustness Exercises

Panel A: Polynomial Order

	$q = 8$	$q = 10$	$q = 12$	$q = 14$	$q = 16$
Services	-0.164	-0.192	-0.243	-0.276	-0.277
Construction	-0.088	-0.069	-0.074	-0.079	-0.079

Panel B: Left Bins Excluded

	$R_{left} = 2$	$R_{left} = 4$	$R_{left} = 6$	$R_{left} = 8$	$R_{left} = 10$
Services	-0.302	-0.250	-0.243	-0.163	-0.162
Construction	-0.112	-0.095	-0.074	-0.054	-0.033

Panel C: Right Bins Excluded

	$R_{right} = 12$	$R_{right} = 14$	$R_{right} = 16$	$R_{right} = 18$	$R_{right} = 20$
Services	-0.191	-0.217	-0.243	-0.255	-0.260
Construction	-0.065	-0.068	-0.074	-0.084	-0.069

Notes: This table shows the results of the robustness exercises for the bunching bias estimates. Panel A shows the sensitivity of the bias to different polynomial orders in the specification for imputed counterfactual approach. Panel B shows sensitivity to excluding different bins to the left of the threshold (R_{left}), i.e., the specified bunching region. Panel C varies the excluded bins to the region to the right of the threshold over which manipulation takes place (R_{right}). Services is the splitting-intensive sector. Construction works is the sector where projects are indivisible and splitting is negligible.

Table A.8: Splitting Motivations - Testable Implications

	Efficiency-Promoting	Favoritism
Standardized Procurement	- or null	$\sim$
Seller Selection		
Local Firms	$\sim$	+
Politically Connected Firms	$\sim$	+
Frequent Sellers	+	$\sim$
Buyers Transparency		
Corr(Manipulation, Transparency)	+ or null	-
Post Award Performance	+	-

Notes: This table summarizes the testable implications for the different motivations to manipulate contracts. Implications on seller selection and post-award performance reflect expected effect on potentially split contracts. $\sim$ refers to no implication on the direction of the effect.

Table A.9: Descriptives - Splitting Motivations

	Pre-Reform			Post-Reform		
	Mean	*SD*	*N*	Mean	*SD*	*N*
Seller Selection						
Local *(%)*	28.67	45.22	102 589	27.95	44.88	73 663
Politically Connected *(%)*	8.60	28.04	124 112	8.23	27.48	90 591
Repeated Seller *(%)*	81.59	38.14	124 091	81.59	38.75	90 584
Post-Award Performance						
Expected length *(days)*	245.09	349.54	124 112	269.77	366.78	90 591
Late *(%)*	32.41	46.80	39 687	45.13	49.76	26 216
Price changes *(%)*	8.84	28.39	124 112	6.48	24.62	90 591
Cost Overruns *(%)*	0.62	7.85	124 112	0.38	6.05	90 591
Incomplete Projects *(%)*	2.96	16.94	124 112	2.43	15.39	90 591
Discounts *(%)*	3.09	17.28	124 112	1.94	13.78	90 591

Notes: This table shows descriptive statistics for seller characteristics and procurement performance outcomes. Values are yearly averages. Pre-Reform Period: 2015-2017. Post-Reform Period: 2018-2019. **Seller selection:** A seller is classified as *Local* if established in the same municipality where procurement project is to be carried. *Politically Connected* sellers have one current or ex-elected official as manager. *Repeated sellers* have received at least one procurement contract over the previous two years. **Post-Award Performance:** *Expected length* is the number of days for project delivery established by contract. *Late* is an indicator for whether project conclusion is after the deadline established by contract, or whether the contract publication includes explicit references to delays. *Price changes* is an indicator taking the value one if the total price paid by the buyers differs from the project's contract price. *Cost Overruns* equals one if the price change is large ($\geq$ 125%) and positive. *Incomplete projects* equals one if the final price is significantly lower ($<$ 75%) than the contract price or if there is explicit mention of contract termination. *Discounts* takes value one if final price paid is between 90% and 100% of the contract price.

Table A.10: Selective Manipulation: Bunching Region Sensitivity

Post* × *SG *by Bunching Region*	**Local** (1)	**Politically Connected** (2)	**Repeated Sellers** (3)
$[-750, 0[$	0.0122***	0.0104**	-0.0134***
$[-1000, 0[$	0.0133***	0.0109**	-0.0186***
$[-1250, 0[$	0.0140***	0.0108**	-0.0182***
$\mathbf{[-1500, 0[}$	**0.0149*****	**0.0111****	**-0.0189*****
baseline	$\mathbf{[0.0083, 0.0216]}$	$\mathbf{[0.0002, 0.0220]}$	$\mathbf{[-0.0293, -.0086]}$
$[-1750, 0[$	0.0151***	0.0117**	-0.0188***
$[-2000, 0[$	0.0160***	0.0126**	-0.0225***
$[-2250, 0[$	0.0163***	0.0114*	-0.0231***

Notes: This table shows the sensitivity of the selective manipulation results to different bunching region (BR) definitions. Baseline BR in bold. The first row shows the definition of the BR considered in the regressions. Outcome in all regressions is indicator for contract in the BR. SG is the special interest group under consideration, indicated on top of each column. Post×SG is the shown coefficient of interest, measuring how the probability of awarding a contract for price in the bunching region after the reform changed differentially for the group of interest. 95% confidence intervals from standard errors clustered at the CPV code and execution municipality in parentheses.
Significance levels: 0.1* 0.05** 0.01***

Appendix B Supplemental Appendix

Table B.1: Cross-Validation Results

	Goods and Services			Construction		
Polynomial Order	Cross-Validation Error	Difference to Minimum	Nr of Non-Estimated Parameters	Cross-Validation Error	Difference to Minimum	Nr of Non-Estimated Parameters
2	4.32E-06	1.64E-06	0	2.28E-06	4.20E-07	0
3	3.55E-06	8.70E-07	0	2.27E-06	4.10E-07	0
4	3.15E-06	4.70E-07	0	2.07E-06	2.10E-07	0
5	3.14E-06	4.60E-07	0	2.06E-06	2.00E-07	0
6	3.14E-06	4.60E-07	0	2.01E-06	1.50E-07	0
7	2.97E-06	2.90E-07	0	1.98E-06	1.20E-07	0
8	2.93E-06	2.50E-07	0	1.94E-06	8.00E-08	0
9	2.76E-06	8.00E-08	0	1.91E-06	5.00E-08	0
10	2.73E-06	5.00E-08	0	1.90E-06	4.00E-08	0
11	2.68E-06	0.00E+00	0	1.87E-06	1.00E-08	0
12	**2.68E-06**	**0.00E+00**	**0**	**1.87E-06**	**1.00E-08**	**0**
13	2.68E-06	0.00E+00	1	1.93E-06	7.00E-08	0
14	2.70E-06	2.00E-08	2	1.99E-06	1.30E-07	2
15	2.68E-06	0.00E+00	8	1.86E-06	0.00E+00	3
16	2.68E-06	0.00E+00	5	2.97E-06	1.11E-06	2
17	3.04E-06	3.60E-07	9	2.59E-06	7.30E-07	3
18	2.71E-06	3.00E-08	10	4.57E-05	4.38E-05	4
19	0.0292324	2.92E-02	10	1.76E-05	1.57E-05	4
20	0.00003	2.73E-05	12	3.27E-06	1.41E-06	5

Notes: This Table shows the results for the 5-Fold Cross Validation procedure. The first column shows the polynomial order under consideration. Panel A shows the results for goods and services, Panel B for construction. The cross-validation error is the average mean squared error across the 5 folds, for a given polynomial order. The subsequent column shows the difference to the minimum cross-validation error across all orders. The last column in each panel shows the number of non-estimated parameters due to collinearity in the polynomial terms. The order that minimizes the CV error without dropping estimated parameters is $q = 12$.

5-Fold Cross Validation

The five-fold cross-validation method partitions the dataset into five equal subsets (folds). In each iteration, four folds are used to train the model, while the remaining fold serves as the test set. This process repeats five times, with each fold serving as the test set once. A polynomial of order q is fitted to the binned distribution in the training set, and the squared error is computed for each bin as the squared difference between the observed density in the test set and the predicted value from the training set estimates. These squared errors are averaged across all bins to obtain the error for that iteration. The final performance metric, the Cross-Validation Error, is determined by averaging the error across all five folds. The optimal polynomial order is the one that minimizes the Cross-Validation Error while also limiting the number of estimated parameters to prevent collinearity between terms in higher-order polynomials.

Table B.2: Standardized Products (Bandiera et al., 2009) to CPV Correspondence

Standardized Procurement	CPV Codes
Car Rental	6017- ; 6018-
Photocopier	3012- ; 50313100- ; 50313200- ; 79521000-2
Laptop & Desktop	30000000-9 ; 302-
Office Desk	39121000-6 ; 39121100-7 ; 39121200-8 39122000-3 ; 39122100-4 ; 39122200-5 39130000-2 ; 39263000-3 ; 39263100-4
Office Chair	39112000-0 ; 39113100-8 ; 39113000-7 39113700-4
Landline Contract	64200000-8 ; 6421 -
Projector	30191200-6 ; 38652100-1 ; 38652110-4 38652120-7
Switch Network	6422- ; 32552330-9 ; 32400000-7 ; 3241 - 32500000-8
Cable Network	3242- ; 3243- ; 3244- ; 3251- ; 3252- 3255- ; 3256- ; 3257- ; 3258- ; 48200000-0
Heating Diesel	091-
Motoroil	0921-
Lunch Voucher	30199770-8
Refuse Bin	34928480-6
Paper	33772000-2 ; 301997- ; 30199330-2 30194320-4 ; 30197-
Paper Products	30199- ; 22420000-0 ; 30194320-4
Mobile Phone Contracts	32250000-0 ; 64212-
Software	4821- ; 4822- ; 4830- ; 4831- ; 4832
Printer	30125110-5 ; 30232100-5 ; 30232110-8 30232120-1 ; 30232130-4 ; 30232140-7 30232150-0 ; 48824000-0
Server	48810000-9 ; 48820000-2 ; 48821000-9 48219700-3 ; 48222000-0 ; 48800000-6 48822000-6 ; 48823000-3 ; 48824000-0 48825000-7
Car Purchase	341-* ; 50100000-6 ; 50110000-9 50111000-6 ; 50111100-7 ; 50111110-0 50112000-3 ; 50112100-4 ; 50112200-5 50112300-6
Fax	30192340-6 ; 50314000-9 ; 32581200-1 32581210-4

Notes: This table shows the correspondence between the products identified as standardized procurement items in Bandiera et al. (2009) and the Common Procurement Vocabulary (CPV) codes. A dash "-" indicates that all CPVs starting with the digits indicated before up to te dash are included in the category. *except 34121000-1, 34121100-2, 34121200-3, 34121300-4, 34121400-5, 34144910-0, 34150000-3, 34151000-0, 34152000-7.

Table B.3: List of Expressions for Late and Incomplete Classifications

Panel A: Late Projects

	Original Expression	Translation
Included	Atraso, Atrasos	Delay, Delays
	Prorrogação, Prorrogações, Prorrogado	Extension, Extensions, Extended
	Prolongado	Extended
	Incumprimento dos Prazos	Deadline not met
Excluded	Conformidade, Normalidade, Normalmente	In accordance
	Nao existe alteração	No changes
	Concluida no prazo contratualmente estabelecido No Prazo Contratual, No Prazo Previsto	Finished within the contract deadline
	Antes do Prazo, Antecipação, Antes do Previsto, Concluidos Antes, Terminou Antes, Concluidos Antes, Prazo Inferior, Inferior ao Previsto, Antes da Data	Before the deadline
	Mais Rápido	Faster
	Prazos Cumpridos, Cumprido o Prazo, Dentro do Prazo	Deadline was met

Panel B: Incomplete Projects

	Original Expression	Translation
Included	Rescisão, Rescindido,	Termination, Terminated
	Revogação, Revogado	Termination, Terminated
	Abaixo do valor esperado, Inferior ao previsto Realizou menos, Abaixo do estimado, Redução Consumos, Consumo efetivo inferior, Quantidades Fornecidas Inferiores,	Below the expected value
	Não foram realizadas as horas previstas	Scheduled hours not carried
	Quantidade adjudicada não fornecida Não foram fornecidas Não foram prestados, Realizou menos	Contracted quantity not supplied
	Cessou antes do término	Ceased before the end

Notes: This table shows the list of expressions used to complement the definitions of *Late* and *Incomplete Projects*. For the *Late* variable, the expressions are searched over justifications to deadline changes. For the *Incomplete Project* variable, the expressions are searched over justifications to payment changes.