Pricing Government Demand: Competition, Markups, and the Labor Share *

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Abstract

This paper studies how government demand affects the labor share of income. Winning a procurement contract lowers a firm's labor share. The decline is driven by higher revenue per worker with limited pass-through to wages, and these revenue productivity gains reflect rising firm-level markups rather than improvements in physical efficiency. The effects are strongest for firms winning contracts as the only bidder. Evidence from bids, award prices, and political connections points to limited competition in procurement as the driving force. Government demand thus shapes the division of income between firm owners and workers, with implications for inequality.

Keywords: public procurement, market power, labor share, competition, political connections.
JEL codes: D24, D43, H57, L22, L11

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1 Introduction

The secular decline in the labor share has sparked a major debate about the distribution of income between workers and firm owners (Koh et al., 2020; Kehrig and Vincent, 2021). A prominent macroeconomic literature links this decline to the rise of superstar firms and expanding product-market power, where rising markups drive a wedge between the revenue a firm collects and the compensation it pays to labor (Barkai, 2020; Autor et al., 2020; De Loecker et al., 2020). However, pinning down this relationship causally at the firm level is notoriously difficult. Because firm scale, market power and technology often evolve together, it is challenging to isolate a shock that changes firm scale and observe exactly how the resulting surplus is divided.

This paper overcomes this identification challenge by using public procurement as a laboratory. Government demand provides large, observable shocks to firm scale, while detailed public procurement data allow us to directly observe the intensity of competition at the time the contract is awarded. This setting lets us trace an identified demand shock into the firm's labor share and ask who captures the gains and how that division depends on the pricing power the firm holds.

This setting requires addressing two empirical challenges. The first is selection. Governments do not pick suppliers at random. Firms that win contracts are often larger and on steeper growth paths beforehand, so naive comparisons confound the causal effect of procurement with the underlying characteristics of the winners. The second challenge is observing pricing power directly. Depending on how competitively an award is priced, the exact same contract can act as a neutral demand shock or as a direct transfer from the labor share to firm profits. Resolving both issues requires observing not only who wins a contract, but the degree of competition they faced and the price they ultimately charged; granular objects that standard firm-level datasets simply do not contain.

We study these questions in Croatia, merging the universe of firm financial statements with the administrative registry of public procurement over 2013–2019. For every published contract, the registry records the number of bids received and the procuring body's ex-ante estimate of its cost, so we can measure how competitive each award was and the price paid relative to the procurer's own reference point. This margin is important because contracts attracting a single bid account for 35% of awarded contract value. We address selection with three complementary research designs. The first traces firm outcomes around the first procurement win we observe, comparing winners to firms that have not yet won a contract, in a local-projections difference-in-differences (Dube et al., 2023). The second compares winners to the runners-up who bid for the same construction contracts, holding the decision to bid fixed. The third compares firms whose first observed contract attracted a single bid to those whose contract was competitively awarded, holding the "win" fixed. Because bidder participation is endogenous, we read this comparison as suggestive; sharper identification comes from contract-level price differentials that hold the firm, product, county, and year fixed. Although these designs draw on different samples and different identifying variation, we find similar results.

We find that winning a procurement contract lowers the labor share within the firm. In the year of the first observed win, revenues rise by 15% and revenue per worker rises by 6%; the average wage moves little (−0.1% and statistically insignificant). Since the labor share is the average wage over revenue per worker $\frac{W}{PY/L}$, it falls by 2.1 percentage points, approximately the part of the revenue per worker gain not passed through to wage pay.

The rise in revenue per worker is driven by the rise in firm-level markups that jump by 9.4 percent in the year of the win (10.6 in the winner–runner-up design) and profits over sales rise by 0.8 percentage points.[1] The markup premium is partly driven by limited competition in procurement. Relative to the multi-bid entrants, firms whose first observed contract attracted a single bid see markups 8.6 percent higher in the entry year, the labor share 1.7 percentage points lower, and profits over sales 1.1 percentage points higher. At the auction level, single-bid awards close 6.4 percent of the estimated value above multi-bid awards within the same firm, product, county, and year, a differential that survives restricting to standardized products as used in Bandiera et al. (2009).

Next, we ask who is more likely to win a single-bid contract. We find that these contracts disproportionately go to procurement insiders. Using data on firms' owners and managers, politically connected firms (about 3% of the sample) are 11 percentage points more likely to win a contract in a given year, against a baseline of 4%. The single-bid gradient, however, loads almost entirely on one type of connection. Firms whose owners or managers are, were, or later become high-level officials of public procurement bodies are, conditional on being in procurement, 12 percentage points more likely to win single-bid contracts, while connections to politicians contribute nothing conditional on it.

We discuss the implications of procurement for the sectoral labor share. We find that in the pre-award year, winners' labor shares are indistinguishable from their sector rivals', so the reallocation of economic activity towards winners alone does not

[1] We measure markups with the production approach where the markup is the wedge between a variable input's output elasticity and its revenue share (De Loecker and Warzynski, 2012), with elasticities estimated following De Loecker and Scott (2025), who show how to recover this elasticity when only revenue data are available. Because our designs identify within-firm *changes* in markups around a procurement win rather than cross-firm levels, the usual concerns about markup levels as in Bond et al. (2021) are less relevant, as explained in De Ridder et al. (2022). Appendix A.2 provides details.

change the sectoral labor share. Around 84% of the sector-level decline comes from winners' own labor shares falling. The cross-term, which captures winners simultaneously becoming bigger and lower-labor-share firms, accounts for about 15 percent, and the between-term is negligible.

Our paper contributes to the strand of the literature that follows contracts to the firm. Government demand generates firm growth through reputation, learning, innovation, and relaxed financial constraints (Ferraz et al., 2015; Gugler et al., 2020; Lee, 2022; Ilzetzki, 2024; di Giovanni et al., 2025; Clemens and Rogers, 2026). However, these scale effects can be short-lived (Srhoj and Dragojević, 2024), selling to the government can substitute for private demand (Hoekman et al., 2026), and permanently elevated government purchases can even dampen firm dynamism (Hager and Huber, 2025). While the scale effects of government demand are well documented, little is known about the prices firms charge the government and who captures the resulting surplus. We follow the demand shock into markups, profits, and factor shares within the firm, showing that the division of gains between owners and workers depends on how competitively the contract is awarded.

We contribute to the literature on market power, superstar firms, and the declining labor share. The fall in the labor share is one of the central empirical trends of recent decades (Karabarbounis and Neiman, 2014; Elsby et al., 2013; Barkai, 2020; Koh et al., 2020; Chiavari and Goraya, 2026), accompanied by rising markups (De Loecker et al., 2020) and a reallocation of activity toward large, high-markup, low-labor-share firms (Autor et al., 2020). Government procurement provides a natural laboratory for studying how the competitiveness of demand shapes markups and the labor share. Each contract is an identified demand shock to a specific firm, the number of bids records how much competition the award attracted, and the awarded price can be compared with the procurer's own cost estimate. Kehrig and Vincent (2021) attribute the labor share decline to favorable demand shocks that raise firm value added without commensurate wage increases. Procurement wins reproduce this exact pattern at the firm level, and we trace the resulting wedge directly to its source — the pricing of government demand.

Finally, we build on the empirical literature on procurement prices, competition, and political connections.[2] More broadly, politically connected firms obtain more contracts (Goldman et al., 2013; Titl and Geys, 2019; Baltrunaite, 2020; Brugués et al., 2024), at a cost in allocative efficiency and contract quality (Schoenherr, 2019; Brogaard et al., 2021; Lehne et al., 2018), and connections raise firm value even in low-corruption en-

[2]The literature shows that competition and procurement prices respond to market structure (Carril and Duggan, 2020; Allende et al., 2024), the business cycle (Gugler et al., 2015), policy-driven preferences (Marion, 2009; Carril et al., 2026; García-Santana and Santamaría, 2026), to the bureaucrats running the procuring bodies (Best et al., 2023), and to the discretion they are granted (Coviello et al., 2018; Szűcs, 2024; Carril, 2022).

vironments (Amore and Bennedsen, 2013).[3] Single-bidding, in particular, serves as an objective indicator of corruption risk (Fazekas and Kocsis, 2020). Existing work lacks evidence on firms' profitability and factor shares — a gap emphasized in the recent survey by Decarolis and Srhoj (2026). We follow procurement rents inside the firm, showing that award competition maps into firm-level markups, profits and factor shares — the objects that connect procurement design to the macroeconomic questions of labor share and inequality.

2 Data

We combine multiple administrative data sources on firms and public procurement in Croatia over 2013–2019. The core is the universe of firm-level financial statements collected by the Financial Agency (FINA), which we merge with the administrative registry of public procurement contracts published in the Official Gazette (all contracts above 200,000 Kuna, approximately 26,500 EUR, for goods and services, and 500,000 Kuna for construction), collected by the Ministry of Economy and Sustainable Development. The procurement registry records the awarded value of each contract, the procuring body's ex-ante estimated value, the identity and location of the procuring body, the number of bids received, the procedure type, and the CPV product classification of the goods or services procured. For a subset of auctions in the construction sectors we additionally observe the full list of bids, which we use in Section 5.1. Appendix A.1 describes each source, the merging steps, and the sample cleaning in detail.

Our estimation sample excludes predominantly non-market sectors (agriculture, public utilities, education, health, defense) and the financial sector, and drops firms with zero capital, labor or sales. We define a *procurement firm* in a given year as a firm winning at least one published contract in that year; a contract is a *single-bid* contract if it attracted exactly one bid. Single-bid contracts account for approximately 35% of total awarded contract value (Table 1), while the share by contract count is similar.

Table 1 presents descriptive statistics. Panel A shows that manufacturing, construction, and wholesale & retail account for about 80% of sales and 84% of procurement value in the sample. Procurement firms are substantially larger than non-procurement firms, have higher measured labor productivity and profit rates, and lower labor shares.

Panel B dissects procurement firms by their procurement intensity — total awarded contract value over total sales, both cumulated over the sample period. Two patterns stand out. First, firm size and labor productivity peak among firms for which pro-

[3]See also Mironov and Zhuravskaya (2016), Cingano and Pinotti (2013), Decarolis (2014), Gerardino et al. (2017), Decarolis et al. (2025), Decarolis et al. (2020), and Baltrunaite et al. (2021).

curement is a small share of sales (up to 10%) and are markedly lower at higher intensities, so that firms most dependent on government demand are smaller and less productive than low-intensity winners. Second, the likelihood of winning single-bid contracts rises with procurement intensity, from 58% of firms in the lowest bin to 84% among firms with intensity between 0.5 and 1, before dipping among firms with intensity above one. Together, the two patterns suggest that firms highly exposed to government demand are not the most productive firms in the market, but are the most likely to obtain contracts through non-competitive procedures — a selection pattern we return to in Section 6.4.

3 Institutional framework

All public procurement above the thresholds mentioned in Section 2 is subject to the Public Procurement Law, which is harmonized with European Union Public Procurement directives. According to the Law, public procurement needs to be done through the Electronic Public Procurement Notice (Elektronicki Oglasnik Javne Nabave). The majority of public procurement (85%) is done through an open procedure in which anybody can submit a bid, while in the rest the buyer invites selected sellers to submit a bid. The winner is determined as the most economically advantageous bid, taking into account the price and other factors such as quality.[4] The government can sign various types of contracts with suppliers. Most contracts are standard individual purchase contracts where the government pays for the delivery of previously specified goods or services. Framework agreements can be signed with one or more suppliers for up to four years to reduce administrative costs for recurring needs, but weaken competition through long-term procurer–supplier relationships.

4 Conceptual Framework

We define the labor share of firm f at time t as

$$\lambda_{ft} = \frac{W_{ft}L_{ft}}{P_{ft}Y_{ft}}. \tag{1}$$

[4]Before the change in the Law in 2017, price was the major determinant, while after the Law changed the procuring bodies are instructed to take other factors such as quality and reliability into consideration. The procuring authority can assign how much weight it gives to each factor. However, the lowest price is still the dominant decision factor.

If labor is a flexible input chosen by a cost-minimizing firm, the standard expression for labor share can be written as

$$\lambda_{ft} \;=\; \frac{\theta^L_t}{\mu_{ft}} \cdot \frac{W_{ft}}{MRPL_{ft}}, \tag{2}$$

where θ^L is the output elasticity of labor, $\mu = P/MC$ is the price–cost markup, and the last term is the ratio of the wage to the marginal revenue product of labor ($MRPL = \frac{\theta^L}{\mu}\frac{PY}{L}$). It falls below one when firms exercise wage-setting power. Equation (2) organizes the empirical analysis. Any within-firm labor share response to procurement must operate through technology (θ^L), product-market power (μ), or the gap between wages and labor's marginal revenue product.

To interpret the empirical results, we develop a simple model of a firm selling in two segmented markets, private and government. Appendix B presents the environment and all proofs. In the private market, the firm faces CES demand and charges a constant markup (monopolistic competition), so private demand shocks move quantities only — markups and the labor share are invariant. Government demand is different because of how it is priced. Contracts are awarded in auctions, which we model as the procurer's discrete choice among differentiated bids and an outside option. Each bidder then faces a downward-sloping *expected* demand whose elasticity rises with the number of competing bids. With many bidders, pricing approaches the competitive benchmark; as the number of bidders falls toward one, the competitive margin vanishes and the bid is disciplined only by the procurer's option, so the markup on government sales rises toward the monopoly markup. The firm-level markup we measure in the data is a quantity-weighted average of the two segments' markups, and the labor share is the corresponding revenue-weighted harmonic average, moving inversely with it. Winning a contract therefore shifts the composition of the firm's sales toward the government segment whose price embeds auction concentration. The model delivers four predictions.

1. *Scale without wage growth.* Winning a procurement contract raises the firm's scale and leaves the average wage unchanged if firms have no wage-setting power.

2. *Markups and the labor share.* The firm's measured markup rises and its labor share falls if and only if government demand is priced above the private-market markup — that is, if and only if auctions are insufficiently competitive.

3. *Revenue productivity without efficiency gains.* Revenue labor productivity rises purely through the firm's average price; no change in physical efficiency is required.

4. *Competition gradient.* All effects in Prediction 2 strengthen as auction competition weakens, and are largest in the limit of a single participating bid.

Predictions 1–3 organize the event-study results in Section 6; Prediction 4 is the discriminating one, and Section 6.3 tests it directly using the number of bids and the gap between awarded and estimated contract values.

5 Empirical strategy

We estimate the dynamic effects of winning a first public procurement contract using the local projections difference-in-differences (LP-DiD) estimator of Dube et al. (2025). Treatment timing is staggered, firms can win contracts repeatedly and exit procurement, and treatment effects are plausibly heterogeneous across firms and over time; conventional two-way fixed effects event studies are not robust in this setting (De Chaisemartin and d'Haultfoeuille, 2020). LP-DiD accommodates all three features while remaining transparent. Each event-time coefficient comes from a separate regression of a long-difference on a treatment indicator among treated firms and *clean controls*.

Treatment definition. Let g_f denote the first year in which we observe firm f winning a published procurement contract.[5] We define the treatment indicator as absorbing at first entry,

$$D_{ft} = \mathbf{1}\{t \geq g_f\}, \tag{3}$$

even if the firm subsequently exits procurement. This way of coding the treatment is deliberate. Conditioning the post-treatment indicator on remaining in procurement selects on an endogenous outcome, since firms for which procurement "worked" are more likely to keep winning. Firms in procurement throughout the sample window are excluded; firms that exit procurement after entry are retained.

Estimation. For each horizon $h \in \{-3, \ldots, 3\}$, we estimate

$$y_{f,t+h} - y_{f,t-1} = \beta_h \Delta D_{ft} + \delta_t + \varepsilon_{ft}, \tag{4}$$

on the sample of first-time entrants at t and clean controls — firms that have not yet entered procurement by $t + h$ for post-entry horizons, and have not yet entered by t for pre-entry horizons. This ensures that every control firm remains untreated both at the reference period $t - 1$ and at the measured outcome. The comparison is thus always

[5] Because firm identifiers in the procurement data are reliable only from 2013, g_f marks entry into the observed data rather than verified first-ever entry.

between an entering firm and firms not yet treated within the estimation window; firm fixed effects are differenced out by construction, and calendar-year effects δ_t absorb common shocks. We reweight observations so that β_h estimates an equally weighted average treatment effect on the treated (Dube et al., 2025), and cluster standard errors at the firm level. As a robustness check we re-estimate all specifications with the heterogeneity-robust estimator of De Chaisemartin and d'Haultfoeuille (2024), which targets the same estimand; the two estimators agree closely (Appendix Figure D.1). The identifying assumption is parallel trends between entrants and not-yet-entered firms. We consider the validity of parallel pre-trends with the pre-entry coefficients β_{-3}, β_{-2}. The existence of confounding shocks that simultaneously affect procurement and the labor share is difficult to test for. We argue and provide corroborative evidence that they are unlikely to affect our results. A key identification concern is selection into procurement by firm size. Larger firms are more likely to cover the fixed costs of tender participation (di Giovanni et al., 2025). If productivity shocks increase firm size and induce procurement entry, the subsequent decline in labor share and rise in markups may reflect those shocks rather than procurement. To address this concern, we re-estimate the effects on the sample of winners and runners-up who bid for the same auctions (Section 5.1).

5.1 Winners versus runners-up

The benchmark compares entrants to not-yet-entered firms, and entrants may be selected — they are potentially larger and on different growth trajectories before entry. Our second design addresses this selection directly. Using bid-level data for construction auctions, we compare first-time winners to the top-two runners-up in the same auctions — firms that bid for the same contract and nearly won — in the spirit of Ferraz et al. (2015). Runners-up enter the control group only if they have not won any procurement contract in or before the auction year. At each post-award horizon h we additionally require that a control firm's first win, if any, occurs strictly after year $t + h$. A runner-up that wins its own first contract later in the focal auction year is therefore excluded at every horizon, and the control group remains untreated throughout the comparison window — the same clean-control standard as in the benchmark design.

By drawing winners and runners-up from the same competitive events, we construct a highly comparable sample that naturally aligns the decision to bid, as well as the market, product, and timing of the contract. We then estimate their differential trajectories in a pooled specification, controlling for the year of the auction and the bid gap. The identifying assumption is that, within this matched sample, winning is not systematically related to counterfactual outcome trajectories. We probe this assumption with pre-award placebo coefficients.

How informative the runner-up is as a counterfactual depends on how close the race was. We therefore control for the bid gap between the winning bid and the closest runner-up bid in every regression. We stress test the design further by limiting the sample to close races — auctions where the absolute gap is below 10% of the procurer's estimated value — and obtain similar point estimates for the labor share and markups (Appendix Table D.4). Table D.3 details the construction of the auction sample; the bid-level data cover construction auctions in 2016–2018.

5.2 Competition: single-bid versus competitive contracts

The framework's discriminating prediction (Prediction 4) concerns the competitiveness of the award, so our third design exploits variation in competition *among winners*. We classify entrants by how many bids their first contract attracted: *single-bid entrants*, whose first contract received exactly one bid, and *competitive entrants*, whose first contract received two or more. By benchmarking against competitive entrants, we isolate a descriptive competition margin. Under the model, both entry types should scale up, but the markup, labor share, and profit responses should be strongest where competition at award is weakest.

Which contracts attract only one bid is not random. Thin bidding fields arise where potential competition is limited — specialized products, small local markets — and, as Section 6.4 shows, single-bid contracts flow disproportionately to procurement-dependent and connected firms. Single-bid and competitive entrants may therefore differ in their characteristics and market environments, and not only in the competitiveness of their award. We accordingly read this design as a descriptive within-winners comparison. Section 6.3 then presents evidence from observed contract award prices. Within the same firm, selling the same product category, in the same county and year, we compare the price of a single-bid award to that of a multi-bid award relative to the procurer's cost estimate. Because this approach absorbs permanent firm quality, broad product composition, and local market conditions, it allows us to compare how contracts awarded to the same firm in the same year are priced under different competitive environments.

6 Results

Throughout this section we report each outcome under five specifications: (a) OLS, a within-firm regression of the outcome on procurement status with firm and year fixed effects, which summarizes the correlation in the panel; (b) the LP-DiD benchmark, the dynamic treatment effect of first entry estimated against not-yet-treated controls

(Section 5); (c) the winner–runner-up design, which compares first-time winners to the losing bidders for the same construction contracts (Section 5.1); (d) the winner–runner-up design restricted to close races, where the absolute bid gap is below 10% of the contract's estimated value; and (e) the single-bid head-to-head comparison, which contrasts entrants by the competitiveness of their first contract (Section 5.2).

6.1 Labor share, labor productivity, and wages

The labor share falls on entry into procurement. Panel (a) of Figure 1 shows the event study. The labor share drops by 2.1 percentage points in the year of the first contract, with pre-entry coefficients tightly at zero (Table 2 collects the impact estimates). Over time, as the first contract's revenue share diminishes, the labor share returns toward its pre-entry level. The winner–runner-up design delivers a nearly identical decline of 2.6 percentage points on impact. Through the lens of the model, this is the central non-neutrality result, a demand shock moves the labor share only if government demand is priced above the private-market markup (Prediction 2), so the decline in panel (a) is itself evidence on how procurement contracts are priced — which Sections 6.2 and 6.3 then test directly with markups and award prices.

Next, we find that the revenue labor productivity rises post-procurement contract. Panel (b) shows revenue labor productivity rising by 6% in the entry year. The model interprets the gain as a price effect (Prediction 3). Revenue per worker rises because the firm's average price rises with the contract, not because the firm becomes more efficient — no change in physical productivity is required to generate panel (b).

Average wages do not respond. Panel (c) shows the impact estimate is -0.1 percent with a standard error of 0.7, followed by a small transitory increase in the year after entry that fades thereafter and never approaches the size of the productivity gain. This is in line with Prediction 1 as the firm expands at a given wage. It also speaks directly to the literature on wage-setting power. If labor supply curves were upward-sloping at the firm level, an expanding firm would have to pay higher wages; and if firms shared rents with their workers, the demand shock would pull wages up (Wong, 2026) — we see no discernible change in average wages.[6]

Together, the three panels give an anatomy of the labor share decline. Writing the labor share as the average wage over average labor productivity, $\ln \lambda_{ft} = \ln W_{ft} - \ln(P_{ft}Y_{ft}/L_{ft})$, the denominator rises by 6% while the numerator stays flat. This is the firm-level, identified-shock counterpart of the demand shocks in Kehrig and Vincent (2021), where the falling manufacturing labor share reflects value added rising in establishments whose wage payments do not keep pace.

[6] While we do not observe differences in average wages, we cannot rule out wage change among newly hired versus incumbent workers.

Could the decline in the labor share be driven by outsourcing, where winners book contract revenue while subcontracting production? The evidence suggests not. The labor share also declines relative to value added, $\left(\frac{WL}{PY-P^M M}\right)$, which nets out intermediate inputs, and this pattern is robust across all four estimators (Appendix Figure D.2). The materials share of sales is also essentially unchanged. It increases by about one percentage point in the award year, from a sample mean of 36%, and then reverts immediately to zero (Appendix Figure D.3). Although outsourcing in the initial year may contribute modestly, the effect is too small and transitory to explain the decline in the labor share over the post-award window. Section 6.2 shows that the main force is higher pricing power.

6.2 Markups and profits

Why does so little of the revenue productivity gain reach workers? Panel (d) of Figure 1 shows that markups jump by 9.4 percent in the year of entry and remain about 4 percent higher over the following three years. The winner–runner-up design gives 10.6 percent on impact, and the estimate is stable across the samples and control sets we consider (10.6–12.1 percent). It is also insensitive to the assumed production technology; markups built from the translog elasticities give 8.8–11.0 percent on impact across the same estimators (Appendix Figure D.4).[7] Through the lens of the model, the markup response is Prediction 2's direct test. Under CES pricing, a demand shock cannot move the markup at all, so the markup results measure the premium at which government demand is priced.

Profits confirm that the premium lands with firm owners. Profits over sales rise by 0.8 percentage points on impact (panel (f)), and the probability of positive profits, a measure robust to the extreme skew of profit ratios, rises by 3 percentage points (panel (e)).[8] Both effects fade over the event window as the treated group accumulates firms that exit procurement, and by the end of the window the point estimates turn negative — significantly so in the heterogeneity-robust estimates (Appendix Figure D.1) — consistent with profits reverting once the entry contract's revenue runs out. In the winner–runner-up design the profit responses are imprecise and centered near zero (columns (3)–(4) of Table 2); with 131 observations and the skewness of profits, that design has little power for profit outcomes.

[7]The markup responses are also robust across alternative timing assumptions for labor, instrument sets, dynamic-panel estimators, and first-stage approximations.

[8]We measure profits as earnings before taxes that are reported in the firm's profit and loss statements.

6.3 Competition: single-bid contracts

Prediction 4 says the effects should strengthen as competition at award weakens. We test it within winners, by the competitiveness of the entry contract, and within contracts, with award prices.

Within winners. Specification (e) of Table 2 and the corresponding series in Figure 1 compare entrants head-to-head, using single-bid entrants as the treated group and competitive entrants as the comparison. While this specification does not perfectly align the competitive firms on their own event clocks, it provides a clear descriptive baseline showing the trajectory of single-bid entrants relative to the average competitive entrant. This demonstrates that the markup and profit surges are disproportionately driven by the single-bid awards.

Single-bid entrants outperform on each margin we measure. Relative to the average competitive entrant, markups rise by 8.6 percent on impact, the labor share falls by 1.7 percentage points, profits over sales rise by 1.1 percentage points, and the probability of positive profits by 3.6 percentage points. Single-bid entrants also pay somewhat higher wages (+2.5 percent), so the labor-share differential reflects revenue outpacing pay rather than wage suppression.

A limitation of the firm-level comparison is that the measured markup blends the firm's private and government segments, so the single-bid differential could reflect greater exposure to government demand rather than how that demand is priced. The contract-level evidence resolves this ambiguity. Using the procuring body's ex-ante cost estimate as the benchmark, single-bid awards close 6.4% above multi-bid awards within the same firm, product, county, and year (Table 3, column 1). The differential rises to 7.8% among firms selling in at most three counties, which makes national single-supplier stories unlikely, and remains at 5.4% for standardized products, where quality differentiation is minimal, as in Bandiera et al. (2009) (columns 2–3). Because the firm itself is held fixed, differences in procurement intensity, technology, or segment composition cannot generate the gap. The same firm's output is priced higher when no rival bids.

Taken together, this evidence suggests that the firm-level rise in markups and profits after entry reflects higher markups in the procurement segment relative to the private segment, a differential explained by limited competition in procurement auctions.

6.4 What drives the single-bid phenomenon?

Although Croatian procurement law is harmonized with EU directives on public procurement, corruption remains a major concern. According to the Eurobarometer survey of business attitudes, 77% of respondents think that corruption is widespread in public procurement, well above the EU average (European Commission, 2020). The main concerns discouraging participation were that "the criteria seemed to be tailor-made for certain participants", that "the deal seemed to be done before the call to tender", and that "the procedure seemed too bureaucratic or burdensome". Consistent with perceptions alone suppressing participation, Colonnelli et al. (2024) show experimentally that firms' beliefs about corruption deter them from bidding at all. Corruption, however, is not the only concern. The European Commission has flagged the limited administrative capacity of contracting authorities in Croatia (European Commission, 2017), and a report on single bidding in the EU (European Commission, 2019) suggests that these political and administrative frictions result in a high share of single-bid contracts, finding that Croatia has the highest share among the countries analyzed.

We provide a direct test of this anecdotal evidence using hand-collected data on firms' political connections. We flag firms owned or managed by current or former politicians (first-order connections) and firms whose owners or managers are, were, or later become high-level officials at public procurement bodies (procurement-body connections); see Appendix A.1.5. About 3 percent of firms in our sample are connected, but they are heavily overrepresented in procurement (Table D.2).

Table D.5 provides the correlational evidence. Politically connected firms are 11 percentage points more likely to win a procurement contract in a given year, against a baseline participation rate of about 4 percent, and, conditional on being in procurement, 10 percentage points more likely to win single-bid contracts. Decomposing the measure is revealing (Appendix Table D.6). Entered jointly, both connection types predict procurement participation (8 percentage points each), but the single-bid gradient loads almost entirely on procurement-body connections — firms whose owners or managers are high-level officials of procurement bodies (12 percentage points). Contracts that attract no other bidders disproportionately go to procuring-side insiders, echoing the survey response documented above and the recent evidence in Asai et al. (2021).

One remaining concern is that these are correlations, and the firms with the most to gain from single-bid contracts may be the ones that hire former procurement officials. If the firms use connections to cut through red tape, connections could even be efficiency-enhancing — the "greasing the wheels" argument of Leff (1964). Our descriptive evidence points in a different direction. The probability of winning single-bid contracts is highest among relatively small, procurement-dependent firms that generate lower revenue per worker on average (Table 1). Because our earlier evidence suggests single-bid awards carry a price premium — which would mechanically inflate revenue per worker — this low measured productivity strongly suggests these firms are less efficient, consistent with the "sand in the wheels" view of Shleifer and Vishny

(1993). If so, single-bidding may increase misallocation, shifting resources from more to less productive firms (Hsieh and Klenow, 2009).[9]

6.5 Sector-level evidence

Here, we perform a sector-level decomposition of the labor share into a within-firm component and a reallocation component. Winning a first contract raises the firm's share of its sector's sales by 13.6% on impact, so a procurement win moves the sector labor share through three channels: the winner's own labor share falls (within-term), sales weight shifts toward the winner (between-term), and the transferred weight carries the winner's new, lower labor share (cross-term). Appendix C derives the decomposition and takes it to the data. We document that, at the pre-award year, first-time winners' labor shares are indistinguishable from their sector rivals' average, so the between-term contributes little. Around 84% of the sector labor share decline is the within-term and 15% the cross-term. The sector-level effects of procurement thus reproduces the within-firm –*rising star*– anatomy documented by Kehrig and Vincent (2021).

Finally, we ask whether sectors more exposed to procurement have lower labor shares. Figure D.1 plots the labor share of NACE 4-digit sectors against their procurement intensity — awarded contract values over sales, both cumulated over the sample period. Procurement-intensive sectors have lower labor shares – with a slope of -0.18 (robust standard error 0.16).[10] Procurement's effect on the aggregate labor share thus runs through the within-firm margin documented above rather than through a strong cross-sectoral gradient.

7 Conclusion

This paper asks how government demand affects the division of income between firm owners and workers. We find that procurement scales firms without raising pay. As a result, the labor share falls substantially. This decline is driven by higher firm-level markups and profits. Highlighting the role of limited competition, these effects are strongest among single-bid winners, whose awards are priced 6.4% above comparable multi-bid awards relative to the procurer's cost estimate, and they disproportionately go to firms connected to high-level officials of procurement bodies.

[9] Participation costs can also rationalize single-bidding (Kang and Miller, 2022), collusive market division — by geography or procuring body — can produce uncontested auctions (Conley and Decarolis, 2016). These channels are not mutually exclusive. Our evidence points to the insider margin but does not rule out the others operating alongside it.

[10] The figure defines the sector labor share as the wage bill over value added, consistent with the literature that measures sector-level or aggregate labor share.

At the sector level, government demand may create superstar firms — winners' labor shares fall as their sales share rises. The distributional impact of government demand depends on how competitively it is priced. Competitive pricing is therefore not only a matter of public budgets; it also determines whether public spending reaches workers or accrues to owners, creating a direct link to inequality. These findings suggest that public procurement institutions should be recognized alongside labor-market institutions and product-market competition as determinants of income distribution.

Rent-sharing mandates, such as mandatory profit-sharing, may offer a complementary tool for sharing procurement rents with workers (Nimier-David et al., 2026). They can be particularly attractive where award competition is difficult to restore, as in settings with thin markets.

Tables and Figures

Tables

Table 1: Summary statistics on the matched firm-level data, 2013–2019

Panel A: Output, procurement, and entry across sectors

Sector	Sales share	Procurement share	Firms	Procurement firms	Single-bid share of procurement	Single-bid firms
Manufacturing	0.26	0.14	13,442	1,006	0.41	686
Construction	0.08	0.39	12,502	1,040	0.26	657
Wholesale & Retail	0.46	0.31	25,080	1,563	0.38	1,067
Transportation & Storage	0.04	0.02	5,322	215	0.54	146
Accommodation & Food	0.04	0.00	10,204	100	0.46	71
Information & Communication	0.04	0.05	5,056	359	0.62	293
Real Estate	0.01	0.00	2,159	16	0.46	8
Professional, S & T	0.05	0.06	16,506	1,183	0.41	744
Administrative & Support	0.02	0.03	4,431	225	0.37	138
Aggregate	1.00	1.00	94,702	5,707	0.35	3,810

Panel B: Firm characteristics by procurement intensity

$\frac{\text{Procurement}}{\text{Sales}}$	$\frac{\text{Procurement}}{\text{Total Proc.}}$	Firms	Labor	$\frac{\text{Sales}}{\text{Employees}}$ (thousand)	$\frac{\text{Sales}}{\text{Capital}}$	$\frac{\text{Profits}}{\text{Sales}}$	$\frac{\text{Wage bill}}{\text{Sales}}$	Pr(single-bid firm)	Mean Proc. (million)
0	0.00	88,995	6	474	68	0.03	0.34	0.00	0.00
(0, 10%]	0.10	3,246	49	930	58	0.06	0.20	0.58	4.54
(10%, 20%]	0.11	743	26	730	53	0.07	0.23	0.72	21.66
(20%, 30%]	0.12	471	30	764	69	0.07	0.23	0.79	36.05
(30%, 50%]	0.21	543	29	863	62	0.06	0.22	0.79	56.33
(50%, 100%]	0.30	502	32	758	67	0.07	0.26	0.84	87.27
$> 100\%$	0.17	202	25	783	90	0.07	0.27	0.76	121.17

Note. Panel A shows sector share of economic activity. Sales share is the section's share of total sales in the firm panel. Procurement share is the section's share of total awarded contract value. Firms, procurement firms, and single-bid firms count unique firms over the sample period: all firms in the estimation sample, firms winning at least one published contract, and firms winning at least one contract that attracted a single bid, respectively. Single-bid share of procurement is the value of single-bid contracts over total awarded value in the section, computed at the contract level from the registry. Panel B: firms are grouped by procurement intensity, total awarded contract value over total sales, both summed over 2013–2019; procurement status follows the firm-panel definition of Panel A. Intensities above 100% arise when awarded multi-year contract values exceed a firm's sales recorded within the sample window. Columns report the bin's share of total awarded value, the number of unique firms, and firm-level sample averages: employment, sales per employee (thousand Kuna), sales over capital, profits (EBT) over sales (trimmed to $[-1, 1]$), wage bill over sales, the share of firms with at least one single-bid contract, and the mean total awarded value per firm (million Kuna). Sales per employee, sales over capital, and the wage bill share are winsorized at the 1st/99th percentiles. 1 EUR $\approx$ 7.4 Kuna.

Table 2: Effects of winning a first procurement contract: point estimates across research designs

	(1)	(2)	(3)	(4)	(5)
			Winner vs runner-up		
	OLS (firm, year FE)	LP-DiD (benchmark)	Baseline	Close races (gap $\leq$ 10%)	Single-bid vs competitive
Panel A: Labor share and its components					
Labor share	-0.020***	-0.021***	-0.026**	-0.027	-0.017***
	(0.003)	(0.003)	(0.012)	(0.018)	(0.006)
Log labor productivity	0.076***	0.060***	0.107	0.136	0.073***
	(0.011)	(0.011)	(0.099)	(0.125)	(0.018)
Log average wage	0.018***	-0.001	0.027	-0.007	0.025**
	(0.007)	(0.007)	(0.060)	(0.064)	(0.010)
Panel B: Markups and profits					
Log markup	0.064***	0.094***	0.106***	0.110***	0.086***
	(0.003)	(0.004)	(0.024)	(0.030)	(0.006)
Pr(EBT>0)	0.018***	0.030***	-0.026	-0.027	0.036***
	(0.005)	(0.006)	(0.038)	(0.059)	(0.011)
Profits / sales	0.001	0.008***	0.003	-0.025	0.011**
	(0.003)	(0.003)	(0.025)	(0.033)	(0.005)
Observations	360,461	256,369	131	70	15,411

Note. Each cell reports the estimated effect of winning a first procurement contract on the outcome in the row, from the design in the column. Column (1): static two-way fixed effects OLS of the outcome on the post-entry indicator with firm and year fixed effects, benchmark sample. Column (2): LP-DiD benchmark impact estimate, full sample (Section 5). Column (3): winner vs top-two runner-up impact estimate: first-time auction winners against not-yet-treated runner-up bidders in construction auctions (Section 5.1); the sample is restricted to auctions with an observable bid gap, and the regression controls for the auction-level gap between the closest runner-up bid and the winning bid as a share of the contract's estimated value. Column (4): the same design restricted to close races, where the absolute bid gap is below 10% of the estimated value. Column (5), right of the vertical rule, compares within procurement entrants: LP-DiD impact estimate for firms entering through a single-bid contract, with firms entering through a competitive contract as the comparison group. Columns (1)–(4) estimate effects relative to firms outside procurement; column (5) is identified only from first-time winners. Outcome construction and winsorizing are described in Appendix A.1.2. Standard errors clustered by firm in parentheses. *, **, *** denote significance at the 10%, 5%, and 1% levels.

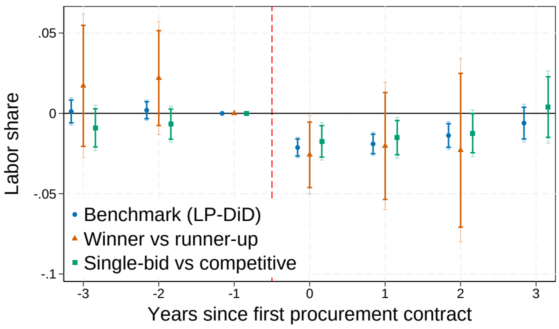
Table 3: Procurement price differentials and competition

	(1)	(2)	(3)
	$\frac{\text{Final value}-\text{Estimated value}}{\text{Estimated value}}$		
Single bidder	0.0641***	0.0776***	0.0539*
	(0.0050)	(0.0065)	(0.0280)
Observations	61,718	25,874	631
R-squared	0.614	0.688	0.592
Firm $\times$ Year $\times$ Product $\times$ County FE	✓	✓	✓
Selling to max 3 counties		✓	
Standardized products			✓

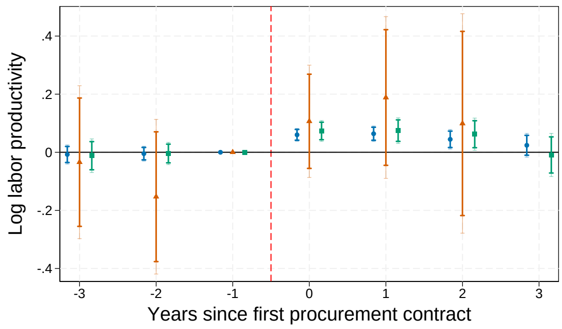
Note. Contract-level regressions with the procuring body's ex-ante estimated values. Columns (1)–(3): the price differential, (awarded value − estimated value)/estimated value, on the single-bidder indicator, absorbing firm×year×product×county fixed effects, so identification comes from the same firm winning contracts for the same product in the same county and year under different levels of competition. Sample restricted to price differentials below 1. Column (2): firms selling to at most 3 counties; column (3): standardized products. Standard errors clustered by firm. *, **, *** denote significance at the 10%, 5%, and 1% levels.

Figures

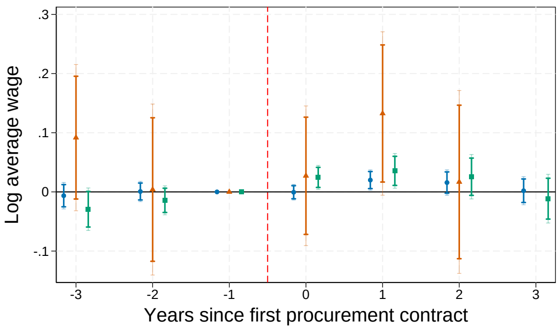
Figure 1: Effects of winning a first procurement contract



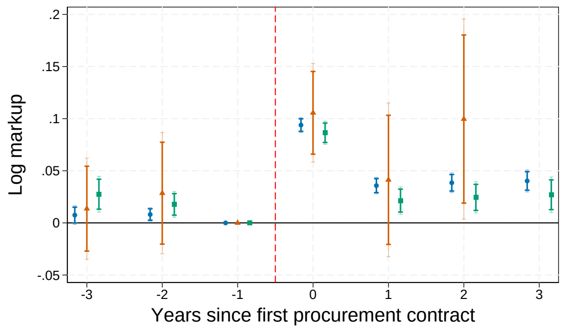
(a) Labor share



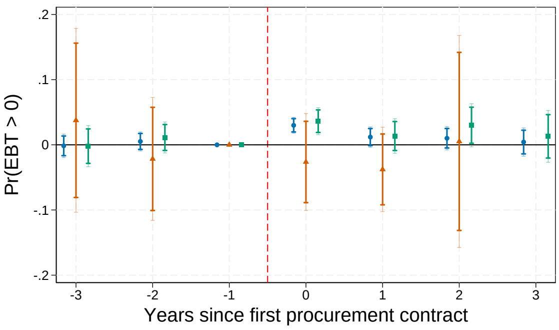
(b) Log labor productivity



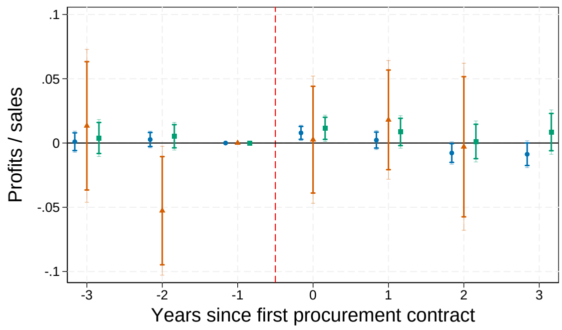
(c) Log average wage



(d) Log markup



(e) Probability of positive profits



(f) Profits / sales

Note. Each panel overlays the three research designs. Blue circles: LP-DiD benchmark, treatment effect of first procurement entry (Section 5). Orange triangles: winner vs top-two runners-up in construction auctions (first-time winners vs runner-up bidders untreated through each horizon), the sample is restricted to auctions with an observable bid gap and the regressions control for the auction-level gap between the closest runner-up bid and the winning bid as a share of the contract's estimated value; the series stops at $h = 2$ because fewer than 30 observations remain at $h = 3$. Green squares: procurement entrants only, firms entering through a single-bid contract against firms entering through a competitive contract. All series: window $-3..+3$, reference period -1, standard errors clustered by firm, 90% (thick) and 95% (thin) confidence bands; the legend appears in panel (a) only. Labor share is the wage bill over sales; labor productivity is sales per employee; average wage is the wage bill per employee; markups are estimated with the production approach (Appendix A.2). Outcome construction and winsorizing are described in Appendix A.1.2.

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Online Appendix

Big G and the Labor Share

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A Appendix

A.1 Data Appendix

A.1.1 Firm-level financial statements

Firm-level financial statements are collected by the Financial Agency (FINA). All non-financial companies in Croatia are obliged to provide their financial reports to FINA. These data are used for statistical purposes by the Croatian Bureau of Statistics and the Croatian National Bank. We exclude predominantly non-market sectors and sectors dominated by the government, such as agriculture, public utilities, education, health and defense. We also remove the financial sector. After dropping these sectors, our dataset contains firm-level data for more than 400 sectors in industry and services according to NACE Rev. 2 four-digit classification. Similarly, we drop all firms with zero capital, labor and sales. After data cleaning, our sample captures 64% of the GVA in the economy. We have access to the data from 2002 to 2019.

A.1.2 Outcome construction and winsorizing

All continuous outcome variables (log sales, log employment, the labor share, log markups, log average wage, and log labor productivity) are winsorized at the 1st and 99th percentiles within the estimation sample. Profits over sales is instead trimmed to $[-1, 1]$. The probability of positive profits is an indicator for positive earnings.

A.1.3 Public procurement registry

Public procurement data is an administrative dataset, collected by the Ministry of Economy and Sustainable Development. It contains all public procurement that was published in the Official Gazette from 2008 until 2019. In our analysis, we use the data starting from 2013 because it includes information on the unique firm identifier and the published data corresponds very closely to official statistics on total public procurement. After 2013 the threshold for required publishing of the tender in the Official Gazette is 200,000 Kuna (approx. 25,000 EUR) for goods and services, and 500,000 Kuna (approx. 70,000 EUR) for construction. Although very small contracts are not published and hence not in our data, we capture the majority of public procurement (see Table D.1).

A.1.4 Bid-level data

For a subset of procurement auctions in the construction sector, we observe the full list of submitted bids, web-scraped from the Electronic Public Procurement Notice (EOJN). Each record is a bid within an auction and contains the bid values with and without VAT, an indicator for the winning bid, the bid's final rank within the auction, a flag for bids excluded as invalid by the procuring body, the raw number of bidders, the procuring body's identity and location (municipality and county), the procuring body's ex-ante estimate of the contract value, the CPV product classification, the award date, the scoring points when the auction used MEAT (most economically advantageous tender) criteria, and the bidder's unique firm identifier (OIB).

We clean these data as follows. We drop bids without a bidder identifier, since they cannot be linked to the firm panel, bids missing both bid values, and bids flagged as invalid by the procuring body. We define the auction as the unit of observation using the official publication identifier, and, where the ex-ante estimated value is recorded on only some bids of an auction, we assign the auction-level estimate to all bids of that auction. We construct product classifications at the eight-, four- and two-digit CPV level. For winning bids, we construct the price differential as the awarded value minus the procuring body's estimated value, relative to the estimated value. Bid-level records are linked to the firm panel through the firm identifier and the award year.

For the winner-versus-runner-up design, we rank the losing bids within each auction by their final rank, breaking ties by bid value, and define the runners-up as the two best-ranked losing bids. We compute the auction-level bid gap as the difference between the closest runner-up bid and the winning bid, expressed as a share of the contract's estimated value; the gap enters every regression as a control.

The control-group rules follow Section 5.1 — runners-up enter only if they have not won a contract in or before the auction year and must remain untreated through each comparison horizon.

Table D.3 reports the sample construction by auction year. The eligibility requirements remove the large majority of matched bidders, and the reason is the structure of the bidder population. The public construction contracts are won overwhelmingly by repeat players. The median winning firm in the bid data wins several auctions over the sample window, and a small group of frequent winners accounts for roughly two-thirds of all winning bids. Because the design identifies the effect of a *first* contract, any winner with a prior procurement history, and any runner-up already active in procurement, is excluded by construction.[11] The final sample is therefore small relative to the raw bid data, but this is the point of the design rather than a defect of it. It aims to isolate the rare auctions in which a genuine first entrant met a genuinely untreated near-winner.

A.1.5 Court registry and registry of politicians

The court registry is a dataset acquired from GONG, a non-governmental organization that focuses on enhancing democratic processes and institutions as well as developing democratic political culture[12]. It contains all managers and owners of firms in Croatia from 2005 onwards. The registry of politicians also comes from GONG, and is web-scraped from webpages of high-ranking public officials that need to disclose their work and ownership histories. This includes parliament members, ministers, deputy ministers, mayors and deputy mayors. The dataset contains their ownership and employment histories[13]. We use these datasets to build two political connection indicators. A firm is *first-order connected* if an owner or manager is a current or former politician. A firm has a *procurement-body connection* if an owner or manager is, was, or later becomes a high-level official at a public procurement body.

A.2 Markup Estimation

We measure firm-level markups using the ratio estimator following De Loecker and Warzynski (2012). In particular, we rely on optimal input demand conditions obtained from cost minimization and the ability to identify the output elasticity of a variable input free of adjustment costs.

[11] An experienced winner's treatment began years earlier, and an experienced runner-up is a contaminated control.

[12] More information can be found on their official webpage: www.gong.hr.

[13] Information about the dataset can be found on https://gong.hr/2018/09/28/mozaik-veza-jer-korupcija-nije-bez-veze-2/.

Specifically, we assume a Leontief production function as

$$Y_{ft} = min[\gamma_{ft} M_{ft}, F(L_{ft}, K_{ft}) exp(\omega_{ft})] exp(\varepsilon_{ft}), \tag{A.1}$$

where Y_{ft} is output of firm f at time t, M_{ft} is material input, K_{ft} is capital stock, L_{ft} is labor input, ω_{ft} is productivity, and ε_{ft} is measurement error in output. We assume that all the firms use the same technology within a sector, however, they can have different productivity ω_{ft}. The Leontief production function has been used for construction sector in Kroft et al. (2025) and the brewing industry in De Loecker and Scott (2025), who found that markups in the production approach were closest to the demand approach (that require data on prices).

The Leontief specification has a key advantage as it gives us an expression for the unobserved productivity term, which can be used to overcome the standard simultaneity bias concern. The fixed-proportion rule, $\gamma_{ft} M_{ft} = F\left(L_{ft}, K_{ft}\right) \exp\left(\omega_{ft}\right)$, implies a purely technological control for productivity: $\omega_{ft} = \log \gamma_{ft} + m_{ft} - \log F\left(L_{ft}, K_{ft}\right)$.

Finally, we turn to firm-level markups. Define revenues as R_{ft}, we can write an expression for markups as;

$$\mu_{ft} := \frac{P_{ft}}{MC_{ft}} = \beta_{\ell} \frac{R_{ft}}{wL_{ft} + \beta_{\ell}\, p^m M_{ft}}.$$

This expression only requires us to estimate the output elasticity of labor and all other items required are directly observed in the data (namely sales, material cost and wagebill). In order to estimate the output elasticity of labor, we assume $F(L_{ft}, K_{ft})$ is Cobb-Douglas.[14] Because the fixed-proportion rule binds at the optimum, observed gross output satisfies $Y_{ft} = F(L_{ft}, K_{ft}) \exp(\omega_{ft}) \exp(\varepsilon_{ft})$. With Cobb–Douglas specification, this implies the estimating equation in logs (the *structural value added* specification):

$$y_{ft} = \beta_0 + \beta_k k_{ft} + \beta_l \ell_{ft} + \omega_{ft} + \varepsilon_{ft}. \tag{A.2}$$

Here, y_{ft} is log deflated revenue, β_{ℓ} is the gross-output elasticity of labor required by the markup expression above; on the right-hand side the parameter vector β must be estimated. The production function is estimated separately by 2-digit NACE sector. Estimating Equation A.2 directly by OLS could potentially suffer from simultaneity bias (as unobserved productivity shocks in ω_{ft} are correlated with input choices), serial correlation bias (if the unobserved productivity ω_{ft} has correlated effects), and selection bias (if, over time, sample selection occurs among exiting low-productivity firms). To deal with these issues, we proceed with the following estimation procedure.

[14] As proposed by De Loecker and Warzynski (2012), we correct the markup formula by eliminating measurement error and use $R_{ft}/exp(\epsilon_{ft})$ as a measure of revenue.

We project log gross output on a second-order polynomial in labor, materials, and capital, together with procurement and single-bid indicators, estimated separately by sector and year. The first stage projects output on materials and the determinants of the materials requirement γ_{ft}, which we assume is fully captured by the sector-year cells and the included indicators (De Loecker and Scott, 2025). This projection serves a single purpose: separating the output the firm anticipates when choosing inputs from measurement error and unanticipated production shocks. Following Ackerberg et al. (2015), no production function parameter is identified at this stage. Given a candidate parameter vector, productivity is recovered in the second stage as expected output net of the contribution of labor and capital. We assume productivity follows a first-order Markov process; projecting productivity on this process yields the innovation — the part of current productivity that the firm could not have anticipated in the previous period. The production function parameters are then estimated by the method of moments, using products of this innovation with appropriately timed inputs. Capital is chosen a period in advance and is therefore orthogonal to the innovation and enters the moments contemporaneously; labor and materials respond to current productivity shocks and enter with a lag.[15]

Alongside the Cobb–Douglas benchmark we estimate a translog technology, $y_{ft} = \beta_\ell \ell_{ft} + \beta_k k_{ft} + \beta_{\ell\ell}\ell_{ft}^2 + \beta_{kk}k_{ft}^2 + \beta_{\ell k}\ell_{ft}k_{ft} + \omega_{ft} + \varepsilon_{ft}$, by the same two-stage procedure with the corresponding lagged instruments. The labor elasticity is then firm-specific, $\theta_{\ell,ft} = \beta_\ell + 2\beta_{\ell\ell}\,\ell_{ft} + \beta_{\ell k}\,k_{ft}$, and enters the same markup formula. Figure D.4 reports the markup event studies under this technology.

B Theoretical Framework

This appendix develops a simple framework for the effect of a procurement demand shock on a firm's measured markup and labor share. It delivers the neutrality benchmark — under standard CES monopolistic competition, demand shocks move quantities, not markups — and shows that the empirical responses identify *how procurement is priced*: the effects operate through the pricing of government demand and are increasing in the concentration of the auction, with all margins sharpest in the limit as the number of bidders falls toward one.

[15] Reassuringly, the markup responses are unchanged when β_ℓ is estimated by dynamic-panel system GMM, which dispenses with the control function entirely.

B.1 Environment

A firm with productivity z produces with constant returns in labor, $y = z\ell$, takes the wage w as given, and has constant marginal cost w/z. It sells in two segmented markets: a private market and a government (procurement) market. Segmentation is natural here because procurement prices are contract-specific by construction, so the firm solves two separable pricing problems against a common, constant marginal cost.[16]

Private market. Private demand is CES with elasticity $\sigma_p > 1$,

$$q_p(p) \;=\; A_p\, p^{-\sigma_p}, \tag{B.1}$$

where A_p is a demand shifter. Profit maximization against (B.1) at marginal cost w/z gives the standard constant markup, $p_p = \mu_p \frac{w}{z}$ with $\mu_p = \sigma_p/(\sigma_p - 1)$. Because μ_p is constant, any shift in private demand moves quantity only: in a one-market CES world, a demand shock leaves the markup and the labor share unchanged. This is the neutrality benchmark against which the empirical results should be read.

Government market. The government buys through auctions. A procurer awards a contract of size g (in units of output) among n participating bidders; bids are differentiated by technical fit, quality, and idiosyncratic evaluation criteria, so we model the award as a discrete choice: the procurer selects one bid or an outside option (re-running the auction, delaying, in-house provision). Demand for a bid is winner-take-all ex post, but a risk-neutral bidder prices against its *expected* demand $P_i \cdot g$, where P_i is its win probability. We give the choice a nested structure: bid i yields the procurer utility $a_i - \alpha \ln p_i$ plus nested Gumbel shocks with dissimilarity parameter $\rho \in (0, 1]$ on the auction nest, so $P_i = P_A\, P_{i|A}$, with P_A the probability the contract is awarded at all.

Lemma 1 (Perceived elasticity). *Holding rival bids fixed, the elasticity of bidder i's expected demand is*

$$\varepsilon(s) \;=\; \eta\,(1-s) + \bar{\sigma}_g\, s, \qquad \eta \equiv \frac{\alpha}{\rho}, \quad \bar{\sigma}_g \equiv \alpha\,(1 - P_A), \quad s \equiv P_{i|A}, \tag{B.2}$$

which is decreasing in s; with n symmetric bidders, $s = 1/n$.

Proof. Write $\ln P_i = \ln P_A + \ln P_{i|A}$ and differentiate with respect to $\ln p_i$, using $\partial v_i/\partial \ln p_i =$

[16] Constant returns matter because with decreasing returns, marginal cost would rise with total output, a contract would spill into the private price, and the composition results below would mix channels.

$-\alpha$ and the log-sum property $\partial \ln \sum_j e^{v_j/\rho} / \partial v_i = P_{i|A}/\rho$. The within-auction share gives $\partial \ln P_{i|A}/\partial \ln p_i = -(\alpha/\rho)(1 - P_{i|A})$; the inclusive value falls by $\alpha P_{i|A}$, so the award probability gives $\partial \ln P_A / \partial \ln p_i = -\alpha(1 - P_A)P_{i|A}$. Summing and changing sign yields (B.2). ■

Three remarks. First, s is the bidder's ex-ante win probability within the auction, not an ex-post share. Second, the ordering $\eta > \bar{\sigma}_g$ is implied, not assumed: $\rho \leq 1$ and $P_A > 0$ give $\alpha/\rho \geq \alpha > \alpha(1 - P_A)$ — rival bids are necessarily better substitutes for each other than the outside option is for the contract. Third, raising a bid loses expected demand through two channels: losing to rivals *within* the auction (strong when bids are close substitutes and rivals are many) and the procurer *walking away* (operative only insofar as the outside option is a live threat).

Each bidder maximizes $(p_i - w/z)\, g\, P_i$ taking rivals' bids as given (Bertrand–Nash); the first-order condition is the Lerner rule $(p_i - w/z)/p_i = 1/\varepsilon_i$, so the government-market markup with n symmetric bidders is

$$\mu_g(n) = \frac{\varepsilon(1/n)}{\varepsilon(1/n) - 1}, \tag{B.3}$$

provided $\varepsilon(1/n) > 1$; otherwise the price is set by the reserve. Because $\varepsilon(1/n)$ is increasing in n, $\mu_g(n)$ is decreasing in the number of bidders. With many bidders, $\varepsilon \to \eta$ and pricing approaches the government segment's internal competitive benchmark, $\frac{\eta}{\eta-1}\frac{w}{z}$. As $n \to 1$, the within-auction margin vanishes entirely and $\varepsilon \to \bar{\sigma}_g$: the bid is disciplined only by the probability that the procurer walks away, and the markup rises to the monopoly markup against the government's own, inelastic demand.[17]

Measured markup and labor share. With private quantity q_p and contracted quantity g, revenue is $R = p_p q_p + p_g g$ and output is $Y = q_p + g$. The firm employs $L = Y/z$ workers, so the wage bill is total output valued at marginal cost, $wL = (w/z)Y$. The labor share follows in three steps: divide the wage bill by revenue, split the numerator across the two markets, and recognize each market's inverse markup,

$$\lambda = \frac{wL}{R} = \frac{(w/z)\, q_p + (w/z)\, g}{R} = \frac{p_p q_p}{R} \cdot \frac{w/z}{p_p} + \frac{p_g g}{R} \cdot \frac{w/z}{p_g} = \frac{1 - s_g}{\mu_p} + \frac{s_g}{\mu_g}, \tag{B.4}$$

where $s_g = p_g g / R$ is the procurement revenue share: *the labor share is the revenue-share-weighted average of the two markets' inverse markups.*

[17] We assume throughout that the procurer's outside option is sufficiently elastic ($\alpha > 1$) to guarantee an interior optimum, even in the single-bid limit. Note that the procurer's published "estimated value" — which we use as a benchmark in the empirical analysis of Section 6.3 — does not function as a binding structural reserve price or price cap in this setting.

Because the two pricing problems are separable, q_p, μ_p, and μ_g are invariant to winning a contract: the within-firm effect of procurement on the measured markup and labor share operates entirely through composition.[18]

B.2 Predictions

Prediction 1 (Scale without wage growth). *Winning a contract raises sales and leaves the wage unchanged.*

Proof. $\partial R/\partial g = p_g > 0$ and $\partial L/\partial g = 1/z > 0$ directly; w is a parameter of the firm's problem. ■

Prediction 2 (Markup up, labor share down). *The measured markup rises and the labor share falls if and only if government demand is priced above the private-market markup, $\mu_g(n) > \mu_p$ — that is, if and only if auctions are insufficiently competitive, $\varepsilon(1/n) < \sigma_p$.*

Proof. Since the measured markup is the quantity-weighted average of the segment markups, $\hat{\mu} = (q_p\,\mu_p + g\,\mu_g)/(q_p + g) = 1/\lambda$, we have $\partial\hat{\mu}/\partial g = q_p(\mu_g - \mu_p)/(q_p + g)^2$, which is positive iff $\mu_g > \mu_p$; $\partial\lambda/\partial g = -\hat{\mu}^{-2}\,\partial\hat{\mu}/\partial g$ has the opposite sign. Since $\mu(\varepsilon) = \varepsilon/(\varepsilon - 1)$ is decreasing, $\mu_g > \mu_p \Leftrightarrow \varepsilon(1/n) < \sigma_p$. If auction competition drives the government markup down to the private-market benchmark ($\mu_g = \mu_p$), the model reproduces exact neutrality. In the many-bidder limit ($n \to \infty$), this exact neutrality is attained if the baseline elasticity of government demand matches that of the private market. ■

In terms of the procurement revenue share, (B.4) implies the exact response $\Delta\lambda/\lambda = -s_g(1 - \mu_p/\mu_g)$ relative to the no-contract benchmark. Two consequences: the labor share response obeys the hard bound $|\Delta\lambda/\lambda| < s_g$ regardless of how large the government markup is, so the composition channel alone cannot generate large labor share declines for firms with small procurement revenue shares; and the response is increasing in procurement intensity.

Prediction 3 (Revenue productivity as a price effect). *If the government markup exceeds the private-market markup ($\mu_g > \mu_p$), revenue labor productivity rises with no change in physical efficiency.*

[18] Consistent with equation (2) in the main text: the wage–*MRPL* ratio equals one here even with two markets, because the contracted quantity g is fixed and therefore *inframarginal* — the marginal worker's output is sold in the private market, where $MRPL = (p_p/\mu_p)\,z = w$. Winning a contract changes the firm's *average* wedge $\hat{\mu}$, not its marginal hiring calculus: revenue per worker rises through the average price while the marginal revenue product stays pinned at the wage, and the entire average–marginal gap is the markup, not a markdown.

Proof. $R/L = z \cdot (R/Y) = z\bar{P}$, where $\bar{P}$ is the firm's average price; it rises with g iff $p_g > p_p$, holding z fixed. ■

Prediction 4 (Competition gradient)**.** *Holding the overall probability of an award fixed, all effects in Prediction 2 are locally increasing in auction concentration, i.e., decreasing in n, and largest in the limit $n \to 1$. Under the bridge assumption that the procurer's published estimated value forecasts the price under effective competition ($p^{\text{est}} \approx p^{\text{comp}}$), the price differential $(p^{\text{awarded}} - p^{\text{est}})/p^{\text{est}} \approx p_g(n)/p^{\text{comp}} - 1$ measures the markup premium over competitive pricing: approximately zero for well-attended auctions, positive and maximal as $n \to 1$, where it is a lower bound on the markup over marginal cost, since p^{est} already contains the normal margin.*

Proof. $\varepsilon(1/n)$ is increasing in n by Lemma 1, so $\mu_g(n)$ and hence $\mu_g(n) - \mu_p$ are decreasing in n; by the proof of Prediction 2, the markup and labor share responses scale with $\mu_g - \mu_p$. As $n \to 1$, $\varepsilon \to \bar{\sigma}_g$, the smallest attainable elasticity, so the premium $p_g/p^{\text{comp}} = \mu_g(n)/\mu^{\text{comp}}$ is maximal. The differential statement follows by substituting $p^{\text{est}} \approx p^{\text{comp}} \geq w/z$. ■

The competition gradient is the prediction that separates our mechanism from a size-based alternative. Suppose instead that markups rise mechanically with firm size, as in superstar-firm models: procurement makes the firm bigger, and bigger firms charge more. Such a mechanism reproduces Predictions 1–3, but it leaves no role for the number of bids: holding firm size fixed, it predicts the same outcomes for single-bid and competitive winners, and it offers no reason why awarded prices should exceed the procurer's own estimate of the competitive cost. Both patterns are in the data. The comparative statics in n treat the overall probability of the contract being awarded (P_A) as fixed, while in full equilibrium the number of bids also moves that probability — mechanically and through prices — so we read the gradient strictly as a local prediction.

C Sector-level labor share decomposition

The sector labor share is the sales-share-weighted average of firm labor shares, $\Lambda_{st} = \sum_{f \in s} \omega_{ft}\lambda_{ft}$ with $\sum_f \omega_{ft} = 1$. Consider a firm with pre-award sector share ω_T and labor share λ_T winning a contract, and let $\bar{\lambda}_N$ denote the sales-weighted labor share of the sector's other firms. The win changes two objects: the winner's labor share, $\lambda_T \to \lambda_T + \delta_\lambda$, and its sector share, $\omega_T \to \omega_T + \Delta\omega$ with $\Delta\omega = \delta_w\,\omega_T$, where δ_λ and δ_w are the estimated labor share (level) and log sector-share responses; rivals' shares fall by $\Delta\omega$ in total. Subtracting the no-win sector labor share from the with-win one:

$$\Delta\Lambda_s = \underbrace{\omega_T\,\delta_\lambda}_{\text{within}} + \underbrace{\Delta\omega\left(\lambda_T - \bar{\lambda}_N\right)}_{\text{between}} + \underbrace{\Delta\omega\,\delta_\lambda}_{\text{cross}}. \tag{C.1}$$

The within term is the winner's own labor share decline, weighted by its ex-ante size. The between term prices *ex-ante selection*: each share point reallocated replaces production paying $\bar{\lambda}_N$ to labor with production paying λ_T, so reallocation lowers the sector labor share only if winners already had below-average labor shares. The cross term captures that the transferred share carries the winner's *new*, lower labor share: the two estimated margins reinforce each other.

Ingredients and measurement. The decomposition takes two estimated responses and three measured objects. First, δ_λ is the LP-DiD effect on the labor share in levels: -2.1pp on impact and -1.5pp averaged over $h = 0..3$, the value used here. Second, δ_w is the LP-DiD effect on the firm's *log within-sector sales share*, $\ln(\text{sales}_{ft}/\text{sector sales}_{st})$, estimated with the same design, sample, and lagged input controls as the sales regression; we report it only here since it enters only this decomposition. Winners gain 13.6 percent on impact and 18.0 percent on average over $h = 0..3$, so $\delta_w = 0.18$. Because the between and cross terms need the share change in *levels*, we convert using the winner's own ex-ante share, $\Delta\omega = \delta_w\,\omega_T$, which assumes the percentage gain is uniform across winner sizes. Third, ω_T, λ_T, and $\bar{\lambda}_N$ are measured in the panel in the year before the award for the 2,370 first-time winners (2014–2019 cohorts) observed at $g-1$: the median winner holds 0.05% of its sector's sales (mean 0.36%), and winners' pre-award labor shares are indistinguishable from sector rivals' sales-weighted labor share.

Measured this way, the between term contributes essentially nothing ($\lambda_T - \bar{\lambda}_N \approx 0$): the decomposition assigns roughly 84% of the sector labor share decline to the within term and 15% to the cross term. The sector's shift toward a large, low-labor-share firm is thus *produced by* the procurement shock rather than selected on.

D Additional tables and figures

Tables

Table D.1: Procurement dataset aggregates

Year	Total published procurement	Total procurement in our dataset	Unpublished procurement
2013	33	25	6
2014	33	31	9
2015	31	26	9
2016	35	35	10
2017	31	31	9
2018	37	37	10
2019	43	43	11

Note: Procurement figures in our dataset are drawn from the Ministry of Economy and Sustainable Development. The overall procurement activity in the economy is taken from the Official Gazette. Numbers are in billions of Kuna. 1 EUR ≈ 7.4 Kuna.

Table D.2: Firm-level descriptive statistics by procurement and connection status

	All firms	Procurement	Connected	Connected & procurement
Revenue (000s)	8,558	59,283	63,006	223,712
Capital (000s)	3,418	15,545	32,442	69,053
Employees	11.2	58.1	67.1	214.3
Procurement Contract Value (000s)		8,458		24,031
Wage bill (000s)	1,086	6,542	7,770	24,920
Observations	384,111	16,570	15,266	2,249
Unique firms	94,702	5,707	3,186	597

Note. Firm-year means, in thousands of Kuna (1 EUR ≈ 7.4 Kuna). Columns classify firm-years: all firms in the estimation sample; firms with at least one procurement contract in the year; connected firms; and connected firms with a procurement contract in the year. A firm is connected if it is owned or managed by a current or former politician, or by a person who is, was, or later becomes a high-level official at a public procurement body; see Appendix A.1.5 for details on the underlying registries. Procurement Contract Value is the mean total awarded contract value per procurement firm-year, from the contract registry; Capital is the stock of tangible fixed assets. Unique firms counts distinct firms ever in the column's group.

Table D.3: Winner-versus-runner-up design: sample construction

Auction year	Auctions	Matched bidders		First-time / untreated		Final sample	
		Winners	Runner-ups	Winners	Runner-ups	Winners	Runner-ups
2016	980	849	1,245	27	55	22	39
2017	998	735	1,410	20	43	18	28
2018	1,108	957	1,517	42	46	26	38
Total	3,086	2,541	4,172	89	144	66	105

Note. Matched bidders are winning bids and top-two runner-up bids linked to the firm panel. First-time winners are winners for whom the auction year is the firm's first procurement year in the panel; untreated runners-up have won no procurement contract up to the auction date. Both additionally require an observable bid gap: an auction with a positive recorded estimated value (Appendix A.1.4). The final sample keeps one observation per firm (the first eligible appearance). Regression samples at each event horizon are further limited to firms with the outcome observed both at the horizon and in the year before the auction, and exclude runners-up already treated by that horizon (controls untreated through $t + h$).

Table D.4: Winner-versus-runner-up design: the role of bid closeness

	(1) Baseline	(2) No bid-gap control	(3) Close auctions (gap $\leq$ 10%)
Labor share	-0.026**	-0.028***	-0.027
	(0.012)	(0.010)	(0.018)
Log markup	0.106***	0.121***	0.110***
	(0.024)	(0.019)	(0.030)
Log average wage	0.027	-0.006	-0.007
	(0.060)	(0.047)	(0.064)
Log labor productivity	0.107	0.068	0.136
	(0.099)	(0.081)	(0.125)
Profits / sales	0.003	0.013	-0.025
	(0.025)	(0.018)	(0.033)
Pr(EBT>0)	-0.026	0.007	-0.027
	(0.038)	(0.035)	(0.059)
Auction bid-gap control	Yes	No	Yes
Observations	131	170	70
First-time winners	66	88	35

Note. Impact estimates ($h = 0$) from the winner-vs-runner-up design of Section 5.1. Column (1) is the baseline specification: auctions with an observable bid gap — the difference between the closest runner-up bid and the winning bid as a share of the contract's estimated value — with the auction-level gap as a control. Column (2) drops both the gap restriction and the gap control, using the full winner–runner-up sample. Column (3) restricts the baseline to close races, where the absolute bid gap is below 10% of the estimated value. Close races are defined by the absolute bid gap; awards in which the winning bid exceeds the runner-up's (1.6% of auctions, reflecting non-price criteria) do not survive the design's eligibility screens. All regressions use long differences relative to the pre-award year, year-of-auction fixed effects, and standard errors clustered by firm (in parentheses); runners-up remain untreated through the comparison horizon. *, **, *** denote significance at the 10%, 5%, and 1% levels.

Table D.5: Political connections and procurement participation

	(1)	(2)	(3)	(4)	(5)	(6)
	Procurement			Single bidder		
Politically connected	0.11***	0.10***	0.10***	0.10***	0.09***	0.10***
	(0.01)	(0.01)	(0.01)	(0.02)	(0.02)	(0.02)
4-digit sector and year FE		✓			✓	
4-digit sector × year FE			✓			✓
Observations	384,111	384,110	383,980	16,570	16,530	16,042
R-squared	0.01	0.06	0.07	0.00	0.11	0.16

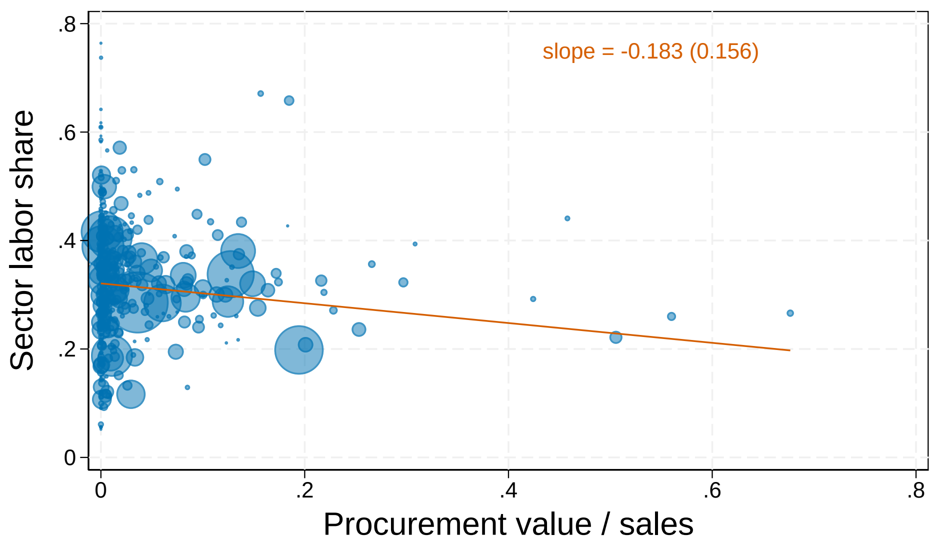
Note. Linear probability models. In columns (1)–(3) the dependent variable is an indicator for winning at least one procurement contract in the year, estimated on the full estimation sample; in columns (4)–(6) it is an indicator for winning at least one single-bid contract, estimated on procurement firm-years only. Politically connected firms are defined as in Appendix A.1.5. Columns (2) and (5) include 4-digit sector and year fixed effects; columns (3) and (6) absorb 4-digit sector × year fixed effects. Standard errors, in parentheses, are two-way clustered by firm and 4-digit NACE Rev. 2 sector × year. *, **, *** denote significance at the 10%, 5%, and 1% levels.

Table D.6: Political connections and procurement participation, by connection type

	(1)	(2)	(3)	(4)	(5)	(6)
	Procurement			Single bidder		
First-order connection	0.126***		0.081***	0.059***		-0.013
	(0.010)		(0.011)	(0.022)		(0.025)
Procurement-body connection		0.099***	0.075***		0.117***	0.122***
		(0.007)	(0.008)		(0.018)	(0.020)
4-digit sector × year FE	✓	✓	✓	✓	✓	✓
Observations	383,980	383,980	383,980	16,042	16,042	16,042
R-squared	0.07	0.07	0.07	0.15	0.16	0.16

Note. Linear probability models decomposing the connection measure of Table D.5 into its two components: first-order connections (firms owned or managed by current or former politicians) and procurement-body connections (firms whose owners or managers are, were, or later become high-level officials at public procurement bodies); see Appendix A.1.5. In columns (1)–(3) the dependent variable is an indicator for winning at least one procurement contract in the year, estimated on the full estimation sample; in columns (4)–(6) it is an indicator for winning at least one single-bid contract, estimated on procurement firm-years only. All columns absorb 4-digit sector × year fixed effects. Standard errors, in parentheses, are two-way clustered by firm and 4-digit NACE Rev. 2 sector × year. *, **, *** denote significance at the 10%, 5%, and 1% levels.

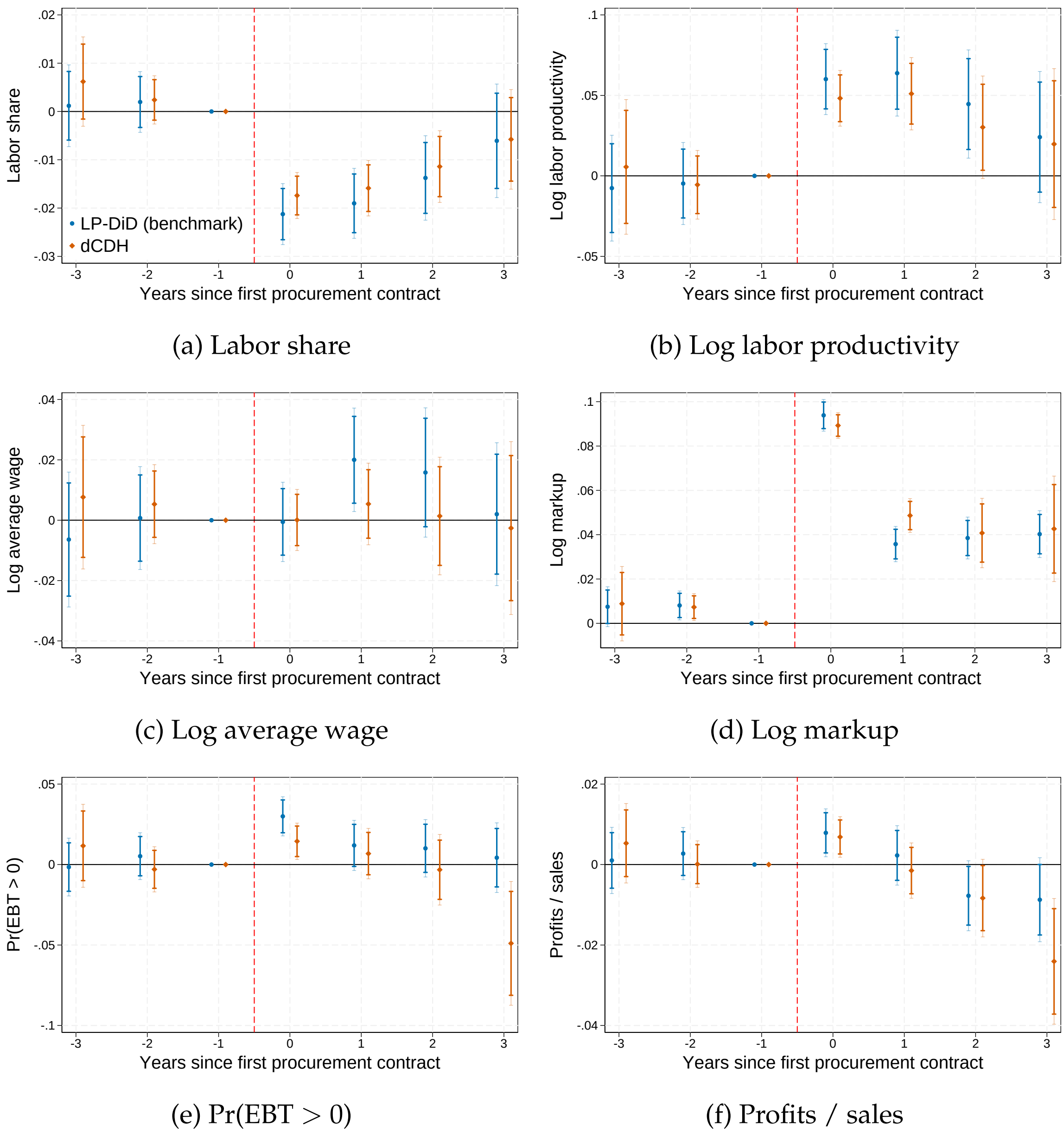
Figure D.1: Sector labor share and procurement intensity



Note. Cross-section of NACE 4-digit sectors. The vertical axis is the sector labor share, the wage bill over value added; the horizontal axis is procurement intensity, awarded contract values over sales, both cumulated over 2013–2019 as ratios of sums. Two sectors with a labor share above one are excluded. Marker area is proportional to the sector's share of firms.

Figures

Figure D.1: LP-DiD versus heterogeneity-robust (dCDH) estimates



(a) Labor share

(b) Log labor productivity

(c) Log average wage

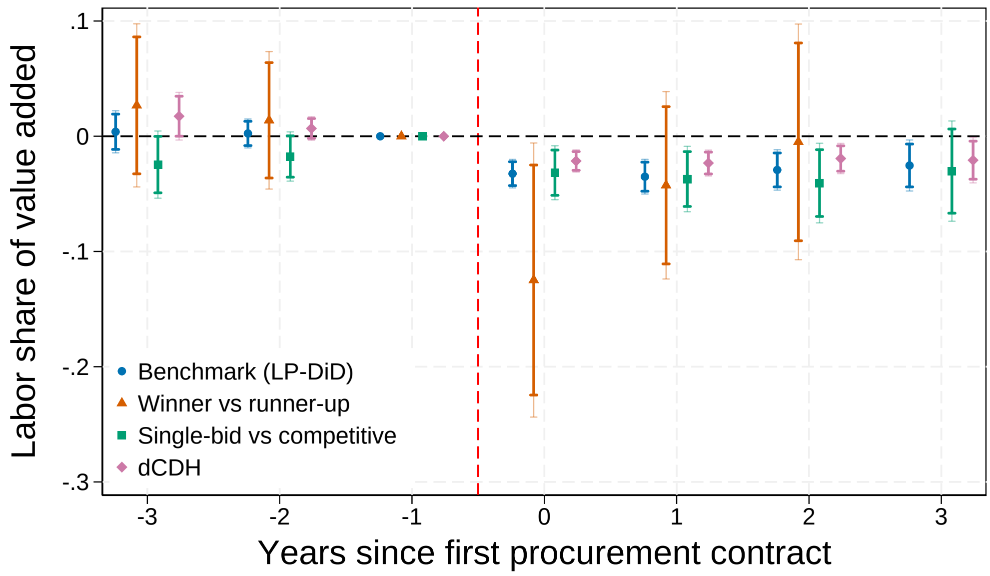
(d) Log markup

(e) Pr(EBT > 0)

(f) Profits / sales

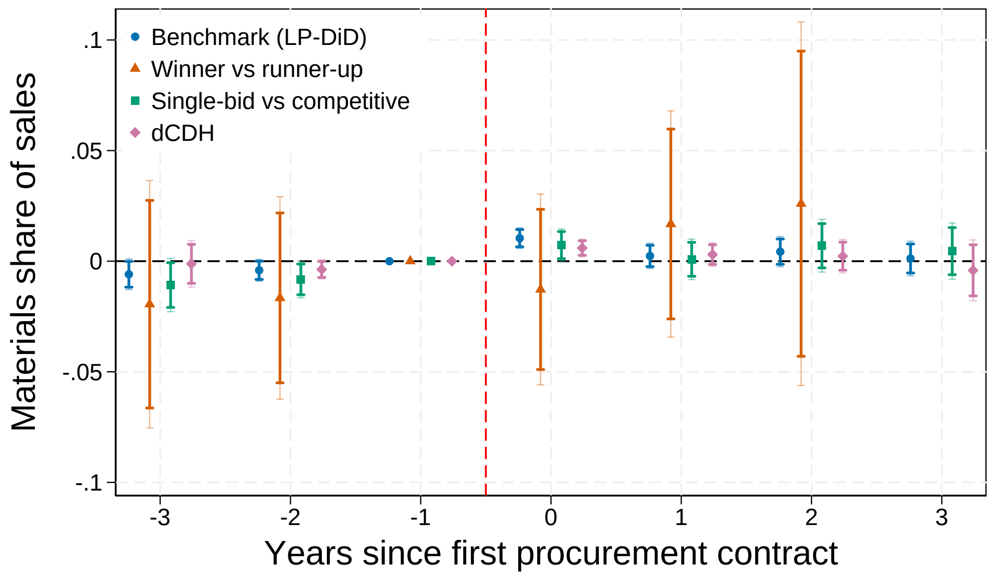
Note. Event-study estimates of first procurement entry: LP-DiD benchmark (blue circles) and De Chaisemartin and d'Haultfoeuille (2024) (orange diamonds), horizons -3 to 3 with -1 the reference period. Bars show 90% (thick) and 95% (thin) confidence intervals; standard errors clustered by firm.

Figure D.2: Robustness: labor share of value added



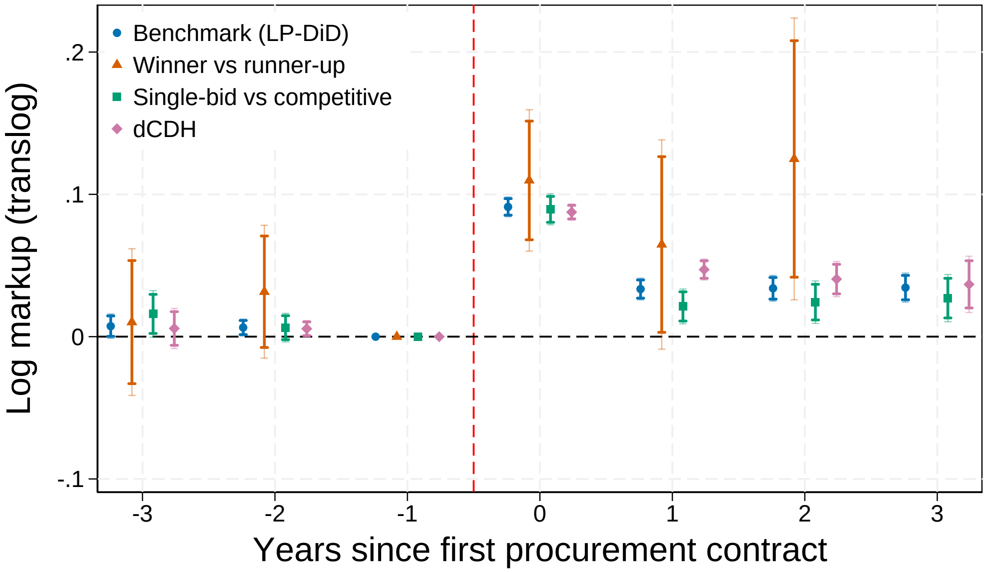
Note. The outcome is the labor share of value added, the wage bill over sales net of material costs, winsorized at the 1st/99th percentiles; negative values (firms whose material costs exceed sales) are retained and winsorized. The figure overlays four estimators: the LP-DiD benchmark (blue circles), winner vs top-two runners-up in construction auctions with controls untreated through each horizon, restricted to auctions with an observable bid gap and controlling for the auction-level bid gap (orange triangles; the series stops at $h = 2$, where fewer than 30 observations remain at $h = 3$), single-bid vs competitive entrants (green squares), and De Chaisemartin and d'Haultfoeuille (2024) (pink diamonds). Window $-3..+3$, reference period -1; standard errors clustered by firm; 90% (thick) and 95% (thin) confidence bands.

Figure D.3: Robustness: materials share of sales, four estimators



Note. The outcome is the materials share of sales, material costs over sales, winsorized at the 1st/99th percentiles (sample mean 0.36). The figure overlays four estimators: the LP-DiD benchmark (blue circles), winner vs top-two runners-up in construction auctions with controls untreated through each horizon, restricted to auctions with an observable bid gap and controlling for the auction-level bid gap (orange triangles; the series stops at $h = 2$, where fewer than 30 observations remain at $h = 3$), single-bid vs competitive entrants (green squares), and De Chaisemartin and d'Haultfoeuille (2024) (pink diamonds). Window $-3..+3$, reference period -1; standard errors clustered by firm; 90% (thick) and 95% (thin) confidence bands.

Figure D.4: Robustness: markups from the translog production function, four estimators



Note. Markups built from the translog technology estimated alongside the benchmark Cobb–Douglas in the production-function step: the labor elasticity is firm-specific, $\theta_{\ell,ft} = \beta_\ell + 2\beta_{\ell\ell}\,\ell_{ft} + \beta_{\ell k}\,k_{ft}$, and enters the same Leontief markup formula with the wage bill as labor expenditure; observations with $\theta_{\ell,ft} < 0.01$ are excluded. Estimators, window, and bands as in Figure D.2.

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