ISSN 2282-6483

Alma Mater Studiorum - Università di Bologna
DEPARTMENT OF ECONOMICS

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Quaderni - Working Paper DSE N° 1218

Public Demand Allocation and Productivity of the Private Sector

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This version: February 2, 2026

We study how variation in the allocation mechanism of public demand shapes firm performance and aggregate productivity. Exploiting the quasi-random implementation of an *efficient* or *lottery-like* auction format in the Italian construction sector, we find that when the same amount of public resources is allocated through the efficient mechanism, recipient firms experience about 8% higher revenue growth within three years. The effect is strongest where contracting authorities exhibit greater screening capacity and in less competitive markets. Efficient allocation targets more productive firms, which subsequently secure a larger amount of future public resources. Simulations suggest that replacing lottery-like mechanisms with efficient ones could raise sectoral productivity by about 4%.

JEL: H57, D22, D61
Keywords: Public demand, Awarding mechanism, Firms, Misallocation

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We thank Giacomo Brusco, Qianmiao Chen, Decio Coviello, Gianmarco Daniele, Francesco Decarolis, Dina Pomeranz, Gabriele Rovigatti, Giancarlo Spagnolo, Frank Staehler, Paola Valbonesi for valuable comments and feedback. This project benefited from presentations at RSIT; NTA annual symposium; University of Padova, Utrecht University; Petralia summer workshop; Bilkent University, CIFREL; Law, Markets and Policy Workshop. We thank Andrea Giglio for excellent research assistance. We ackwnoledge the finacial support of the University of Tubingen (PSP: 4141909177). Piemontese acknowledges the financial support from the MSCA (Project FIBRE 101063844). Any mistakes are our own.

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Non-Technical Summary

This paper studies whether the way public contracts are allocated across firms affects firm growth and aggregate productivity. Using data from the Italian construction sector, the analysis compares two procurement mechanisms used for medium-sized public works. One mechanism awards contracts to the lowest bidder and selects the most efficient firms. The other relies on a rule based on average bids, introducing a lottery-like element into the selection of winners.

We rely on an institutional setting that generates quasi-random variation in which mechanism is used. Whether a contract is awarded through the efficient or the lottery-like mechanism depends on the number of eligible bids received, a number that cannot be predicted ex-ante by firms or contracting authorities. Exploiting this feature, we compare firms that win their first public contract through the efficient mechanism to otherwise similar firms that win through the lottery-like mechanism.

We find that efficient allocation substantially improves firm performance. A 1% increase in the size of a public contract awarded through the efficient mechanism raises firm revenues by about 8% within three years, relative to firms receiving comparable contracts through the lottery-like mechanism. These effects emerge quickly after contract award and persist in the short run. They are economically meaningful, as public contracts represent a large share of firms' revenues in this sector.

The gains from efficient allocation are strongest when contracting authorities display greater capacity to screen bids and in local markets with limited competition. In highly competitive markets, the difference in the effects between allocation mechanisms is smaller, consistent with competition itself already favoring more productive firms.

The paper shows that efficient auctions select firms that are more productive rather than simply larger firms. After winning, these firms grow further by securing additional public contracts, without reducing their private-sector revenues. The results therefore reflect a selection mechanism, not mere differences in the amount of public funds received or improved access to finance.

Finally, we design different simulation exercises to translate the firm-level effects into aggregate implications. Replacing lottery-like mechanisms with efficient ones for medium-sized contracts would raise productivity in the construction sector by approximately 4%. Overall, these findings show that the design of public procurement rules has significant consequences for firm growth and aggregate efficiency, independently of the level of public spending.

1 Introduction

Government spending accounts for an increasingly large share of national income in advanced economies, influencing not only aggregate demand but also the evolution of private markets and firms.[1] A large portion of this expenditure consists of public spending on goods, services, and infrastructure provided by private firms, which directly shapes industrial structure and firm performance. While a growing literature shows that government demand can foster firm growth and innovation (e.g., Lee 2022), less is known about how *well* public resources are allocated and what this implies for economic performance. If public resources are directed toward more efficient firms, the same level of spending could generate higher firm growth and, in turn, stronger aggregate outcomes. In this sense, the efficiency in the allocation of public demand, and not merely its scale or composition, may play a key role in fostering economic growth, particularly as governments increasingly use public spending to support large-scale structural transformations in the economy.

This paper takes an important step toward understanding how the efficiency in the allocation of public funds shapes economic outcomes. We exploit an institutional setting, contingent on the Italian construction sector, in which exogenous shifts in allocation rules alter how public resources are distributed, enabling a causal identification of their effects on firm performance and, in turn, on sectoral productivity. Specifically, we test whether firms grow differently after receiving public funds awarded through an efficient mechanism, favoring the most efficient one, rather than through a lottery-like format (Decarolis 2018).[2] Crucially, in this context, neither the contracting authority nor the participating firms know ex-ante exactly which rule will determine the award: while firms form expectations about the likely rule to allocate public resources, there remains a positive probability that it will change before the award, rendering the final allocation effectively quasi-random.

We find that allocating public resources efficiently significantly boosts firm growth in the short run. Specifically, a 1% increase in project size awarded under the efficient mechanism raises firm revenues by about 8% within three years, relative to comparable firms winning under the lottery-like rule. These effects are driven by firms' behavior in the public market: efficiently selected firms are more successful at securing additional public

[1]For example, in 2023, public spending accounted for approximately 35% of GDP in the United States—an increase of about 8 percentage points relative to 2000. In the same year, the share reached 57% in France (up 9 points), 44% in the United Kingdom (up 19), 48% in Germany (up 7), and 54% in Italy (up 17), with all changes measured relative to the year 2000 (IMF website).

[2]Similar awarding rules are used in other institutional settings (e.g., China, some US states) and are not specific to the Italian construction sector. Section 2 discusses the details.

funds without substituting their private revenue. The magnitude of these gains, however, depends on the characteristics of the contracting authority and the extent of competitiveness of the public market. The positive impact of efficient allocation is concentrated among public bodies with stronger screening capacity, i.e., those better able to identify credible bidders, and in local public markets with lower competition, whereas in highly competitive markets, competition itself tends to attract a pool of generally more efficient firms. Finally, placing these findings in a broader context, simulation exercises suggest that universal adoption of the efficient mechanism, rather than the lottery-like mechanism, would raise sectoral aggregate productivity by approximately 4%.

Our empirical analysis focuses on the Italian construction sector and, in particular, on medium-sized public auctions, that is, contracts with a reservation price between €40,000 and €5 million, which represent roughly one-fifth of total public contracts during our period of analysis. A distinctive feature of the Italian procurement system makes this context an ideal laboratory for our analysis. Specifically, the auction mechanism is determined by a regulatory threshold based on the number of eligible bids that pass an ex ante screening verifying compliance with formal requirements. Auctions initially published under the lottery-like mechanism must instead be executed through the efficient mechanism if the number of eligible bids falls below the regulatory threshold. Because neither firms nor contracting authorities know ex ante whether the threshold will be reached, this institutional feature generates quasi-random variation in the allocation mechanism.

In this context, we use a newly assembled dataset that matches information on the universe of firms operating in the Italian construction sector with data on all medium-sized public construction auctions conducted in Italy between 2007 and 2016. We implement a dynamic difference-in-differences design (de Chaisemartin and d'Haultfoeuille 2024) comparing firms selected through the efficient mechanism with those chosen through the lottery-like mechanism. To mitigate concerns that pre-existing differences between firms might drive our results, we restrict the analysis to firms receiving public resources for the first time. We conduct several validity checks to support this identification strategy. We find no evidence of manipulation in the number of applicants around the regulatory threshold, and projects just above and below it are statistically comparable in size and complexity. Moreover, firms winning contracts around the threshold are similar in observable characteristics, such as revenues and firm size, further supporting the comparability of treated and control firms at baseline. These results confirm that the variation we exploit can be credibly interpreted as quasi-random, corroborating our causal interpretation of the estimates of the effect of efficient allocation on firm performance.

Beyond the average effects, we document substantial heterogeneity across contracting authorities and identify the firm-level mechanisms driving our baseline results. The benefits of efficient allocation are strongest when contracting authorities possess greater screening capacity and operate in less competitive markets, where random assignment is more likely to misallocate resources. This underscores the importance of institutional quality and market structure in shaping the effectiveness of public spending. At the firm level, we show that efficient mechanisms systematically select inherently more efficient firms, which are similar in size but with higher profitability, and that these firms subsequently expand by winning additional public contracts without decreasing their private activity.
These results suggest that allocative efficiency, by improving firm performance, can generate aggregate gains through the reallocation of public resources toward more efficient firms. To quantify these broader implications, we use our firm-level estimates in two simulation exercises: one focusing on the market of firms participating in auctions and another extending to the entire construction sector. The first exercise focuses on the market formed solely by firms participating in the public auctions under analysis, and shows that increasing the share of efficiently allocated contracts by ten percentage points raises aggregate productivity within this group by about 2.7%. The second exercise extends the analysis to the entire construction sector, encompassing a much larger set of firms, and finds that a complete shift from lottery-like to efficient allocation would increase aggregate productivity in the *overall* sector by roughly 4%.
Taken together, our results show that how governments allocate public funds matters as much as how much they spend. Enhancing the efficiency in the allocation of public spending can strengthen firm performance and magnify the growth effects of government expenditure, providing valuable insights for policymakers in the design of future policies like green investments, or post-crisis recovery strategies.

Contribution to the literature. This paper contributes to different strands of literature on the determinants of firm performance. On the supply side, credit constraints limit investment and growth (Chodorow-Reich 2013; Greenstone et al. 2020; Gabriel 2024), while integration into multinational supply chains fosters substantial productivity gains (Alfaro-Ureña et al. 2022). On the demand side, new market access is equally critical: exports can reshape firm trajectories, whereas preferential treatment of specific firms can distort competition (Atkin et al. 2017; Almunia et al. 2021; Carril and Guo 2023). Within this debate, public demand has received particular attention, as access to public resources can substantially enhance firm growth and profitability (Pozzi and Schivardi 2016; Fadic 2020; Goldman 2019; Gugler et al. 2020; Ferraz et al. 2021; Ravenda et al.

2021; Cappelletti et al. 2024; Lee 2022; Verhoogen 2023; Mensah et al. 2024; Ye et al. 2025), with effects further shaped by features of the institutional environment, such as the informational role of public demand (De Silva et al. 2009) or the presence of capacity constraints (Ilzetzki 2024). We contribute to this literature by demonstrating that the effectiveness of public spending depends critically on *who* receives it: assigning public contracts to more efficient firms generates stronger performance gains than lottery-like allocation.

Our findings also connect to the literature linking firm dynamics to broader, aggregate, consequences. A substantial literature highlights how allocation of inputs among productive units can foster or hamper aggregate growth (Restuccia and Rogerson 2008a; Hsieh and Klenow 2009; Schoenherr 2019), and how the distribution of government spending influences the magnitude of fiscal multipliers (Ramey 2011; Di Giovanni et al. 2022; Cox et al. 2024; Antolin-Diaz and Surico 2025; Fornaro 2025). Our contribution bridges rigorously identified firm-level effects and broader patterns of economic performance. We show that institutional features of the public spending process can be altered in ways that direct resources toward better-performing firms, and that doing so can have measurable consequences at the aggregate level. The mechanisms driving this result is that efficiently selected firms subsequently secure a larger share of future public resources, which reinforces their growth trajectory, while at the same time maintaining stable private revenues.

Finally, our work belongs to the literature examining institutional rules governing the allocation of public resources. One strand examines the effects of different awarding mechanisms on execution and winner characteristics (Decarolis 2014; Chang et al. 2015; Branzoli and Decarolis 2015; Palguta and Pertold 2017; Coviello et al. 2018; Carril 2021; Bessonova 2023; Hoekman and Onur Taş 2024; Szucs 2023), while other focus on collusion and corruption (Baldi et al. 2016; Conley and Decarolis 2016; Chassang et al. 2022; Hang and Zhan 2023; Chen 2024; Decarolis et al. 2025). A further line of work emphasizes the role of public body expertise (Decarolis et al. 2020; Bosio et al. 2022) and the influence of local competition (Celentani and Ganuza 2002; Hong and Shum 2002; Ganuza 2007; Li and Zheng 2009; Onur et al. 2012; Kang and Miller 2021). We innovate this literature by shifting attention away from project-level outcomes toward firm-level performance. We show that different methods to allocate public resources impact not only the execution of spending but also the long-run trajectory of recipient firms. Moreover, we demonstrate that these effects are strongest when resources are distributed by more experienced public bodies and in less competitive environments, highlighting how institutional and market characteristics jointly shape the effectiveness of public spending.

2 Background

Government spending constitutes a substantial share of GDP worldwide, and the majority of this spending is allocated through competitive procurement procedures in which firms submit offers to carry out publicly funded projects or services (ECA 2023). The selection of contractors is governed by predefined allocation rules, which can differ substantially across and within institutional settings. In what follows, we first describe the institutional framework of our empirical analysis, focusing on Italy. We then develop a conceptual framework to examine the potential consequences of adopting alternative allocation rules for public contracts.

Institutional Setting. In Italy, public procurement plays a significant role, accounting for more than 40% of total public spending and about 12% of GDP (OECD). Our analysis focuses on the construction sector, where roughly 50% of public funds are allocated through sealed-price tenders (Decarolis 2011). In these auctions, participants confidentially submit bids expressed as discounts on a reservation price announced by the contracting authority. After the auction concludes, all bids are disclosed and the contract is awarded. Within this context, we compare two alternative sealed-price tenders used to allocate medium-sized contracts: the First Price Auction (FPA), i.e., what we call the *efficient* mechanism, and the Average Bid Auction (ABA), i.e., what we call the *lottery-like* mechanism, which differ in allocative efficiency. The use of ABA expanded substantially over time, particularly in the construction sector. By the late 2000s, it accounted for the majority of public auctions in northern Italy, reaching about 75% of contracts in that region (Decarolis 2018).[3]

Both mechanisms operate under the same legal framework and are applied to highly comparable contracts, yet they rely on distinct criteria for selecting the winner. In FPAs, contracts are awarded to the lowest bidder, conditional on bids not being deemed abnormally low. By contrast, the ABA determines the winner based on a function of the average submitted bids, introducing a lottery-like component into the allocation process

[3] ABAs have been implemented internationally, including in China, Peru, Taiwan, and the United States (e.g., in New York State). Although regulatory details differ, the core logic of the mechanism is broadly similar across contexts (Spagnolo et al. 2006). Originally proposed in the civil engineering literature (Ioannou and Leu 1993) and later endorsed by major institutions (EU 2002), the ABA was intended to moderate price competition and reduce risks of cost overruns or execution failures in publicly financed projects.

(Decarolis 2018). The coexistence of these two mechanisms within a single institutional environment,applied to a large and economically significant sector,offers an informative setting for assessing how allocation rules that differ in efficiency affect firms' ex-post outcomes and, in turn, can generate aggregate effects.

While small contracts (i.e., below €40,000) are typically *directly awarded* to a specific contractor, and large contracts (i.e., those above the European Union threshold for EU-wide tendering, €5 million) must be allocated through FPAs, medium-sized contracts can be awarded using either the FPA or the ABA. The latter format can only be employed under specific conditions established by the Italian regulatory framework (D.lgs. 163/2006 and its subsequent amendment, D.lgs. 152/2008), relating to (i) the maximum contract size and (ii) the minimum number of eligible bids. One bid is considered eligible if it complies with formal requirements, such as number of documents to be submitted, which do not concern the bid itself. These criteria are used in Sections 3 and 4 to define our pool of auctions of interest (criterion (i)) and our treatment variable (criterion (ii)).

Specifically, until 2008, ABAs were permitted only for contracts below €5 million and in auctions with at least five eligible bids. In 2009–2010, the maximum contract size was reduced to €1 million, while the minimum number of eligible bids was increased to ten. Finally, between 2011 and 2016, the maximum contract size reverted to €5 million, and the ten-bid requirement remained in place.

Conceptual Framework. FPAs and ABAs differ in their allocation rules, implying that the same contract may be awarded to different firms under each mechanism. When firms differ in their efficiency, allocating public resources to one firm rather than another can lead to different outcomes: more efficient firms are better able to expand production and improve performance after winning than less efficient ones. Therefore, what matters is not only *whether* resources are allocated, but also *how* they are allocated. In what follows, we first provide an illustrative example of how the two allocation rules operate in practice. We then discuss the potential consequences for winning firms, which we examine empirically in Sections 4 and 5.

In an FPA, the contract is awarded to the bidder offering the lowest price, provided the bid is not deemed unreasonably low.[4] In contrast, the ABA relies on a two-step average-bid

[4]In actual fact, this mechanism is a variant of the standard FPA, which introduces an ex post verification stage in which bids considered "abnormally low" can be excluded. As shown by Decarolis (2018), this screening mitigates the risk of selecting bids that are unsustainable or strategically distorted. In this paper we don't make a distinction between FPA and FPA with ex-post screening. Although our data do not allow us to verify whether an auction was conducted under the former or the latter format, post-auction information indicates that none of the winning firms defaulted.

rule. After eliminating the highest and lowest deciles of bids, the contracting authority computes a first average of the remaining offers. All bids above this average are then excluded, and a second mean is calculated; the winning bid is the lowest offer strictly above this second average. We build on the findings of Decarolis (2018), treating FPAs as *efficient* auctions since they tend to award contracts to the most cost-efficient firms, and ABAs as *lottery-like* auctions, given their quasi-random allocation pattern, which is due to the fact that this procedure weakens the deterministic link between firms' efficiency and the allocation of contracts.

To illustrate the implications of the two allocation mechanisms, consider an auction with reservation price x and ten firms that differ in their production efficiency. Each firm i submits a sealed bid b_i, expressed as a fraction of x, with more efficient firms offering lower prices. Importantly, bidders cannot adjust their offers in anticipation of the specific allocation rule that will ultimately apply. The choice between the two mechanisms depends on the total number of submitted bids—an outcome that becomes known only after all bids are received. Consequently, firms cannot tailor their bidding strategies to the mechanism in place ex ante. Instead, they form expectations over the possible formats, acknowledging that the final rule (e.g., FPA or ABA) will be determined only once participation is closed (Stähler and Tulli 2025).

Table 1 reports a simple configuration of bids.[5] Under the FPA allocation rule, the contract is awarded to Bidder 1, i.e., the most efficient bidder with a bid of $0.62x$. Under the ABA allocation, bids are first ordered, and the highest and lowest deciles are discarded. The contracting authority then computes a first trimmed mean A_1 of the remaining bids and excludes all offers above it. A second mean A_2 is computed from the remaining bids, and the winner is the lowest bid strictly above A_2. Applying this rule to the same set of bids yields $A_1 = 0.736$ and $A_2 = 0.686$, so the winning bid is $0.69x$, submitted by Bidder 4, who in this example ranks fourth in terms of efficiency.

This simple example illustrates two key effects of choosing one auction mechanism over the other. On the one hand, opting for the FPA rather than the ABA generates a *selection* effect: the FPA awards the contract to the lowest bidder, that is, the most cost-efficient firm, whereas the ABA introduces a quasi-random component into the allocation process. Because the winning bid depends on an endogenous threshold derived from the average

[5] In theory, the Nash equilibrium of the ABA implies that all firms should bid the reservation price, since any deviation is either excluded in the trimming stage or does not increase the probability of winning (Decarolis 2018). In practice, however, bids exhibit substantial dispersion. Stähler and Tulli 2025 provide a theoretical explanation for this pattern, showing that firms incorporate into their bidding strategies the possibility that the auction may ultimately be implemented as an FPA, which reintroduces the incentive to undercut competitors.

Table 1: Illustrative Example

Bidder	**Cost** (c_i/x)	**Bid** (b_i/x)
1	0.58	0.62
2	0.61	0.65
3	0.63	0.67
4	0.66	0.69
5	0.68	0.70
6	0.70	0.72
7	0.73	0.75
8	0.76	0.78
9	0.80	0.83
10	0.85	0.88

Notes: The table reports a simple configuration of bids for a contract of an auction with reservation price x. All bids are expressed as fractions of the contract value. Under the FPA, the winner is Bidder 1 with a bid of $0.62x$. Under the aba, after applying the two-step averaging rule ($A_1 = 0.736$, $A_2 = 0.686$), the winning bid is $0.69x$ (Bidder 4).

of submitted bids rather than on efficiency, firms with higher costs can win with positive probability. As a result, the FPA tends to select more efficient firms, while the ABA weakens the efficiency–allocation link.

On the other hand, opting for the ABA rather than the FPA gives rise to a *level* effect. Since the probability of winning under the ABA is less sensitive to offering the lowest price among all bids, the resulting contract size is larger. In this example, the contract size (i.e., the winning bid) under the ABA is about 11% higher than under the FPA, meaning that the contract involves a larger amount of public funds.

Taken together, these differences imply that the FPA and the ABA not only allocate contracts to different sets of firms but also do so at systematically different price levels. In what follows, we provide evidence that the *selection* effect dominates, while the *level* effect is of second order. We then empirically assess how efficient versus lottery-like allocation rules affect firm-level outcomes and, consequently, can have aggregate effects.

3 Data and Preliminary evidence

In this section, we present the dataset used in the empirical analysis, which combines firm-level accounting data with information on public contracts. Following this, we offer a preliminary description of the data and examine the empirical association between the targeting of public resources toward more performing firms and aggregate outcomes.

3.1 Data

Firm data. We assemble a novel dataset covering the universe of Italian firms operating in the construction sector by combining two complementary sources: the ORBIS and CADS databases.[6] The former provides detailed accounting information for medium and large firms. The latter offers broader coverage in terms of firm size, including all registered limited liability companies; however, since these firms are not required to file full accounting statements, the set of available variables is more limited. The final dataset spans the period 2007–2016 and includes firm-level information such as tax ID, year of establishment, revenues, profit margins, return on assets (ROA), employment, cost of raw materials, and asset values for the universe of Italian firms in the construction sector.

Auction data. We complement the firm-level data with auction-level information from the Italian Anti-Corruption Authority (ANAC) on sealed-price public tenders. Specifically, we collect data on the universe of medium-sized construction contracts awarded between 2007 and 2016 through either the efficient or the lottery-like mechanism. This subset represents a significant portion of public procurement activity, accounting for approximately 22% of total public spending on contracts in Italy over the period of analysis. For each auction, we record detailed information on the contracting authority (including its location), the contract identifier, and the identity of the winning firm (name and tax ID). We also gather key contractual characteristics such as the publication date, reservation price, awarding rebate, number of eligible bids, bid submission deadline, description of the project, and the standardized classification of the procurement type (CPV code, henceforth).

Unfortunately, the data do not report which allocation rule, i.e., efficient or lottery-like, the contracting authority selected ex ante. Consequently, we classify all tenders with a number of bids below the regulatory threshold (either 5 or 10, depending on the year, see Section 2 for details) as allocated through the efficient rule, since Italian regulations explicitly prohibit the use of the lottery-like mechanism in such cases, regardless of the awarding body's initial intention. For tenders with a number of bids exceeding the threshold, we assume they are allocated through the lottery-like rule, consistently with prevailing practice in the sector and excluding the rare instances in which contracts with many participants may still be awarded under the efficient rule (Decarolis 2011). We discuss the implications of this classification strategy and its central role in our identification approach, including the potential for measurement error, in Section 4.

[6] The two datasets are provided by Bureau van Dijk and CERVED Group, respectively.

We complement these data with detailed information on contract execution, including the number of suspension days (i.e., project delays), the occurrence of renegotiations, and the presence of subcontracting. These variables allow us to capture key dimensions of ex post contract performance and implementation quality.

In constructing the final dataset, we merge the auction-level information with firm-level data using the tax identification number, which uniquely identifies each winning bidder. This linkage enables us to track the performance of firms over time, both before and after the receipt of public contracts, thereby allowing a consistent analysis of how different allocation mechanisms affect firm outcomes.

Descriptive statistics. Appendix A reports descriptive statistics on overall procurement activity in the construction sector and the associated firm characteristics, as well as summary statistics for the sample of contracts and firms used in the analysis.[7] Table A.1 summarizes procurement activity by province and balance-sheet characteristics of construction firms. On average, each province awards 53 public tenders per year, accounting for over half of public contract expenditure and about 40% of the number of medium and large contracts, with roughly half of tender expenditure allocated through sealed-price mechanisms. Contracts are characterized by sizable rebates and frequent renegotiation and subcontracting. Overall, the construction sector is dominated by small firms, reflecting the fragmented structure of the Italian construction industry.

Instead, Table A.2 reports descriptive statistics for contracts awarded around the threshold and for the firms winning those contracts. These projects are smaller, on average, than contracts awarded through sealed-price public tenders in the construction sector as a whole, and are less subject to cost overruns, time suspensions, and subcontracting. At the same time, the winning firms are larger than the average firm operating in the construction sector. This evidence is consistent with the fact that the lottery-like mechanism applies to medium-sized contracts and that relatively larger firms are more likely to participate in public procurement markets.

Importantly, there are no substantial differences in these characteristics between the treated and control groups. Contracts awarded under the efficient mechanism are slightly larger and more likely to experience cost overruns, time suspensions, and subcontracting, and are awarded to marginally larger firms. As shown in Section 4, however, these differences are not systematic once outliers and fixed effects are taken into account.[8]

[7] For ease of exposition, we provide additional useful descriptive figures in the Online Appendix A.

[8] Specifically, Section 4 estimates the specifications using logarithmic transformations of these measures, and Appendix B further includes year fixed effects to account for common shocks.

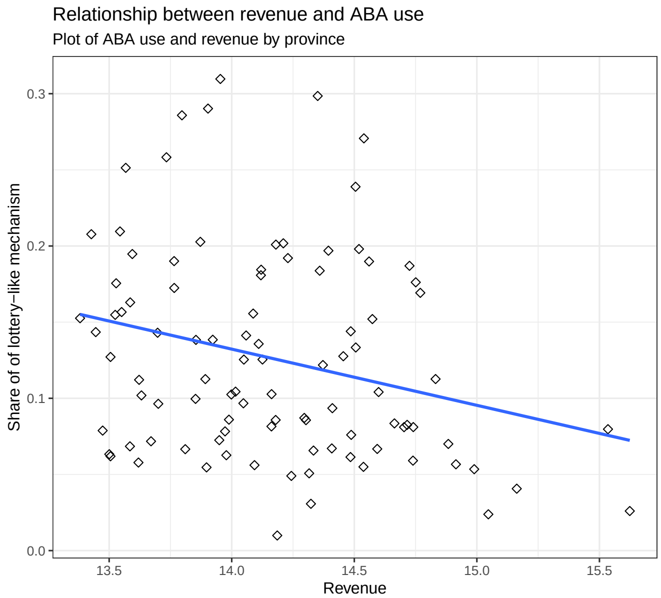
3.2 Preliminary evidence

As a preliminary step, we explore the relationship between the relative use of the two auction formats and aggregate indicators of performance in the construction sector. Figure 1 presents a scatter plot to examine the correlation between the provincial average revenues of construction firms and the share of public expenditure allocated through lottery-like mechanisms. The results reveal a clear negative association between the prevalence of lottery-like awarded contracts and province-level average revenue. This pattern suggests that weaker aggregate performance in the construction sector may be linked to the way public funds are allocated: if lottery-like auctions tend to select less efficient firms (see Section 2), their post-auction revenue growth may be lower than that observed under allocations based on efficient mechanisms. If this pattern holds systematically, it could translate into measurable effects at the aggregate level.

In a second exercise, we examine the correlation between an annual proxy of aggregate productivity, capturing the extent to which more productive firms tend to have larger market shares, and the prevalence of efficient mechanisms in public auctions. Specifically, we estimate OLS regressions in which the dependent variable is the province-level Olley–Pakes covariance between firm productivity and firm size in the construction sector (see Section 6 for details) in year t, and the main regressor is the share of medium-sized auctions executed through the lottery-like mechanism in year $t-1$. The results, reported in Table 2, reveal a negative and statistically significant correlation, robust to increasingly demanding specifications that replace year fixed effects (columns 1 and 3) with region–year fixed effects (columns 2 and 4), and to the inclusion of province-level controls for construction-sector characteristics, such as average firm size, average assets, average material costs (all in logs), and the number of firms (columns 3 and 4).

Overall, this evidence suggests that, since public entities represent a major source of demand in the construction sector, the way in which public contracts are awarded can have substantial economic implications. In the next sections, we conduct a micro-level analysis to identify the causal impact of winning an auction through the efficient rather than the lottery-like mechanism on firm-level revenue growth (see Sections 4 and 5). We then place these causal findings in a broader context by simulating their aggregate implications for the overall efficiency of the construction sector (Section 6).

Figure 1: Average revenues



Notes: The figure depicts the province-level correlation between average revenue (in logs.) (x-axis) and the share of expenditure awarded with the lottery-like mechanism among all the public tenders at provincial level.

Table 2: Aggregate productivity

	(1)	(2)	(3)	(4)
	aggregate productivity (t)			
share of lottery-like auctions (t-1)	-0.023**	-0.023*	-0.031**	-0.032*
	(0.01)	(0.012)	(0.013)	(0.017)
Controls			X	X
Province FE	X	X	X	X
Year FE	X		X	
Region X Year FE		X		X
Observations	831	704	778	655
R-squared	0.299	0.522	0.312	0.518

Notes: This table reports OLS estimates of the correlation between the province-level Olley–Pakes (OP) covariance between firm productivity and firm size in the construction sector and the share of medium-sized auctions executed through the lottery-like mechanism in year t $t1$. All regressions include province fixed effects. Column (1) includes province and year fixed effects. Columns (2) and (4) replace year fixed effects with region–year fixed effects. Columns (3) and (4) add province-level construction sector controls: (log-) average firm size (number of employees), (log-) average assets, (log-) average material costs, and the number of firms. Robust standard errors are clustered at the province level.

4 Empirical strategy

We develop a contract-level identification strategy to examine how winning a public contract allocated through a more efficient mechanism, as opposed to a less efficient one, affects firm-level outcomes. Specifically, we compare sealed-price public tenders awarded either under the efficient mechanism or under the lottery-like mechanism. By analyzing the post-award revenue growth of firms that win contracts in each setting, we assess how differences in the efficiency of allocation translate into differences in firm performance.

As described in Section 2, our analysis focuses on medium-sized contracts that can be awarded through either mechanism. Since the lottery-like rule can be applied only when the number of eligible bids exceeds (or equals) a regulatory threshold, i.e., five until 2008 and ten thereafter, we restrict the sample to contracts that received a number of bids close to these thresholds.

In what follows, we outline our identification strategy, beginning with a discussion of potential threats to its validity and proceeding to the description of the empirical model.

4.1 Identification strategy

Our key treatment variable is an indicator for whether a contract is awarded through an efficient or a lottery-like mechanism. Because the awarding mechanism is not directly observable, we classify all contracts with a number of bids below the institutional threshold as efficiently awarded, which as argued in Section 2, it is an institutionally guaranteed assignment. Contracts with a number of bids above the threshold are classified as awarded through a lottery-like mechanism, in line with the administrative rule documented by Decarolis (2018). This classification may introduce measurement error if a subset of contracts exceeding the threshold was designed to be awarded efficiently from the outset, despite receiving a large number of bids. In such cases, contracts that are in fact efficiently awarded would be misclassified as lottery-like. This would mechanically attenuate differences in outcomes between efficiently and lottery-awarded contracts, implying that our estimates should be interpreted as a lower bound on the true effect of allocating contracts through an efficient mechanism. To tackle this concern, we restrict attention to contracts above the relevant threshold for which at least 20 percent of bids are excluded, making it substantially more likely that the lottery-like mechanism is applied (see Figure B.1).

Our identification strategy exploits the quasi-random change in the allocation mechanism for contracts with a number of bids around the threshold, together with the panel structure of our firm-level data. We therefore examine in detail the assumptions required for the validity of this strategy, distinguishing for clarity between those related to the behavior of the *demand side*, i.e., public bodies, and those related to the behavior of the *supply side*, i.e., firms.[9]

Regarding the demand side, a potential concern is that public bodies may influence the exact number of bids an auction receives, and that the scope for such influence may be correlated with the contract characteristics or the procurement experience of the contracting authority. For example, contracting authorities could strategically allocate larger or more complex projects on one side of the threshold, or more experienced public bodies could systematically attract more bidders through less stringent contract design, advertising, or eligibility requirements. If such practices were correlated with placement of contracts on one side of the threshold, observed differences across allocation mechanisms could reflect endogenous sorting rather than causal effects. The data do not support these concerns. We find no evidence of systematic sorting of contracts around the threshold, with a similar number of auctions and comparable distributions of contract characteristics and public-body experience on either side of the cutoff (Figures B.2–B.4).

Concerning the supply side, a first threat to the validity of the identification strategy is that firms may strategically adjust participation to increase the likelihood of being awarded contracts under a preferred mechanism. This would require firms to systematically and accurately predict whether auctions with participation close to the threshold will ultimately fall just below or just above it. While firms may form broad expectations about participation intensity, forecasting the exact number of bids is considerably more demanding due to uncertainty about rivals' entry decisions and the presence of non-coordinated bidders. A related concern is the existence of cartels aimed at fine-tuning participation around the threshold. While coordination cannot be entirely excluded, achieving precise control over the realized number of bids would necessitate extensive contract-specific information on expected participation, accurate forecasts of last-minute entry or exit, and credible enforcement against deviations. Under these conditions, even a single unexpected entrant could shift a contract across the threshold, rendering systematic manipulation implausible. Consistently, firms winning contracts just below and just above the threshold are similar in observable characteristics such as revenues, assets, input costs, and employment (Figure B.5). As we show below, the only systematic difference concerns firm efficiency (Section 5.3), which we interpret as evidence that the efficient mechanism in-

[9]For ease of exposition, we defer detailed explanations and supporting tables and figures to Appendix B and here focus on the core message. Further evidence is presented in Online Appendix B.

deed selects more efficient firms on average. This pattern would threaten identification only under strong assumptions about firms' ability to precisely target contracts, which we consider unlikely.

Second, our identification relies on the assumption that winning firms cannot influence the timing of contract awards. This assumption is plausible in this institutional setting. While firms choose when to submit bids, the evaluation and award process is governed by complex administrative procedures set by the contracting authority, over which firms have no control. Even if limited timing manipulation were theoretically possible, identification would be threatened only if firms winning contracts under the efficient mechanism systematically differed from lottery-like winners in their ability to influence award timing, which we consider unlikely.

Third, the identification strategy could be threatened if a time-varying factor affected both the probability of winning a public tender and the firm's subsequent performance. To mitigate this potential source of selection bias, we compare changes in performance between the two groups of winning firms, assuming that any time-varying factors affecting both the likelihood of receiving public funds and future revenues are either comparable across the two groups or vary only at the regional level.

Finally, differences in unobservable experience with publicly funded contracts could matter only if they translated into systematically different winning probabilities under the two allocation rules. In such a case, comparisons of post-award outcomes would be biased, as winning firms would not be directly comparable. To address this concern, we restrict the sample to firms receiving public resources for the first time, thereby ensuring that the comparison is conducted between firms with no prior exposure to public tenders. This restriction allows us to isolate the effect of the allocation rule by comparing efficiently and lottery-like selected winners around the threshold under comparable baseline conditions.

4.2 Estimation strategy

In our estimation, we analyze the performance of firms that win contracts around the regulatory threshold. Our empirical analysis leverages the quasi-random assignment of contracts awarded efficiently (i.e., with the number of bids just below the threshold) and focus on firms receiving a public contract for the first time, either through the efficient or with the lottery-like mechanism, and the number of bids falls within a narrow window around the threshold.[10]

[10] In our main specification, we focus on contracts with a number of bids within plus or minus four bids from the relevant threshold.

In addition, to mitigate measurement error in the treatment variable, as discussed in Section 4.1, we further restrict the sample of contracts above the threshold by excluding cases in which fewer than 20% of bids are excluded, consistent with the institutional features of the lottery-like allocation mechanism described in Section 2. Contracts above the threshold that do not satisfy this criterion are therefore less likely to have been awarded through the lottery-like mechanism. Imposing this restriction allows us to sharpen the distinction between efficiently awarded and lottery-like awarded contracts, thereby reducing misclassification of the treatment status. This strict selection yields a final sample of approximately 200 contracts.[11]

We compare the revenue trajectories of firms selected through the efficient mechanism with those selected through the lottery-like procedure over the same time horizon following contract award. To identify this differential effect, we implement a difference-in-differences estimation strategy that measures the change in revenue for treated firms, i.e., those whose first public contract was awarded under the efficient mechanism, relative to a control group of firms whose first contract was assigned through the lottery-like procedure. The estimation approach is formalized in Equation 1:

$$\begin{aligned} Y_{f,t} = {} & \alpha_f + \alpha_t \\ & + \sum_{k \neq 0} \gamma_k \, \text{Proj Size}_f \times \mathbf{1}\{t - T_f = k\} \\ & + \sum_{k \neq 0} \beta_k \, \text{Proj Size}_f \times \text{Efficient Award}_f \times \mathbf{1}\{t - T_f = k\} \\ & + \delta X_{ft} + \epsilon_{f,t}. \end{aligned} \tag{1}$$

where $Y_{f,t}$ is the logarithm of the revenue of firm f in year t, α_f and α_t represent respectively firm and year fixed effects, Proj Size$_f$ is the logarithm of the awarded project size.[12] T_f is the year of the award of the contract for firm f, therefore $\mathbf{1}\{t - T_f = k\}$ is an indicator taking value 1 when the year t is equal to $T_f + k$. Dummy Efficient Award$_f$ is our treatment variable, taking value 1 if the contract awarded to the treated firm is awarded efficiently, i.e., public tenders with number of eligible bids below the regulatory threshold,

[11] Although the institutional setting appears well suited for a regression discontinuity design, we adopt a difference-in-differences approach due to the limited sample size, because only approximately 200 auctions fall within the relevant window around the threshold and are allocated to firms available in our data, which prevents obtaining reliable local estimates.

[12] When referring to Proj Size, we use the reservation price defined in Section 2, as it provides an exogenous proxy for the amount of public resources received by the winning firm and allows comparisons across contracts of comparable complexity. In Section 5, we also focus on the total amount of awarded resources, which is more endogenous to firms' bidding and execution behavior, and show that the results remain unchanged.

and 0 otherwise. X_{ft} are firm-level controls, such as number of employees, fixed assets, the value of raw materials, and the firm's age. Finally, all the results include regional trends (when specified) to account for common regional shocks to firm performance. Standard errors are clustered at the firm level and bootstrapped. Since the contracts are awarded at different times for different winning firms, we estimate Equation 1 with the event-study estimator proposed by de Chaisemartin and d'Haultfoeuille (2024) and de Chaisemartin et al. (2024), which accounts for treatment heterogeneity.

The coefficients β_k are our parameters of interest and capture the differential effect of a one percent increase in the size of a project awarded efficiently on firm revenues, measured relative to the year in which the contract is awarded.

5 Results

In this section, we present the main empirical results. For clarity, we organize the discussion into three parts. First, we present our baseline results that estimate the average effect of awarding public resources through the efficient rather than the lottery-like mechanism. Second, we explore heterogeneous effects that are relevant for policy implications and for guiding the simulation of aggregate results in the next section. Specifically, we show that the effect is stronger when contracting authorities display greater ability to screen and discard non-credible bids, and in markets with lower levels of competition. Finally, we explore the mechanisms behind our main findings. First, we show that firms selected efficiently already performed better before receiving public resources, despite being similar in size to firms selected with lottery-like auctions. This indicates that the efficient allocation rule effectively identifies more performing firms. Second, we find that these firms outperform their peers after the award, not only by securing additional public contracts but also by maintaining their revenues in private markets. By contrast, we find no significant differences in credit constraints or fixed capital investment.[13] Overall, the stronger ex-post performance of efficiently selected firms reflects their greater ability to generate revenue across both public and private markets, rather than improved access to finance or investment capacity.

5.1 Baseline results

Event study. Figure 2 presents the event-study estimates of the coefficient β coefficients from Equation 1, which captures the average effect of a one-percent increase in the size

[13]These are alternative mechanisms often used to explain the positive effects of public demand on firm performance (e.g., Hebous and Zimmermann 2021; Cappelletti et al. 2024).

of projects awarded through the efficient mechanism on the logarithm of firm revenues. Revenue trajectories for treated and control firms are statistically indistinguishable before the award, confirming the absence of differential pre-trends and supporting the validity of the comparison. After the award, a one percent increase in the size of the project allocated efficiently leads treated firms to experience a significant and immediate increase in revenue relative to the control group. This effect persists in the short run, indicating that efficient allocation of public funds results in substantially higher firm revenue compared to lottery-like awarding rule. Over time, the differential impact attenuates in both magnitude and precision, becoming statistically indistinct after three years.

Average effect. Panel A of Table 3 reports the estimated coefficient β from Equation 1 for the first three years following the contract award. The results show that a one-percent increase in the size of a public project allocated through the efficient mechanism is associated with roughly an 8% higher revenue growth for the winning firm, relative to firms selected through the lottery-like procedure. This effect is both statistically significant and economically meaningful, indicating that even small differences in how public resources are allocated can translate into substantial differences in firm revenues over a relatively short period.

The magnitude of this effect reflects the substantial weight of these contracts in firms' balance sheets: the median contract in the sample accounts for nearly 20% of the median firm's annual revenue.[14] Consequently, a one-percent increase in the size of the awarded project represents a considerable boost to a firm's available budget. The results remain robust when controlling for firm-level characteristics (column 2) and for regional linear trends (column 3).

Overall, our findings imply a strong *selection* effect (see Section 2): contracts allocated through the efficient mechanism are systematically awarded to more efficient firms, which subsequently exhibit higher revenue growth. This confirms that the efficiency of allocation rules has important implications for the overall effectiveness of public procurement.

A remaining concern relates to the potential presence of a *level* effect, which may arise if, for projects of similar size, firms selected through the lottery-like mechanism receive mechanically a larger volume of public funds to execute their contracts (see Section 2, Table 1). In such cases, differences in post-award performance between efficiently and lottery-like selected firms could partly reflect disparities in the amount of resources allocated, rather than the efficiency of the selection process itself. In addition, the estimates in Panel A of Table 3 rely on the project value (i.e., the reservation price) and do not

[14]See Table A.2.

control exactly for the amount of awarded resources, including the potential ex-post renegotiations.[15] Neglecting the final size of the allocations may introduce measurement error in the explanatory variable and obscure the interpretation of the estimated effects.

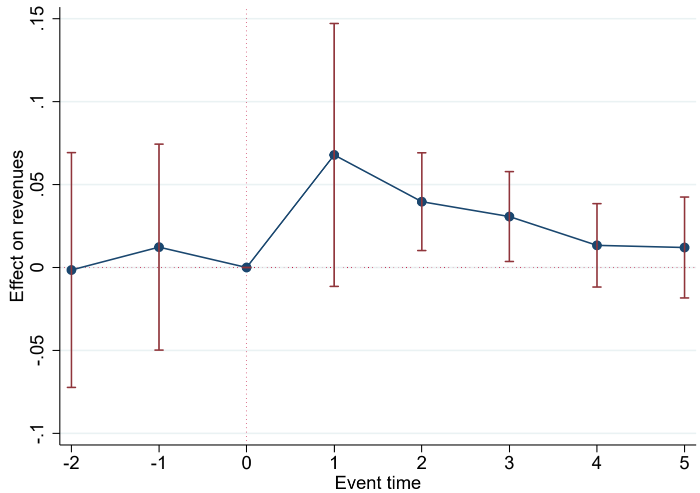
To address this issues, we re-estimate Equation 1 using the same sample of firms but redefine our main explanatory variable. Instead of using the logarithm of the project size, we use the logarithm of the total public resources received by each firm, combining the original value of the award and any supplementary transfers resulting from renegotiation. This approach allows us to compare revenue trajectories between efficiently and lottery-like selected firms that receive the same overall amount of public funds, regardless of whether these were allocated at the time of award or subsequently.

This specification, however, weakens the causal interpretation, since the total funds received are no longer exogenously determined: firms may influence both their initial bid and the probability or extent of renegotiation. The results from this exercise should therefore be interpreted as suggestive rather than fully causal. Nonetheless, the estimates reported in Panel B of Table 3 are remarkably similar to those in Panel A, both in magnitude and statistical significance. This consistency suggests that our key result, i.e., the observed performance gap between winners of efficient auctions and winners of lottery-like auctions, is not driven by differences in the amount of resources allocated, i.e., by the *level* effect, but rather by the *selection*, that is, the advantage of allocating public resources to more performing firms.

Robustness checks. Here, we outline several test we develop to validate our main findings. For ease of exposition, relevant tables and figures are relegated to Online Appendix E. First, we restrict the sample to contracts awarded within a narrower window around the regulatory threshold (i.e., plus or minus three bids). Second, we include control variables that proxy the experience of the public body, in order to account for potential endogeneity in the choice between the efficient and the lottery-like mechanism. Third, we extend the analysis by considering alternative outcome variables related to firm revenues. Fourth, we lengthen the time horizon, focusing on average effects within five years of the award rather than three. Finally, we re-estimate the results using the TWFE estimator.

[15] In practice, contracts may be adjusted during implementation, leading to additional transfers that can alter the total amount of funds allocated to the winning firm.

Figure 2: Baseline specification: event study



Notes: The Figure shows the even-study coefficients of β_1 from Equation 1, with the dependent is the logarithm of firm revenues. The β_1 represents the average impact of a one percent increase in the amount of public resources awarded efficiently on firm's revenues. The model includes firm and year fixed effects. Control variables include firm characteristics such as firm assets, value of the raw materials, number of employees, and firm age. Standard errors are clustered at the firm level and bootstrapped.

Table 3: Average effect

	(1)	(2)	(3)
		Revenue	
		Panel A: Baseline results	
Proj. size X Efficient award	0.058	0.080***	0.076**
	(0.042)	(0.030)	(0.032)
		Panel B: Only *selection effect*	
Overall Value X Efficient Award	0.059	0.081***	0.077***
	(0.043)	(0.031)	(0.032)
Controls		X	X
Regional trends			X
Observations	1,646	1,538	1,538

Panel A – Notes: This panel reports the β_k coefficients from Equation 1, where the dependent variable is log firm revenue measured three years after contract award. Specifications include firm and year fixed effects, regional linear trends where indicated, and firm-level controls (assets, raw material costs, employment, and age). Standard errors are clustered at the firm level and bootstrapped.

Panel B – Notes: This panel uses the same specification as Panel A, but replaces the key explanatory variable with the log of total public resources received by the firm, defined as the sum of the initial award and any additional funds allocated during contract execution.

5.2 Heterogeneous results

In this section, we investigate whether our main findings mask heterogeneity along two dimensions: (i) the contracting authority's screening ability to evaluate the credibility of bids, and (ii) the degree of competition in the (local) market for public contracts.

These characteristics play a crucial role in shaping the overall impact of more efficient resource allocation on firm performance. A stronger capacity to screen out unreliable bids increases the likelihood that public resources are allocated to firms capable of delivering high-quality outcomes. Likewise, in less competitive environments, where random allocation is more likely to produce suboptimal matches, the advantages of efficient selection become more pronounced. For ease of exposition, we report the relevant tables and figures in Online Appendix C.[16]

Screening ability. We find that our results depend on the contracting authority's ability to screen bids, which we proxy with bid-evaluation length. Table OA.C.1 and Figure OA.C.6 suggest that when screening capacity is weak, i.e., when public bodies have short bid-evaluation periods, allocating contracts efficiently does not generate statistically significant differences in post-award revenue growth relative to lottery-like allocation. Conversely, when public bodies exhibit stronger screening capacity with longer bid-evaluation periods, efficient allocation substantially amplifies firm performance. In this case, a 1% increase in the size of public resources allocated efficiently leads to an increase in firm revenues of approximately 11% over the subsequent three years. These results highlight the central role of institutional capacity in translating efficient awarding rules into real economic gains.

Market competition. We also document substantial heterogeneity across markets with different levels of competition, which we proxy with auction winners' turnover. Table OA.C.2 and Figure OA.C.7 suggest that the efficient allocation rule has its strongest impact in less competitive markets, where public resources are less likely to be disciplined by competitive forces alone. In these environments, a 1% increase in contract size awarded efficiently raises firm revenues by about 13% relative to lottery-like allocation. In contrast, in more competitive markets the estimated effect is smaller and statistically indistinguishable from zero, consistent with the idea that competition itself already performs an allocative role by directing contracts toward higher-performing firms.

[16] In Online Appendix C we also show that our main results remain stable across other relevant characteristics, such as contract and firm size, but differ depending on whether the contracting authority belongs to the local or central government.

5.3 Mechanisms

In this section, we explore the mechanisms underlying our baseline results (Section 5.1). We begin by examining firm-specific *ex ante* characteristics to assess along which dimensions winners of efficient auctions differ from those of lottery-like auctions. We then turn to *ex post* behavior to shed light on whether factors beyond inherent firm characteristics contribute to the higher growth of winners of efficient auctions.

Firm characteristics (ex ante). Having established that winners are comparable in size (see Figure B.5), we next compare indicators of firm performance within our sample of winning firms.[17] Table C.3 reports these comparisons. Columns (1) and (2) present differences in profit margins (in percentage points), columns (3) and (4) examine return on assets (ROA), and columns (5) and (6) consider firm age (measured as years since establishment).[18] Across all measures, firms selected through the efficient mechanism outperform those chosen through the lottery-like mechanism, despite being similar in size. Overall, these patterns indicate that the efficient allocation mechanism systematically selects more performing firms.

Firm behavior (ex post). Then, we examine the mechanisms underlying the revenue growth differences documented in Figure 2. The corresponding regression tables are reported in Appendix C, and the event-study figures testing these mechanisms are in Online Appendix D. We begin by analyzing whether firms selected under the efficient mechanism differ in the additional public resources they subsequently obtain through further contract awards via auctions, direct assignments, or negotiated procedures—within the construction sector. Because firms in our sample had no prior exposure to public procurement, this analysis is particularly informative: it reveals whether initial access to public contracts generates systematically different post-award trajectories in firms' responses to a demand shock.

Panel A of Table C.4 reports average effects within three years of the first contract award. A 1% increase in project value is associated with an approximately 20% increase in revenues from subsequent public contracts. This result suggests that allocating public resources to more efficient firms not only improves the immediate efficiency of public

[17]Further evidence is presented in Online Appendix D.

[18]Profit margins and ROA are particularly informative indicators of efficiency in this context. Profit margins capture how effectively firms convert revenues into profits after accounting for operating costs, while ROA measures how efficiently they use their asset base to generate returns. Given that we focus on a relatively homogeneous set of small construction firms operating under comparable cost structures and limited price-setting power, these indicators primarily reflect differences in cost efficiency rather than market power or accounting practices. Finally, age proxies for experience, which is often associated with higher efficiency and more stable performance.

spending but also facilitates sustained access to public procurement, thereby supporting longer-term firm growth.

Building on Lee (2022), we next examine whether firms selected through the efficient mechanism also experience higher private revenues, i.e., revenues unrelated to public demand—following contract award. This analysis is important because public contracts may affect private activity through competing channels: they can absorb capacity and crowd out private business, or alternatively enhance specialization, reputation, and complementarities that strengthen private-market performance. Panel B of Table C.4 shows that efficiently selected firms exhibit a modest, though imprecisely estimated, increase in private revenues within three years of the award. Taken together, these results indicate that allocating public resources to more efficient firms strengthens their position in public procurement while also supporting private business expansion, with no evidence of capacity crowding or displacement.

Finally, in Online Appendix D we show that the revenue effects documented in Figure 2 are unlikely to be driven by two alternative mechanisms emphasized in the literature: the relaxation of credit constraints following contract award and the realization of large, irreversible capital investments that permanently boost firm performance (e.g., Goldman 2019, Di Giovanni et al. 2022, Hebous and Zimmermann 2021). These channels are central in studies comparing winners to non-winners, where access to public resources itself drives performance differences. By contrast, our design compares two groups of winning firms receiving comparable contracts but selected through different procedures, which helps explain why we find no evidence supporting these mechanisms in our setting.

6 Aggregate implications

We conclude our empirical analysis exploring the potential aggregate implications of our micro-level findings. A vast literature on misallocation has shown that aggregate productivity growth is closely related to the efficiency with which inputs are allocated among productive units (see, for example the seminal contributions of Restuccia and Rogerson (2008b); Hsieh and Klenow (2009)). Following a similar reasoning applied to the allocation of public funds, a larger implementation of the efficient mechanism should improve aggregate productivity of the overall construction sector, not only because public contracts are allocated to more efficient firms but also because, as shown in Section 5, winners selected efficiently experience a higher growth than winners of lottery-like auctions.

In this perspective, we conduct two simulation exercises to assess how aggregate productivity would change under alternative scenarios with different degrees of efficient and lottery-like allocation mechanisms. The first exercise focuses on the market composed of firms participating in the set of contracts used in our main analysis, i.e., *public contract-level simulation*. In the second exercise, we broaden the definition of the market to encompass the entire construction industry, i.e., *sector-level simulation*. In both exercises, we simulate the allocation of medium-sized contracts under either efficient or lottery-like mechanisms, drawing winners from firms comparable to those in our main estimation sample. The resulting sector-level outcomes thus isolate the implications of changing the allocation rule for this class of contracts, holding all other features fixed.

Our measure of aggregate productivity is that introduced by Olley and Pakes (1996), who expresses it as the weighted sum of the productivity of the firms, where the weight is the size of the firms. In this formulation, aggregate productivity can be decomposed into: (i) unweighted average of firms' productivity; (ii) a "covariance" component, which measures the extent to which most productive firms are larger than the less productive ones. Specifically, in a market populated by N firms, the aggregate productivity is:

$$\Omega \equiv \sum_{i \in N} \theta_i \omega_i = \bar{\omega} + \sum_{i \in N} ((\theta_i - \bar{\theta})(\omega_i - \bar{\omega})) \tag{2}$$

where Ω denotes aggregate productivity, ω_i and θ_i represent firm-level productivity and size, respectively, and overbars indicate unweighted firm-level averages. This decomposition implies that, in a market with a given average productivity, aggregate productivity increases when more productive firms are also the larger ones.[19] The counterfactual analyses that follow, in which we simulate both efficient and lottery-like auctions, will affect this second term. In fact, given the same group of participants in a given auction, these two formats select different winners who will experience different revenue growth.

To proxy firm size, we use market share, and we measure firm-level productivity using profit margins, computed as sales minus expenses over revenues. Although profit margins may also capture elements of pricing power, this measure is appropriate in our context because we focus on a homogeneous set of construction firms operating in similar markets and under comparable regulatory and cost conditions, i.e., mainly small construction firms (Table A.2) within the same province and time period. In this environment, variation in profit margins primarily reflects differences in cost efficiency rather than market power, which, as shown in Section 2, is key to winning public contracts.

Public contract-level simulation. We conduct a Monte Carlo simulation to assess how

[19]See Piemontese (2023) for an analytical proof.

the share of efficiently allocated contracts affects aggregate productivity in the market for public construction works. Specifically, we replicate the 200 auctions included in our main sample. For each simulated auction, we draw the observed number of participants from a given pool of potential bidders, defined on the basis of province, year, and firm characteristics, such as number of employees and assets. The auction format is then determined probabilistically from a binomial distribution with mean $\lambda \in [0, 1]$, where λ represents the probability that an auction is conducted under the efficient mechanism. Conditional on the auction format, the winning firm is selected as follows: under the efficient mechanism, the most productive participant wins the contract; under the lottery-like mechanism, the winner is chosen randomly. Each winner is then assigned the observed contract size, which determines its post-award revenue growth.

We simulate two alternative scenarios. In the first, the only difference between the efficient and lottery-like mechanisms lies in how the winning firm is selected. In the second, consistent with our empirical estimates in Section 5.1, we assume that revenue growth following an efficiently awarded contract is 7.6% higher than under the lottery-like counterfactual (Table 3, column 3). In both cases, we update each firm's post-auction market share and compute aggregate productivity using Equation 2. Each auction simulation is iterated 1000 times for every value of λ.

Figure 3 Panel A presents the results. The dashed line represents the first scenario, which isolates the selection effect. Moving from a setting with only lottery-like auctions to one with only efficient auctions leads to a sizable improvement in market-level productivity, which increases by about 23%. The relationship is nearly linear: a 10-percentage-point increase in the share of efficient auctions raises aggregate productivity by roughly 2.3% on average. When we incorporate the higher post-award growth estimated in our firm-level analysis, the overall productivity gain reaches approximately 27%, meaning that the average increase for every 10-percentage-point rise in the probability of conducting an auction through the efficient mechanism equals 2.7%.

Sector-level simulation. This second exercise simulates the broader implications of exclusively using efficient auctions to allocate medium-size contracts across the entire construction sector. To this end, we replicate each medium-size auction conducted in Italy between 2007 and 2016 through a Monte Carlo simulation. As in the previous exercise, for each simulated auction, we draw the observed number of bids from a constructed pool of potential bidders, defined by province, year, and firm characteristics such as employment and asset size. Each auction is simulated 1000 times, and in every iteration the winner is selected according to either (i) the efficient mechanism—that is, the firm with the highest profit margin, or (ii) the lottery-like mechanism. Each winner is then assigned the observed contract size, which determines its post-award revenue growth. In the efficient case, this growth is augmented by 7.6%, following our main results in Table 3, column (3). Finally, we update each firm's post-auction market share and compute aggregate productivity for the entire construction sector using Equation 2.

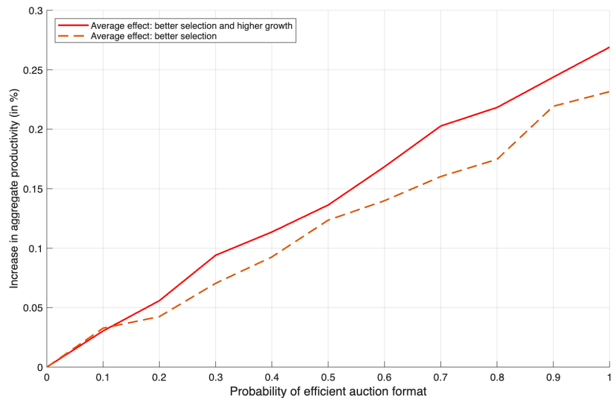
Moreover, in the spirit of the heterogeneity analysis developed in Section 5.2, we perform two additional simulations in which the post-award growth associated with efficient auctions is calibrated using the estimates reported in Tables OA.C.1 and OA.C.2. In both exercises, we divide auctions into two groups based on the characteristics of the contracting authority where the auction took place: (i) high versus low screening ability, and (ii) high versus low competition. In the first case, simulated efficient auctions conducted by contracting authorities with low screening ability generate no additional growth for winners (Table OA.C.1, column 3, where the coefficient is not statistically different from zero), while those promoted by authorities with high screening capacity yield an additional 10.9% increase in post-award revenue (Table OA.C.1, column 6). Similarly, in markets characterized by low competition, the adoption of efficient auctions leads to an extra 13% increase in revenue (Table OA.C.2, column 3), whereas in competitive environments the effect is statistically indistinguishable from zero (Table OA.C.2, column 6).

Figure 3 Panel B presents the results in a bar plot. Each bar represents the average increase in the aggregate productivity of the construction sector that would result if all medium-size auctions conducted between 2007 and 2016 were carried out using the efficient mechanism instead of the lottery-like one. The red lines indicate the 95% confidence intervals. In the baseline case (left bar), the increase in aggregate productivity is approximately 4.5%. In the alternative specifications, the increase is about 3.5% when auctions are split according to the level of competition faced by the contracting authority (center bar), and around 4.1% when distinguishing by the contracting authority's screening ability (right bar).

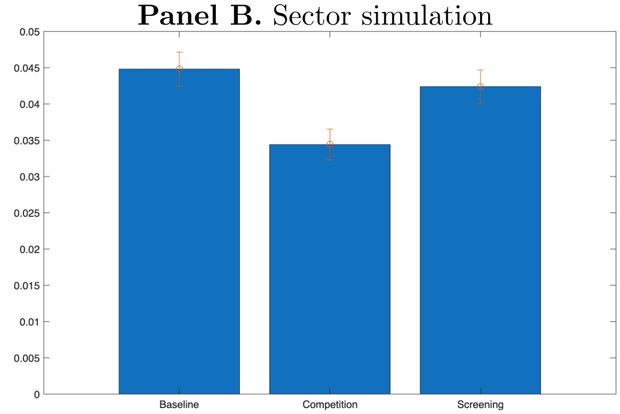
Overall, although these findings are derived from simulations, they highlight that the firm-level effects documented in Section 5 can translate into meaningful aggregate implications. When extrapolated to the entire construction sector, the systematic use of efficient auctions could generate productivity gains that are both sizable and economically relevant. While we cannot directly estimate the aggregate impact of the widespread reliance on lottery-like mechanisms for medium-size contracts during 2007–2016, our simulations indicate that the associated sectoral productivity losses may have been substantial.

Figure 3: Aggregate productivity effects of efficient allocation mechanisms

Panel A. Public contract simulation



Panel B. Sector simulation



Panel A — Notes: This figure shows the simulated increase in aggregate productivity as the probability of conducting an auction through the efficient mechanism rises. The dashed line isolates the selection effect, while the solid line incorporates post-award growth effects. Aggregate productivity is computed following Olley and Pakes (1996) as the weighted sum of firm-level productivity, proxied by profit margins and weighted by firms' market shares. The simulation consists of a Monte Carlo experiment that replicates 200 auctions 1,000 times, varying the probability that each auction is conducted through the efficient rather than the lottery-like mechanism. In each iteration, winners are selected according to the auction format, firm market shares are updated, and aggregate productivity is recalculated for each value of the efficiency probability λ.
Panel A — Notes: This figure reports results from Monte Carlo simulations replicating all medium-sized construction auctions conducted in Italy between 2007 and 2016. In each iteration, the observed number of bids is drawn from a pool of potential bidders defined by province, year, and firm characteristics. Winners are selected either through the efficient or the lottery-like mechanism, and post-award revenue growth is assigned accordingly. In the efficient scenario, growth is increased by 7.6% based on the estimates in Table 3. Aggregate productivity for the construction sector is computed using Equation 2. Additional simulations incorporate heterogeneity by calibrating post-award growth according to contracting authorities' screening ability and local market competition, as estimated in Tables OA.C.1 and OA.C.2. Bars report average yearly changes in sectoral productivity; vertical lines indicate 95% confidence intervals.

7 Conclusion

This paper shows that the design of public procurement mechanisms can meaningfully shape firm performance and, consequently, have aggregate implications. Exploiting quasi-random assignment of Italian construction contracts with efficient or lottery-like auctions, we find that efficiency in allocation matters: firms winning through the former mechanism experience substantially higher post-award revenue growth, particularly where contracting authorities have stronger screening capacities and competition is limited.

These effects stem from a clear selection mechanism: efficient auctions systematically identify more productive firms, which subsequently perform better across both public and private markets. Extending these findings to the sectoral level, our simulations suggest that exclusive use of efficient auctions could have increased sectorial productivity by about 4%, implying that past reliance on lottery-like mechanisms for medium-sided contracts may entailed notable efficiency losses.

The mechanisms we identify are not specific to our institutional context; rather, they hinge on the extent to which auction designs select firms based on underlying productivity. When allocation rules weaken this mapping, public resources are more likely to be misallocated, particularly in environments with limited screening capacity or low competitive pressure. These conditions are not unique to Italy, therefore our results apply to environments in which allocation rules weaken the selection of more productive firms (e.g., Lee (2022)), and/or in which contracting authorities differ in their ability to screen bidders. Thus, our results can speak more generally to settings in which public demand represents a sizable firm-level shock and allocation mechanisms play a meaningful role in determining which firms receive the public resources.

Clearly, the magnitude of the effects may depend on firms' prior exposure to public procurement. Our analysis focuses on first-time winners, for whom entry into public markets constitutes a substantial demand shock. In settings where procurement is dominated by "incumbents", growth effects may be attenuated because of lower marginal returns to public demand. Nonetheless, even in such environments, more efficient allocation rules can improve aggregate outcomes by reallocating resources toward more productive firms.

Finally, these results carry important policy implications: the design of public procurement mechanisms is not merely a matter of administrative efficiency but a key determinant of aggregate growth. In periods of post-crisis recovery, whether from pandemics, wars, or the transition toward climate resilience, the ability to allocate public resources efficiently can play a pivotal role in sustaining productivity.

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Appendix A

This Appendix reports the descriptive statistics discussed in Section 3. Table A.1 presents information on public procurement by province, as well as construction firms characteristics. On average, each province awards 53 public tenders annually, accounting for over half of the yearly public contract expenditure and about 40% of the total number of medium and large contracts, i.e., with a reservation price above €40,000.[20] Roughly 50% of the expenditure on public tenders is awarded through a sealed-price mechanism, representing nearly 65% of all tenders.

Concerning the execution of these contracts, we observe an average rebate of approximately 25%, an average delay of 15 days, about four renegotiations, and roughly one and a half subcontracting instances per province. Finally, more than 70% of contracts are awarded to building companies, while infrastructure firms account for only 4%, and other types of firms for about one quarter. These firms are on average small: they employ around five workers, generate annual revenues of roughly €1 million, and hold capital assets of about €150,000. This is in line with the structure of the Italian construction sector, dominated by small firms with limited capitalization but widespread participation in public procurement markets.

Table A.2 focuses on contracts awarded around the efficient/lottery-like mechanism threshold, and on the firms winning these contracts. First, these contracts are, on average, smaller than the typical contracts awarded through sealed-price public tenders in the construction sector. They are also less frequently associated with cost overruns, time suspensions, and subcontracting. This pattern is consistent with the institutional design of the lottery-like mechanism, which applies to medium-sized contracts, and indicates that the contracts used for identification are not among the most complex or execution-intensive projects in the sector.

Second, firms winning contracts around the threshold are larger than the average firm operating in the construction sector. In particular, they exhibit higher revenues, larger input expenditures, and greater employment. This is consistent with the fact that participation in public procurement involves fixed administrative and compliance costs, which are more easily borne by firms with a larger scale of operations, even when contracts are of medium size.

Third, when comparing treated and control contracts around the threshold, some small

[20] The €40,000 threshold is relevant under Italian procurement law. During the period of analysis, public bodies could directly award contracts smaller than €40,000 without a tender. As a result, the vast majority of such contracts were directly awarded to firms (Tulli 2025).

differences in observable characteristics emerge. Contracts awarded under the efficient mechanism are slightly larger, more likely to experience cost overruns, time suspensions, and subcontracting, and are awarded to firms that are marginally larger. While some differences are present, they do not pose a concern for identification, as they are not systematic once appropriate adjustments are taken into account. As shown in Section 4, when comparisons are conducted using logarithmic transformations of contract and firm characteristics, the observed disparities largely disappear. Moreover, in Online Appendix B we show that including year fixed effects to control for common shocks yields similar conclusions. Overall, this evidence suggests that residual differences between treated and control groups are driven by outliers and common time shocks rather than by systematic selection, reinforcing the comparability of contracts and firms around the threshold and supporting the validity of the identification strategy (discussed in Section 4).

Table A.1: Descriptive statistics (sector)

	Mean	Median	SD
Contracts characteristics			
N public tenders	53	29	102
% exp. with public tenders	0.56	0.58	0.28
% N awarded with public tenders	0.37	0.29	0.28
% exp. in sealed-price tenders (as a sh. of all public tenders)	0.52	0.51	0.31
% N. sealed-price tenders (as a fraction of all public tenders)	0.64	0.67	0.22
Sealed-price tenders contract size (in th. Euro)	2,146	0.779	8,105
Share of discount (i.e. rebate)	0.24	0.25	0.07
Time overrun (in days)	14.8	0.45	47.9
Cost overrun (number)	4	1	12
Subcontract (number)	1.5	0	4.9
Firms characteristics			
% contracts won by building comp.	0.70	0.71	0.08
% contracts won by infrastructure comp.	0.04	0.05	0.09
% contracts won by others	0.26	0.25	0.05
Revenue (in th. Euro)	1,049	248.9	15,324
Employees	5.1	1.6	110.6
Fixed assets (in th. Euro)	148.7	110.7	128.0
Raw materials (in th. Euro)	444	112	4,138

Notes: The table presents descriptive statistics for relevant procurement behavior on the construction sector (i.e., CPV 45) and firm characteristics. First, we calculate the average number of public tenders issued annually in each province, followed by the share of overall expenditure and the number of public contracts awarded through public tenders. We then compute the proportion and number of tenders issued with sealed-price mechanisms (i.e., either efficiently or with the lottery-like mechanism), along with the average contract size for these auction formats.
Next, we provide statistics on the execution of public works, including the rebate value, the number of delay days, and the average number of cost overruns and subcontracting instances. Regarding firm characteristics, we begin by showing the sectoral distribution of awarded contracts, followed by the average revenue, number of employees, and cost of inputs.

Table A.2: Descriptive statistics (sample)

	Mean	Median	SD	N
Treated group				
Contracts characteristics				
Project size (in th. Euro)	480.9	259.3	638.2	179
Rebate (%)	0.179	0.173	0.104	179
Time overrun (in days)	6.26	0	37.8	179
Cost overrun (number)	0.07	0	0.33	179
Subcontract (number)	0.01	0	0.11	179
Firms characteristics				
Revenue (in th. Euro)	3,556	1,484	5,850	133
Employees	15.4	8	26.8	133
Fixed assets	224.3	49.9	669.4	133
Raw materials (in th. Euro)	1,153	498.3	1,899	133
Control group				
Contracts characteristics				
Project size (in th. Euro)	416.0	281.6	374.9	102
Rebate (%)	0.19	0.18	0.10	102
Time overrun (in days)	0.43	0	4.35	102
Cost overrun (number)	0.01	0	0.09	102
Subcontract (number)	0.01	0	0.10	102
Firms characteristics				
Revenue (in th. Euro)	2,658	1,412	3,577	64
Employees	12.3	7	17.03	64
Fixed assets	104.9	34.4	213.4	64
Raw materials (in th. Euro)	800	407	1,228	64

Notes: The table reports descriptive statistics for the contracts and firms in our sample. A total of 281 contracts are awarded around the threshold to firms winning a public procurement contract for the first time; however, balance-sheet information is available for only about 200 of these firms, which are subject to mandatory financial disclosure. Descriptive statistics are presented separately for the treated group (contracts with a number of bids below the threshold) and the control group. On the contract side, we report the mean, median, and standard deviation of project size, rebate, time overruns, the number of cost overruns, and the number of subcontracts. On the firm side, we report the same statistics for revenues, employment, fixed assets, and raw material costs.

Appendix B

In this Appendix, we discuss in greater detail the credibility of our key identifying assumption that the number of bids is not manipulable by neither the contracting authorities nor the auction participants. We also report and discuss the figures described in Section 4.

We begin by establishing the role of the threshold in determining the allocation rule. Figure B.1 shows that the threshold in the number of bids is directly relevant for the applicability of the lottery-like mechanism, with contracts above the cutoff being substantially more likely to be allocated through this rule. This evidence confirms that the threshold induces a discrete change in the allocation mechanism, which constitutes the basis of our identification strategy.

We then discuss the key identifying assumption of non-manipulation of contracts around the threshold. This assumption is central to the paper and requires that neither the demand side (public bodies) nor the supply side (firms) can precisely manipulate the number of bids received by a contract so as to endogenously determine the awarding mechanism.

Demand side. Figure B.2 shows no evidence that contracting authorities manipulate the overall number of public tenders around the threshold. While this absence of bunching is reassuring, it is not by itself sufficient to rule out more subtle forms of manipulation. In particular, even if the distribution of bids appears smooth, contracts located just above and just below the threshold could still differ systematically in their underlying characteristics, rather than differing only because of the allocation rule triggered by the threshold. For example, contracting authorities could deliberately assign larger, technically more demanding, or administratively complex projects to one side of the cutoff—such as keeping them below the threshold—while allocating smaller or simpler contracts above it. In such a scenario, the bid distribution would remain continuous around the threshold, yet contracts on the two sides would differ along dimensions that are directly relevant for procurement outcomes, such as size, complexity, or execution risk. A related concern is that contracting authorities may differ substantially in procurement experience and administrative capacity. More experienced public bodies may be better able to design tenders, prepare documentation, advertise contracts effectively, or set eligibility requirements in ways that attract a larger pool of potential bidders. As a result, these public bodies may systematically receive higher bidder participation than less experienced authorities, independently of the awarding mechanism itself. If more experienced public bodies were also more likely to place contracts on one side of the threshold, variation in the number of bids around the cutoff could reflect differences in public-body characteristics rather than the causal effect of the allocation rule.

In this perspective, Figures B.3 and B.4 directly address these concerns. They show that contracts awarded just below and just above the threshold are comparable in terms of project size and project complexity, and that they are issued by public bodies with similar levels of procurement experience. This evidence supports that contracting authorities do not systematically sort contracts around the threshold based on observable characteristics, nor do differences in administrative capacity explain variation in bidder participation near the cutoff.

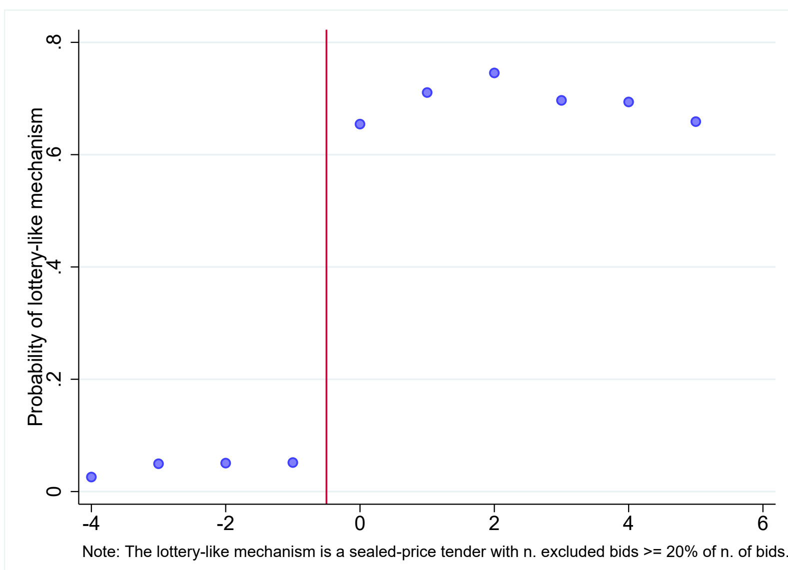
Supply side. One potential threat to the validity of our identification strategy is that firms may strategically adjust their participation in procurement procedures based on expectations about whether a contract is likely to fall just below or just above the threshold that determines the awarding mechanism. In other words, firms might choose to participate or refrain from participating in tenders they expect to be awarded under a preferred mechanism. Such behavior would not require firms to observe the realized number of bids ex ante, but it would require them to form sufficiently accurate expectations about which side of the threshold a contract is likely to end up on.

While firms may plausibly form coarse expectations about participation—such as whether a tender is likely to attract relatively few or many bidders—translating these expectations into reliable predictions about whether the final number of bids will fall just below or just above the cutoff is substantially more demanding. This difficulty arises from uncertainty about rivals' entry decisions, heterogeneity in firms' bidding costs, and the presence of non-coordinated bidders whose participation is hard to anticipate. As a result, even if firms condition their participation on expected competitiveness, precisely targeting contracts that end up on a specific side of the threshold remains challenging. A related concern is that firms could engage in coordinated entry or cartel-like behavior to influence participation more directly. In principle, coordination could allow cartel members to discourage participation by some firms, thereby affecting the total number of bids and increasing the likelihood that a contract falls on a particular side of the threshold. However, translating coordinated behavior into precise control over the realized number of bids would require detailed, contract-specific information about the expected participation of all potential competitors, including non-cartel firms. It would also require the ability to anticipate last-minute entry or withdrawal decisions and to enforce strict compliance among cartel members. Even a single unexpected entrant or deviation would be sufficient to shift the realized number of bids across the threshold and alter the applicable awarding mechanism.

Consistent with the view that firms cannot systematically sort participation with this degree of precision, Figure B.5 shows that firms winning contracts around the threshold are comparable in observable characteristics such as size, revenues, input costs, and employment. The absence of systematic differences between firms winning contracts just below and just above the threshold suggests that firms with different observable attributes do not selectively participate in tenders in a way that places contracts on a specific side of the cutoff.

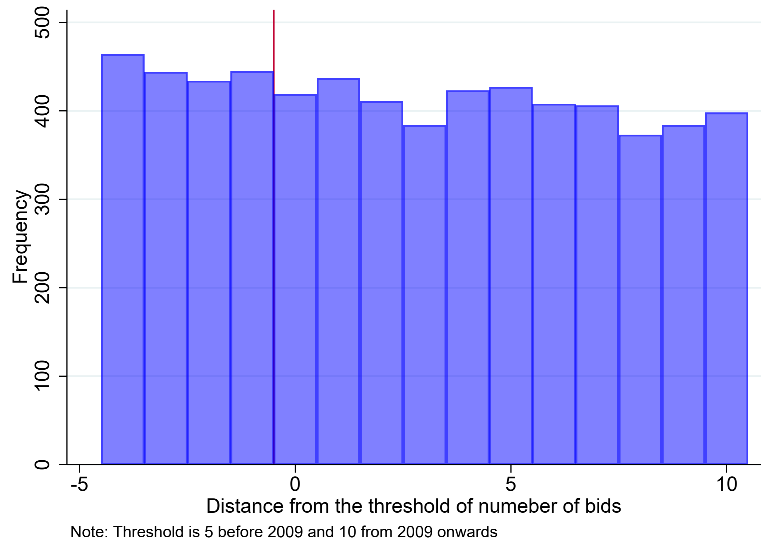
Finally, Table C.3 shows that the only systematic difference between firms winning contracts around the threshold concerns firm efficiency. We interpret this result as evidence that the efficient allocation mechanism is indeed able to successfully selects more efficient firms on average. This pattern would invalidate the identification strategy only if more efficient firms were systematically able to anticipate which contracts would be awarded under the efficient mechanism and adjust their participation accordingly. Given the absence of sorting in other observable firm characteristics and the limited ability of firms—whether acting individually or through coordination—to precisely control participation around the threshold, we consider this possibility unlikely and rule it out by assumption.

Figure B.1: Probability of excluding a bid



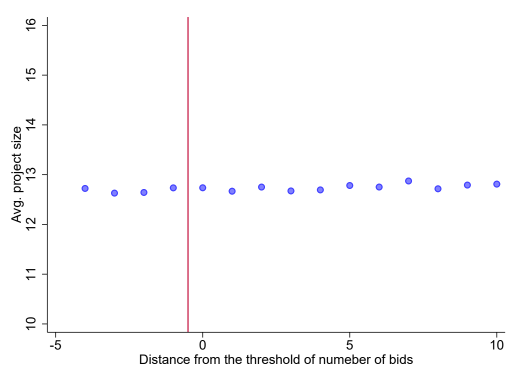
Notes: The figure plots the probability that contracts with a number of eligible bids around the regulatory threshold for the efficient mechanism exclude at least 20% of submitted bids. Before 2008, the threshold was five eligible offers, and it increased to ten thereafter. This probability is close to zero for contracts below the threshold but increases sharply once the number of bids reaches or exceeds the cutoff for the lottery-like mechanism, which requires the exclusion of at least 20% of bids.

Figure B.2: Frequency of public tenders by number of bids

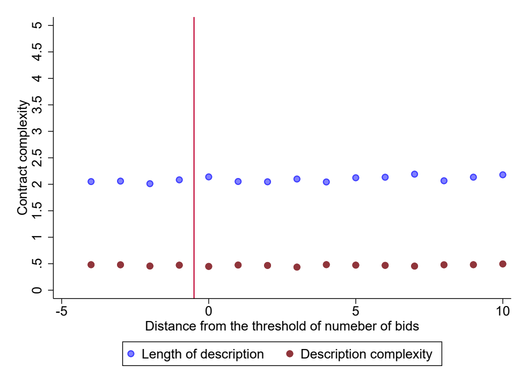


Notes: The figure shows the distribution of medium-size public tenders (either awarded efficiently or with the lottery-like mechanism) by number of eligible bids (x-axis). The x-axis is normalized with respect to the threshold of applicability of the lottery-like mechanism. Therefore, contracts on the left-hand side of the threshold are awarded efficiently, while the contracts on the right-hand side of the threshold can be awarded with a the lottery-like mechanism.

Figure B.3: Average contract characteristics by number of bids



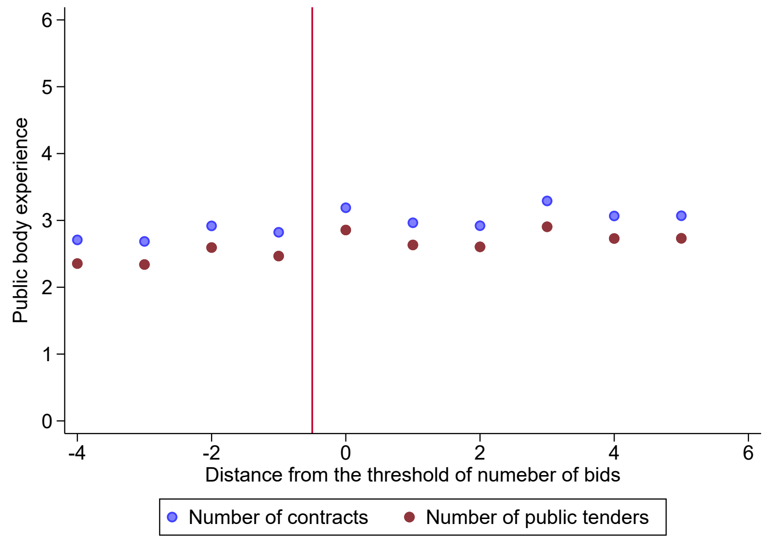
(a) Project size



(b) Contract complexity by number of bid

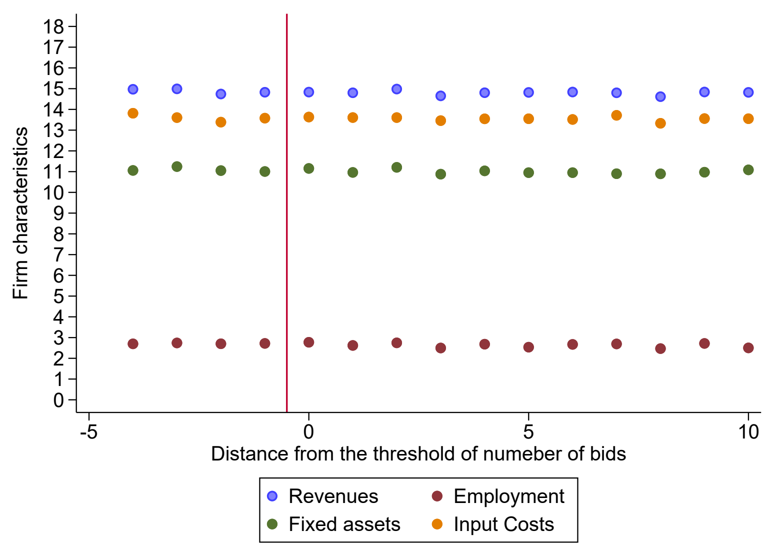
Notes: The figure reports average contract and public-body characteristics by number of bids, controlling exclusively for year fixed effects. The panels display: (log of) average project size in panel (a); the (log of) number of words in the contract description and the share of unique words in the contract description in panel (b).

Figure B.4: Public body characteristics by number of bid



Notes: The figure reports average contract and public-body characteristics by number of bids, controlling exclusively for year fixed effects. The panel displays the (log of) total number of contracts previously awarded by the public body, and the (log of) total number of public tenders previously awarded by the same public body in panel (c).

Figure B.5: Firm characteristics by number of bids



Notes: The figure reports average contract and public-body characteristics by number of bids, controlling exclusively for year fixed effects. The panels display: (log of) firm revenue; the (log of) input costs; and (log of) number of employees.

Appendix C

We report here the tables described in Section 5.3. First, Table C.3 shows that firms selected through the efficient mechanism are, on average, more efficient than those selected through the lottery-like mechanism even before the contract award, while remaining comparable in size (see also Section 4). In particular, firms in the treated group exhibit higher profit margins and returns on assets (ROA), and are on average older than firms in the control group. These differences persist even after including year and sector fixed effects to account for common shocks and sectoral heterogeneity in firm performance. Overall, these results highlight that the efficient mechanism is effective at selecting more efficient firms relative to the lottery-like mechanism.

Table C.4 shows that the award of a public contract leads to greater subsequent public procurement revenues for firms selected through the efficient mechanism, without crowding out revenues from private markets.

Table C.3: Comparison of pre-award characteristics: Performance measures

	(1)	(2)	(3)	(4)	(5)	(6)
	Profit margin	Profit margin	ROA	ROA	Age	Age
Efficient award winner	3.62**	3.05**	2.03**	1.90**	3.29**	3.30**
	(1.48)	(1.40)	(0.95)	(0.93)	(1.66)	(1.65)
Sector FEs		X		X		X
Year FEs		X		X		X
Observations	703	703	703	703	750	750

Notes: The table reports differences in the conditional means between winners selected through the efficient mechanism and those chosen via the lottery-like mechanism across several balance sheet variables proxying firm performance. We focus on the years preceding the award of public resources and, where indicated, include year and sector fixed effects. The outcomes considered are profit margin, return on assets (ROA), and firm age.

Table C.4: Average effect of the efficient allocation on firm public and private revenue

Panel A: Public revenue	(1)	(2)	(3)
Proj. size X Efficient Award	0.236***	0.205***	0.205***
	(0.032)	(0.028)	(0.039)
Controls		X	X
Regional trends			X
Observations	1,646	1,538	1,538

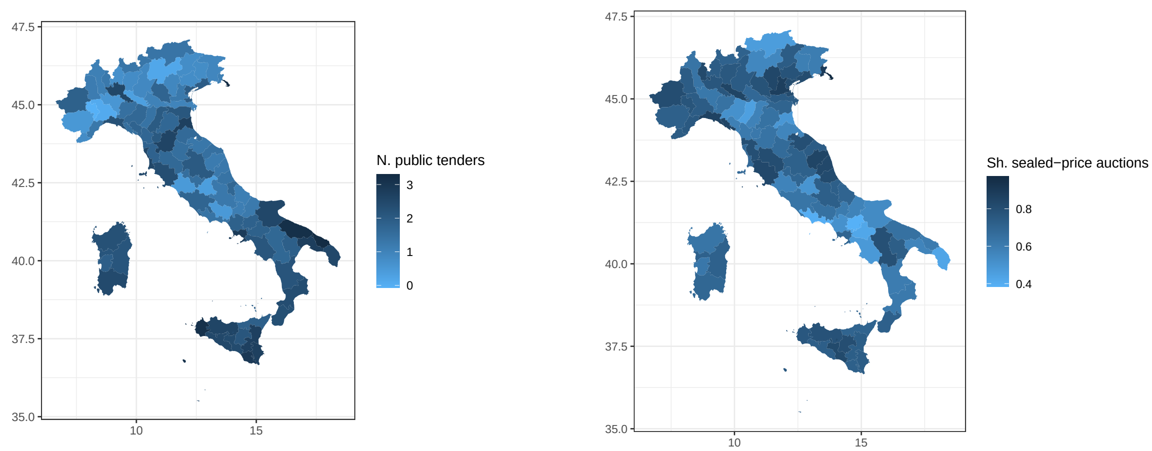
Panel B: Private revenue	(1)	(2)	(3)
Proj. size X Efficient Award	0.015	0.044**	0.037
	(0.041)	(0.025)	(0.027)
Controls		X	X
Regional trends			X
Observations	1,646	1,538	1,538

Notes: The table shows the coefficient β resulting from estimating Equation 1 and focusing on the change in revenue three years after the award. The dependent variable is the total public revenue excluding that from the initial award (Panel A) and private revenue (Panel B), measured in logarithms. The model includes firm and year fixed effects, as well as regional linear trends (where specified). Control variables include firm characteristics such as firm assets, cost of raw materials, number of employees, and firm age. Standard errors are clustered at the firm level and bootstrapped.

Online Appendix A

Figure OA.A.1 summarizes the geographical distribution of construction-sector auctions, reporting tender activity and the share of medium-sized sealed-price tenders across provinces. The use of alternative allocation rules is broadly balanced geographically, supporting the comparability of provinces in the empirical analysis.

Figure OA.A.1: Geographical distribution of procurement contracts



(a) N. public tenders

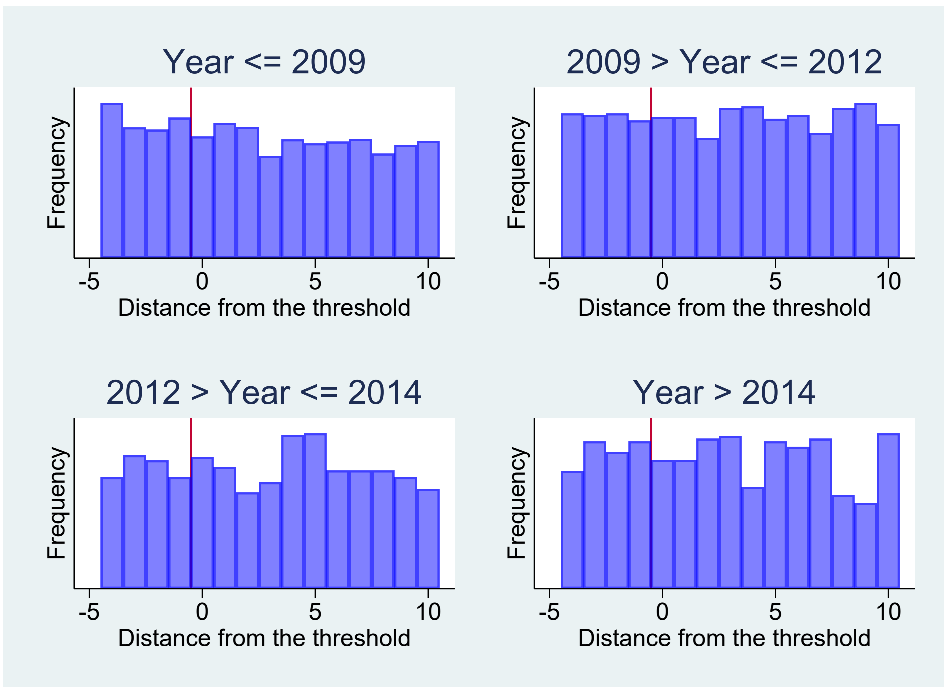
(b) Sh. sealed-price medium-size auctions

Notes: Geographic distribution of public tenders and allocation mechanisms. Panel (a) shows the average annual number of tenders—encompassing both sealed-price and scoring formats—conducted by each public body in every province. Panel (b) reports the provincial average yearly share of contracts awarded through sealed-price tenders, i.e., auctions that may be conducted under either the efficient or the lottery-like mechanism.

Online Appendix B

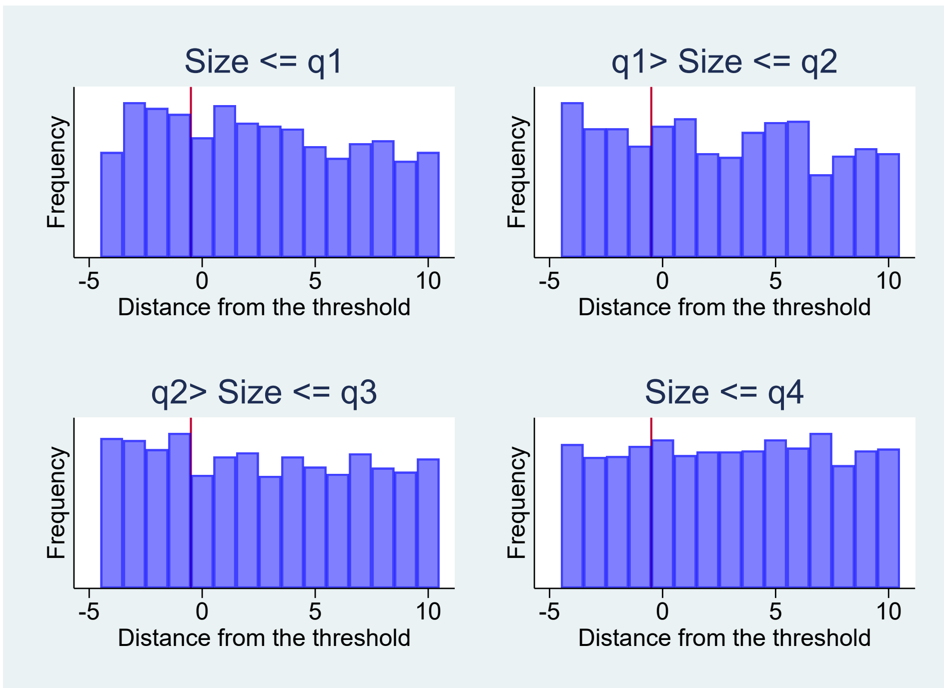
This Appendix presents additional balance evidence around the threshold. Figures OA.B.2 and OA.B.3 report alternative versions of Figure B.2, splitting contracts by year and by project size, respectively. Figure OA.B.4 extends Figure B.3 by controlling for year fixed effects. Figure OA.B.5 shows that firm characteristics remain balanced around the threshold. Together, these figures provide further support for the identifying assumptions discussed in the main text.

Figure OA.B.2: Frequency of public tenders by number of bids in separate years



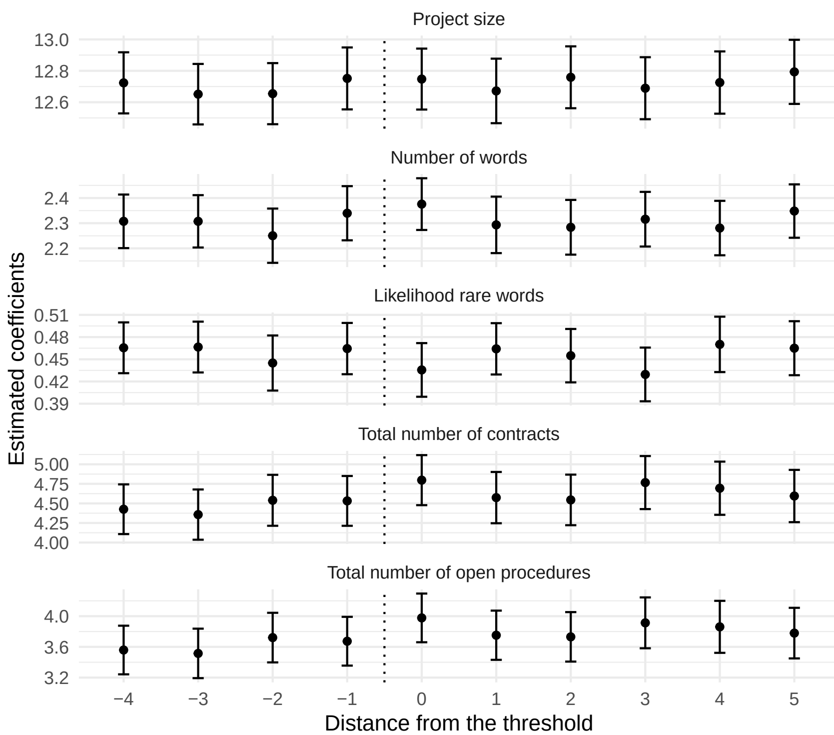
Notes: The figure shows the distribution of the public tenders by number of bids (x-axis) by different years. The x-axis is normalized with respect to the threshold of applicability of the lottery-like mechanism. Therefore, contracts on the left-hand side of the threshold are awarded efficiently, while the contracts on the right-hand side of the threshold are awarded with the lottery-like mechanism. The top-left panel reports the distribution for contracts awarded between 2007 and 2009, the top-right for those between 2010 and 2012, the bottom-left for 2013–2014, and the bottom-right for 2015–2016.

Figure OA.B.3: Frequency of public tenders by number of bids of different sizes



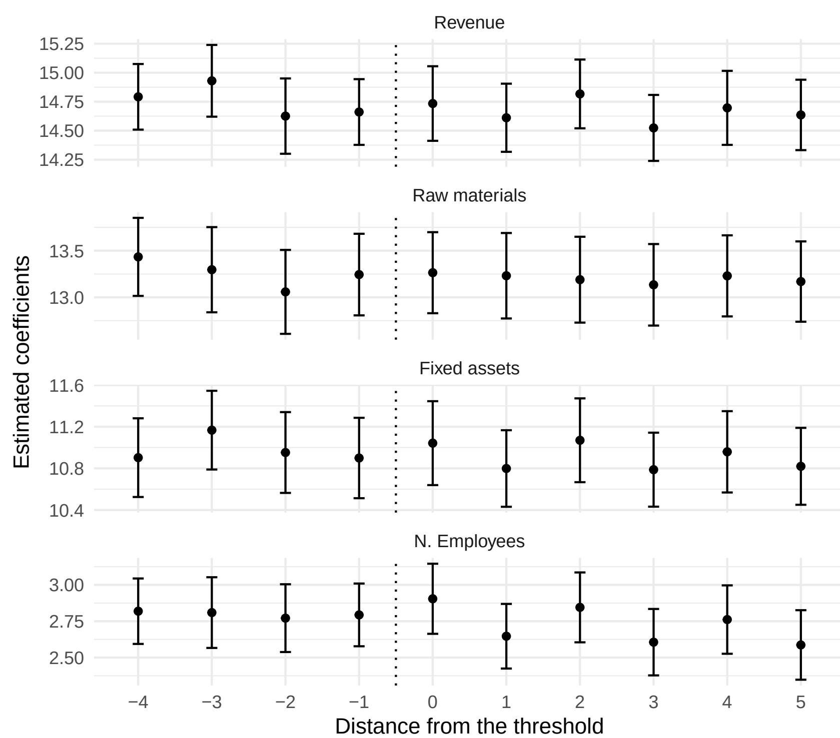
Notes: The figure shows the distribution of the public tenders by number of bids (x-axis) by different contract sizes. q1,q2,3, and q4 stand respectively for the first, second, third and last quartile. The x-axis is normalized with respect to the threshold of applicability of the lottery-like mechanism. Therefore, contracts on the left-hand side of the threshold are awarded efficiently, while the contracts on the right-hand side of the threshold are awarded with the lottery-like mechanism. The top-left panel reports the distribution for contracts smaller than the first quartile, the top-right for those between the first and second quartile, the bottom-left between the second and third quartile, and the bottom-right for the contracts larger than the third quartile.

Figure OA.B.4: Average contract and public body characteristics by number of bids



Notes: The figure reports average contract and public-body characteristics by number of bids, controlling exclusively for year fixed effects. From top to bottom, the panels display: (log of) average project size; the (log of) number of words in the contract description; the share of unique words in the contract description; the (log of) total number of contracts previously awarded by the public body; and (log of) the total number of public tenders previously awarded by the same public body.

Figure OA.B.5: Average firm characteristics by number of bids



Notes: The figure reports average firm characteristics by number of bids, controlling exclusively for year fixed effects. From top to bottom, the panels display: (log of) revenue; the (log of) input costs; and (log of) the number of employees.

Online Appendix C

Screening ability. Following Decarolis (2014), we proxy a public body's screening capacity using the average number of days between the bid submission deadline and the contract award date for all other tenders it issued in the construction sector. A longer interval is interpreted as allowing more time for bid evaluation and is therefore indicative of greater screening capability. We classify contracting authorities as having *high screening* ability when their average screening time exceeds the sample median.

Figure OA.C.6 and Table OA.C.1 present the results based on Equation 1, where the sample is divided according to the screening ability of contracting authorities. Table OA.C.1 shows that our average effects (Table 3) mask substantial heterogeneity depending on the screening capability of the contracting authority. When the awarding body has worse ability to adequately assess and exclude non-credible bids, the difference in revenue growth between firms selected efficiently and those selected with the lottery-like mechanism is statistically insignificant (Columns 1–3). By contrast, when the contracting authority exhibits stronger screening capacity, allocating public resources through an efficient mechanism yields a markedly larger increase in firm revenues (Columns 4–6). In these cases, a 1% increase in the size of public resources allocated efficiently is associated with roughly an 11% rise in firm revenue over the following three years.

Figure OA.C.6 Panel (a) presents the estimated effects for contracting authorities with lower screening capacity, while Panel (b) displays the corresponding effects for those with higher screening ability. In both cases, the revenue trajectories of firms selected with efficient and lottery-like mechanisms are comparable prior to the award and vanish approximately four years later.

Market competition. We next show that the aggregate results mask important heterogeneity across markets with different levels of competition. To measure competition for each contracting authority, we focus on entities that awarded at least three construction tenders during the study period. For these authorities, we calculate the share of contracts allocated to distinct winners and classify markets as competitive when this share exceeds the sample median, with the remaining cases labeled as less competitive. We then examine how this distinction translates into differences in the dynamic effects of efficient allocation. Table OA.C.2 reports estimates of Equation 1, focusing on the average impact within three years of the contract award. Over this horizon, allocating public resources efficiently has a positive effect on winning firms' revenues, particularly in markets with lower competition: the revenues of efficiently selected firms increase by about 13% more

Table OA.C.1: Results on revenue by screening ability

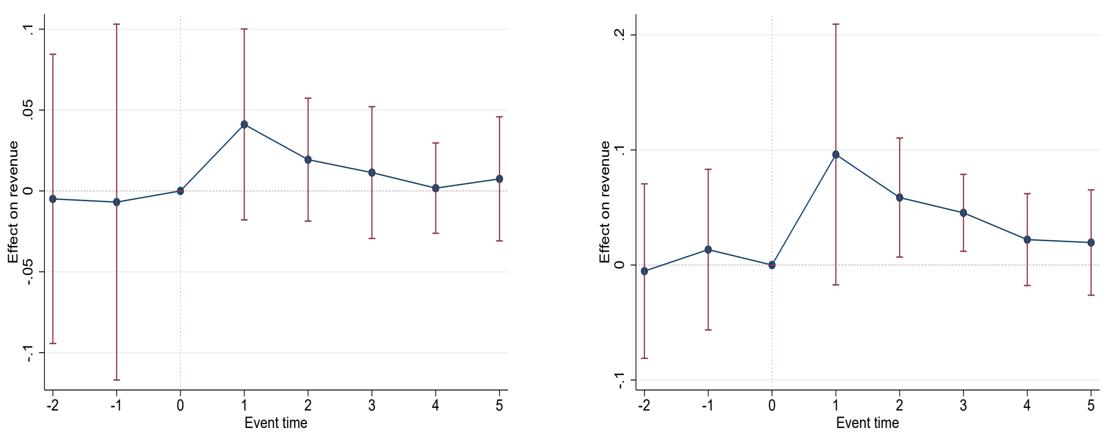
	Low screening t.			High screening t.		
	(1)	(2)	(3)	(4)	(5)	(6)
	Revenue	Revenue	Revenue	Revenue	Revenue	Revenue
Proj. size X Efficient award	0.004	0.037	0.040	0.113**	0.116**	0.109**
	(0.073)	(0.037)	(0.044)	(0.052)	(0.050)	(0.055)
Controls		X	X		X	X
Regional trends			X			X
Observations	800	739	739	834	789	789

Notes: The table presents the results of estimating Equation 1 focusing on the change in revenue three years after the award, distinguishing between the contracts awarded by public bodies with either low (Columns 1 to 3) or high screening ability (Columns 4 to 6). The dependent variable is the logarithm of the firm revenue. The model includes firm and year fixed effects, as well as regional linear trends (Columns 3 and 6). Control variables include firm characteristics such as firm assets, cost of raw materials, number of employees, and firm age. Standard errors are clustered at the firm level and bootstrapped. The screening ability is computed as the average day that the public body takes between the last day to participate in the tender and the awarding date, as suggested by Decarolis (2014). The median is 60 days.

Figure OA.C.6: Results by screening ability of the public body

(a) Low screening ability

(b) High screening ability



Notes: The Figure shows the even-study coefficients of the Equation 1 split by public bodies with different screening ability. The dependent variable is the logarithm of the firm revenue. The model includes firm and year fixed effects. Control variables include firm characteristics such as firm assets, cost of raw materials, number of employees, and firm age. Standard errors are clustered at the firm level and bootstrapped. The screening ability is computed as the average day that the public body takes between the last day to participate in the tender and the awarding date, as suggested by Decarolis (2014). The median is 60 days.

Table OA.C.2: Results on revenue by competition in procurement market

	Low competition			High competition		
	(1)	(2)	(3)	(4)	(5)	(6)
	Revenue	Revenue	Revenue	Revenue	Revenue	Revenue
Proj. size X Efficient award	0.132*	0.133**	0.133*	0.027	0.056	0.043
	(0.073)	(0.065)	(0.081)	(0.050)	(0.040)	(0.045)
Controls		X	X		X	X
Regional trends			X			X
Observations	565	537	537	1,077	998	998

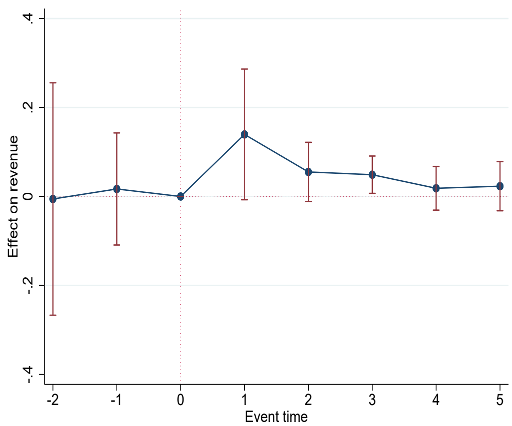
Notes: The table presents the results of estimating Equation 1 focusing on the change in revenue three years after the award, distinguishing between the contracts awarded in more or less competitive procurement markets. The dependent variable is the logarithm of the firm revenue. The model includes firm and year fixed effects, as well as regional linear trends (Columns 3 and 6). Control variables include firm characteristics such as firm assets, cost of raw materials, number of employees, and firm age. Standard errors are clustered at the firm level and bootstrapped. The degree of competition is computed as the share of different winners for the public tenders in the construction sector computed for each public body with at least three public tenders.

than winners of lottery-like auctions for each 1% increase in contract size. These results are highly robust to the inclusion of firm-level controls and regional trends. By contrast, in more competitive markets, the estimated effect is smaller and statistically indistinguishable from zero, consistent with competition already channeling public resources toward high-performing firms.

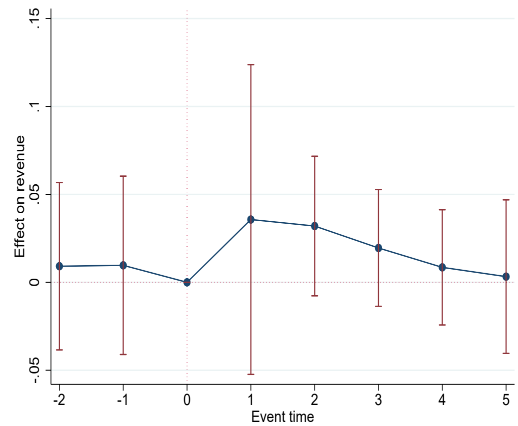
Figure OA.C.7 reports the corresponding event-study estimates from Equation 1, splitting the sample according to the local market competitiveness. The panel (a) presents the effects of awarding public resources efficiently in low-competitive markets, while panel (b) reports the results for high-competitive markets. In both cases, the pre-award revenue trends of winning firms are comparable. Following the award, efficiently selected firms exhibit higher revenues within the first three years in less competitive markets, although the effect gradually dissipates thereafter. By contrast, in more competitive markets, the impact of efficient allocation is smaller and estimated with less precision.

Figure OA.C.7: Results by local public procurement market

(a) Low competition



(b) High competition



Notes: The Figure shows the even-study coefficients of the Equation 1 split by local public procurement markets. The dependent variable is the logarithm of the firm revenue. The model includes firm and year fixed effects. Control variables include firm characteristics such as firm assets, cost of raw materials, number of employees, and firm age. Standard errors are clustered at the firm level and bootstrapped. The local public procurement market is computed as the share of different winners on the overall number of contracts. The median is 0.9.

Other characteristics. We complement the analysis in Section 5.2 with additional heterogeneity analyses aimed to assess further characteristics that might be relevant for making efficient public spending more effective in enhancing firm performance. We first focus on project size. Table OA.C.3 reports the estimates of Equation 1 separately for projects below and above the median size. While increasing project size by 1% has a statistically significant effect on firm performance primarily for larger projects, the estimates for smaller projects are similar in magnitude though less precisely estimated. Therefore, we find no conclusive evidence on whether the effects of efficient allocation of public contracts is mainly driven by larger projects.

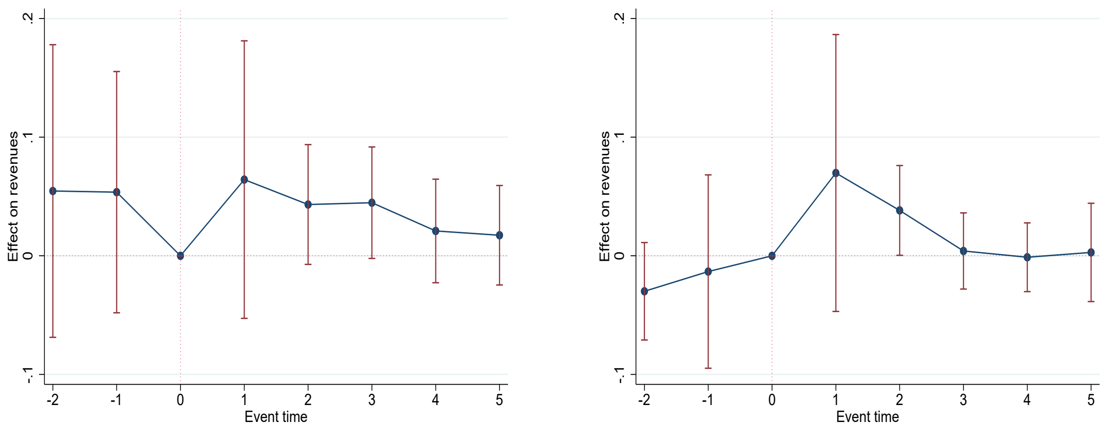
Second, we test the role of firm size. The idea is to understand whether a more efficient targeting of public resources is more effective among small firms, which are on average less performing but that might hide greater growth potential, relative to already established and larger firms. We split the sample based on the number of employees each firm employ and we divide them among those with a number of employees larger (or smaller) than the median, i.e., 8 employees.

Figure OA.C.9 and Table OA.C.4 show how the efficient award of public resources has a positive effect on firm revenue irrespective of firm's size, even though the estimates are less precisely estimated than in the case of Figure 2, in which we compute the average

Figure OA.C.8: Results by project size

(a) Low Proj. size

(b) High Proj. size



Notes: The Figure shows the even-study coefficients of the Equation 1 split by project size. The model includes firm and year fixed effects. Control variables include firm characteristics such as firm assets, cost of raw materials, number of employees, and firm age. Standard errors are clustered at the firm level and bootstrapped. The median size of the projects in the sample is 228,494 Euro.

effect on all the firms.

Finally, we compare the effects of efficient allocation in local and central administrations. This comparison is motivated by the fact that local administrations may face larger informational frictions and greater variation in firm performance than central administrations, so the effects of allocating public resources to more efficient firms could be more pronounced at the local level. Both Table OA.C.5 and Figure OA.C.10 show that the effect is primarily driven by local public demand i.e., public resources allocated by local administrations, while firms selected by central administrations grow at a comparable rate regardless of the allocation method.

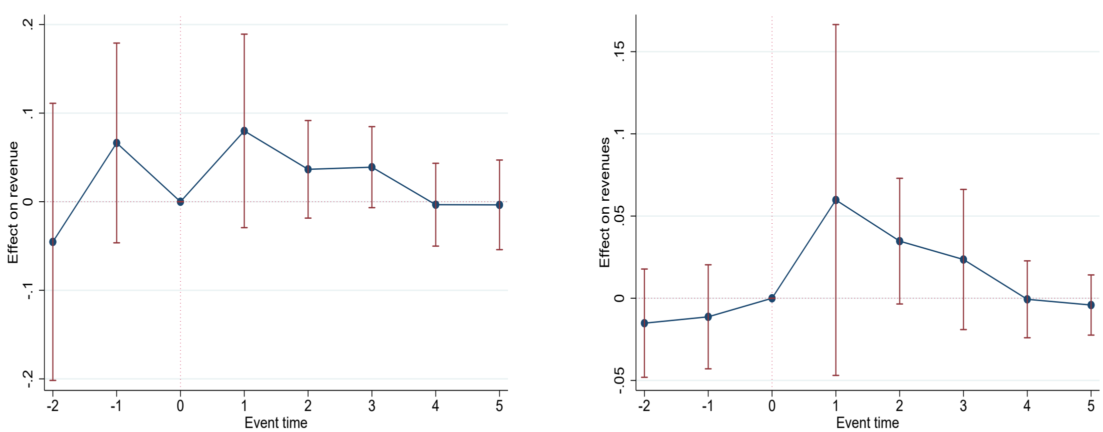
Table OA.C.3: Results on revenue by project size

	Small			Large		
	(1)	(2)	(3)	(4)	(5)	(6)
	Revenue	Revenue	Revenue	Revenue	Revenue	Revenue
Proj. size X Efficient Award	0.063	0.092*	0.082	0.052**	0.075**	0.067*
	(0.080)	(0.056)	(0.066)	(0.030)	(0.039)	(0.042)
Controls		X	X		X	X
Regional trends			X			X
Observations	820	736	736	818	794	794

Notes: The table presents the results of estimating Equation 1 focusing on the change in revenue three years after the award, distinguishing between smaller projects (Columns 1 to 3) or larger ones (Columns 4 to 6). The dependent variable is the logarithm of the firm revenue. The model includes firm and year fixed effects, as well as regional linear trends (Columns 3 and 6). Control variables include firm characteristics such as firm assets, cost of raw materials, number of employees, and firm age. Standard errors are clustered at the firm level and bootstrapped. The median project size is 228,494 Euro.

Figure OA.C.9: Results by firm size

(a) Low firm size (b) High firm size



Notes: The Figure shows the even-study coefficients of the Equation 1 split by large and small firms measured on the number of employees. The model includes firm and year fixed effects. Control variables include firm characteristics such as firm assets, cost of raw materials, number of employees, and firm age. Standard errors are clustered at the firm level and bootstrapped. The median number of employees is eight.

Table OA.C.4: Results on revenue by firm size

	Small			Large		
	(1)	(2)	(3)	(4)	(5)	(6)
	Revenue	Revenue	Revenue	Revenue	Revenue	Revenue
Proj. size X Efficient Award	0.133***	0.088	0.054	-0.026	0.055	0.037
	(0.047)	(0.061)	(0.075)	(0.069)	(0.044)	(0.055)
Controls		X	X		X	X
Regional trends			X			X
Observations	799	745	745	728	675	675

Notes: The table presents the results of estimating Equation 1 focusing on the change in revenue three years after the award, distinguishing between small firms (Columns 1 to 3) or large ones (Columns 4 to 6). The dependent variable is the logarithm of the firm revenue. The model includes firm and year fixed effects, as well as regional linear trends (Columns 3 and 6). Control variables include firm characteristics such as firm assets, cost of raw materials, number of employees, and firm age. Standard errors are clustered at the firm level and bootstrapped. The median number of employees is eight.

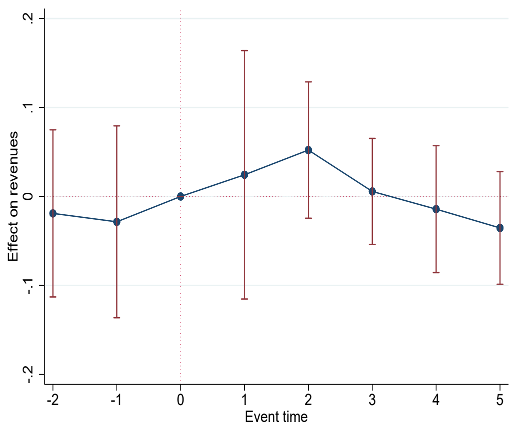
Table OA.C.5: Results on revenue by type of government

	Central		Local		
	(1)	(2)	(3)	(4)	(5)
	Revenue	Revenue	Revenue	Revenue	Revenue
Proj. size X Efficient Award	0.038	0.049	0.065	0.088***	0.081*
	(0.049)	(0.083)	(0.057)	(0.037)	(0.043)
Controls		X		X	X
Regional trends					X
Observations	233	197	1,419	1,347	1,347

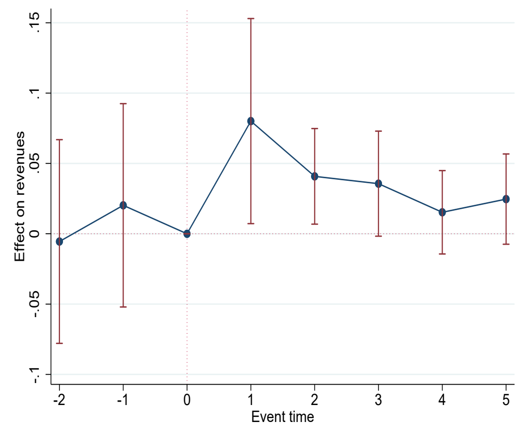
Notes: The table presents the results of estimating Equation 1 focusing on the change in revenue three years after the award, distinguishing public spending issued by the central administration (Columns 1 to 2) and the local governments (Columns 3 to 5). The dependent variable is the logarithm of the firm revenue. The model includes firm and year fixed effects, as well as regional linear trends (Columns 3 and 6). Control variables include firm characteristics such as firm assets, cost of raw materials, number of employees, and firm age. Standard errors are clustered at the firm level and bootstrapped. The regressions focusing on central governments do not include specifications with regional trends, since with only one central government these would be collinear with the year fixed effects.

Figure OA.C.10: Results by central and local governments

(a) Central gov.



(b) Local gov.



Notes: The Figure shows the even-study coefficients of the Equation 1 split by central and local governments. The model includes firm and year fixed effects. Control variables include firm characteristics such as firm assets, cost of raw materials, number of employees, and firm age. Standard errors are clustered at the firm level and bootstrapped.

Online Appendix D

This Appendix presents additional tables and event-study analyses related to the mechanisms discussed in Section 5.3. Table OA.D.6 shows that firms selected under the two allocation rules are comparable in pre-award size characteristics. Figures OA.D.11 and OA.D.12 report event-study evidence on public and private revenues, while Table OA.D.7 and Figures OA.D.13–OA.D.15 examine alternative mechanisms, for which we find no evidence.

Table OA.D.6: Comparison of pre-award characteristics: firm's size

	(1) Revenue	(2) Revenue	(3) Employees	(4) Employees	(5) Assets	(6) Assets	(7) Material cost	(8) Material cost
Efficient award winner	0.173	0.263	0.208	0.246	0.260	0.307	0.174	0.259
	(0.411)	(0.377)	(0.195)	(0.182)	(0.273)	(0.270)	(0.272)	(0.226)
Sector FEs		X		X		X		X
Year FEs		X		X		X		X
Observations	750	750	750	750	750	750	700	700

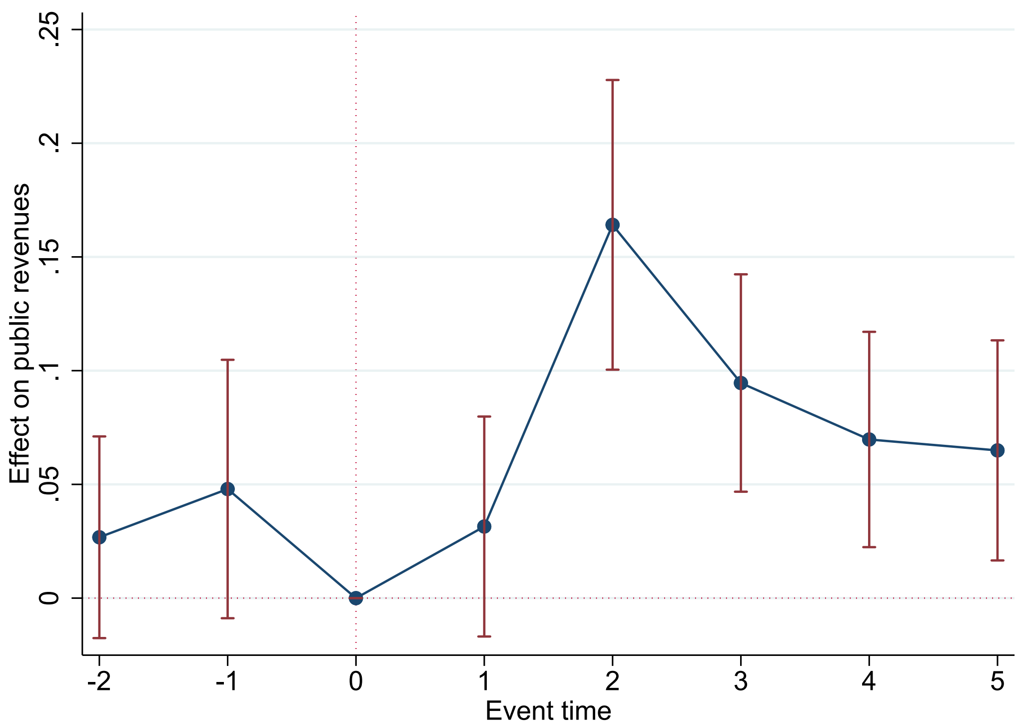
Notes: The table reports differences in the conditional means between winners selected through the efficient mechanism and those chosen via the lottery-like mechanism across several balance sheet variables proxying firm size. We focus on the years preceding the award of public resources and, where indicated, control for year and sector fixed effects. The outcomes considered are the logarithm of firm revenue, number of employees, value of fixed assets, and cost of raw materials.

Table OA.D.7: Results on other mechanisms

	(1) Short debt	(2) Short debt	(3) Long debt	(4) Long debt	(5) Capital investment	(6) Capital investment
Proj. size X Efficient Award	0.004	0.013	-0.022	-0.011	-0.005	-0.004
	(0.058)	(0.070)	(0.052)	(0.055)	(0.016)	(0.013)
Controls	X	X	X	X	X	X
Regional trends		X		X		X
Observations	1,546	1,546	1,546	1,546	1,546	1,546

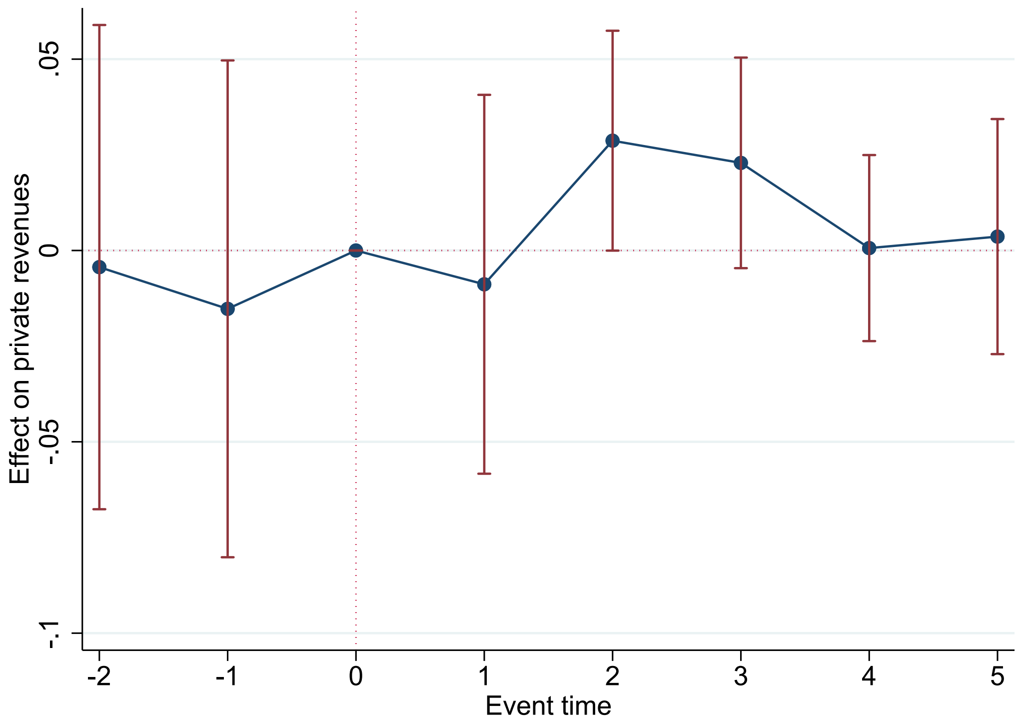
Notes: The table presents the results of estimating Equation 1 focusing on other relevant mechanisms. The dependent variable is the logarithm of the short-term debts, long-term debts, and fixed assets. The model includes firm and year fixed effects, as well as regional linear trends (Columns 3 and 6). Control variables include firm characteristics such as firm assets, cost of raw materials, number of employees, and firm age. Standard errors are clustered at the firm level and bootstrapped. The regressions focusing on central governments do not include specifications with regional trends, since with only one central government these would be collinear with the year fixed effects.

Figure OA.D.11: Effect of the efficient allocation on revenue from public bodies



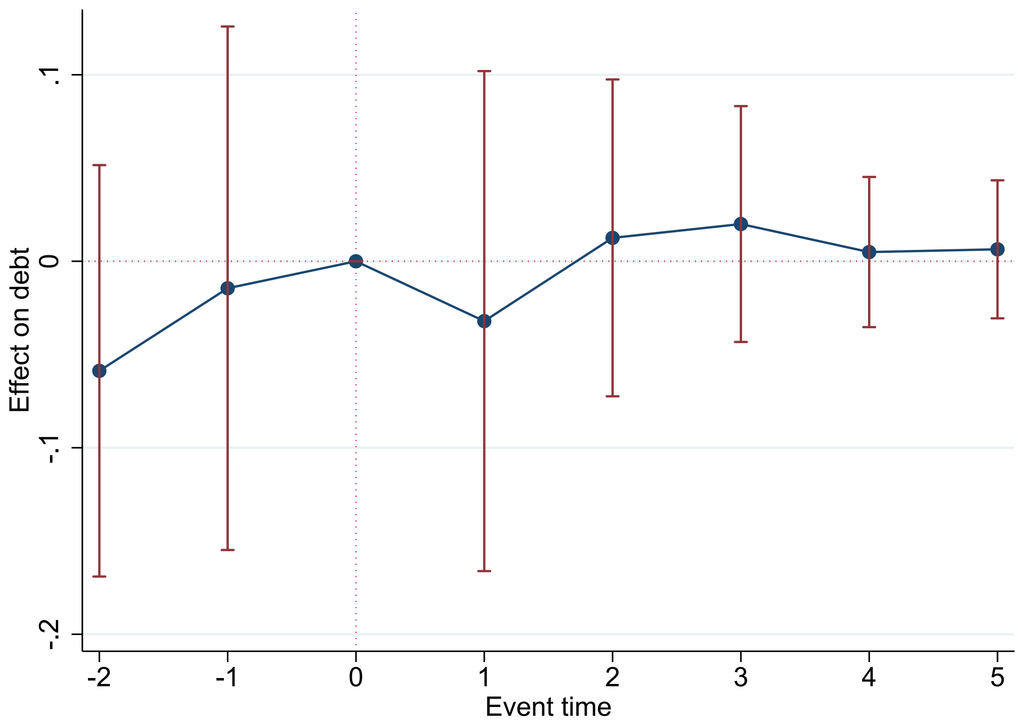
Notes: The figure plots the event-study coefficients of β_1 from Equation 1, where the dependent variable is total public revenue excluding that from the initial award, measured in logarithms. The β_1 represents the average impact of a one percent increase in the amount of public resources awarded efficiently on firm's public revenue. The model includes firm and year fixed effects. Control variables include firm characteristics such as firm assets, cost of raw materials, number of employees, and firm age. Standard errors are clustered at the firm level and bootstrapped.

Figure OA.D.12: Effect of the efficient allocation on revenue from private sector



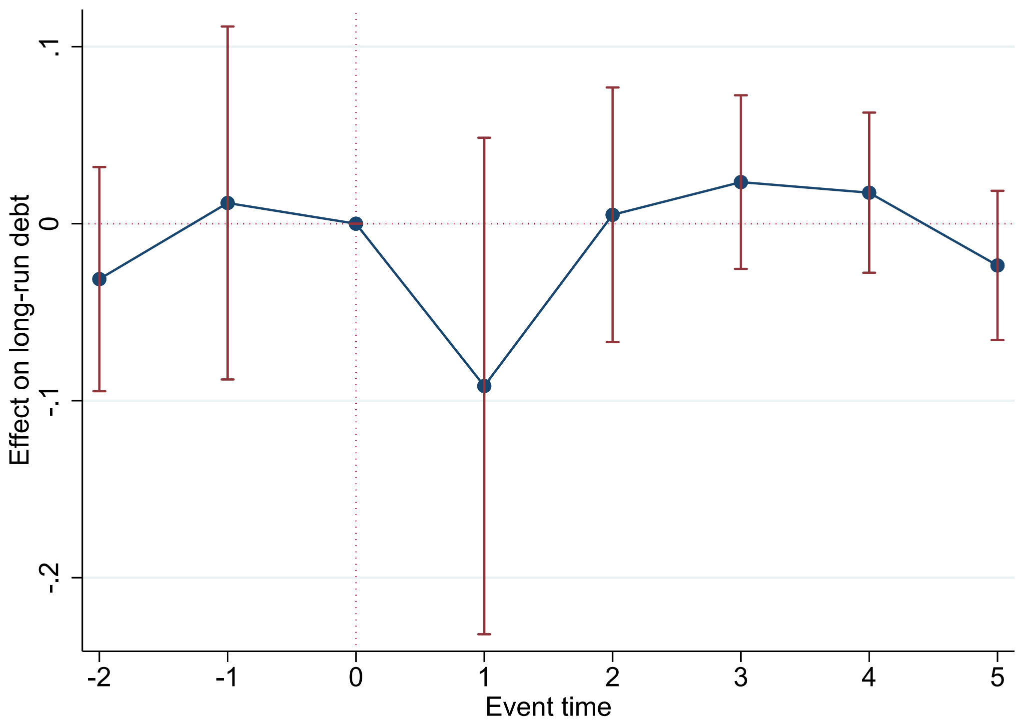
Notes: The figure plots the event-study coefficients of β_1 from Equation 1, where the dependent variable is total private revenue, measured in logarithms. The β_1 represents the average impact of a one percent increase in the amount of public resources awarded efficiently on firm's private revenue. The model includes firm and year fixed effects. Control variables include firm characteristics such as firm assets, cost of raw materials, number of employees, and firm age. Standard errors are clustered at the firm level and bootstrapped.

Figure OA.D.13: Average effect on firm short-term level of debts



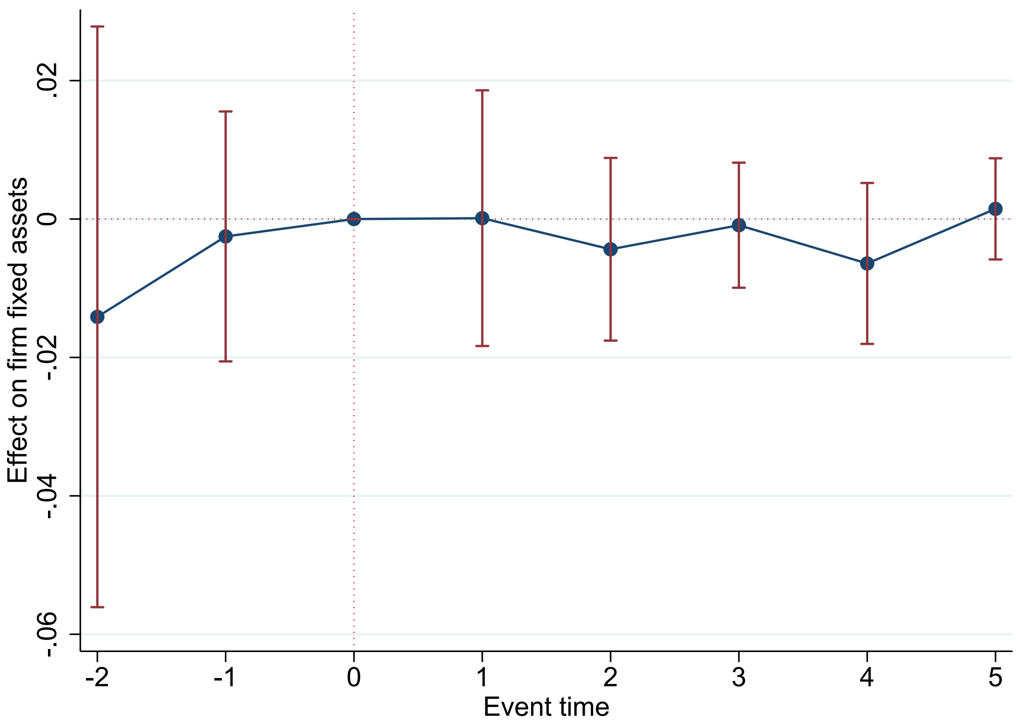
Notes: The Figure shows the even-study coefficients of the Equation 1. The dependent variable is the short-term level of debts, measured in logarithms. The model includes firm and year fixed effects, as well as regional linear trends (where specified). Control variables include firm characteristics such as firm assets, cost of raw materials, number of employees, and firm age. Standard errors are clustered at the firm level and bootstrapped.

Figure OA.D.14: Average effect on firm long-run level of debts



Notes: The Figure shows the even-study coefficients of the Equation 1. The dependent variable is the long-run level of debts, measured in logarithms. The model includes firm and year fixed effects, as well as regional linear trends (where specified). Control variables include firm characteristics such as firm assets, cost of raw materials, number of employees, and firm age. Standard errors are clustered at the firm level and bootstrapped.

Figure OA.D.15: Average effect on firm fixed assets



Notes: The Figure shows the even-study coefficients of the Equation 1. The dependent variable is the value of the fixed assets, measured in logarithms. The model includes firm and year fixed effects, as well as regional linear trends (where specified). Control variables include firm characteristics such as firm assets, cost of raw materials, number of employees, and firm age. Standard errors are clustered at the firm level and bootstrapped.

Online Appendix E

In this Appendix, we describe the robustness checks outlined in Section 5. First, we re-estimate Equation 1 focusing on firms selected in tenders with a number of competitors close to the eligibility threshold (i.e., plus or minus three bids). Table OA.E.8 and Figure OA.E.16 confirm that the results remain similar even when restricting the sample to tenders close to the threshold of eligible bids.

Table OA.E.8: Average effect with contracts closer to the threshold

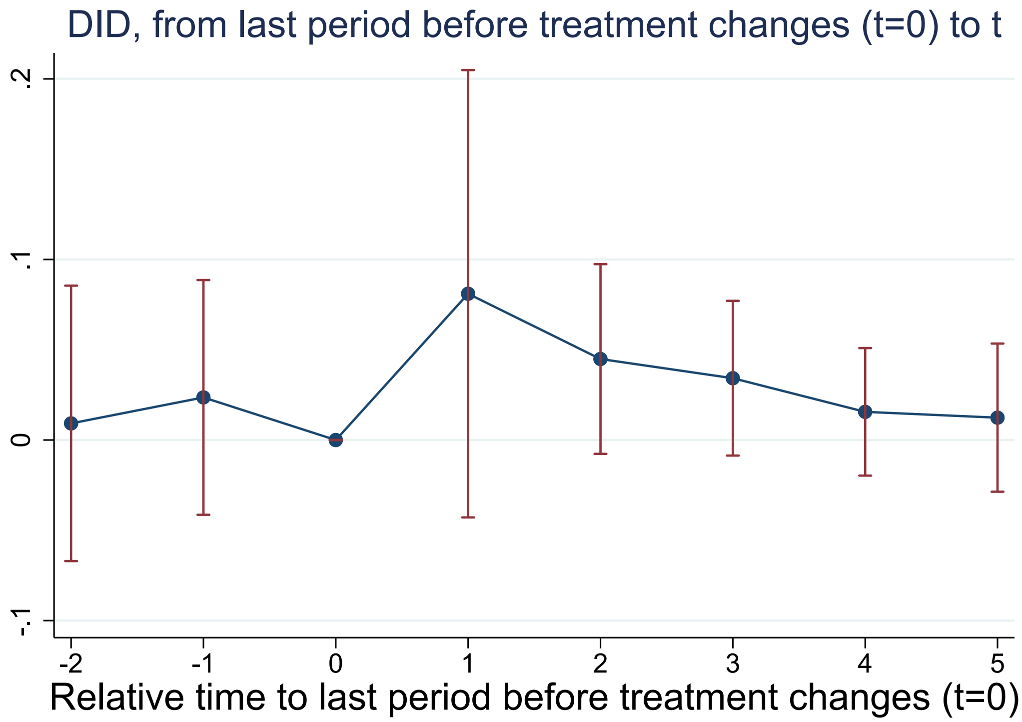
	(1) Revenue	(2) Revenue	(3) Revenue
Proj. size X Efficient award	0.055	0.090**	0.086**
	(0.054)	(0.041)	(0.042)
Controls		X	X
Regional trends			X
Observations	1,675	1,566	1,566

Notes: The table shows the coefficient β_1 resulting from estimating Equation 1 and focusing on the change in revenue three years after the award. The dependent variable is the logarithm of the firm revenue. The model includes firm and year fixed effects, as well as regional linear trends (where specified). Control variables include firm characteristics such as firm assets, cost of raw materials, number of employees, and firm age. Standard errors are clustered at the firm level and bootstrapped. We restrict the sample to a narrower window of contracts around the threshold, specifically those within plus/minus three bids.

Next, we address potential endogeneity concerns arising from the choice of public bodies to allocate resources through the lottery-like mechanism (irrespective of whether it is ultimately implemented, which requires a sufficiently large number of bids and forms the basis of our identification strategy). One concern is that public bodies with different levels of experience in managing public resources may also differ in their preferred allocation method, and that these preferences could be correlated with firm characteristics, thereby biasing our estimates. For instance, if the lottery-like mechanism were used predominantly by less experienced public bodies, and if firms contracting with them were on average less productive,[21] then the estimates in Section 5 might confound the effect of efficient

[21] Descriptive evidence in Section 2 suggests that such a pattern does not exist. For example, Figure OA.A.1 shows that this procedure was widely used across all types of public bodies when allocating resources in the construction sector, consistent with the findings of Decarolis (2018).

Figure OA.E.16: Main effect with contracts closer to the threshold



Notes: The Figure shows the even-study coefficients of β_1 from Equation 1, with the dependent variable being firm revenue, measured in logarithm. The β_1 represents the average impact of a one percent increase in the amount of public resources awarded efficiently on firm's revenue. The model includes firm and year fixed effects. Control variables include firm characteristics such as firm assets, cost of raw materials, number of employees, and firm age. Standard errors are clustered at the firm level and bootstrapped. Differently from Figure 2, here we restrict the sample to a narrower window of contracts around the threshold, specifically those within plus/minus three bids. The results are qualitatively similar to those in Figure 2, although estimated with less precision.

allocation with the effect of operating in a generally less efficient environment.

Table OA.E.9 addresses this concern by comparing the baseline estimates from Table 3 (Column 1) with results from Equation 1 that include additional controls for the number of tenders and contracts previously awarded by each public body. These variables proxy for public body experience in the construction sector. The results remain stable after including these controls, indicating that the findings in Section 5.1 are not driven by differences in public body experience and are unlikely to be severely biased.

Table OA.E.9: Average effect controlling for public body experience

	(1) Revenue	(2) Revenue
Proj. size X Efficient award	0.076**	0.073*
	(0.032)	(0.041)
Controls	X	X
Regional trends	X	X
Public body controls		X
Observations	1,538	1,538

Notes: The table shows the coefficient β_1 resulting from estimating Equation 1 and focusing on the change in revenue three years after the award. The dependent variable is the logarithm of the firm revenue. The model includes firm and year fixed effects, as well as regional linear trends (where specified). Control variables include firm characteristics such as firm assets, cost of raw materials, number of employees, and firm age. Column 2 includes controls of public body experience measured as the total number of public tenders, and the total number of contracts issued up to that point in time by each public body in the construction sector. Standard errors are clustered at the firm level and bootstrapped. We restrict the sample to a narrower window of contracts around the threshold, specifically those within plus/minus three bids.

Finally, we present three additional robustness checks. First, Table OA.E.10 and Figure OA.E.17 report the estimation of Equation 1 using sales and productivity (measured as revenue per worker) as alternative outcomes. Second, Table OA.E.11 extends the analysis to the first five years after the award, rather than the first three only. Third, Table OA.E.12 re-estimates Equation 1 using the TWFE estimator.

Neither the use of alternative outcomes, nor the longer time horizon, nor the choice of estimator alters our main conclusions. Allocating public resources efficiently has a positive impact on the performance of winning firms in the short to medium run. This effect is driven by the selection of more efficient firms, which are able to leverage additional public resources to expand their activity in the market for public spending, while maintaining their private revenue.

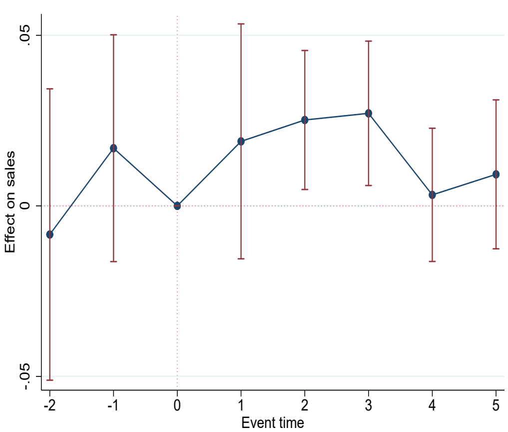
Table OA.E.10: Results on other outcomes of performance

	(1) Sales	(2) Sales	(3) Sales	(4) Productivity	(5) Productivity	(6) Productivity
Proj. size X Efficient Award	0.032	0.051**	0.047**	0.063**	0.079***	0.074***
	(0.039)	(0.022)	(0.021)	(0.033)	(0.034)	(0.029)
Controls		X	X		X	X
Regional trends			X			X
Observations	1,646	1,538	1,538	1,646	1,538	1,538

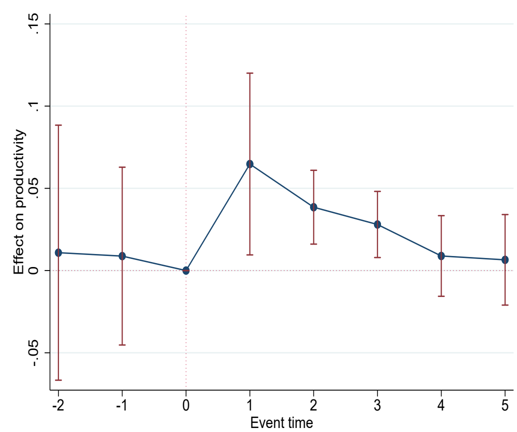
Notes: The table presents the results of estimating Equation 1 focusing on other outcomes of firm performance, such as sale, productivity (i.e., revenue per worker), The model includes firm and year fixed effects, as well as regional linear trends (Columns 3 and 6). Control variables include firm characteristics such as firm assets, cost of raw materials, number of employees, and firm age. Standard errors are clustered at the firm level.

Figure OA.E.17: Results with other outcomes

(a) Sales



(b) Productivity



Notes: The Figure shows the even-study coefficients of the Equation 1 using as outcome another measure of revenue and productivity (measured as revenue per worker). The model includes firm and year fixed effects. Control variables include firm characteristics such as firm assets, cost of raw materials, number of employees, and firm age. Standard errors are clustered at the firm level and bootstrapped.

Table OA.E.11: Results on longer time interval

	(1) Revenue	(2) Revenue	(3) Revenue
First Award X FPA Award	0.022	0.048**	0.041*
	(0.043)	(0.023)	(0.023)
Controls		X	X
Regional trends			X
Observations	1,777	1,656	1,656

Notes: The table presents the results of estimating Equation 1 focusing on the five years after the award of the public funds. The dependent variable is the log of the firm revenue. The model includes firm and year fixed effects, as well as regional linear trends (Columns 3 and 6). Control variables include firm characteristics such as firm assets, value of the raw materials, the number of employees, and age. Standard errors are clustered at the firm level and bootstrapped.

Table OA.E.12: Results on revenue using the TWFE estimator

	(1) Revenue	(2) Revenue	(3) Revenue
Proj. Value X Efficient Award	0.065	0.072**	0.069*
	(0.042)	(0.036)	(0.036)
Observations	2,221	2,064	2,064

Notes: The table presents the results of estimating Equation 1 focusing on the change in revenue three years after the award using the TWFE estimator. The dependent variable is the logarithm of the firm revenue. The model includes firm and year fixed effects, as well as regional linear trends (Columns 3 and 6). Control variables include firm characteristics such as firm assets, cost of raw materials, number of employees, and firm age. Standard errors are clustered at the firm level.

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