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Procuring Survival

Abstract

We investigate the impact of public procurement spending on business survival. Using Italy as a laboratory, we construct a large-scale dataset on firms—covering balance-sheet, income-statement, and administrative records—and match it with public contract data. Employing a regression discontinuity design for close-call procurement auctions, we find that winners are more likely to stay in the market than marginal losers after the award and that the boost in survival chances lasts longer than the contract duration. We document that this effect is associated with earnings substitution rather than increased business scale. Regardless of size, contracts that are long-lasting and awarded by decentralized buyers are more impactful for survival prospects. Survivors experience no productivity premium but rather an improvement in their credit position.

JEL-Codes: D250, D440, H320, H570.

Keywords: firm survival, firm dynamics, public demand, public procurement, demand shocks, productivity, credit, auctions, regression discontinuity design.

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I Introduction

A firm's life expectancy at birth is low—across countries and sectors, most startups survive the first year, but less than half remain in the market after seven years (Agarwal and Gort, 2002; Bartelsman et al., 2009; Calvino et al., 2016). Although these statistics are partially ascribed to the natural selection of the most efficient firms, a mechanism essential to a thriving economy, business survival has intrinsic value for socioeconomic cohesion—as recently demonstrated by government support packages for firms in response to the economic fallout from the Covid-19 pandemic. The former observation and the latter consideration have spawned a body of literature examining the determinants of business survival. Among the many underlying forces, demand constraints are among the least analyzed (Syverson, 2011; Pozzi and Schivardi, 2016; Foster et al., 2016).

In this paper, we focus on the underexplored role of public demand—as opposed to market-based, private demand—in promoting firm survival.[1] At the macroeconomic level, government spending, its optimal level, and its structural role in guiding the economy have been at the center of debate for decades (Ramey, 2019). Several contributions have shown how shifting the amount of public spending has cascading effects throughout the productive sector, making it the most effective policy tool to prop up the economy during downturns. At the microeconomic level, however, the impact of procurement spending—i.e., a specific component of government outlay—on firm outcomes has been studied only recently (e.g., Ferraz et al., 2015; Gugler et al., 2020) and its effect on business survival vastly underinvestigated (De Silva et al., 2009). A priori, a survival effect is uncertain given that public contracts do not necessarily entail higher profits than private ones (and profitability tends to be a better predictor of survival (Jovanovic, 1982)). Indeed, higher revenues from public sales could be associated with higher costs due to administrative burden—lowering the profitability of public demand. On the other hand, firms selling to the government may see frictions reduced—e.g., they may have easier access to credit since the certainty of a government-backed cash flow decreases their implied risk (di Giovanni et al., 2022). In fact, we find that (i) receiving a government contract boosts firms' survival odds, (ii) this effect is large and extends well beyond the median contract expiration date, and (iii) the main channel of transmission is winners rebalancing their source of income toward public money and experiencing easier access to credit.

We address the research question empirically using a novel combination of extensive and highly detailed data on Italy, the laboratory for this study. We combine individual balance-sheet and income-statement records on the quasi-universe of limited companies with administrative data reporting official business registration (i.e., market entry) and deregistration (i.e., market exit). We match this panel of firms—including records on survival, age, revenues, employment, and labor productivity—with a database on government procurement contracts provided by the National Anti-Corruption Authority (hereafter ANAC), which is the public procurement regulator in the country. The database contains comprehensive information on all tenders with a value of more than €40 thousand and the related contracts, totaling a cumulative yearly value of €156 billion—representing 9% of GDP and 90% of total procurement spending.[2] This includes the contract value

[1] In 2018, government procurement spending in the median OECD country amounted to 13% of GDP and 41% of total government outlay (OECD, 2019).

[2] The reporting threshold is lower than any category-specific EU regulatory threshold and, in particular, much lower than that for public works contracts, which is around €5.5 million in 2019.

and duration, the procurement category, the award mechanism, and, most importantly, the winner's identity.

Thanks to the granularity of our data, we can pinpoint firms that receive public money under procurement contracts ("*procurement firms*") and compare them to firms that receive no public contracts. Altogether, the former are bigger and older than the latter. In addition, they have better survival prospects, even controlling for age, size, and productivity. However, regardless of the included controls, any comparison between procurement and other firms suffers from endogeneity issues. First, as just mentioned, the former are fundamentally different from the rest of the sample, making the two groups incomparable; second, there might be unobservable firm characteristics that correlate with both the probability of participating in a public auction and winning as well as the firm's ability to stay in the market (e.g., management quality, political connections).

To address these identification concerns, we focus on auctions in the construction sector—which accounts for 19% of procurement spending and 13% of firms in our dataset—and extract information about the bidding process (i.e., individual bids, the identity of bidders, final ranking) directly from the official documents when available. We leverage the gap between the winning and losing bids to define a running variable and a cutoff (i.e., at the runner-up bid) to implement a regression discontinuity (RD) analysis and compare firms that win a public contract with firms that lose by a small margin. The identifying assumptions are that (i) firms cannot perfectly manipulate the award assignment around the cutoff, (ii) the award is as good as random for bids in the vicinity of the cutoff, and (iii) winners and runners-up are ex ante exchangeable. In the paper, we provide evidence that validates these assumptions. We find a substantial increase in survival probability as a response to the awarding of contracts. We estimate that winning a government contract—whose median duration is about six months—causes an increase in the 24-month (36-month) survival probability by 1.8 (3.4) p.p. on top of a baseline 97.9% (96%) survival rate—i.e., an 85% decrease in exit rate. Among other concerns, we show that the results are robust to the risk of collusive bidding behavior around the cutoff—a concern when assuming quasi-random contract allocation—as well as to the risk of contamination of runners-up with concurrent awards in the same year.

The estimated effect may arise from the combination of a potential scale effect (i.e., additional revenues from the contract award) and a composition effect, which rebalances the firms' source of income toward public money. We find that there is no scale effect at play and that firms absorb the public demand boost ($\approx$+10%) by substituting approximately 17% of their revenues from the private demand in the award year. Thus, we can interpret the boost in survival effect as depending on the *nature* of the demand shock (i.e., public instead of private) rather than the earnings it generates. We find that contracts that explain survival are the longest regardless of their size and those awarded by decentralized buyers—suggesting the removal of specific survival constraints triggered by political connections, in the spirit of Akcigit et al. (2022)'s findings on Italian firms. We also find that winning a contract has a stronger impact on firm survival when the construction category is one in which public demand is, on average, more important than private demand for suppliers (i.e., civil work industry) and, therefore, a public order is less easily substituted by a private order.

To investigate the implications of our results, we explore other firm-level outcomes to understand which firms survive in the wake of public demand. First, we replicate the

RD analysis to examine the role of labor productivity, a relevant aspect to consider because of its aggregate impact on the economy (Baier et al., 2006)—especially for an economy like Italy with its sluggish and increasingly dispersed productivity (Calligaris et al., 2016)—and its importance as a predictor of survival in the private market (Ugur and Vivarelli, 2021). We find no "public procurement premium" (i.e., *ex post*), since we estimate no significant difference in lead labor productivity levels between winners and runners-up. Accordingly, public demand helps firms survive longer, but it does not necessarily select the most efficient source. Second, we investigate whether public demand shocks mitigate different layers of financial constraints on firms. Winners of public tenders have been shown to use contracts as collateral to access external credit more easily since their future earnings are more secure than without the contract (di Giovanni et al., 2022). Consistently, we find that awards increase the uptake of credit and improve the existing credit quality. All in all, sales to the public do not affect labor productivity dynamics; rather, they act as a gateway to improve borrowing capacity. This result also suggests that—intentionally or unintentionally—winners are kept artificially alive by the government (Banerjee and Hofmann, 2018).

Related Literature By examining the government's role in firm survival, this paper joins a long-standing debate on the effectiveness of fiscal policies. Most of the existing evidence comes either from innovation and investment subsidies (Cerqua and Pellegrini, 2014; Criscuolo et al., 2019) to firms or from place-based policies (Becker et al., 2010; Kline and Moretti, 2014). Little is known about the implications of demand-based policies on firm performance. To contribute to this scholarship, we add to the more general empirical literature studying the effect of a demand shock on firms' outcomes (Pozzi and Schivardi, 2016; Foster et al., 2016), which hinges on solid theoretical predictions (Arkolakis et al., 2018; Gourio and Rudanko, 2014; Drozd and Nosal, 2012). In particular, we are interested in public demand shocks channeled to the private sector through procurement markets. Across different contexts, exposed firms—*conditioning on survival*—are found to experience a persistent boost in revenues and employment growth with evidence from Austria (Gugler et al., 2020), Brazil (Ferraz et al., 2015), Ecuador (Fadic, 2020), and South Korea (Lee, 2017).[3] A positive public demand shock is also found to induce more capital investment (Hebous and Zimmermann, 2021), easier access to external borrowing (di Giovanni et al., 2022), and more innovation (Czarnitzki et al., 2020). If the shock is negative, firms consistently respond by cutting capital (Coviello et al., 2021). Our paper complements this empirical literature by focusing on survival, productivity, and credit as additional firm-level outcomes affected by procurement contracts. Although industrial policy should be targeted in such a way as to support the most efficient players, we show that public procurement demand might instead be biased towards the laggard players in the market (Acemoglu et al., 2018).

Our work also directly advances the scholarship that studies the drivers of firm survival. Theoretical predictions and empirical evidence stress that the marginal survival probability increases with age and size (Hall, 1986; Evans, 1987a,b; Dunne et al., 1989; Clementi and Hopenhayn, 2006). Yet the relationship between growth and the likelihood of survival is not as simple as it appears at first glance. For example, Agarwal and Audretsch (2001) shows that the variance of realized growth rates is found to decrease with size,

[3]This effect is found to be relevant for domestic firms only in a cross-country analysis in Sub-Saharan Africa performed by Hoekman and Sanfilippo (2018).

conditioning on survival. The empirical evidence provided by the authors suggests that the association is shaped by technology and the stage of the industry life cycle. While the likelihood of survival for small entrants is generally less than that of their larger counterparts, the relationship does not hold for mature product life cycle stages or in technologically-intensive products. In mature industries that are still technologically intensive, entry may be less about radical innovation and more about filling strategic niches, negating the impact of entry size on the likelihood of survival. In short, increased scale is not necessarily associated with increased survival odds. The forces affecting survival can be more generally divided into industry characteristics (Zingales, 1998), geography (Choi et al., 2020), macroeconomic conditions (Byrne et al., 2016), product life cycle (Esteve-Pérez et al., 2018), exposure to trade (Kao and Liu, 2022) and shocks (Brata et al., 2018), all of which interact with those arising from the idiosyncratic characteristics of the firm (Audretsch and Mahmood, 1995; Ortiz-Villajos and Sotoca, 2018). To explain firm survival, less attention has been paid to institutional features in general (Pessina, 2020; Cevik and Miryugin, 2021; Byrne et al., 2016) and demand constraints in particular (Syverson, 2011; Pozzi and Schivardi, 2016; Foster et al., 2016).

We contribute to this scholarship by spotlighting the role of public-sourced sales in predicting firm survival. In line with the existing contributions from De Silva et al. (2009), De Silva et al. (2017), and Kosmopoulou and Press (2022), we find that construction spending has long-term benefits to the survival of firms in the industry. Our contribution to this scholarship entails relying administrative instead of bidding records to define firm survival, focusing on an entire national market—with multiple levels for governments, construction types, and auction formats considered—and a novel exploration of drivers and effect channels. Moreover, we use a different reduced-form approach (i.e., an RD design) with close-call auctions for which we find comparable firm characteristics, implying that a public contract award *by itself* affects survival probabilities. Instead, the above works observe differential bidding behavior between entrants and incumbents, with the former more prone to underestimate their costs and underbid. Indeed, in previous work, it is the resulting losses that influence survival differentials.

The rest of the paper unfolds as follows. Section II describes the data and sketches stylized facts; Section III presents the identification strategy; Section IV displays the results, which are discussed in Section V. Section VI concludes.

II Data

We gather and combine data on firms and public procurements at the most detailed level available in Italy. The source for the former is the Company Accounts Data System (CADS), a yearly collection of individual balance sheets covering the quasi-universe of limited liability companies. We complement it using administrative data on the firms' market entry and—if applicable—exit date, with the reason as provided by the Chambers of Commerce (i.e., the official business register, *Infocamere* from now on). As for the procurement side, we employ the full list of tender and associated contract records provided by ANAC (i.e., the *OpenANAC* database). The two databases are matched via the winning firms' tax code. To determine the analysis sample, we complement the contract data with two additional data sources on the bids and bidders' records on construction procurement auctions. In this subset of data, we are able to merge procurement data with losing participants, making our firm-procurement dataset unique for Italy.

Finally, we also add credit information coming from a confidential dataset (i.e., the *Central Credit Register*) collected by the Bank of Italy which includes the universe of bank-firm credit relationship records at the monthly level, including major features of lending channels and, crucially, the exposure amount per credit type (e.g., self-liquidating or upon-maturity) and the quality of credit (e.g., impaired or expired loans).[4] We will present this data content in further detail in Section V.

II.1 Firm-level Data

CADS Produced and distributed by the Cerved Group, the CADS is a proprietary repository of balance-sheet and income-statement data.[5] It covers the population of limited liability companies—except for the finance and agriculture sectors—accounting for around 70% of the total yearly business turnover in the country. The data reports revenues, employment, financial debt stock, capital, among many other pieces of information at the firm-year level.

Infocamere The Chambers of Commerce gather data on the universe of active businesses (irrespective of its legal form) in the country, record their registration date (i.e., entry) as well as their de-registration date (i.e., exit) including information on the reason—e.g., bankruptcy, acquisition by another business, or relocation.[6] We use the latter information to build the "survival" variables that we use as an outcome for the empirical analysis. In particular, we set the record to "missing" whenever we observe that the firm de-registers due to a merger or relocation, as the de-registration does not involve market exit, and we cannot track future performance. By contrast, we label a firm de-registration as an exit for all other reported reasons, notably bankruptcy. In addition, from the year of registration, we can retrieve firm age.

Procurement versus Non-Procurement Firms Table D1 in Appendix D reports a selection of firm characteristics for the full 2008–2018 firm sample. Despite our paper only using a subsample of these firms (see below), it is useful to compare procurement and non-procurement firms across all sectors to display structural differences. Overall, we observe 5.86 million unique non-procurement firm-year pairs and about 0.64 million procurement firm/year observations. Not surprisingly, firms that only operate in the private market differ structurally from those that also sell to the government. The former tend to be younger (13 years old on average compared with about 17 for procurement firms) and of much smaller scale in terms of the number of employees (9 vs. 47), revenues (€2.63 vs. 16.43 million), but also in terms of capital and debt stocks. Labor productivity—which we obtain by dividing the added value by the number of employees—is also higher for procurement firms. This difference in observables characteristics is important to be considered for the selection issues and associated identification concerns for our empirical strategy described in Section III.1. Procurement firms win 6.66 auctions in the pooled sample—i.e., 0.6 contracts per year on average.

[4]The data has been extensively used in the financial as well as in the banking literature, e.g. by Rodano et al. (2018).

[5]`www.cerved.com`.

[6]`www.infocamere.it`.

II.2 Contract-level Data

OpenANAC Since September 2020, ANAC has started publishing a large amount of previously privately retained data on Italian public procurement. The OpenANAC database constitutes the single largest source of this type of data ever available in the country.[7] The data includes all tenders solicited by any public authority above €40,000 as a reserve price—a monetary value much lower than any sector-specific publicity thresholds for EU law—all the awarded contracts linked to them, and, more importantly, the winner's identity and tax code.

The data report records of (i) the tender—e.g., the category of purchase, the reserve price, the awarding mechanism and the contracting authority; (ii) the award—e.g., the winning discount to the reserve price and number of bidders; and (iii) the post-awarding phase—e.g., contract duration. Among the many other pieces of information reported, the OpenANAC dataset allows us to identify whether the winning firms are part of a temporary partnership of firms (i.e., a consortium), which are typically created with the sole purpose of participating in single tenders and are either immediately dismantled if failing to win the auction, or persist until the contract expiration date. Through this information, we are able to assign the correspondent share of amount of the contract to the firm participating in a consortium.

The full sample (see Table D2 in Appendix D) comprises 1,274,979 contracts totaling a cumulative yearly value of €157 billion—representing about 9% of GDP and 90% of total procurement spending.[8] The mean contract amounts to €1.36 million, receives 4.4 bids, and lasts 585 days; medians are €130,000, 1, and 299, respectively, thus highlighting the pretty skewed distributions typical of public contract data. We report summary statistics for the overall sample, and for works. Construction contracts are relevant in terms of the overall procurement market: Throughout the 11-years period covered by our full data, approximately 40% of procurement firms were awarded at least one construction contract, representing around 60% of the cumulative 1.73 trillion euros of public procurement spending tracked by OpenANAC. Consortia represent 6% of the winners. About 20% of the contracts are awarded via auctions—the awarding mechanisms we focus on in the rest of the paper and the setting for our identification strategy.

Additional Sources The openly available dataset *Banca Dati Amministrazioni Pubbliche* (BDAP) allows us to retrieve one additional but crucial piece of information for this work, which was not available in OpenANAC at the time of the selection of the PDF for the bid-extraction process (see next subsection for details).[9] In particular, for the subset of tenders covered in BDAP—i.e., the work contracts between 2012 and 2017—we sourced data on the identity of all participants in the auctions along with their tax codes (but, notably, not their bids). In order to complement the information on the bidding process, we rely on proprietary data. More specifically, we purchased from *Telemat* the scanned version of tender documents for public works contracts solicited and auctioned off between 2012 and 2017, when available.[10] Through Telemat data, we can link OpenANAC con-

[7] `https://dati.anticorruzione.it/opendata`.

[8] The downloaded dataset dates back to first release of OpenAnac in the fall 2020.

[9] `https://openbdap.rgs.mef.gov.it/`.

[10] Telemat is a corporate division of DBInformation S.p.A.—a private company that provides multimedia services to Italian companies to support their development. One of its activities is collecting, scanning, and providing the digitalized version of official documents of the Italian public procurement

tracts data to the tender documentation by the unique tender ID (i.e., the *CIG* code). In a subset of these documents, alongside the identity of the bidders, the contracting agency reports the individual bids submitted—be it a discount in the case of price-based auctions, or the points obtained in scoring auctions. We extract this information to create a bid-level dataset by merging the bids with the firm-level information from CADS/Infocamere and the contract-level data from OpenANAC/BDAP. We refer the reader to Appendix C for the details on the extraction process of digitalized tender-document records.

II.3 Descriptive Statistics

We focus on the 11,078 contracts available both in BDAP and Telemat and, for the subset of those with available documentation—i.e., 1,896 contracts—we reconstruct the bid distribution. We define it as the *analysis sample*. We also drop the contracts when (i) they do not include the amount of the winning bid, or (ii) we cannot identify the winner. Our final working sample comprises 1,247 contracts. We merge the extracted bid data with contract-level data (i.e., OpenANAC) and firm-level data (i.e., CADS) via the CIG code, building a bid-level dataset featuring the full distribution of bids alongside the indication of winners as well as the business history of all participants.

Table 1 Panel A reports the mean, median, and p-values for the t-test on differences across the CADS and *analysis* sample of construction firms. We classify firms as construction firms if *winning* construction auctions and being awarded correspondent contracts in the full dataset. On top of scale variables and age, we augment the firm comparison with procurement-specific metrics. First, *Public Revenues* reports the cumulative yearly amount of contracts awarded.[11] Second, # Awards reports the yearly number of contracts awarded. Third, we construct the variable *Share Public Revenues* as the ratio of *Public Revenues* over Revenues.[12] Finally, we define *Share Direct Award* as the share of public contracts awarded through a direct award (i.e., without auctions or negotiations) relative to the total amount of contracts awarded by the firm in a given year. In Panel B, we compare the OpenANAC versus the *analysis* sample of auctioned construction contracts (amount, duration, bids received).

As for the balance-sheet and administrative data, differences in means of firm variables are found to be not statistically significant at the 5% significance level, with the exception of number of workers. The average firm-procurement metrics are comparable for Public Revenues and # Awards but tend to statically differ in terms of Share Public Revenues and Share Direct Awards: Winners in the analysis sample tend to rely slightly more on public sales and slightly less on direct awards in contracting. We find that contracts in the analysis sample have a similar amount and duration. Yet, differences between the two datasets rise for competition intensity as the analysis sample features fewer bids on average.

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tender—which are publicly available but only in paper format. See `https://www.telemat.it/`.

[11]The amounts of multi-year contracts are assigned to the firm-year as follows. We assume that a multi-year contract value is uniformly split into the years of contract duration. For instance, the contract i is assigned at t and ends at $t+2$. The corresponding yearly contribution to the winner's Public Demand equals $amount/3$. This mechanic also applies to consortia.

[12]We emphasize again the distinction between public money flowing to private firms in the form of public contracts (i.e., public procurement) and public *subsidies*, whether in the form of investment programs, direct transfers, or tax cuts. We consider only the former as counterparts to private demand. See Cingano et al. (2022) for a recent overview of the impact of subsidies on firm outcomes.

Table 1: *Quasi-universe* vs. *Analysis* Sample

Panel A:
CADS vs. Analysis Sample – Firms Winning Construction Auctions

	CADS		Analysis		
	Mean	Median	Mean	Median	t-test
Age (Years)	18	15	18	14	0.323
# Workers	66	11	25	11	0.008
Revenues (€, 000)	29,376	2,037	10,705	1,843	0.394
Capital (€, 000)	6,724	192	1,105	157	0.431
Labor Productivity	109.28	52.30	68.89	50.47	0.261
Financial Debt (€, 000)	17,928	451	3,564	395	0.420
Public Revenues (€, 000)	3,769	523	2,349	610	0.379
# Awards	7	3	6	3	0.752
Share Public Revenues	0.45	.	0.48	.	0.066
Share Direct Award	0.07	.	0.05	.	0.000
Observations	22,806		881		

Panel B:
OpenANAC vs. Analysis Sample – Construction Contracts (Auctions Only)

	OpenANAC		Analysis		
	Mean	Median	Mean	Median	t-test
Amount (€, 000)	1,398	308	1,388	310	0.986
# Bids	44.53	20.00	37.02	17.00	0.000
Duration (Days)	429.73	308.00	388.36	305.00	0.071
Observations	30,757		1,247		

Notes: The Panel A reports the mean value, median as well as the p-value for the conducted t-test, for different firm characteristics for the CADS dataset and the RD sample. We compare the analysis sample of winners with the original sample of winners appearing in CADS. The observation is at the firm-year level. Note also that for CADS, for comparability, we consider only the years 2012 to 2017 for this table, as this is the time span for the RD sample. We also restrict our attention to construction firms, as the RD sample includes only public works. We label firms in CADS as construction firms by means of the *NACE* code.

III Empirical Analysis

In this Section, we outline our methodology after presenting the identification concerns in our setting.

III.1 Identification Concerns

The link between the survival of firms and their access to public contracts is not trivial, in particular when dynamic considerations are included. A naive approach would be to project a firm-level indicator function for survival after k periods onto an indicator of contract(s) recipience—or, equivalently, the amount of public revenue received in levels or as a share of revenues—controlling for a variety of firm characteristics and fixed effects.[13] However, we would be overlooking the main feature of the public procurement market, which is firms' decision to participate in a public auction and the probability of being awarded the contract (conditional on participation). These two dimensions are strongly correlated with other firm characteristics, both observable—e.g., firm size and location—and unobservable—e.g., management quality and political connections. In fact, firms sequentially evaluate two elements when deciding to join the public procurement market. The first is the expected benefits of winning a public contract (in terms of, e.g., their survival chances) and the expected costs of participation.[14] Second, they evaluate the intensity of potential competition in the auction to assess the ex ante probabilities of winning. Hence, at each point in time, the population of firms is composed of those who (i) self-select to participate in the public procurement market (who also choose which and how many auctions to participate in), whose group is in turn split into those who win and those who ultimately do not win, and (ii) compete only in the private market. In developing an identification strategy, we must necessarily consider such sequence of self-selection to overcome resulting endogeneity problems.

We list three examples. First, demand shocks in the private market might affect the public market's participation rate. Indeed, because of capacity constraints, firms might be temporarily more (less) inclined to bid for government contracts if their private-sector demand gets weaker (stronger). In our setting, this type of selection bias might hold even after controlling for private-market revenues, given that procurement firms are intrinsically different from those that decide not to participate in the public procurement market, as shown in Section II. Second, following the analysis in Akcigit et al. (2022), we know that politically connected firms are more likely to be awarded a contract irrespective of their productivity and they also survive longer. As reported in Appendix A, in our sample the procurement firms also survive longer. If the degree of firms' political connection evolves over time, omitting this information would yield upward-biased estimates for the parameter of interest. Third, participation decisions may be driven by the struggle to survive. Consider the case of limited liability firms facing the risk of bankruptcy: *because* they are likely to exit the market, they may decide to engage in public auctions and bid aggressively (see, e.g., Board, 2007 and Calveras et al., 2004). Such "bidding for resurrection" effect downward-biases the estimates.

We cannot rule out these sources of endogeneity in a nonexperimental context unless we assume that participation decisions and procurement contracts are randomly distributed across firms. In this ideal experimental scenario, we could simply contrast the survival rates of procurement and non-procurement firms at both the extensive and intensive margins employing a difference-in-differences (DiD) strategy.[15] Due to data limitations, and the endogenous source of selection in the market and auctions, we cannot conduct a DiD analysis. A possible alternative strategy is to assign certain projects as unexpectedly

[13]Table A1 in Appendix A reports the results of this exercise. The estimates show a positive correlation between the probability of survival and public revenues, and a smaller effect of typical business predictors for firms that receive procurement contracts.

[14]Firms approaching the public procurement market face both fixed and variable costs in the form of investments required to gather knowledge about the bureaucratic processes involved, analyze auction-specific documents, or build political connections (see, e.g., Akcigit et al., 2022).

[15]The relatively recent literature on "new" DiD includes estimators designed for multiple time periods (Callaway and Sant'Anna, 2021), staggered adoption (Athey and Imbens, 2022), or heterogeneous treatment effects (De Chaisemartin and d'Haultfoeuille, 2020).

assigned to winners instead of close-losers by exploiting the regulatory framework and auction design. This would additionally allow us to control for the fact that winners may be structurally different from losers within the same auction. To this end, we focus on the subset of public works contracts awarded through open auctions for which we observe both winning and losing bidders and the full distribution of bids, i.e., the *analysis* sample. With the bid-level data, we can compare auction-by-auction winners and losers (i.e., the runners-up and the third-ranked). In this way, we account for the decision of firms to participate in the market and in the particular auction; zooming in around the most competitive bids, firms have the same ex ante probability of winning but were awarded the contract quasi-randomly. To quantify the impact of the winning bid, we use a RD analysis whose main elements are tailored to the Italian legal framework.

III.2 Institutional Background: Construction Firms and Auction Mechanism

From 2012 to 2017, Italian contracting authorities were required to select contractors through sealed-bid auction contests, which could feature the automatic exclusion of anomalous bids via an algorithm (average-bid auctions or ABAs) or award the contract to the lowest bid (first-price auctions or FPAs).[16] In both cases, the contracting agency announces a project description and a reserve price; then, firms submit sealed bids with discounts on the reserve price.

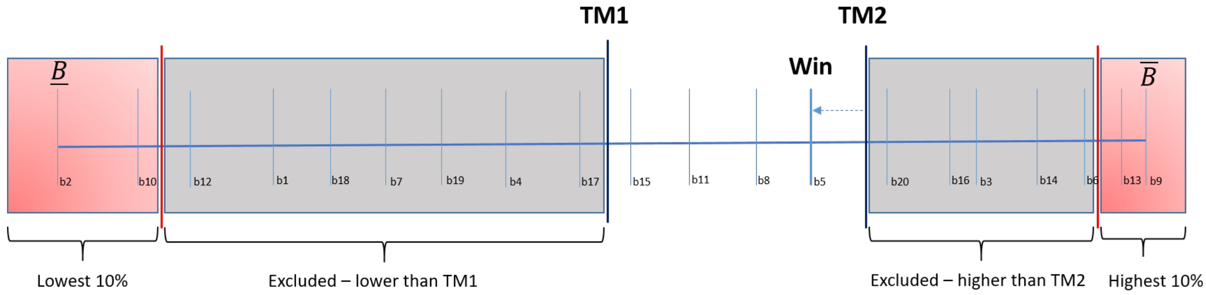
The idea underlying the ABAs is that, in the context of auctions with several participants, some bids are "too-good-to-be-true"—i.e., can be associated with underbidding or poor quality bidders and later poor performance—and therefore contracting authorities would be better off by selecting more expensive bidders. The algorithm underlying the ABA procedure essentially eliminates all offers above a mechanically calculated threshold close to the average bid and awards to the highest discount in the interval. Figure 1 offers a visual representation of the ABA mechanism in a fictional 20-bid auction. The winner is determined as follows: (i) bids are ranked from the lowest to the highest discount; (ii) a trimmed mean (TM1) is calculated excluding the 10 percent highest and the 10 percent lowest discounts; (iii) a second trimmed mean (TM2) is calculated as the average of the discounts strictly above TM1; (iv) the winning bid is the highest discount strictly lower than TM2.[17] The regulatory default format is the FPA; however—even though not compulsory—public buyers *could* choose to employ an ABA (and hence exclude anomalous offers) when they receive more than ten offers, or the reserve price is below the EU statutory threshold.[18] The auction format is not known in advance by bidders nor perfectly predictable.

[16]Contracting authorities can also use scoring rule auctions to select the winner—up to 100 points are assigned to most economically advantageous offer in terms of "quality" and price. We consider scoring rule auctions as FPAs because their award mechanics is equivalent for the sake of our econometric analysis: the firm obtaining the highest score (instead of the highest discount) is awarded the contract.

[17]We refer to Conley and Decarolis (2016) for a thoughtful discussion of the Italian ABA mechanism.

[18]Note that the ABA rules changed slightly in May 2017. The details of these changes are discussed in Section IV.3. In Panel I of Table 2 we test our results against the exclusion of 2017 auctions.

Figure 1: Visual Representation of the ABA Mechanism



Notes: This is an example of ABA with 20 bids, reported in increasing order between $\underline{B}$ and $\bar{B}$. Red areas represent the tails of the bid distribution (±10%), which are excluded to compute the average TM1. Focusing on bids higher than TM1, a second average is computed (TM2). The winning bid is the *nearest* but *lower* bid to TM2 (b5 in the example).

III.3 Identification Strategy

In order to ensure the identification of the public demand effect, we exploit the logic of quasi-random allocation of a contract to firms in the vicinity of the winning bid in a RD fashion. The idea is to compare the outcomes of winning and losing bidders under the assumption that—except for the fact that the former has been awarded a public contract—the two groups are ex ante identical (Cattaneo et al., 2020). To do that, we propose a RD framework that pools togethers multiple auctions with a cutoff depending on the value of the winning and runner-up bids.[19]

Despite that, and provided that there are no observable variables that influence the treatment probability, units with values of the running variable just below the cutoff can be used as a control group for treated units with values at or just above to estimate the (local) treatment effects on the outcomes of interest. In the rest of this section, we discuss the characteristics and the assumptions of our RD design.

The Cutoff Consider a *sharp RD* setting, with a forcing variable B_i and a cutoff $B^\star$ which informs the running variable $X_i = B_i - B^\star$. In this framework, only subjects with positive values of the running variable—i.e., if $X_i > 0$—are treated. This is equivalent to claiming that the probability of treatment (i.e., $Pr(D_i)$) is one whenever B_i strongly exceeds the cutoff level—i.e., $Pr(D_i = 1|B_i > B^\star) = 1$. In the context of procurement auctions pooled together, there is no "fixed" cutoff like $B^\star$ to be used in the definition of the running variable, as long as the discount of the winning bids differs depending on the bid distribution, the contract amount, the local market conditions, and so forth. Hence, we use a normalized, auction-level cutoff ($B^\star_a$) with the same characteristics as the one above, namely:

[19] This idea is not new in the literature, similar firms in the same auction hints at similar unobserved costs and/or similar information regarding the auction (e.g., Kawai et al., 2022). Recently, Kong (2021) employs this empirical strategy to isolate synergy from affiliation effect in sequential auctions. Moreover, multiple cut-off RDs are typical in the education literature for estimating the effect of school quality on different pupils' outcomes. For instance, Sekhri (2020) exploit a threshold that is year, college and stream specific. Similarly, Pop-Eleches and Urquiola (2013) use a cutoff score that is school and track specific for the admission into secondary education. Finally, Lucas and Mbiti (2014) utilize as a threshold the score of the last student admitted at a school-district level.

$$Pr(D_{i,a} = 1 | B_{i,a} > B_a^\star) = 1, \tag{1}$$

and change the definition of the running variable accordingly ($X_{i,a} = B_{i,a} - B_a^\star$).[20] We define the auction-level cutoff by leveraging the institutional features presented in Section III.2 and our bid-level data. More specifically, for each auction, we rank the bids and pinpoint the winning (i.e., B_a^1), runner-up (i.e., B_a^2), and higher-order bids ($B_a^3, .., B_a^N$). Consider the case of FPAs: conditional on the observed bid distribution up to the runner-up's, any discount exceeding B_a^2 wins the contest—in formula:

$$Pr(D_{i,a} = 1 | B_{i,a} > B_a^2) = 1, \tag{2}$$

and an immediate comparison between Equations (1) and (2) reveals that a straightforward choice of the auction-level cutoff is $B_a^\star = B_a^2$.

When it comes to ABA, once excluding the tails and computing the trimmed averages, all bids in the TM2-TM1 interval are treated as in FPAs, and the winner is the one offering the largest discount (see Figure 1). Therefore, conditional on the observed bid distribution, and focusing on the TM2-TM1 interval only, we rank the bids from the highest to the lowest discount ($B^1_{a,TM}, B^2_{a,TM}, .., B^N_{a,TM}$) and define the cutoff as $B_a^\star = B^2_{a,TM}$. Note that, in defining such cutoff, we are implicitly modifying the definition in Equation (1) to reflect the fact that a winning firm should overbid the runner-up discount, but not exceed TM2—expressed in a formula: $Pr(D_{i,a} = 1 | B_{i,a} > B^2_{a,TM} \forall B_{i,a} < TM2) = 1$.[21] Finally, the peculiarities of ABA auctions generate cases in which the absolute distance between the winning and the runner-up bid (as defined above) is larger than the absolute distance between the winning and the nearest absolute excluded bid. In a robustness check, we define the cutoff using the nearest bid with unaltered findings.

The Running Variable The running variable takes up the following values: $X_i = 0$ for the runner-up (i.e., there is a mass point of untreated observations at the cutoff), $X_i > 0$ for the winners, and $X_i < 0$ for all other losing bidders. Considering A auctions, we observe one point per auction (i.e., totaling A) to the right of the cutoff representing the winning bids (i.e., positive scores), A points massed at zero, and $N_l = \sum_{i=1}^{A} N_i$ to the left (i.e., negative scores)—where N_i is the number of losers in the auction i—representing losing bidders other than the runner-up.

The RD Sample In the spirit of Gugler et al. (2020), we argue that the comparison between the winner (i.e., treated) and the runner-up plus the third-ranked bidder (i.e., controls) provides a valid counterfactual to estimate the effect of winning a procurement auction on firms' outcomes. There are two reasons for restricting our sample up to the third-ranked bidder: on the one hand, it provides us with firms that are very similar

[20] Cattaneo et al. (2016) present a class of RD models with multiple cutoffs close to the one that we propose, and discuss three common applications in the empirical literature: running variables informed by vote shares, population, and test scores. Applications encompass close call elections (Cerqua and Pellegrini, 2014) and school admissions (Hoekstra, 2009).

[21] We stress that our interest is in the ex post analysis of bid distribution, hence we can safely condition our analysis on the observed bids and ignore the fact that different values of $B_{i,a}$ would modify TM1 and TM2 and move the very definition of runner-up with its relative cutoff.

to the winners not only in terms of bid distance but also in terms of the underlying characteristics. In Appendix D, we show that some of the firm characteristics around the cutoff are no longer similar (and are jointly different) if we keep the full spectrum of bids—even though the main estimation results hold (see Panel F of Table 2). On the other hand, the choice of keeping only up to the third-ranked bid better balances the number of observations on both sides of the cutoff—as long as adding losing bids would only inflate the sample to the left of the threshold.

The RD Model After defining the cutoff, the running variable, and the sample of bids we can implement a sharp RD by pooling the auction-specific scores. The regression model reads

$$Y_{i,a(t)} = \alpha + \tau D_{i,a(t)} + f_l(B_{i,a(t)} - B^*_{a(t)}) + D_{i,a(t)} f_r(B_{i,a(t)} - B^*_{a(t)}) + \epsilon_{i,a(t)}, \tag{3}$$

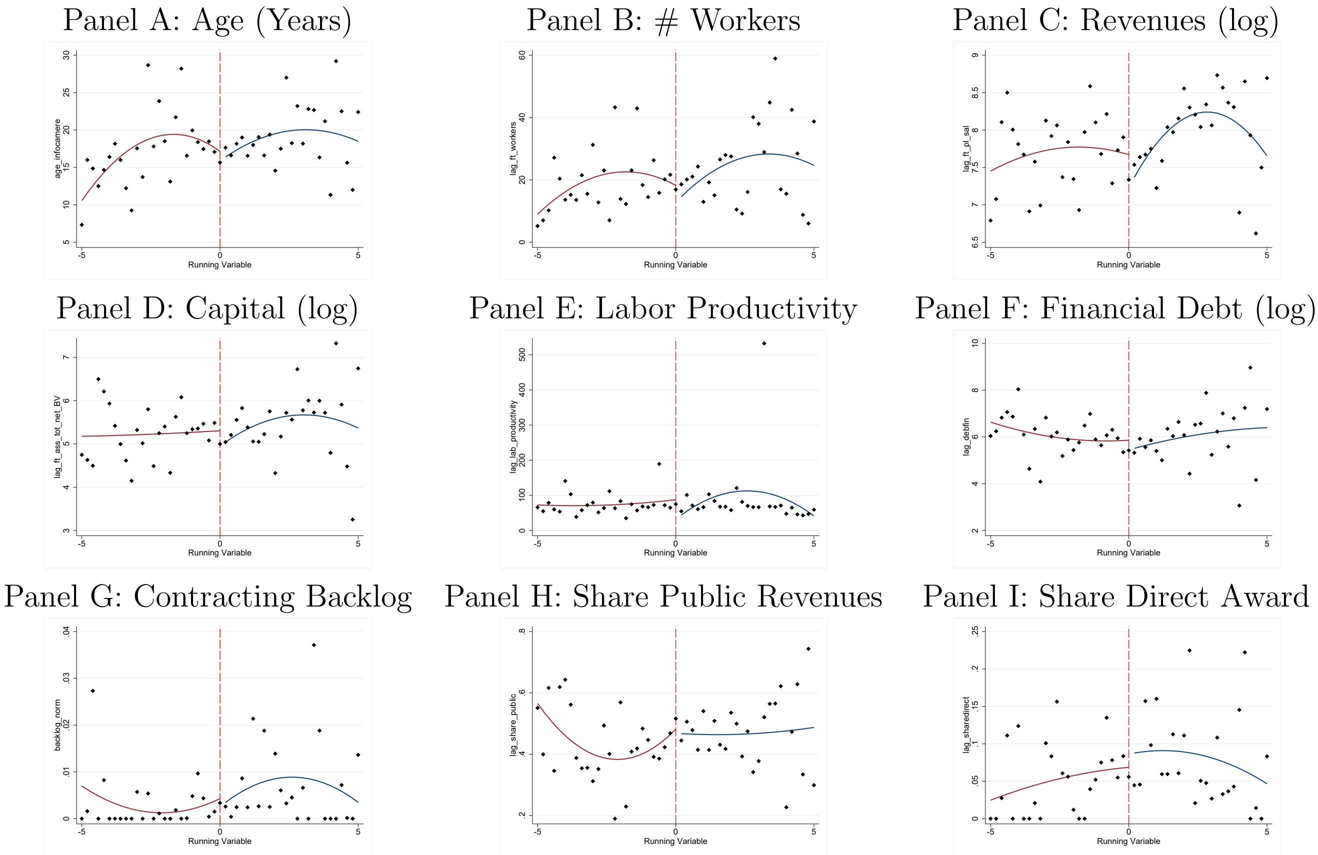
where $Y_{i,a(t)}$ is the outcome of interest—e.g., in the baseline analysis it is an indicator for survival after the award ($Surv_{i,t}^{t+m}$ and $m = [12, 24, 36]$ months).[22] More specifically, we look at the probability of a firm i being alive 12, 24, and 36 months after participating in the auction a (at a specific point in time t). The variable $f_k(B_{i,a} - B^\star_a)$ stands for a second-degree polynomial function, which we let vary on the left and right side of the cutoff ($k \in \{l, r\}$). $B_{i,a}$ is the bid submitted by firm i in auction a, $B^\star_a$ is the auction-specific cutoff value, $D_{i,a}$ is an indicator function for winning the contract—i.e., $D_{i,a} = \mathbb{I}[B_{i,a} > B^\star_a]$—and τ is the estimand treatment effect. Given the time spanned by the data and in order to rely on the same sample of observations for all outcomes, we limit our analysis to 36 months.

Testing the RD Assumptions The first identification assumption is that agents cannot manipulate the assignment around the cutoff. Therefore, the main confounding factor to the causal interpretation of the model from Equation's (3) is the possibility that bidders change their score strategically and are assigned to their preferred treatment condition (McCrary, 2008). In our context this is not the case, as firms participating in the auctions cannot *perfectly* control their distance to the runner-up and therefore their ranking, which is the key ingredient for the definition of the cutoff. This is especially true in our sample, which features, on average, 36 bids in competitive contests (see Table 1). A potential concern, though, arises from the possibility of collusive behaviors by cartel members, who may manipulate their bids—and their ranking—even in the proximity of the cutoff. In Appendix B, we discuss the manipulation concern, and we provide evidence that the results do not suffer from the risk of collusion. The second key element in an RD is the randomization assumption, namely that the regression functions $E[Y_i(0)|X_i = x]$ and $E[Y_i(1)|X_i = x]$ are continuous in x at $B^\star_a$. In Section IV we propose a placebo exercise that does not falsify this assumption. The third condition is that there is a (sharp) discontinuity in the treatment probability at the cutoff. This condition is ensured by construction for auctions that assign the treatment (i.e., the contract) to bidders with the lowest bid. Fourth, the groups are assumed to be exchangeable around the cutoff. In other words, treated and control firms are supposed to be ex ante identical, differing only by treatment status, in the absence of which they would exhibit the same dynamics

[22] For the rest of the section we omit the subscript (t) given that there is a one-to-one mapping between the specific auction a and the time t.

of outcome variables. Hence, any difference between the average response of treated and control units around the cutoff is fully attributed to the (local) average effect of the treatment. This assumption is usually tested by looking at the continuity of the relevant characteristics before the event for firms around the cutoff. More specifically, we graphically compare the pre-event variables of winners and losers in Figure 2 where we plot the mean values of several characteristics the year prior to the auction.

Figure 2: Firm Characteristics: Winners and Marginal Losers at $t-1$



Notes: Firm-level characteristics for winners (blue line) and marginal losers (maroon line, include runners-up and third-ranked) prior to the contract award. The running variable is rescaled to reflect the distance from the runner-up bid ($X_i = B_{i,a} - B_a^2$). All variables are lagged one year except contracting backlog, which is measured on the award day, as it is a snapshot of firm backlog at the daily level. Balance-sheet variables are transformed in natural logarithms. Each point represents the average of the covariates for a given non-overlapping bin.

We test the continuity of firms' lagged characteristics that correlate most likely with the probability of winning and surviving: age, employment, labor productivity (see Appendix A). We also include scale controls such as revenues, capital and financial-debt stocks.[23] In addition, we include metrics for behavior in public procurement. This exercise allows us to mitigate the risk of capacity constraints, corruption, and firms connections biasing our results, as argued in Section III.1. *Contracting backlog* is the residual backlog of ongoing contracts at the exact date of the award normalized by the revenues. It accounts for firms that rely more on public procurement and therefore are more likely to win,

[23] A similar age between winners and close-losers is particularly important for identification in this context as entrant firms tend to bid more aggressively and win with significantly lower bids compared to incumbents (De Silva et al., 2003, 2009). We want therefore to compare firms with similar experience in the construction market.

either because of experience or because of political connections. Notably, in the spirit of Kawai et al. (2022), its discontinuity at the cutoff can be indicative of bid rigging (see Appendix B.) We also look at the share of direct awards received. This measure proxies the degree of political connectedness and might signal the presence of relational contracts with buyers (Calzolari and Spagnolo, 2009), both cases in which firms may be more likely to receive direct awards. Winners and close-losers do not display significant differences along any of the above dimesions. All in all, the plots confirm the lack of systematic difference between winners and losers at the cutoff. We refer the reader to Appendix B for a detailed discussion on how these exercises are reconciled in the discussion on the risk of manipulation and collusion in our auctions.

As we consider several covariates, some discontinuities could be statistically significant (or close to) by chance. Therefore, to test against a continuity pattern jointly, we perform an auxiliary exercise inspired by Lee and Lemieux (2010). We execute seemingly unrelated regressions, where each regression features one of the nine covariates considered above. Specifically, we regress a binary model with an indicator equal to one when the observation is treated, i.e., if it lies above the threshold, on each of the nine covariates from Figure 2. We then perform a χ^2-test for the estimated coefficients being jointly equal to zero. We cannot reject the null, which corroborates that observable characteristics are jointly continuous at the cutoff. Winning and losing is therefore "as-good-as-random" conditional on close bids.

Altogether, our empirical design bolsters a causal interpretation of the RD results, which we present in the next subsection.

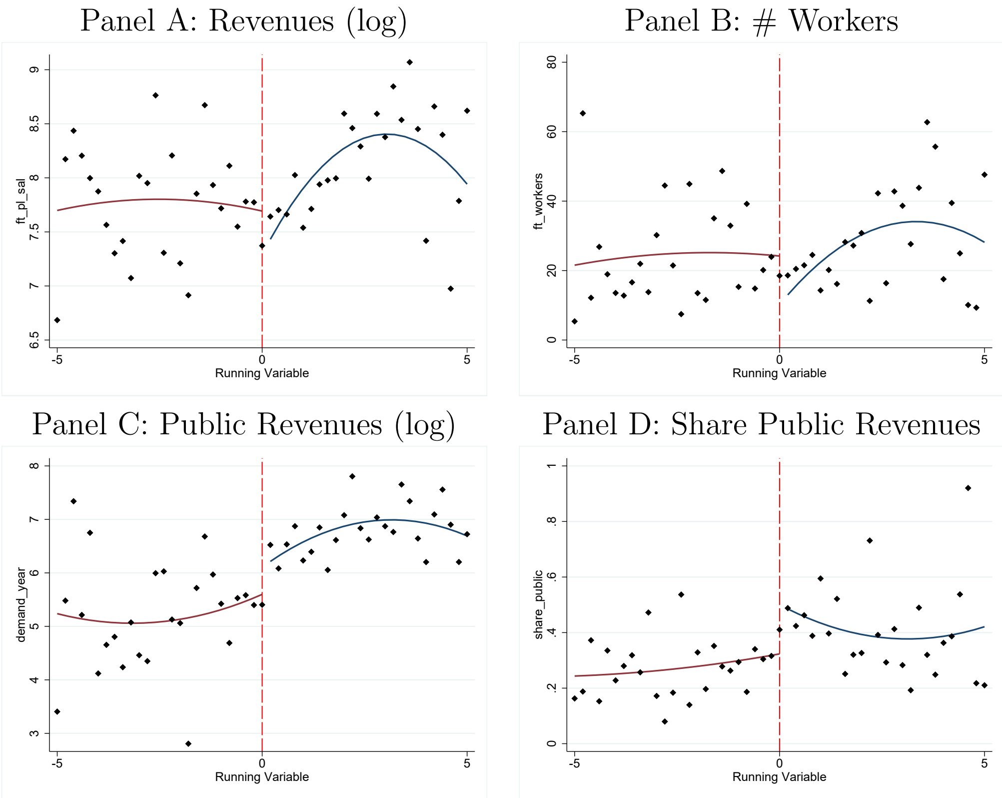
IV Results

In this section, we present the baseline results of our RD model and the tests for robustness. Before that, we show the short-run impact of public contract awards on winner activity.

IV.1 "First stage": Short-run Responses

Winning a public contract secures a source of earnings while taking up part of the existing productive input. In response, a firm can either expand its activity in order to keep its exposure to the private market unchanged or react to the congestion by reducing its private commissions, thereby substituting them with public revenues. Distinguishing the two strategies allows the correct interpretation of the RD estimates. Indeed, they quantify the "gross" impact of public demand on survival probability which combines, and potentially conflates, both a *scale effect* (i.e., additional revenues coming from the contract award) and a *composition effect* (firms' rebalancing sources of income toward public money). To test whether winning firms expand their business after a procurement award, we replicate the comparison of winners versus marginal losers from Figure 2 but at time t and for variables related to firm scale and business decisions.

Figure 3: “First Stage” Effects



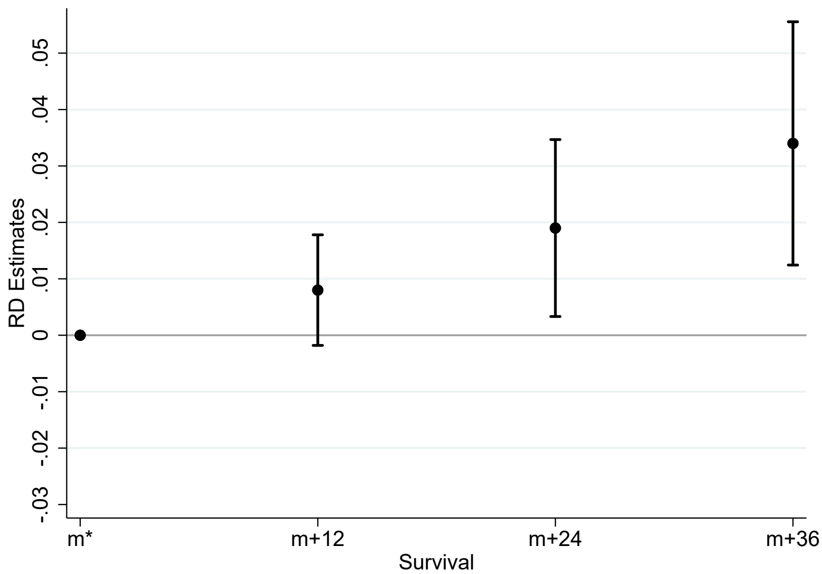
Notes: Visual representation of the RD estimate of being awarded a public contract on revenues (Panel A), number of workers (Panel B), public revenues (Panel C) and the share of public revenues (Panel D), all measured at t.

In Figure 3 we plot the visual effect for revenues (Panel A) and employment (Panel B) to observe winners' short-term response compared to close-losers. The former provides direct information on whether the additional income from public contracts adds to the bulk of income or tends to crowd out private activities. At the same time, an increase in employment would signal the presence of a scale effect. Neither measure, however, shows any significant changes in the award year, suggesting a zero-scale effect. Panels C and D present Public Revenues and Share Public Revenues to further explore the strategic response of firms in terms of revenue reallocation. They show significant jumps in public revenues ($\approx +$ 10%) and share ($\approx +$17%) when firms receive a public contract, regardless of the size. All in all, this evidence suggests that (i) being awarded a public contract induces a strategic response and (ii) a composition effect seems to be at play: firms absorb the higher public demand by shifting some of their sales from private to public customers with no apparent scale adjustment. Therefore, we can interpret the increase in the survival effect shown below as being related to the nature of the demand shock (i.e. public *vis-à-vis* private) rather than to the revenue it generated.

IV.2 Main Results

We begin by showing the visual relationship between the increase in the probability of survival and the contract award event. In Figure 4, we plot the estimated increase in the probability of staying in business 12, 24, and 36 months after the award. The estimated parameters are local as resulting from winners compared to the two closest losers (i.e., runners-up and third-ranked) with bids that are no more than 5 p.p. of discount on the reserve price away from the runner-up threshold.[24] We observe an accruing effect over time. Part of the effect mechanically reflects the contract duration. However, as the median contract in the RD sample lasts approximately ten months—or 300 days, as shown in Table 1—the boost to survival goes well beyond it.[25] Hence, public awards positively affect the survival probability of the winning bidders in the medium run compared to control bidders. Confidence intervals, computed at the 95% confidence level, widening with the time horizon suggest a contamination concern in our control group of marginal losers that we formalize in Section IV.3

Figure 4: Visual Representation of RD Point Estimates



Notes: RD estimated coefficients and standard errors (95% confidence level) of survival boost m months after the participation in auction a. The coefficients are bias-corrected and the computed standard errors are robust.

We report the results on the three survival outcomes in Table 2. We show the results of our baseline specification in Panel A.[26] The estimates show that being awarded a public contract has a positive effect on the probability of surviving both 24 and 36 months after the award date. Survival increases by 1.9 and 3.4 p.p. from baseline values of 97.7 and 95.7 p.p., respectively. Looking at the 36-month survival rate, winning a contract allows a firm in our sample to reduce its exit rate from approximately 4% to 1%, corresponding to a 75% reduction in market exit odds.[27]

[24]To include at least one within-auction comparison always, we exclude in a robustness regression those auction observations where the winners' bid exceeds the 5-p.p.-discount distance from the runnerup (i.e., approximately 12% of the bids). Our findings are robust to this sample selection.

[25]For contracts above €150K, OpenANAC also provides information on renegotiations and delays so that we are able to compute the real duration of the contracts in our analysis sample. 90% of the contracts that we are able to merge appear to be on time.

[26]We employ a triangular kernel and a second-order degree polynomial in the focal specification. On the one hand, the chosen kernel gives more weight to observations close to the cutoff. On the other hand, the chosen polynomial allows us to account for non-linearities in the scores on both sides of the cutoff. We refrain from using higher-order polynomials as they can lead to noisy weights and poor confidence intervals (Gelman and Imbens, 2019).

[27]We employ heteroscedasticity-robust standard errors. Yet it is common in the empirical literature using RD studies to define standard errors as clustered by the running variable (Kolesár and Rothe,

Table 2: RD Regressions—Baseline and Robustness Checks

	Window	Polynomial	Kernel	Survival m+12	Survival m+24	Survival m+36
Panel A: Baseline	± 5 p.p.	Quadratic	Triangular	**0.008**	**0.019**	**0.034**
				(0.005)	(0.008)	(0.011)
				0.991	0.977	0.957
				2,532	*2,532*	*2,532*
Panel B: Linear	± 5 p.p.	Linear	Triangular	**0.008**	**0.017**	**0.032**
				(0.005)	(0.007)	(0.011)
				0.991	0.977	0.957
				2,532	*2,532*	*2,532*
Panel C: Epanechnikov	± 5 p.p.	Quadratic	Epanechnikov	**0.009**	**0.019**	**0.033**
				(0.005)	(0.008)	(0.012)
				0.991	0.977	0.957
				2,532	*2,532*	*2,532*
Panel D: 1 percentage point	± 1 p.p.	Quadratic	Triangular	**0.009**	**0.018**	**0.047**
				(0.003)	(0.009)	(0.011)
				0.993	0.982	0.967
				2,166	*2,166*	*2,166*
Panel E: 9 percentage points	± 9 p.p.	Quadratic	Triangular	**0.008**	**0.015**	**0.025**
				(0.005)	(0.007)	(0.011)
				0.991	0.976	0.954
				2,681	*2,681*	*2,681*
Panel F: All percentage points (optimal bandwidth)	All p.p.	Quadratic	Triangular	**0.008**	**0.018**	**0.044**
				(0.004)	(0.008)	(0.012)
				0.989	0.973	0.948
				2,878	*2,878*	*2,878*
Panel G: No contamination (control)	± 5 p.p.	Quadratic	Triangular	**0.025**	**0.067**	**0.131**
				(0.015)	(0.024)	(0.041)
				0.996	0.986	0.969
				270	*270*	*270*
Panel H: No contamination (treatment)	± 5 p.p.	Quadratic	Triangular	**-0.000**	**0.004**	**-0.008**
				(0.013)	(0.026)	(0.036)
				0.991	0.975	0.953
				675	*675*	*675*
Panel I: Without 2017	± 5 p.p.	Quadratic	Triangular	**0.009**	**0.017**	**0.024**
				(0.005)	(0.008)	(0.011)
				0.991	0.978	0.958
				2,236	*2,236*	*2,236*
Panel J: Alternative runner-up	± 5 p.p.	Quadratic	Triangular	**0.009**	**0.019**	**0.035**
				(0.005)	(0.007)	(0.011)
				0.992	0.977	0.958
				2,607	*2,607*	*2,607*
Panel K: No Consortia	± 5 p.p.	Quadratic	Triangular	**0.008**	**0.018**	**0.032**
				(0.005)	(0.008)	(0.012)
				0.991	0.977	0.957
				2,446	*2,446*	*2,446*

Notes: The RD coefficients (first row of each panel, in bold) are bias-corrected and the robust standard errors are in parentheses (second row). We also report the mean of the dependent variable (third row), as well as the number of observations (fourth row). The observation is at the auction-bid level. Given our selection, the number of auctions in each regression corresponds to one-half to one-third of the observations, depending on the share of auctions with two participants (winner and runner-up only) or more (third-ranked also). For all specifications, we use the bandwidth minimizing the MSE.

2018). This means that observations with the same realization of the running variable are defined as members of the same cluster. A cluster-robust procedure is then used to estimate the variance of the estimator. Accordingly, in an auxiliary analysis, we cluster the standard error at the auction level with

IV.3 Robustness Checks

We run the analysis with alternative model specifications to test whether the results are sensitive to arbitrary choices on the functional form. In particular, we show the results when using either a local linear regression on each side of the cutoff (Table 2, Panel B) or an alternative non-parametric specification of the kernel (i.e., Epanechnikov, Panel C). In both cases, we obtain very similar results, both qualitatively and quantitatively. We also change the window of scores around the threshold: In Panels D and E, we restrict and extend the window by 4 p.p. on both sides of the cutoff, respectively. The more we zoom in on the score space around the cutoff, the more we keep auctions where the first three bids are very close, the more the number of observations decreases relative to the baseline. However, the smaller the window, the larger and more significant (but also more local) the estimated effect. When using a $\pm$1-p.p. window, the results on +36 months increase in magnitude, indicating a stronger survival boost. When we expand the window to $\pm$9 p.p around the cutoff, or when we do not impose any window in the running variable space and employ a robust bias-corrected RD—as in Panel F—the estimates hold comparable nonetheless. The inclusion of less comparable firms discussed in Section III.3 does not seem to affect our estimates but does indeed affect their validity. We then keep our preferred window specification of $\pm$5 p.p. to maximize the trade-off between the locality of estimates and their validity.

After ruling out the concern that our RD results are sensitive to the functional form definition, we address the concerns about the identification assumptions. In this section, we discuss those related to contamination sample selections and definition issues presented in Table 2, Panels G-K. We refer the reader to Appendix B for tests for robustness concerning bid rigging, as well as auction-rules-related sample selections.

Although losers and winners are similar in terms of pre-treatment exposure to public procurement, the contamination problem is underpinned by the fact that the longer the period after the award event, the greater the chances for both losers and winners to win other contracts (control and treatment contamination, respectively). In this scenario, the comparison between losers and winners could become increasingly contaminated over time. We propose a series of exercises to show the robustness of our estimates to the contamination problem. Panel G shows the scenario with no contamination in the control group—i.e., excluding *all* runners-up and third-ranked that do not receive a contract starting at t through the following three calendar years. To perform this exercise, we use the entire OpenANAC data to make the firm selection independent of the analysis sample of contracts. As expected, the survival coefficient is much stronger—as we compare the winners (who may receive more contracts) to the ”never winners”—but the sample size decreases dramatically; never-winners are indeed few—i.e., 16.8% of the firms in the analysis sample). However, these point estimates should be taken with caution: although they show the remarkable robustness of our baseline results, the increase in the parameter value could be due to the adverse selection of poor quality controls. By restricting attention to firms that do not receive a future contract we could boost the treatment parameter "endogenously." In Panel H, we propose a mirror approach, i.e., we exclude only winners that received other contracts ("no-more-winners"). As expected, the mechanical absence of contamination in the treatment but the presence of contamination in the control quickly causes the comparison of no-more-winners to "winning-losers" to

virtually unchanged results.

become nonsignificant.

These findings speak for themselves: there is a risk of contamination both in treatment and control groups and the issue becomes bigger as long as we focus on longer outcome leads. If we jointly remove control and treatment contamination, we are left with no sufficient power for estimation; yet, even if we could, we would be including two-layers of suppliers selection which would make the comparison biased in an unpredictable way. An alternative approach is to *control for contamination*. In fact, if there is a risk of contamination but alike in both control and treated firms, our baseline RD would be estimating the local average treatment effect in an unbiased manner as comparing firms with similar future exposure to procurement contracts *excluding* the contract under analysis. In a robustness check, we replace the survival outcome variable with a binary indicator signalling at least one award after t and up to $t+m$, for all leads, and could not estimate any significant effect (Table 4, Panel A). In other words, the probability of obtaining contracts after time t is the same for control and treated firms, and such zero-effect confirms that winners and losers around the cutoff are in fact fully comparable, also in terms of exposure to public procurement sales after auction a. Thus, despite the risk of contamination, our RD design is capable of estimating the effect of public demand at t for winners.

The other robustness exercises are associated with the auction mechanism rules. In Panel I, we exclude the 2017 auctions from the sample as the rules of ABA changed slightly in May 2017. From then on, before opening the sealed bids, the buyer proceeded with a random draw among five criteria to assess an offer as anomalously low and some criteria were not coherent with the definition of the ABA mechanism discussed in Section III.2.[28] In Panel J, we use an alternative definition of the runner-up for the ABAs. The ABA mechanism can yield situations where the absolute distance between the winning and the runner-up bid is larger than the absolute distance between the winning and the nearest excluded bid.[29] We define the cutoff using the absolute-nearest bid instead of the baseline ABA's runner-up bid. The further specification does not induce a different pattern in the results. As an additional robustness check for the winner and runnerup-definition, we exclude in Panel K consortia from the sample of winners and losers and we obtain similar results, both qualitatively and quantitatively. All in all, these exercises confirm the robustness of the baseline findings against the risks for the validity of the RD associated with contamination and auction rules.

We propose additional exercises to corroborate our findings. First, in Table D4 and D5, we augment our baseline model specification by controlling for covariates.[30] Specifically, in Table D4, we add as controls in the RD model alternatively the nine firm-level variables measured at $t-1$, which we used to show pre-treatment similarity of bidders in Section III.3. In Table D5, we instead include seven contract- and auction-level controls, namely contract duration, reserve price, reserve price over revenues, reserve price over employment, North-regions dummy (vs. rest of the country), type of contracting authority (i.e., central vs. local government), and construction category (i.e., buldings vs. other constructions). All these exercises but one—i.e, controlling for share of direct procurement awards, which restricts the sample to procurement winners in the year t mechanically—show robust estimates of τ over the time leads.

[28]The details on the "new" ABAs are presented by Conley and Decarolis (2016).

[29]For instance, see "b20" in Figure 1.

[30]Controlling for additional covariates is also instrumental to explore the conditional average treatment effects (Calonico et al., 2019). See Section V.1 for an in-depth analysis of effect heterogeneities.

Second, we run a battery of placebo RD regressions that replicate the baseline model and the functional-form robustness checks (i.e., Table 2, Panels A to F), by ruling out the winners from the sample and replacing them with the runners-up. The third-ranked in the original regression sample turn to be the runners-up in the placebo exercise, and so forth. The results, reported in Table D3 in Appendix D, show that all of the coefficients, except for one, are no longer statistically significant, which advocates that the effect identified in our regressions are indeed triggered by the exogenous demand shock rather than by other confounding factors.

V Discussion

In this section, we explore possible drivers and implications of our results. First, we investigate which contract characteristics matter the most in terms of survival boost. Second, we study the effect of public awards on the evolution of two key determinants of survival at the firm level: productivity and financial performance.

V.1 Effect Heterogeneity

Which public contracts are more impactful for firm survival? Within our data, we observe a highly heterogeneous sample of contracts in terms of size, content, and buyer characteristics. To investigate heterogeneous treatment effects, we split the data into a constellation of subsamples and separately estimate τ, providing estimates conditional on a specific set of observable orthogonal contract characteristics.

To accommodate concerns over multiple testing and invalid inference on heterogeneity in the sharp RD framework (Imbens and Lemieux, 2008), we perform a subsample-regression approach—in opposition to an interaction-term analysis conditioning on pre-treatment outcomes. Moreover, we consider the lack-of-power-versus-coarseness trade-off raising from subsampling in an RD framework. On the one hand, if the subgroups are too finely discretized, the subsample regression method can lose power. On the other hand, coarsely defining groups can let important information on treatment effect heterogeneity be lost (Hsu and Shen, 2019). We maximize this trade-off by splitting the sample into three or four groups depending on the source of variation. Specifically, in the case of continuous variables, we group observations in quantiles according to the median of the variable of interest (e.g., the reserve price or expected duration) and define four cross-subgroups (i.e., below reserve price median *and* above expected duration median). In the case of categorical variables, we typically assign groups based on three selected meaningful elicited categories (e.g., government layers). We separately estimate the original regressions for these new sub-samples across the three time leads and assess whether the estimated effects differ from one another and vis-à-vis the baseline's.

As reported in Table 3, we pin down heterogeneous effects on contract size *and* duration (Panel A), buyer type (Panel B), and construction category (Panel C).

Regardless of the reserve price, the survival boost of awards is significant and stronger only for long contracts, while it is not significant for short small contracts and short large contracts. In other words, winners of contracts that are long and small (i.e., above the median expected duration and below the median reserve price) or long and large (i.e., above the median expected duration and above the median reserve price) have

Table 3: RD Regressions– Heterogeneity

	Survival		
	m+12	m+24	m+36
Panel A: Contract Duration and Size			
Short and Small	**0.007**	**-0.004**	**0.008**
	(0.005)	(0.015)	(0.020)
	0.994	0.981	0.961
	737	*737*	*737*
Short and Large	**0.006**	**0.012**	**-0.019**
	(0.006)	(0.009)	(0.041)
	0.994	0.984	0.968
	358	*358*	*358*
Long and Small	**-0.000**	**0.012**	**0.053**
	(0.000)	(0.009)	(0.019)
	0.995	0.984	0.968
	389	*389*	*389*
Long and Large	**0.007**	**0.026**	**0.051**
	(0.017)	(0.019)	(0.027)
	0.985	0.965	0.938
	670	*670*	*670*
Panel B: Buyer Type			
Central Government	**0.027**	**0.042**	**-0.006**
	(0.014)	(0.017)	(0.044)
	0.991	0.980	0.957
	317	*317*	*317*
Local Government	**0.005**	**0.012**	**0.034**
	(0.006)	(0.010)	(0.013)
	0.992	0.977	0.957
	1,677	*1,677*	*1,677*
Other Local	**0.009**	**0.032**	**0.050**
	(0.006)	(0.013)	(0.026)
	0.990	0.976	0.958
	523	*523*	*523*
Panel C: Construction Type (CPV)			
Civil Works	**0.005**	**0.018**	**0.038**
	(0.007)	(0.011)	(0.013)
	0.992	0.976	0.955
	1,820	*1,820*	*1,820*
Buildings	**0.014**	**0.014**	**-0.004**
	(0.008)	(0.008)	(0.024)
	0.992	0.986	0.969
	498	*498*	*498*
Others	**0.023**	**0.033**	**0.112**
	(0.016)	(0.020)	(0.035)
	0.989	0.971	0.945
	201	*201*	*201*

Notes: Contract size is defined based on auction reserve price, while contract duration on expected duration. Bias-corrected RD coefficients (first row of each panel, in bold), robust standard errors (second row, in parentheses), average of the dependent variable (third row), and number of observations (fourth row) are obtained on cross-quantiles of reserve price and expected duration distributions (Panel A), different types of buyer (B), and different construction categories (C). The observation is at the auction-bid level. In all regressions we use a triangular kernel, the second-order polynomial, the bandwidth minimizing the MSE, and only consider observations within 5 p.p. around the cutoff.

a survival advantage over losers. On the one hand, these results suggest that a firm survives because the awarded contract is active. However, we are not overly concerned about the mechanicalness of such an effect because i) we observe in our data that firms are awarded contracts quite regularly, and a pure mechanical effect would entail that they never exit the market, which is not the case; ii) the estimated survival effects after three years still far exceed the median contract duration *within* any "long duration"" subsample (i.e., 422 and 590 days for small and large long contracts, respectively); and iii) no legal constraint forces a firm to postpone declaring bankruptcy and exiting the market during the execution of a public contract until its end. Most importantly for our work, the mechanicalness concerns are softened because the contract size proxied by the reserve price does not matter whatsoever. We interpret such lack of effect to mean that, regardless of the size of the award, winners use contracts as a source of secured income and collateral to marginally improve their credit position—and thus indirectly their survival prospects. For example, in the event of a symmetric shock at the industry level, procurement firms could use their earnings-based collateral to access credit more easily and be more likely to survive. This would happen regardless of the income size and only because of the (public and therefore secured) nature of it.

Interestingly, contracts auctioned off by local buyers, irrespective of being on behalf of local government (i.e., region or municipalities) or the central government (e.g., universities) impact winner's survival; instead, contracts awarded by central administrations (e.g., ministries) are associated with an effect dissipating after the second year, despite awarding, on average, larger contracts. This suggests that "locality" plays a role in the survival effect, as long as comparable contracts have longer-lasting (even though weaker) effects when awarded by local authorities. Assuming that firms are more likely to have political connections with local rather than central authorities, and with the idea that political connections help firms remove certain frictions, this would reconcile our heterogeneous results with those of Akcigit et al. (2022) who show that firms with political connections survive longer in the market.[31]

Finally, we divide our sample of construction contracts according to the Common Procurement Vocabulary (CPV), which is adopted in Italy as well as other EU member states. In particular, we group contracts in Civil Works (i.e., CPV 452), Buildings (i.e., CPV 454), and Other Constructions. In this case, we signal the lack of effect for buildings. This finding can be motivated by the different weights of public customers for the total turnover in the civil work industry and building industry in Italy we assess in our data. In fact, the average share of public versus private spending in the public works construction market is higher than in the construction of buildings (i.e., 27 versus 23% respectively). The award (lack) of public contracts in the former case likely benefits (hurts) firm business more than in the latter, as buildings companies have additional sale opportunities in the private building market and might replace a missed public with a new private customer more easily. Winners of public building contracts, therefore, tend to display no differential survival prospects compared to losers.

We have explored further subgroups analyses along other dimensions. The results are displayed in Table D6 in Appendix D. For instance, we split the sample in terciles of contract reserve price distribution relative to winner's revenues (Panel A) and the same relative to employment (Panel B) to normalize contract size to suppliers size. In addition, in Panel

[31]We rule out blocking competition as a channel for this effect because we restrict our analysis to auctioned contracts. An example of potential barriers that could be removed by connectedness are looser credit constraints from local banks.

C, we associate the contracts to the geographical area of the buyer to capture possible unobserved drivers related to local institutions and divided the country in northern regions (pooling NUTS1 Northwest Italy and NUTS1 Northeast Italy), central regions (NUTS 1 Central Italy, which includes the capital city of Rome), and southern and islander regions (NUTS1 South Italy plus NUTS1 Insular Italy) according to the subdivision of the country in adopted by the National Statistic Office and European Statistical Office. We find no detected heterogeneous effect along all these dimensions.

V.2 Which Firms Survive?

Productivity Dynamics Labor productivity is a strong predictor for business survival (see Table A1). Panel B of Table 4 reports the estimated parameters for labor productivity one, two, and three years after the award. The level of the observation is the firm-year given that, unlike for survival, we only observe productivity once a year through the balance sheet data. Essentially, we compare the future values of the productivity of firms receiving contracts at any point in t with those of the runners-up and the third-ranked. We can only run this exercise only for firms active in the market in each lead—that is to say, all results hold conditioning on survival. The estimated difference between the groups includes both the effect due to the evolution of labor productivity and the one driven by the differential in survival rates.

We find no effect of public contracts on the labor productivity dynamics of survivors. Lead productivity does not seem to be affected by shocks in public sourced demand. This, in turn, implies that neither our estimated increase in survival rate is channeled through an increase in productivity. This result is consistent with several non-competing explanations offered both in the policy practice and in the academic literature. First, there is evidence that government-linked firms invest less in intellectual capital and tend to grow less (Cohen and Malloy, 2016). Second, the government may have incentives to protect inefficient firms through public contracts and shield them from market competition because their existence meets policy goals or dynamic considerations—e.g., this is the case, for example, with "set-aside" programs in the US, where about a quarter of the federal government procurement budget is allocated to support small, disadvantaged or local firms (Cappelletti and Giuffrida, 2022). Yet neither the requirements nor the explicit goals for these programs take into account the impact on business productivity.

Credit Dynamics Another channel through which winning firms could see their probability of survival increased is through an improvement of their financial position. Indeed, it is broadly established that an improvement in financial conditions is a reliable predictor of medium- to long-term survival (see e.g., Blattner et al., 2022 and Schiantarelli et al., 2020). Indeed, being awarded a public contract could i) facilitate access to credit (e.g., by securing future revenues) and ii) help overcome current financial constraints. Recently, di Giovanni et al. (2022) proposed a procurement model for financially constrained firms, which shows that both effects are at play. Specifically, their model predicts that a financially constrained firm that receives government contracts can use this future earning stream to improve its current financial position and, in turn, increase its ability to receive more contracts in the future. In terms of our approach, this means that contract awardees may survive longer *because* they improve their financial conditions. We first provide evidence that winners' credit position responds quickly and steadily to an award.

Table 4: RD Regressions—Additional Outcomes

	Outcome		
	t+1	t+2	t+3
Panel A: Winning a Contract	**0.041**	**0.011**	**0.027**
	(0.032)	(0.033)	(0.035)
	0.522	0.486	0.454
	2,532	*2,532*	*2,532*
Panel B: Labor Productivity	**-15.888**	**-6.943**	**-1.128**
	(20.632)	(4.425)	(5.846)
	61.038	61.094	61.364
	2,154	*1,829*	*1,505*

Notes: Bias-corrected RD coefficients (first row of each panel, in bold), robust standard errors (second row, in parentheses), average of the dependent variable (third row), and number of observations (fourth row) relative to the winning indicator variable (A) and labor productivity (B). The observation is at the auction-bid level. In all regressions we use a triangular kernel, the second-order polynomial, the bandwidth minimizing the MSE, and only consider observations within 5 p.p. around the cutoff. Labor productivity is added value divided by the number of workers. t stands for the award year.

Then, we argue that credit position is likely a major transmission channel for survival dynamics by showing how differently credit categories evolves for winners and losers.

To explore short-run financial responses, we exploit monthly frequency data from the credit registry introduced in Section II. Over these sets of exercises, we focus on winners as opposed to "non-contaminated" losers—being awarded no contracts before and after the month of award. We use a symmetric time window of 24 months around the award month to further minimize contamination coming from differences in survival probability estimated beyond 12 months after the award and hinging of non-discontinuous firm characteritics at $t-1$ as discussed in Section IV.2 and III.3, respectively. The bidders' selection, the monthly frequency of records, and the tight rolling window allow us to eyeball the short-term dynamics triggered by the award itself, even though the medium- and long-term effects in terms of presence in the market take time to materialize.

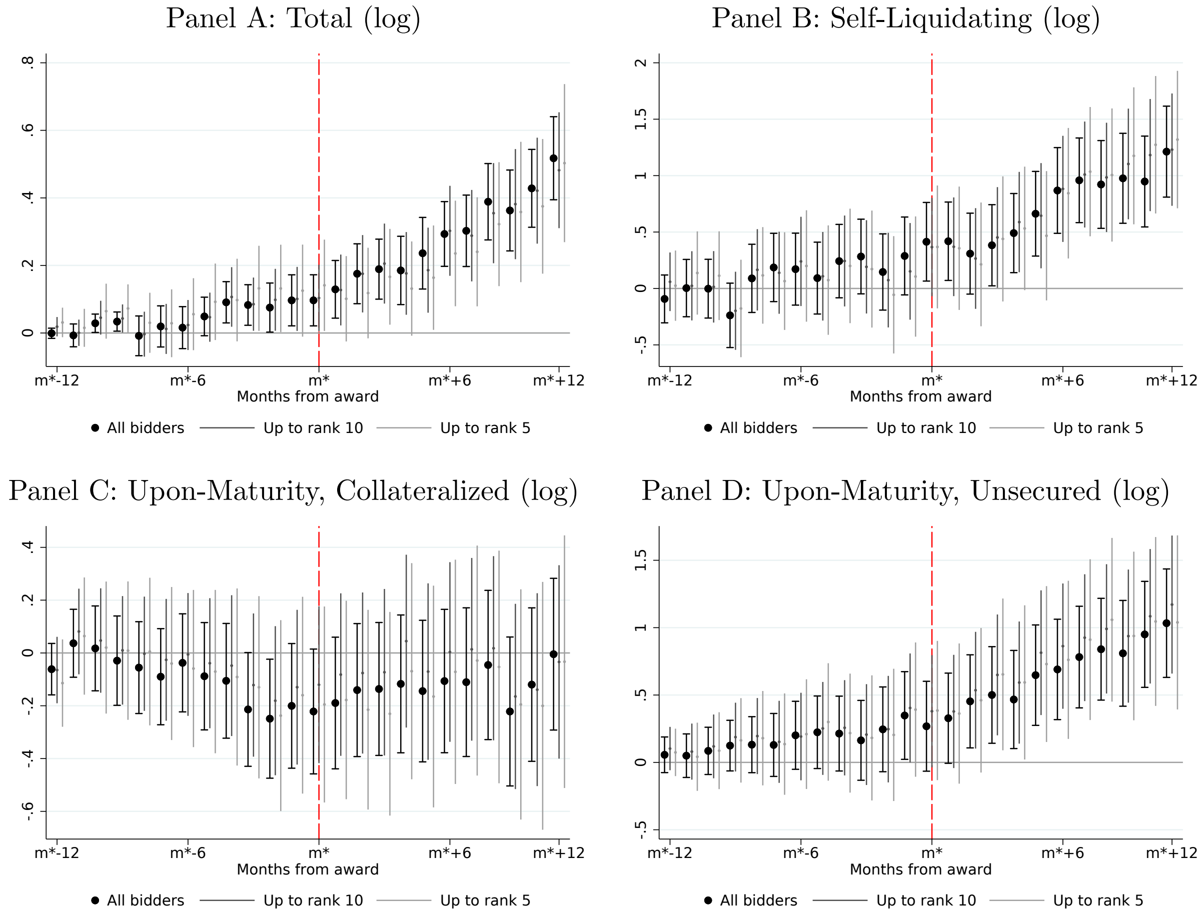
In Figure 5, we plot the coefficients estimated in a regression of the credit variable(s) on month fixed effects interacted with the binary indicator for winners. In Panel A, we show the estimated difference between winners and losers in (log) total credit around the award date with a different selection of the control group rankings: all (black), the first 10 (dark gray), or the first 5 bidders (light gray).[32] While there is little or no difference before the event, the coefficients begin widening significantly as early as two months after the award, with a clear upward trend up to 0.80% until 12 months, when the baseline survival effect is still not significant. Due to decreasing power, the more we restrict the ranking of losers, the longer it takes for the difference to become significant—at last, 6 months for the more restrictive selection—even though the magnitude does not change substiantially.

To nail down the mechanism that links public awards to firm survival in the medium-run, we further break down the total credit into different categories. First, we look at the self-liquidating credit, which consists of short-term loans typically granted for investments and whose amount is paid back with the profits generated by the investment itself—or,

[32]The sample of non-contaminated runners-up and third-ranked is limited—totaling one-third of the treated firms—and does not allow for meaningful comparisons.

ceteris paribus, with the income coming from a secured contract. We also focus on upon-maturity loans, and further distinguish between collateral-backed and unsecured credit, as long as the expected revenue inflow provided by the awarded contract matters for the latter, but not for the former.

Figure 5: Credit Stock Dynamics

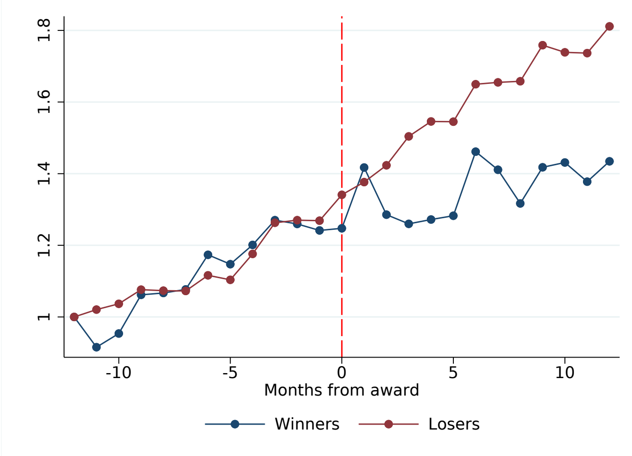


Notes: Estimated differences between bidders awarded and not awarded public contracts in terms of total credit (Panel A), self-liquidating credit (Panel B), and in credit upon-maturity which required (Panel C) and did not require collateral (Panel D). For each panel, we plot coefficients estimated in a regression of the credit variable(s) on month fixed effects interacted with the binary indicator for winners. Sample include all bidders (black line) or winner plus losers up to rank 10 (dark gray) and 5 (light gray).

The amount of self-liquidating credit exhibits a steep pattern after the awarding date for the winners—i.e., +0.8% after 6 months and up to +1.2% after 12 months—but the gap widens significantly after $m + 3$, suggesting that winning firms leverage the advance invoices (or award notices) to borrow just after signing the contract. On the other hand, the credit upon maturity shows two distinct patterns. While the collateral-backed loans do not record any difference between winners and losers, the unsecured credit for winners increases significantly after only 2 months, and up to +1% at $m + 12$. Accordingly, the difference in total credit dynamics seems to be driven by both unsecured loans and self-liquidating credit: the former is responsible for the shortest-run effect, while the latter becomes more relevant from the fourth month onwards down until the last payment is disbursed (i.e., until and likely beyond contract expiration).

For the sake of consistency, we check whether the easier access to credit helps firms overcome existing financial constraints and avoid the deterioration of their financial situation. In Figure 6, we compare winners (blue line) and losers (red line) in terms of the monthly average value of the variable *credit issues*, which is an indicator that flags the presence of bad, impaired, or expired loans in the firm's account.[33] The figure illustrates how the common rising pre-award trend is projected smoothly for losers, while it flattens for winners. Hence, winning firms are effectively able to leverage the contract they receive to avoid delayed payments, deterioration, and, ultimately, market exit.

Figure 6: Credit Quality Dynamics



Notes: Indicator function of any bad, impaired, or expired credit for auction winners and losers (jointly, credit issues dummy). We rescale the series to be 1 at $m^* - 12$.

Taken together, these results suggest that in our data credit stands out as a mechanism underlying the survival effect of public contracts, and already plays a role in the very short run, before survival prospects are affected by awards, and likely beyond, as suggested by the stronger effect for self-liquidating credit. Combined with the short-run earning substitution effect we display in Section IV.1, firms exposed to public demand seem to replace sources of income in favor of public customers to overcome financial constraints by taking out new loans right after contract signature—in particular unsecured and self-liquidating ones—and improve their current and prospective financial position.

VI Conclusions

This paper quantifies the effect of public purchases on supplier survival dynamics. We construct a unique dataset on firms, contracts, and auctions and focus primarily on the survival likelihood—an aspect mostly overlooked in the extant literature, which instead tends to analyze the effects of procurement contracts on business performance *conditioning* on survival. In doing so, we inform the debate on how the public sector's monopsonistic power could be an effective fiscal measure for the government to intervene in the economy and affect business performance.

[33]For the sake of clarity and convenience, we present the series rescaled to be 1 at $m - 12$. We obtain comparable results when we use the (unreported) regression approach as in Figure 5. Tables and figures are available from the authors upon request.

Our results indicate that winning a contract per se increases a firm's survival probability above and beyond the contract expiration. We show that this effect is associated with a recomposition of revenues from private to public customers rather than a pure scale effect induced by the award. Regardless of size, long-lasting contracts or awards by decentralized buyers are more impactful for survival prospects. To explain the implications of these results, we examine the impact of public demand on different firm outcomes. Labor productivity is unaffected by the demand shocks. This result suggests that public procurement helps firms stay in the market longer. However, it does not necessarily support the growth potential, nor, for that matter, is it designed to award contracts to the most productive firms. To get a better sense of why procurement firms are not necessarily forced to exit, we rely on evidence showing that public contracting revenues protect them from competing with more efficient firms in the private market (Akcigit et al., 2022). Relatedly, we find evidence that procurement firms survive more by leveraging public contracts to gain easier access to credit and improve their credit score.

Are these effects accounted for when regulators design the spending policies and allocate the budget? If intentional, do the results highlight an attempt to address a market failure or, rather, the use of contracts as an indirect tool to pursue other goals? If unintentional, what is the role of bureaucratic incompetence or corruption? While the latter could be at least partially at play, the fact that governments have traditionally pursued restrictive purchasing policies to procure domestically and favor local actors explicitly may suggest that the former is also possible. At the same time, however, the government could target firms that make the survival boost more rewarding (e.g., more productive survivors), which appears not to be the case in our data.

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A Appendix: Regression-based Evidence

From the descriptive statistics in Table D1, Panel A, we know that procurement firms are i) a rather small subsample of the total—they amount to less than 4% of the population—and ii) older and larger. Hence, an unconditional comparison of the survival odds of these two groups is not an informative exercise, as long as the two groups differ along multiple dimensions that in turn predict survival rate, as discussed in Section III. Nonetheless, we want to exploit the wealth of our data to check whether the association of survival and public demand is confirmed in the full sample of firms and contracts and verify whether the positive association procurement-firm survival identified in Section IV is confirmed in a larger sample and longer panel (i.e., 2008-2018). Thus, we run a static, regression-based exercise that looks at the *conditional* survival probability. More specifically, we regress an indicator function for firm staying in the market two or three years ahead against an indicator variable for any contract awards in the year (i.e., $\mathbb{I}\{PubWinner_{i,t}\}$), plus observables.[34] The resulting linear probability model reads:

$$\begin{aligned}\mathbb{I}(Surv)_{i,s,t+j} = \alpha + \beta_1 \mathbb{I}\{PubWinner_{i,t}\} + \\ + \beta_2 LabProductivity_{i,t} + \beta_3 \#\text{ Workers}_{i,t} + \zeta_i + \zeta_{t,s} + \epsilon_{i,s,t},\end{aligned} \tag{4}$$

where $j \in [2;3]$, $LabProductivity_{i,t}$ is labor productivity. Let ζ_i and $\zeta_{t,s}$ be firm and year-sector fixed effects, respectively. The former account for time-invariant heterogeneity among firms and nest all unobserved sub-industry- and geographical-specific effects; the latter capture all time-variant local macroeconomic variables common among all firms within an industry. For instance, during periods of economic turmoil or due to temporary local shocks, the government may be more lenient in awarding contracts to local firms to sustain the local economy. In such a way, we deal with potentially time-dependent impacts of government spending.[35] Let β_1 be the parameter of interest capturing the effect of being awarded at least a contract at t on the probability of staying in the market, conditional on firm size, productivity, and all sector- and local-related characteristics captured by the battery of fixed effects.[36] In Table A1 column (1), we report the results for two years of survival: $\hat{\beta}_1$ is positive and strongly significant, meaning that awards make less likely for the same firm to exit the market. Its effect amounts to 2.4 p.p.—i.e., about half of the mean two-years exit probability—and appears to be a major driver of business survival.[37] The table shows another remarkable fact: Firm scale—proxied by employment—and productivity predict survival but matter more when the firm is exposed to public procurement contracts (column 2 versus column 3). Columns (4) to (6) replicate the same analysis for a three-year survival with overlapping takeaways.

[34] To study the effects of procurement contracts at the firm level, a lead time of at least two years is required to avoid mechanical effects. The average and median contract leads in the full contract sample are 585 and 364 days, respectively, as reported in Section II.2.

[35] The procurement sector is represented by the 2-digits CPV code.

[36] Age as shown in Section II and discussed in Section III, strongly correlates with receiving public demand and is a predictor for survival. However, our fixed-effect model makes age collinear with firm fixed effects as age mechanically increases by one every year for every firm and we omit it from the model.

[37] Even though interpreting linear probability parameters as marginal effects is a challenging exercise for a number of reasons (e.g., Horrace and Oaxaca, 2006), our results indicate that, all else equal, being awarded at least one public contract in a given year is associated with a boost in survival probability corresponding to that of expanding the employment by around 2,400 employees.

Table A1: Firm Survival – Static Regressions, Full Sample

	Two-Years			Three-Years		
		Proc	Non-proc		Proc	Non-proc
	(1)	(2)	(3)	(4)	(5)	(6)
$\mathbb{1}\{\text{PubWinner}\}$	0.024			0.022		
	(0.001)			(0.001)		
# Workers (000)	0.001	0.014	0.001	0.001	0.021	0.001
	(0.000)	(0.003)	(0.000)	(0.000)	(0.004)	(0.000)
Labor Productivity (000 000)	0.003	0.007	0.003	0.018	1.386	0.018
	(0.003)	(0.051)	(0.003)	(0.010)	(0.771)	(0.010)
Observations	4,544,345	192,917	4,320,525	4,068,993	169,433	3,869,487
Unique Firms	738,141	42,749	726,836	696,572	39,800	685,197
Mean Y	0.95	0.98	0.95	0.92	0.95	0.92
Year*Sector FE	✓	✓	✓	✓	✓	✓
Firm FE	✓	✓	✓	✓	✓	✓

Notes: Columns (1) and (4): results of Equation (4) on the full sample using two and three years as the survival horizon. Results of Equation (4) are replicated for procurement firms (columns 2 and 5) and non-procurement firms (columns 3 and 6), respectively. The observations are at the firm-year level. All models feature year-sector and firm fixed effects. Standard errors are clustered at the year-area-sector level.

B Appendix: Manipulation and Collusion

One key assumption for our identification strategy is that firms do not manipulate the assignment around the cutoff—i.e., firms behave competitively. More specifically, since bidders' ranking is key to selecting treated and control bidders, we require that firms do not agree on manipulating their ranking strategically. If collusive agreements are at play, bidders are more likely to change their bid and ranking strategically and be assigned to their preferred treatment condition. The presence of cartels in our sample of auctions could be an issue depending on the interplay between a bid-rigging strategy around the threshold and the award mechanism. If the manipulation only occurs among losing bidders, though, this would not undermine the correct identification.

Ideally, we would like to exclude from the sample all auctions in which bidders are found to be part of collusive agreements. In the absence of such records, we propose in this appendix a series of empirical exercises to corroborate the validity of the identification assumptions. Considered altogether, these exercises suggest that our findings are robust against manipulation concerns. We structure our argument in three complementary parts.

Regression-based exercises The stability of a cartel is arguably more likely when "the cake is shared", that is, when all members are awarded a contract at some point in time. As a result, we would expect cartels' members to win at least one contract every year. In Panel A of Table B1, we repeat the baseline RD exercise excluding all auctions whose runners-up or third-ranked bidders win another contract in the same year of the award under analysis. To implement this, as for the contamination exercises in Section IV, we employ the entire OpenANAC data to make firm selection independent from the analysis sample of contracts. By excluding the "winning losers" at time t, we exclude auctions potentially awarded to cartel members from the sample and only keep firms that participate in contests that are more likely to be competitive. The effects are stronger and more significant despite the halved sample size. Panel G of Table 2 presents the ideal exercise to exclude collusive practices in the auctions from the viewpoint of an eventual cartel's stability: we keep runners-up and third-ranked bidders that are never awarded a contract until $t+3$, despite the multiple award opportunities over time. The effect holds stronger and the results are already commented in Section IV.

The second regression exercise we propose is inspired by the results of Decarolis et al. (2016) and Chassang et al. (2020), considered together. The former discuss how the risk of collusive behavior in Italian public procurement auctions is particularly relevant for ABAs, as they provide vigorous incentives to manipulate the bid distribution. Since the rules allow each firm to submit at most one bid, firms that submit multiple bids must game the system by creating shadow subsidiaries. Alternatively, a bidder may also seek to coordinate with other companies to form a bidding ring and pilot TM2 (see Section III.2). For the strategy to work, cartel members must participate in a sufficient number. By contrast, non-coordinating firms do not have incentives to participate jointly. However, it is a safe strategy to focus only on FPAs where rigging bids do not entail manipulation of the average bid. We report the relative results in Panel B of Table B1: The medium-term effects are bigger in magnitude despite the much-restricted sample. Conversely, despite the larger sample, when focusing on ABAs only, the effect tend to dilute.

According to the collusion detection literature, a signal of bid-rigging in FPAs would be the variance of all bids (Abrantes-Metz et al., 2006), which is not necessarily located around the threshold.[38] To corroborate these results on the FPA sample, we propose below an empirical exercise based on the frontier collusion detection tool from Chassang et al. (2020), whose takeaway is no evidence consistent with the null hypothesis of collusion in the FPAs in the analysis sample. This is understandable as long as cartel members have the possibility of participating in ABAs, where bid-rigging was easier. Finally, Panel C splits the sample depending on the number of submitted bids below versus equal and above 10—the latter being a necessary (but not sufficient) condition for the procurers to opt for an ABA. In the case of more competed contracts—regardless the awarded mechanism—the effect on survival is weaker, consistently with the idea that firms more likely to manipulate procurement awards concentrate in auctions where the probability of ABA implementation—and the actual employment of collusive schemes—is positive.

Collusion detection algorithm Figure B1 replicates the visual test for collusion proposed by Chassang et al. (2020). When a cartel participates in a first-price sealed-bid procurement auction, colluding firms designate a winner among themselves and have the other firms submit intentionally losing bids. To decrease the chance of error and increase the cost of betraying the cartel, especially in a very competitive market, Chassang et al. (2020) and Imhof et al. (2018) argue that the difference between the designated winning bid and others is typically larger than it would be in a collusion-free auction.

The idea is that colluding firms rig the planned-to-be-losing bids, but they might do so far away from the designated winning bid. This creates a suspicious drop in the density of the bid-to-bid distance around zero. Chassang et al. (2020) exploit this behavior to detect collusion by plotting the distribution of Δ, the proportional difference between each bid and the winning bid in that auction.[39] A fair and competitive auction will show increasing bid density as this difference approaches zero, while a colluded auction will exhibit missing mass near $\Delta = 0$.

Unlike the results from Chassang et al. (2020) for Japanese auctions, and despite focusing on the public construction sector as well, we observe no missing bids near $\Delta = 0$—suggesting that the behavior in our sample of FPAs is not the same as in the auctions in Japan. Our data exhibit the highest bid density slightly above zero, suggesting that many auctions have one or more losing bids very close to the winner—inconsistent with the behavior seen in the source paper, where collusive firms arrange for intentional losing bids to be significantly higher than the designated winner's bid. The lack of missing mass near $\Delta = 0$ persists even if we only consider the subset of bids greater than $-0.10 < \Delta < 0.10$ of the reserve price, where the incentive to collude is highest. However, the distribution of bids is significantly wider in our context than in the data used in Chassang et al. (2020).

In the paper, the bulk of observations were contained in the interval $-0.05 < \Delta < 0.05$ p.p. of the reserve price. The authors note that this is usually associated with a very competitive market and one where a small change in bid is associated with a large change in expected profit. The distribution has higher kurtosis, due to heavier tails in our data, and we believe this has two implications. First, there would be less incentive to collude since an efficient firm could take advantage of low competition to increase profits without resorting to collusion. Second, if collusion *were* present, it would be less important that

[38]This pattern is observed in the field. De Leverano et al. (2020) show that the collapse of a cartel in the road pavement market in Montreal after the start of the investigation caused the standard deviation of bid differences in auctions to increase dramatically.

[39]For the winning bid, the difference is from the second-lowest, this creating negative values of Δ'.

the cartel enforces a “no-bid mass near zero” rule since the incentive to deviate is lower. Panel B of Figure B1 further examines the density falloff with a window three times larger than Chassang et al. (2020)’s (i.e., $-0.15 < \Delta < 0.30$ insrtead of $-0.05 < \Delta < 0.05$) with overlapping conclusions.

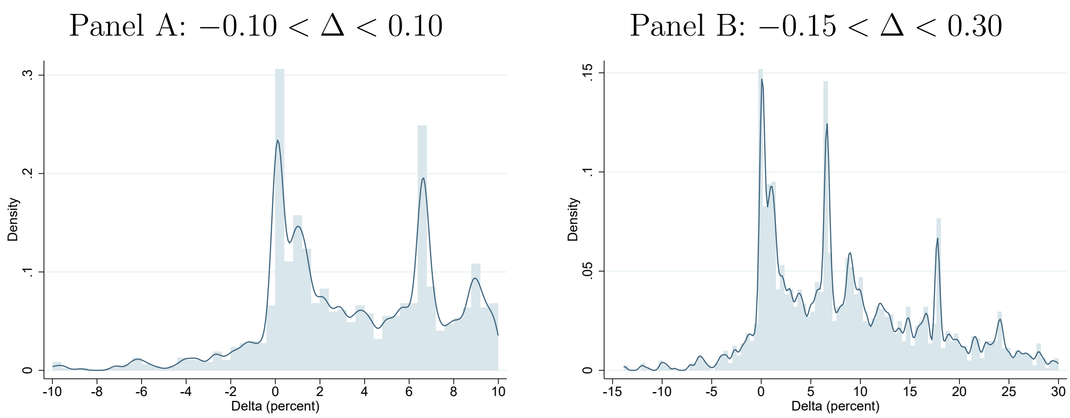
Pre-treatment firm characteristics Continuous firm features in the vicinity of the cutoff exclude the presence of shill bidders created by cartels to better manipulate the allocation, particularly the average bid in the case of ABAs. A shill bidder is a firm created only for this illegal purpose and closed down afterward; therefore, it is hardly comparable with established “real” firms. Following Kawai et al. (2022), observing a discontinuity at the threshold in the level of pre-award backlog—as defined in Section III.3— can be also a sign of bid ridding. Indeed, the backlog proxies the costs of participation in the auction and, in the case of a cartel using bid rotation, all else equal, those with a higher backlog might be less likely to win in a given auction. Given that we find no sign of discontinuity at the cutoff in Section III.3, we can further rule out concerns on possibly colluding bidders.

Table B1: RD Regressions – Collusion Checks

	Survival		
	m+12	m+24	m+36
Panel A: ”Cake is shared”	**0.019**	**0.025**	**0.049**
	(0.009)	(0.012)	(0.018)
	0.983	0.966	0.943
	1,252	*1,252*	*1,252*
Panel B: Auction Type			
FPA	**-0.000**	**0.009**	**0.036**
	(0.000)	(0.009)	(0.017)
	0.984	0.966	0.922
	344	*344*	*344*
ABA	**0.007**	**0.012**	**0.015**
	(0.005)	(0.008)	(0.012)
	0.991	0.977	0.957
	1,672	*1,672*	*1,672*
Panel C: Number of Offers			
< 10 bids	**0.012**	**0.036**	**0.045**
	(0.006)	(0.010)	(0.048)
	0.988	0.974	0.931
	731	*731*	*731*
≥ 10 bids	**0.008**	**0.015**	**0.026**
	(0.005)	(0.008)	(0.012)
	0.992	0.978	0.959
	1,801	*1,801*	*1,801*

Notes: RD estimates are executed keeping auctions in which losers are not awarded other contracts at t (Panel A), splitting the sample of auctions conditioning on the award format (Panel B) and the number of bids received (below 10 versus 10 or above, Panel C). Given our selection, the number of auctions in each regression corresponds to one-half to one-third of the observations, depending on the share of auctions with two participants (winner and runner-up only) or more (third-ranked also). We replicate in each subsample Table 2 Panel A.

Figure B1: Chassang et al. (2020)'s Visual Test for Collusion – Distribution of Bid Difference in FPAs



Notes: Distribution of bid difference Δ from the winning bid. The parameters of the estimated density match those of the original paper, with a smoothing width of 0.75%. Panel A includes all bids: Panel B focuses on bids within 10 p.p. discount from the threshold.

C Appendix: Telemat Document Extraction Procedure

In order to extract the information on the distribution of the bids—from the PDFs documentation provided by Telamat—we had to proceed in several steps. We started with downloading tenders' outcomes PDFs from Telemat's website using Python. In particular, we downloaded only those present both in the Telemat and BDAP database, as the latter data provided us with the name and tax number of auctions participants necessary for the merge with CADS-firm data. The merged data consisted of 11,079 unique contracts. As the documents were not standardized, we had to proceed in several steps. First of all, we had to select the documents containing the list of bids. Note that the downloaded PDFs were more than the number of contracts as, for each contract, more than a document can be produced by the contracting officer. Using Python, we searched among the over 16,000 downloaded documents (corresponding to 10,000 contracts) to select only those containing the list of participants, which BDAP provided. As the documents were not standardized, this was the only characteristic that all PDF documents with the distribution of bids have in common. Then, the 8,348 Python-selected documents for such contracts were inspected manually and with Python, and the bids placed by each auction participant were recorded to create a unique dataset. Given that placed bids appear in a table, we mainly used the package Camelot in Python to extract tables containing the bids from 3,686 machine-readable documents. We had to proceed with manual data extraction for about 4,580 PDFs, namely for those documents that were scanned PDFs and were therefore not machine-readable. However, not all the Python-selected documents reported the bids information, as many reported only the participants' list but not the placed bids. We were able to retrieve bids information for 1,743 contracts (about 16% of the sample for which we had participant information).

D Appendix: Additional Figures and Tables

Table D1: CADS Summary Statistics on Firms: Full Sample 2008-2018

	Non-procurement			Procurement		
	Mean	Median	sd	Mean	Median	sd
Age (Years)	12.92	9.00	12.11	17.41	14.00	13.29
# Workers	8.96	2.58	319.86	47.16	8.76	459.88
Revenues (€,000)	2,634.64	391.00	55,798.56	16,387.86	1,387.00	261441.45
Capital (€, 000)	796.66	43.89	39,802.45	7,192.64	121.52	340169.26
Labor Productivity	84.49	38.33	32,322.42	141.02	49.58	13,635.45
Financial Debt (€, 000)	1,048.84	77.00	14,818.75	7,896.19	248.00	292722.13
Observations	5,859,034			645,723		
Unique Firms	1,046,930			74,399		

Notes: The table reports summary statistics of the 2008–2018 CADS dataset for both non-procurement and procurement businesses. Only *Age* is sourced from Infocamere. Labor productivity is defined as added value divided by employment. The observation is at the firm-year level and we report the corresponding unique number of firms.

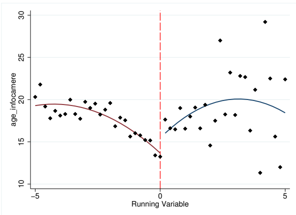
Table D2: OpenANAC Summary Statistics on Contracts: Full Sample 2008-2018

	Overall			Construction		
	Mean	Median	sd	Mean	Median	sd
Amount (€, 000)	1,357	130	82,705	1,411	151	52,463
# Bidders	4.44	1.00	48.82	13.06	4.00	36.57
Duration (Days)	585	364	1,049	326	231	484
Direct Award	0.27	.	0.44	0.12	.	0.32
Open Procedure	0.19	.	0.39	0.21	.	0.41
Negotiated Procedure	0.32	.	0.46	0.46	.	0.50
Consortium	0.06	.	0.24	0.09	.	0.28
Observations	1,274,979			324,533		

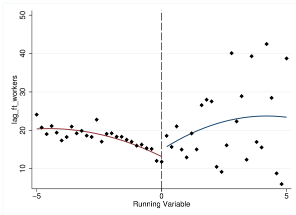
Notes: The table presents summary statistics for the cross-section of OpenANAC data. The level of observation is a contract awarded between 2008 and 2018. The *Overall* column refers to the entire dataset, while the second columns refer to construction.

Figure D1: (Discontinuous) Firm Characteristics': Winners and Marginal Losers at $t-1$ (All Bidders)

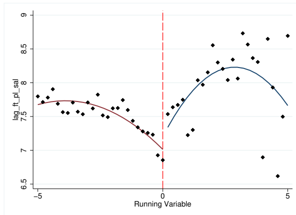
Panel A: Age (Years)



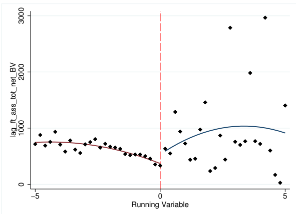
Panel B: # Workers



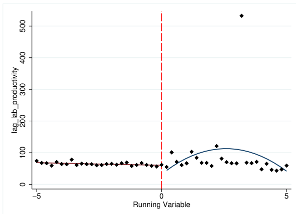
Panel C: Total Revenues (log)



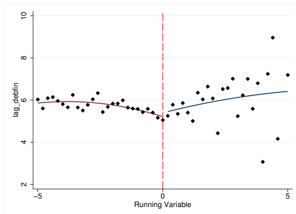
Panel D: Capital



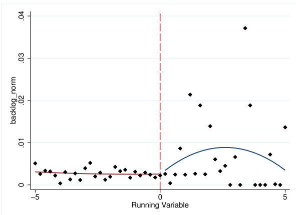
Panel E: Labor Productivity



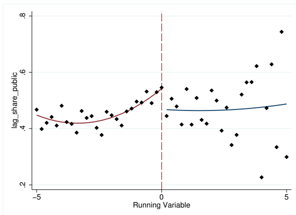
Panel F: Financial Debt (log)



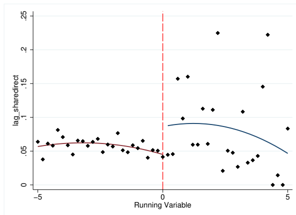
Panel G: Contracting Backload



Panel H: Share Public Revenues



Panel I: Share Direct Award



Notes: We replicate Figure 2 including all bidders.

Table D3: RD Regressions—Placebos

	Window	Polynomial	Kernel	Survival m+12	Survival m+24	Survival m+36
Panel A: Baseline	± 5 points	Quadratic	Triangular	**-0.004**	**-0.008**	**0.014**
				(0.007)	(0.011)	(0.014)
				0.991	0.977	0.957
				2,417	*2,417*	*2,417*
Panel B: Linear	± 5 points	Linear	Triangular	**-0.003**	**-0.008**	**0.014**
				(0.007)	(0.011)	(0.014)
				0.991	0.977	0.957
				2,417	*2,417*	*2,417*
Panel C: Epanechnikov	± 5 points	Quadratic	Epanechnikov	**-0.004**	**-0.008**	**0.014**
				(0.008)	(0.011)	(0.014)
				0.991	0.977	0.957
				2,417	*2,417*	*2,417*
Panel D: 1 point	± 1 point	Quadratic	Triangular	**0.009**	**0.009**	**0.036**
				(0.007)	(0.012)	(0.015)
				0.993	0.982	0.966
				2,067	*2,067*	*2,067*
Panel E: 9 points	± 9 points	Quadratic	Triangular	**-0.001**	**-0.003**	**0.017**
				(0.007)	(0.010)	(0.013)
				0.991	0.976	0.954
				2,547	*2,547*	*2,547*
Panel F: All points (optimal bandwidth)	All points	Quadratic	Triangular	**-0.002**	**-0.005**	**0.015**
				(0.007)	(0.011)	(0.014)
				0.989	0.973	0.948
				2,686	*2,686*	*2,686*

Notes: We replicate Table 2 by dropping the winning bid from each auction, replacing it with the runner-up, establishing the third-ranked as the runner-up and the fourth-ranked as the third-ranked.

Table D4: RD Regressions—Pre-treatment Firm-level Covariates

	Survival		
	m+12	m+24	m+36
Panel A: Age (Years)	**0.000**	**0.010**	**0.022**
	(0.004)	(0.008)	(0.012)
	0.995	0.983	0.964
	2,264	*2,264*	*2,264*
Panel B: # Workers	**0.001**	**0.011**	**0.022**
	(0.004)	(0.008)	(0.012)
	0.995	0.983	0.963
	2,202	*2,202*	*2,202*
Panel C: Revenues (€,000)	**0.001**	**0.012**	**0.024**
	(0.004)	(0.008)	(0.012)
	0.995	0.983	0.963
	2,202	*2,202*	*2,202*
Panel D: Capital (€,000)	**0.001**	**0.012**	**0.024**
	(0.004)	(0.008)	(0.012)
	0.995	0.983	0.963
	2,202	*2,202*	*2,202*
Panel E: Labor Productivity	**0.001**	**0.012**	**0.025**
	(0.004)	(0.008)	(0.012)
	0.995	0.983	0.963
	2,183	*2,183*	*2,183*
Panel F: Financial Debt (€,000)	**0.000**	**0.008**	**0.022**
	(0.005)	(0.010)	(0.014)
	0.995	0.982	0.962
	1,754	*1,754*	*1,754*
Panel G: Contracting Backlog	**0.002**	**0.012**	**0.025**
	(0.004)	(0.008)	(0.012)
	0.995	0.983	0.964
	2,262	*2,262*	*2,262*
Panel H: Share Public Revenues	**0.006**	**0.016**	**0.025**
	(0.005)	(0.009)	(0.013)
	0.994	0.980	0.960
	2,165	*2,165*	*2,165*
Panel I: Share Direct Award	**0.009**	**0.012**	**0.009**
	(0.007)	(0.010)	(0.015)
	0.992	0.977	0.956
	1,784	*1,784*	*1,784*

Notes: We replicate Table 2 Panel A by separately adding firm covariates measured at $t-1$ from Figure 2.

Table D5: RD Regressions With Contract-level Covariates

	Survival		
	m+12	m+24	m+36
Panel A: Duration (Days)	**0.008**	**0.012**	**0.024**
	(0.006)	(0.008)	(0.013)
	0.991	0.977	0.957
	2,154	*2,154*	*2,154*
Panel B: Amount (€, 000)	**0.008**	**0.019**	**0.035**
	(0.005)	(0.008)	(0.011)
	0.991	0.977	0.957
	2,519	*2,519*	*2,519*
Panel C: Amount/Revenues	**0.001**	**0.012**	**0.025**
	(0.004)	(0.008)	(0.012)
	0.995	0.983	0.964
	2,251	*2,251*	*2,251*
Panel D: # Bids	**0.009**	**0.017**	**0.024**
	(0.005)	(0.008)	(0.011)
	0.991	0.977	0.957
	2,532	*2,532*	*2,532*
Panel E: Geographic Area	**0.006**	**0.014**	**0.025**
	(0.005)	(0.008)	(0.011)
	0.991	0.977	0.957
	2,514	*2,514*	*2,514*
Panel F: Buyer Type	**0.009**	**0.019**	**0.034**
	(0.005)	(0.008)	(0.011)
	0.991	0.977	0.957
	2,517	*2,517*	*2,517*
Panel G: CPV	**0.008**	**0.019**	**0.034**
	(0.005)	(0.008)	(0.011)
	0.991	0.977	0.957
	2,519	*2,519*	*2,519*

Notes: We replicate Table 2 Panel A by separately adding reported contract covariates.

Table D6: RD Regressions– Heterogeneity Analyses 2

	Survival		
	m+12	m+24	m+36
Panel A: Reserve Price over Revenues			
Lower Tercile	**0.009**	**-0.001**	**0.017**
	(0.005)	(0.021)	(0.027)
	0.992	0.974	0.949
	661	*661*	*661*
Middle Tercile	**-0.012**	**0.014**	**0.023**
	(0.012)	(0.016)	(0.024)
	0.992	0.975	0.955
	635	*635*	*635*
Upper Tercile	**0.015**	**0.022**	**0.010**
	(0.007)	(0.009)	(0.016)
	0.991	0.982	0.968
	667	*667*	*667*
Panel B: Reserve Price over # Workers			
Lower Tercile	**0.006**	**0.001**	**0.024**
	(0.004)	(0.018)	(0.023)
	0.994	0.978	0.954
	645	*645*	*645*
Middle Tercile	**-0.002**	**0.021**	**0.026**
	(0.011)	(0.015)	(0.022)
	0.992	0.977	0.958
	661	*661*	*661*
Upper Tercile	**0.016**	**0.023**	**0.005**
	(0.008)	(0.009)	(0.019)
	0.989	0.977	0.961
	657	*657*	*657*
Panel C: Nord/Centro/Sud			
Nord	**-0.005**	**-0.008**	**-0.020**
	(0.015)	(0.023)	(0.030)
	0.982	0.953	0.911
	1,035	*1,035*	*1,035*
Center	**0.020**	**0.036**	**0.013**
	(0.010)	(0.013)	(0.033)
	0.989	0.973	0.947
	453	*453*	*453*
South and Islands	**0.003**	**0.004**	**0.014**
	(0.002)	(0.006)	(0.011)
	0.994	0.985	0.971
	1,369	*1,369*	*1,369*

Notes: We split the sample of auctions depending on the distribution tercile of reserve price over firm revenues (Panel A), reserve price over firm employment (Panel B), geographical areas (Panel C). We replicate in each subsample Table 2 Panel A.