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Procuring Survival

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Abstract

Public spending (i.e., "G") enables governments to fulfill their fiscal policies. This paper takes a micro perspective and quantifies the impact of procurement spending—a specific component of G—on firm survival. We find that firms that receive public contracts survive longer, *ceteris paribus*, and that this effect accrues over time, reaching 20 percentage points after ten years. Our results are based on a novel dataset for Italy that combines balance sheet data on the universe of limited liability firms with administrative records on market entry and exit and quasi-universe of public contract data between 2008 and 2018. For construction auctions, we also rely on bid-level data to inform a regression discontinuity analysis. We find that the survival rate of winners relative to marginal losers is 70% higher after 36 months—or after two years and half of the median contract expiration. We explore several alternative channels that could rationalize our findings. We find that recipients do not become more productive, and their earnings become increasingly dependent on sales to public customers.

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

JEL: D44, H32, H57.

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I Introduction

The life expectancy of a firm at birth is low. Across all countries and markets, the vast majority of 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 vital economy, the survival of firms has intrinsic value for socioeconomic cohesion, as recently demonstrated by government support packages in response to the economic fallout from the Covid-19 pandemic. The former observation and the latter considerations have spawned a large body of literature examining the determinants of business survival. Theoretical predictions and empirical evidence stress that survival probability increases with age and size (Evans, 1987a,b; Clementi and Hopenhayn, 2006; Hall, 1986; Dunne et al., 1989) but, among the many forces that determine survival probability, demand constraints are the least analyzed (Syverson, 2011; Pozzi and Schivardi, 2016; Foster et al., 2016). However, given that the demand for firms' products and services mechanically translates into revenues, they play a crucial role in influencing survival prospects.

In this paper, we focus on the relatively unexplored role of public demand—as opposed to market-based, private demand—in promoting firm survival.[1] At the macroeconomic level, government spending (G), 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 private business during economic downturns and prevent general crises. At the microeconomic level, however, the effect of public demand on the survival of private firms is unexplored, and little is known about its effect on firm behavior. Moreover, such a survival effect is *a priori* uncertain as long as higher public demand does not necessarily entails more profits, which tend 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 if contract execution involves significant red tape or renegotiations. On the other hand, firms selling to the government may have access to cheaper capital, since the certainty of government-backed cash flow decreases a lender's exposure to risk (di Giovanni et al., 2021).

We address this question empirically using a novel combination of extensive and highly detailed datasets on Italy, the laboratory for this study. We show that, *ceteris paribus*, an additional euro coming from sales to the government positively affects survival more than one earned through sales to private buyers.[2] We also document how this differential effect builds up over time, leading to substantial differences in survival rates (up to +20 p.p. after ten years) between firms that do and do not enter the public procurement market, controlling for size, age, and productivity. In the medium term we estimate, using close-call auctions in a regression discontinuity (RD) setting, that winning a government contract increases the 24-month (36-month) survival probability by almost two (three) p.p.—on top of a baseline 98% (96%) survival rate. The effect is identified from one and a half to three and a half years beyond the end of the contract. Finally, we use the same empirical strategy to examine how public demand shocks affect other firm-level metrics. We find that public sales do not affect TFP dynamics but acts as a gateway to future procurement performance.

[1] In 2018, government procurement spending in the OECD median country amounts to 13% of GDP and 41% of total government spending.

[2] We emphasize 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. (2021) for a recent overview of the impact of subsidies on market outcomes.

To construct the dataset, we combined individual balance sheet information and income statement records on the universe of limited companies with Chambers of Commerce data reporting official registration (i.e., market entry) and deregistration (i.e., market exit). In this way, we created a panel of 1,121,328 distinct firms over 11 years—including information on revenues, employees, debt, and TFP. We match these data with a comprehensive database on government procurement contracts provided by the National Anti-Corruption Authority (hereafter Anac), the official public procurement regulator. The database contains comprehensive information on all tenders published as of 2008 with a value of more than €40 thousand, and the related contracts.[3] This includes the contract value and its duration, the procurement category, the award mechanism, and, most importantly, the winner's identity. For the period 2008-2018, our data track about 140 thousand companies that participated in government tenders and won public contracts for supplies, services, or works, with a total yearly value of €156 billion, representing 8-9% of the country's GDP during this period and 80-90% of procurement spending.

Thanks to the granularity of our data, we can distinguish firms that receive public money under procurement contracts ("*procurement firms*") and quantify the share of their annual revenues that comes from the government, in addition to observing the day in which they exit the market. In this way, we can compare the (quasi-)universe of procurement firms to the (quasi-)universe of firms that do not receive a public contract, controlling for revenues, size, age, and productivity, and holding the industry and the geographical area fixed. In doing so, we find that the former are more likely to survive, conditional on all observable characteristics. In particular, we stress that among the procurement firms, neither the most productive nor the biggest and oldest firms are necessarily the ones with the lowest hazard rates, in line with the recent findings of Akcigit et al. (2018). This, in turn, suggests that the competitive mechanism (i.e., the cleansing effect laid out in Caballero and Hammour, 1994) and the main drivers of survival are altered by the public procurement. Nevertheless, this empirical exercise might suffer from endogeneity issues. First, there might be unobservable firm characteristics that are correlated with both the probability of winning a public auction and the firm's ability to stay in the market (e.g., political connections, but also management quality); second, the presence of a public contract might mechanically prevent firms from exiting the market for administrative reasons; third, procurement firms might be fundamentally different from the rest of the sample, making the two groups incomparable.

To address these endogeneity issues, we focus on auctions in the construction sector—which accounts for 19% of procurement spending and 13% of firms in 2012-2017 period—and extract information about the bidding process (e.g., individual bids, the identity of bidders, final ranking) directly from the original auction documents when available. Using the sample of auctions with the entire bid distribution, we exploit the gap between the winning and losing bidders in terms of the bid submitted to define a running variable, a cutoff (i.e., at the runner-up bid), and implement a RD analysis by comparing firms that win a public contract by a small margin with firms that barely make it but ultimately do not win. We are thus able to causally estimate the gross effect of winning a public auction on survival as a firm outcome, finding a huge increase in survival probability in the medium run.

The identifying assumption of the RD that we propose is that if a bid is placed close to the "threshold," the award is as good as random. To validate this assumption, we show that the features of the winners—in terms of the main predictors of survival—are indistinguishable from those of the close losers. Moreover, the Italian regulatory framework imposes additional challenges on our identification, and we perform a series of robustness checks to ensure its validity. Indeed, the available mechanisms for awarding public contracts include both the lowest-

[3] The reporting threshold is lower than any category-specific EU regulatory one and, in particular, much lower than that for public works contracts, which is around €5.5 million in 2019. The average contract value in construction is also slightly above the threshold, making our dataset a near-universal sample of this category.

price and the average-bid auctions.[4] We show that under both formats, the bids of losers are always higher than those of winners and our results hold even if we focus on subsamples of the data obtained by excluding one of the awarding mechanisms or in a subset of the time span. Finally, we show that the baseline results are robust to the risk of collusive bidding behavior around the cutoff (a concern for the assumption of quasi-random contract allocation assumption), the risk of contamination of the control group of losers with concurrent awards, and the remaining days before exiting as an alternative survival outcome.

Which firms survive, and how? We address these questions by discussing the implications of our results for firm characteristics. In particular, we examine the role of TFP, a relevant aspect to consider because of its aggregate impact on the economy (Baier et al., 2006)– especially for an economy like Italy with sluggish and increasingly dispersed productivity (Calligaris et al., 2016)—and its importance as a predictor of survival (Ugur and Vivarelli, 2021).[5] We find that procurement firms have no significant productivity difference from all other firms (i.e., *ex ante*), but we also find no "public procurement premium" (*i.e., ex post*)—our RD estimates on lead productivity show negative future patterns, if any. Accordingly, public demand helps firms survive longer, but it does not necessarily select the most efficient firms. These results are consistent with several non-competing explanations offered both in policy practice and in the academic literature. First, the government "nurtures" inefficient firms through public contracts and protects them from market competition because their existence meets policy goals or dynamic considerations—this is the case with set-aside programs in the U.S., where about a quarter of the federal procurement budget is allocated to small or minority-owned firms. Neither the requirements nor the explicit goals for these programs consider the implications for firm productivity. Second, as described by Akcigit et al., 2018, firms might trade future productivity for current access to the public procurement market—in the model, firms invest in political connections rather than productive capital; the same would apply in the presence of other high barriers to entry into the public market. In fact, we find increasing dependence of firms on the public contracts.

Related Literature By examining the role government plays in affecting firm survival, in addition to the papers cited above, our paper relates to a long-standing debate on whether industrial policy can spur firm responses. Through fiscal policy—such as subsidies, training, production reservations, and place-based interventions—governments directly and indirectly support certain firms, sectors, and localities. Despite their prevalence, most of the existing evidence on the effectiveness of such policies comes from either innovation or investment subsidies (Cerqua and Pellegrini, 2014; Criscuolo et al., 2019) to firm or place-based policies (Becker et al., 2010; Kline and Moretti, 2014). Little is known about the implications of demand-based policies on firm performance, especially in a cross-industry spectrum. We contribute to a growing empirical literature that studies the effect of a demand shock on firms outcomes (Pozzi and Schivardi, 2016; Foster et al., 2016), which hinges on solid predictions of theoretical results (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. Exposed firms—*conditioning on survival*—are found to experience a persistent boost in revenues and employment growth. This effect is found in Brazil (Ferraz et al., 2015), South Korea (Lee, 2017), Austria (Gugler et al., 2020), and Ecuador (Fadic, 2020).[6] Firms exposed to public demand shock are also found to have easier access to external borrowing (Hebous and

[4]See Conley and Decarolis (2016) and Decarolis (2018) for a detailed account of the latter mechanism.

[5]We compute the labor productivity (i.e., the average output per worker) and use it as an alternative metric. The conclusions we draw remain unchanged.

[6]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).

Zimmermann, 2020; di Giovanni et al., 2021), cut capital (Coviello et al., 2019), invest less in physical and intellectual capital (Cohen and Malloy, 2016), but innovate more (Czarnitzki et al., 2018). Our paper complements this literature by focusing on survival and productivity as further firm outcomes affected by procurement contracts and across a complete set of procurement categories and firm sectors involved in the Italian government procurement market. Although industrial policy should be targeted in such a way as to support the most efficient players, we show public procurement demand might instead be biased towards the laggard players in the market (Acemoglu et al., 2018). Hence, our result provides further evidence supporting the argument of Pozzi and Schivardi (2016), that demand dominates productivity in determining firm outcomes.

Our work also contributes to a long-standing and established scholarship studying the drivers of firm survival. The relationship between growth and the likelihood of survival is not as simple as at a first casual look. The variance of realized growth rates is found to decrease with size, conditioning on survival (Agarwal and Audretsch, 2001). 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 possibly more about filling strategic niches, negating the impact of entry size on the likelihood of survival. In a nutshell, growing firms do not always see their survival odds increase. 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), 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 investigating the role of public-sourced sales in predicting survival.

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

II Data

We gather data at the most granular level available, both for firms and public procurements. In Italy, the main source for the former is the Company Accounts Data System (CADS) database, a yearly collection of individual balance sheets covering the universe of limited liability companies. We complement it using administrative data on the firm's market entry and—if applicable—exit date, with the reason as provided by the Chambers of Commerce (*Infocamere*). As for the procurement side, we employ the full list of tenders and associated contracts provided by Anac. The two databases are combined via the winning firms' tax code.

II.1 Firm-level data

CADS Produced and distributed by the CERVED Group, the CADS is a proprietary database for credit risk evaluation. Built on detailed balance-sheet and income-statement data, it fully covers limited firms, which overall account for around 70% of the revenues in the private, nonfinancial sectors. The yearly data span every sector and report revenues, employment size, debt stock, among other information.

Table 1: Summary Statistics: Firms

Panel A): All Firms

	Non-procurement		Procurement	
	mean	sd	mean	sd
Age	12.92	12.11	17.41	13.29
# Workers	8.98	319.96	47.11	459.47
Revenues t (€, 000)	2638.72	55,884.51	16,372.06	261,469.11
Capital (€, 000)	796.79	39,799.28	7,201.31	340,432.14
TFP (ACF, 2015)	2.51	2.56	2.65	4.77
Value Added	523.42	10,150.74	3,615.85	51,411.91
Short-term Debt (€, 000)	447.88	7,291.85	3,424.78	124,794.37
Total Debt (€, 000)	1,291.88	18,548.55	9,208.76	247,534.96
Observations	5,860,032		644,725	

Panel B): Construction Firms

	Non-procurement		Procurement	
	mean	sd	mean	sd
Age	10.32	9.59	16.59	12.90
# Workers	3.91	8.02	34.30	406.36
Revenues t (€, 000)	826.61	4,667.24	11,736.25	29,4990.49
Capital (€, 000)	281.75	8,030.39	9,945.09	505,448.61
TFP (ACF, 2015)	3.38	0.71	3.33	3.56
Value Added	200.03	925.08	2,933.44	47,347.43
Short-term Debt (€, 000)	563.56	10,688.89	4,637.49	179,644.54
Total Debt (€, 000)	950.00	8,402.90	8,341.75	168,276.66
Observations	675,534		265,559	

Notes: The table reports CADS data summary statistics for both non-procurement and procurement businesses. The observation is at the firm-year level. The top panel reports all firms appearing in the CADS data, whereas the bottom panel presents only construction firms, namely firms that are described by the *NACE* code as construction firms. Firm productivity (TFP) is estimated à la Ackerberg et al. (2015).

Infocamere We backup the firm-level information in CADS with the *Infocamere* dataset coming from the Chambers of Commerce, i.e., the official business register. The dataset reports historical data on 5,130,744 firms since 1900. The Chambers collect data on each active firm in the country, recording their registration date (i.e., entry) and de-registration date (i.e., exit)—if applicable—including information on the delisting reason—e.g., bankruptcy, acquisition by another business, relocation. We use the latter information to build the "survival" variable that we use as an outcome in our baseline estimations. In particular, we set to missing the record 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 outcomes of the firm. On the other hand, we code a firm de-registration as the exit for all other reported reasons, notably bankruptcy or similar. Also, we retrieve firm age from the year of registration.

Descriptives Table (1) reports a selection of firm characteristics for the 2008-2018 firm sample. We compare procurement and non-procurement firms across all sectors (Panel A) and for the construction sector only (B). Overall, we observe 5.86 million unique non-procurement firm-year pairs and almost 0.65 million conditioning on the procurement status. Not surprisingly, firms that operate only in the private market tend to be different from those selling to the government. The latter tend to be older—17-years-old on average compared to about 13 for non-procurement firms— and much larger in terms of the number of workers (47 vs. 9), but revenues, capital, and value-added. On the other hand, TFP—which we obtain by estimating the production function à la Ackerberg et al. (2015)—is similar across the two groups, though more dispersed for procurement firms. Finally, procurement firms tend to have much higher short-term debt—3,424 euro vs. 447 euro—and higher total debt—9,209 euro vs. 1,292 euro. The comparison among firm types is similar if we restrict our focus on construction firms, as we do in Panel B).[7]

II.2 Contract-level data

OpenAnac In September 2020, Anac published a large amount of previously privately retained data on Italian public procurement. The OpenAnac data constitute the single largest source of this type of data ever available in the country. The data includes all tenders published in Italy above the €40,000, the subset of all awarded contracts linked to them, and, more importantly, the winner(s)' identity and tax code. While OpenAnac is thought to contain public contracts from early 2000s on, data quality was uneven before 2008. Therefore, we limit ourselves to procurement data from years 2008.

Table 2: Summary Statistics: Contracts

	Overall	Supplies	Works	Services
Yearly Amount (€, 000,000)	155,900	54,669	41,357	59,874
# Yearly Contracts	114,746	46,706	29,205	38,835
Amount (€, 000)	1,359	1,171	1,416	1,542
# Bidders	4.45	1.41	13.06	1.62
Direct Award	0.27	0.36	0.12	0.27
Open Procedure	0.19	0.19	0.21	0.17
Negotiated Procedure	0.32	0.23	0.46	0.32
Duration (Days)	475	644	207	671
Subcontracting	0.19	0.03	0.59	0.09
Observations	1,262,208	513,768	321,251	427,189

Notes: The table presents summary statistics for the OpenAnac data. The level of observation is a contract awarded between 2008 and 2018. The *Overall* column refers to the entire dataset, while the three additional columns refer to supplies, works, and services samples.

The data report all the relevant information on (i) the tender—i.e., which category of purchase it refers to (i.e., supplies, services, or public works), the reserve price, the awarding mechanism, and scoring rule adopted, the contracting authority, and whether the funding is at the local, regional, or national level; (ii) the award, with information on, e.g., the winning discount and the number of accepted bidders; (iii) post-awarding phase—with limited information, mainly related to project duration and subcontracting. Among the many other pieces of information reported, the OpenAnac dataset allows us to identify whether the winning firms are part of a temporary group of firms (*Associazione Temporanea di Imprese*), which are typically created with the sole purpose of participating in single tenders and are either immediately

[7]We define construction firms as described in the raw data by the *Ateco* code, which is the Italian version of the NACE, the European classification of the economic activities. Whenever we calculate within-industry variables—for example, when we estimate the TFP—we do so through the two-digits NACE code—e.g., in the "F—Construction" code, we employ the three subcategories "41 - Construction of buildings", "42 - Civil engineering" and "43 - Specialized construction activities".

dismantled if failing to win the auction, or persist until the contract expiration date. Through this information, we are better able to map firm-level info and public income data.

Descriptives Table (2) shows a selection of contract characteristics included in the OpenAnac 2008-2018 sample. We report summary statistics for the overall sample, as well as per sector. The sample comprises 1,262,208 contracts, for a total average yearly amount of €156 billion awarded to 139,965 unique firms. On average, contracts amount to €1.36 million and received on average 4.45 bids. Contracts lasted on average 475 days, and 19% of them are associated with a subcontract to other firms. However, most subcontracting took place for construction contracts, where the share of subcontracting is 59%. Most contracts in the overall sample (about 32%) are awarded through a negotiated procedure, where the contracting officer invites only some firms to present an offer. Open procedures are slightly more often used when awarding public works (21%).

Additional data sources The openly available dataset *Banca Dati Amministrazioni Pubbliche* (BDAP) allows us to retrieve one additional but crucial auction information that was not included in OpenAnac at the time we selected the PDF for the bid-extraction process (see next subsection for details). In particular, for the subset of tenders covered in BDAP—i.e., the public works contract between 2012 and 2017—we sourced data on the identity of all participants to the tender auctions and their tax code (but not their bids). In order to complement the contract-level data with further info on the bidding process, we rely on proprietary data. More specifically, we purchased from *Telemat*, i.e., the main private provider of this type of data, the list of all public works contracts auctioned off between 2012 and 2017 and their associated auction documents (in .PDF format).[8] For each tender, we merge Telemat information with Open Anac through the unique tender ID (*Codice Identificativo Gara* or CIG). Hence, we observe "redundant" information, such as the awarding procedure, the winning discount or total score, the reserve price, the identity of the winning firm (i.e., name and address), the expected contract duration, the contract type, a brief description of the contract object, and some additional information about the tender and the contracting authority.[9] Crucially, through Telemat data, we can link contracts to the auction outcome documentation. 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.[10] We extract this information, when available, 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.

II.3 Data cleaning and sample selection

The BDAP dataset plays an key role in this analysis, as it allows us to retrieve the identity of participants in open procedure tenders, i.e. auctions, and bridge OpenAnac to Telemat data. We focus on the 11,078 contracts available both in BDAP and Telemat and, for the subset of those with available documentation, i.e, 1,745 contracts, we extracted the bid distribution. We also drop the contracts when (i) they do not include the amount of the winning bid, or (ii)

[8] `https://www.telemat.it`.

[9] The two datasets are sourced in different ways: while Open Anac is based on information directly input by procurement officials, the Telemat data is gathered by employees based on published tender documents. In an additional exercise, we use the redundant information to benchmark our main data source and run a few sanity checks on the data quality, which yield positive results.

[10] About 16% of our contracts contains the bid distribution—with marked variation across regions and public administration types.

we cannot identify the winner. Therefore, our final working sample for this part comprehends 1,247 contracts. We refer the reader to Appendix C for the details on the PDF-documents extraction process. We merge the extracted bid data with contract-level (OpenAnac) and firm-level (CADS) data, 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 3: *Analysis* and *Quasi-universe* sample differences

A) Analysis sample vs. CADS construction Firms			
	CADS	Analysis	t-test
Age (Years)	18	18	0.390
# Workers	50	29	0.253
Revenues (€, 000)	19,682	10,947	0.547
Capital (€, 000)	13,382	1,129	0.420
TFP (ACF, 2015)	3	4	0.005
Value Added	4,763	1,685	0.161
Short-term Debt (€, 000)	9,222	1,637	0.410
Total Debt (€, 000)	13,940	8,789	0.540
# Awards	26	53	0.206
Public Demand (€, 000)	2,867	5,463	0.028
Contracting Workload	165,818	85,944	0.831
Share Public Demand	0.37	0.65	0.000
Share Direct Award	0.10	0.06	0.000
Observations	65,662	894	
B) Analysis sample vs. OpenAnac contracts			
	OpenAnac	Analysis	t-test
Avg Amount (€, 000)	608	1,394	0.005
# Bids	11.59	33.45	0.000
Duration (Days)	191.20	228.62	0.022
Subcontracting	0.58	0.83	0.000
Observations	182,354	1,247	

Notes: The Panel A reports the mean value, as well as the p-value for the conducted t-test, for different firm characteristics for the CADS dataset and the RDD sample. We compare the RD analysis sample of winners with the original sample of winners appearing in CADS, used for the first empirical part of this paper in Section III. The observation is at the firm-year level. Note also that for CADS, 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 only, as the RD sample includes only public works. We label firm in CADS as construction firms by means of the *Ateco* code—which is the Italian version of the NACE, the European classification of the economic activities—as construction firms.

In order to understand whether the resulting analysis sample is representative in terms of the firm and contract characteristics, Table (3) reports the means and the p-values for the t-test on differences with the CADS and *analysis* samples (Panel A) and the OpenAnac and *analysis* sample (Panel B). In Panel A), *# Awards* reports, by firm-year, the number of awarded contracts. Second, *Public Demand* accounts for the cumulative yearly amount of contracts awarded. Third, we construct a metric of contracting workload by looking at the daily amount of contracting workload. To do so, we divide the amount of each contract awarded by firm i by the days of duration N. We assume that the workload associated with a contract is uniformly split into the contractual days. By construction, looking at the date of the first award, the workload amounts to $amount/N$. Then, we sum up all daily workload associated with every active contract in the year t to obtain *Contracting Workload.* Fourth, by firm-year, we construct the variable *Share Public Demand* by dividing *Public Demand* by the firm's total revenues, i.e., coming from both procurement and non-procurement contracts. Finally, *Share Direct Award* is the share of contracts awarded through direct award over the total amount of contracts awarded by the firm in a given year.

We find that firms in the bid sample are similar to the overall population. Indeed, the difference in mean and distribution of only four variables—out of thirteen—is found to be statistically significant. The firms in the two samples appear to differ in productivity, public demand, the share of public demand, and the share of direct award - all of which tend to be higher in the analysis sample, except for the last one. In addition, by merging OpenAnac and CADS, we can improve the definition of construction firm from Table (1) by complementing the information provided by the Ateco code. Indeed, we add the construction label to all firms that *participate* in construction auctions, irrespective of their main activity as such via the Ateco code.[11] Panel B) focuses on contract characteristics. We find that contracts in the analysis sample tend to be bigger, longer, receive more bids, and are subcontracted more often. Notably, the differences between the two datasets are not statistically significant until 2015, so the differences in the average amount are due to 2016 and 2017 only. We test the robustness of our findings in Section IV.5 by excluding 2016 and 2017, and we will find that the estimates hold.

III Exploratory Analysis

This section explores the relationship between public demand—that we capture through the amount of public spending in contracts awarded to each firm—and survival odds for the universe of limited liability firms. We do so by first outlining a few, "static" facts on procurement vs. non-procurement firms, and then "dynamically" via a full-fledged survival analysis, in which we control for several firm-level observables. This analysis is exploratory in nature and provides a first empirical support to the answer to our main research question.

III.1 Stylized Facts on Procurement Firms

From the descriptive statistics in Table (1), Panel A), we know that procurement firms are i) a rather small subsample of the total—they amount to less than 4% of overall observations and ii) older and bigger in terms of turnover, capital, and employment. 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 I. Then, to gather evidence on the role of public demand on firm survival, we run a series of static exercises by looking at the *conditional* survival probability. More specifically, we regress an indicator function for firm exit two years ahead against an indicator variable for procurement firms (i.e., $\mathbb{I}\{PubWinner_{i,t}\}$), plus observables.[12] The resulting linear probability model reads:

$$\mathbb{I}(exit)_{i,t+2} = \alpha + \beta_1 \mathbb{I}\{PubWinner_{i,t}\} + \beta_2 \text{Revenues}_{i,t} + \beta_3 \Omega_{i,t} + \beta_4 \text{Age}_{i,t} + \beta_5 \#\ \text{Workers}_{i,t} + \zeta_t + \zeta_s + \zeta_a + \epsilon_{i,t}, \tag{1}$$

[11] This more precise definition of construction firms explain the different number of firm-year observation that we observe in Table (3) than in (1).

[12] Focusing on at least two-years lead to investigate firm-level implications of procurement contracts is necessary to avoid mechanical effects as the average contract duration in the population is 475 days from Table (2).

Table 4: Static effect of public demand on firm survival

						Proc	Non-proc
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
PubWinner	-0.026	-0.022	-0.026	-0.025	-0.021	.	.
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(.)	(.)
# Workers (000)	-0.004				0.001	-0.001	0.002
	(0.001)				(0.001)	(0.001)	(0.001)
Firm Age		-0.001			-0.001	-0.000	-0.001
		(0.000)			(0.000)	(0.000)	(0.000)
Value Added (€,000,000)			-0.045		-0.036	-0.011	-0.089
			(0.005)		(0.006)	(0.006)	(0.010)
Ω^{ACF}				-0.003	-0.002	0.001	-0.003
				(0.000)	(0.000)	(0.000)	(0.000)
Observations	4436632	4436603	4436632	4436632	4436603	211151	4225452
Mean Y (2-years)	0.05	0.05	0.05	0.05	0.05	0.26	0.51
Year FE	✓	✓	✓	✓	✓	✓	✓
Area FE	✓	✓	✓	✓	✓	✓	✓
Sector FE	✓	✓	✓	✓	✓	✓	✓

Notes:*Notes:* columns (1) to (5): results of equation (1) on the full sample. Columns (6) and (7): results of equation (1) on procurement and non-procurement firms, respectively. All models features year, area and sector fixed effects. Standard errors are clustered at the year-area-sector level.

where $\Omega_{i,t}$ is ACF productivity, ζ_t, ζ_s and ζ_a are year, sector and region fixed effects, respectively. β_1 is the parameter of interest and captures the gross effect of being awarded at least a contract at t on the probability of exiting the market, conditional on firm size, productivity, age, and all sector- and local-related characteristics captured by the battery of fixed effects.[13] In Table (4)—columns (1) to (5)—we report the results: in all models, β_1 is negative and strongly significant, meaning that procurement firms are much less likely to exit the market. Its gross effect amounts to around two p.p.—i.e., 40% of the overall death probability—and appears to be the main individual driver of survival for Italian firms.[14]

The table shows some other remarkable facts. First, the firm size—proxied with the number of employees—does not matter for survival once we account for the age, productivity, and value added (see columns 1 and 5). Second, firm productivity plays a minor role in explaining survival, even though associated with a parameter that is significantly different from zero. Most interestingly, the parameter sign is reversed if we look at the procurement (column 6) vs. non-procurement firms (column 7): the former appear to be *more likely* to exit the market whenever they are more productive and we also find no detectable association of survival prospects and total value added. In order to shed light on such puzzling results, we must move from a static correlation analysis to a dynamic survival-type set up.

[13] For sake of simplicity, the table does not report the parameters obtained including a number of additional firm-level characteristics like leverage, capital stock, investment level, etc. All results are available from the authors upon request.

[14] Even though interpreting linear probability parameters as marginal effects proves to be a challenging exercise for a number of reasons (e.g, Horrace and Oaxaca, 2006), our results indicate that, *ceteris paribus*, being awarded a public contract is associated to a boost in survival probability greater than that of increasing the value added by half a million €.

We emphasize that the results presented in Table 5 are largely descriptive and do not account for dynamic considerations and omitted factors that simultaneously affect survival and the level and dynamics of key firm characteristics. Indeed, consider an economy in which firms, at each time t, must: (i) observe their own productivity, the market structure, and the regulatory framework; (ii) decide whether to enter the procurement market (i.e., participate in the public auctions) or to the private market or both; (iii) after production and sale, decide whether or not to exit the market. Hence, at each time t there is a subset of firms self-selecting for participation in the public sector, an even smaller group of firms that *win* a contract (and actually work with the public sector), and the remaining firms that compete only in the private market. We can safely assume that firms are primarily interested in their own survival. Hence, they evaluate two elements in their decision to participate in the public procurement market. First and foremost, whether and to what extent winning a public contract increases the survival chances. Second, how intensive the potential competition in the auctions is, in order to evaluate the ex-ante probabilities of winning and adjust their survival prospects accordingly—i.e., the expected probability of surviving to $t + 1$ is simply the probability of surviving having won the contract, reweighted by the probability of winning.[15] We will further discuss the implications of omitting this information in Section IV.1.

Although brief, this description of firms' decisions to enter the procurement market highlights two key facts that inform our analysis in the following section. First, the decision to participate in the public market depends on and influences the dynamics of firm characteristics. In turn, it means that the "static" difference in survival prospects between procurement and non-procurement firms accrues over time, generating two pools of firms that differ along many dimensions as highlighted in Section II—e.g., procurement firms survive longer, and hence they are older, on average. Second, the drivers of survival in the public and private domain are possibly rather different—e.g., productivity might play a minor role once awarded a public contract. Among these drivers, a crucial element is the marginal boost to survival of winning a contract *per se*. In the next section, we examine the first element by moving from a static to a dynamic survival model while we examine the second element with an RD analysis causally in Section IV.

III.2 Survival Analysis

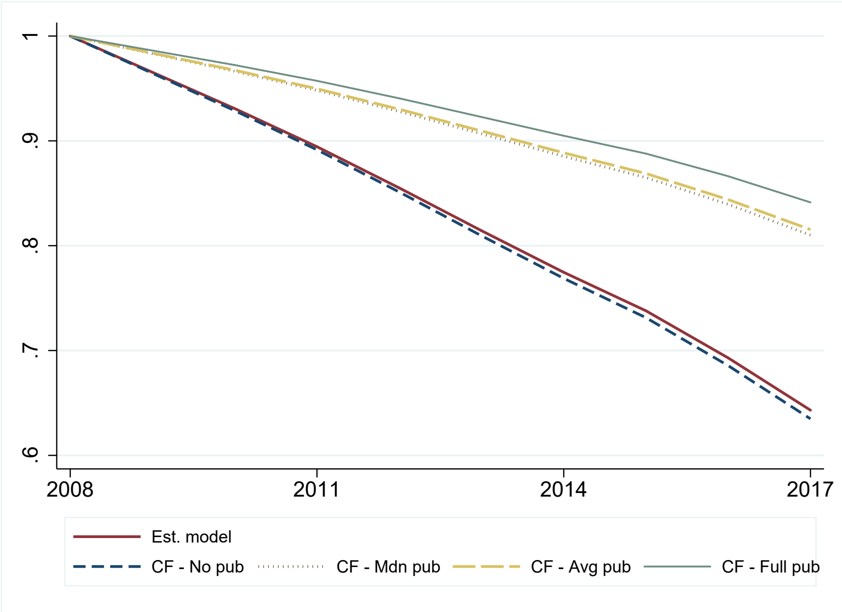
Figure (1) shows a graphical representation of the stylized fact just described. Over a 10-year period, it plots the survival odds for the average Italian firm– solid maroon line—alongside four "counterfactuals". First, we look at the same firm without public contracts—dashed blue—whose survival prospect appears to be very similar to the baseline. We then consider two additional counterfactuals with increasingly higher shares of public demand (i.e., with the median and average values of public demand in the data, respectively)—dotted and dashed yellow lines—as well as a "full procurement" firm—solid green. The latter counterfactuals confirm the direction indicated by the static figures of Table (4). When conditioning on revenues, size, productivity, and age, the main driver of survival is—by far—whether firms are awarded a public contract. In particular, the cumulative difference in the hazard rate is up to +30% (or 20 p.p.) at the end of the period.

In order to have a (gross) estimate of the public procurement dynamic effect on survival odds, we estimate a duration model whose hazard depends on the share of revenues coming from public procurement, plus firm characteristics related to their size, age, and productivity.[16]

[15] This may also include private information on competitors' productivity - which informs the assessment on the number of potential procurement firms - or connections with public authorities (see, e.g., Akcigit et al. (2018) for a model which takes into account such connections).

[16] We use an empirical approach that resembles the one proposed by Pavan et al. (2020).

Figure 1: Procurement and non-procurement firms survival



Notes: hazard rate of the estimated model - solid maroon - plus four counterfactual exercises: no public (dashed blue), median and average value of public revenue share (dashed and dotted yellow), and fully public (solid blue).

The observation is a firm-year pair and the "death" occurs whenever the firm exits the market. the regression model reads:

$$\lambda(t, X_i) = \exp\Big(\beta_0 + \beta_1 \mathbb{I}\{PubWinner_{i,t}\} + \beta_2 \text{SharePub}_{i,t} + \beta_3 \text{Revenues}_{i,t} + \beta_4 \Omega_{i,t} + \beta_5 \text{Age}_{i,t} + \beta_6 \#\ \text{Workers}_{i,t}\Big) \cdot \lambda_0(t), \tag{2}$$

where $\lambda_{0(t)}$ is the baseline hazard, i denotes the firm, t the year and, notably, all covariates are time-varying. More specifically, the parameters of interest are β_1 and β_2, which capture the effect of winning at least a public contract—irrespective of its size—and the share of public revenue on the total, respectively. In addition, we control for the size of the firm (proxied by both total revenues and number of workers), its age, and productivity.

Results In Table (5), we report the results of model (2). In column (1) we only control for procurement-related variables: the parameter for winning a contract is, as expected, strongly negative and significant—i.e., firms that start working for the public sector will very likely *not* exit the market in the short run. On the other hand, the share of public revenues pushes in the opposite direction, with a positive estimate. Controlling for proxies of firm size in column (2) has two major consequences: first, a decrease in the β_1, given that bigger firms are also more likely to win contracts; second, and most importantly, a reversal of β_2, which becomes negative and strongly significant, this signaling an effect at the intensive margin on top of the extensive. The results are qualitatively the same when we also control for productivity - column (3) - and firm age in column (4). We interpret this as evidence of unobserved variables that dynamically link firm size, age, productivity, and the choice to participate in the public procurement market. Among them, both unobservable factors like managerial practices and the long-term effects of the decisions to position the firm in the procurement market play arguably an important role. The magnitude of the estimated parameters reflects how crucial winning a public contract can be for firm survival. Indeed, we observe that—while the amount of total revenues reduces the hazard by a factor of 0.04 per million € for the average firm—being awarded a public contract is "the real deal", as it has an effect around 20 times stronger, given the estimated parameter of 0.75 p.p. every year, plus the effect of public demand on the revenue share. Such a huge effect explains the different survival rates observed for public and private firms (see Figure 1).

Table 5: Dynamic effect of public demand on firm survival

	(1)	(2)	(3)	(4)
Public Procurement				
$\mathbb{1}\{PubWinner_{i,t}\}$	-1.193	-0.872	-0.770	-0.740
	(0.019)	(0.020)	(0.020)	(0.020)
Share public	0.231	-0.256	-0.216	-0.227
	(0.043)	(0.045)	(0.047)	(0.047)
Firm Characteristics				
Total Revenues (€,000)		-0.071	-0.049	-0.044
		(0.001)	(0.001)	(0.001)
Workers (000)		0.010	0.009	0.009
		(0.003)	(0.002)	(0.002)
Ω^{ACF}			-0.001	-0.001
			(0.001)	(0.001)
Age				-0.006
				(0.000)
Observations	6,864,562	6,864,562	5,315,296	5,315,279

Notes : The table reports the hazard ratios from a Cox proportional hazard model à la Cox (1972). The observation is a firm. The controls for participation in the public procurement market are $\mathbb{1}$ {Pub_winner} and Share_pub, which control for (any) contract awarded and the share of public revenues on total annual revenues, respectively. Column (1) reports a plain model in which we only control for successful participation in public procurement. We add controls for firm characteristics which capture the effect of size - proxied by *Total Revenues* and *Number of Workers*, column (2) - productivity, as reflected in the productivity variable (column 3) and age of the firm - column (4), baseline. The robust standard errors are clustered at the firm level. The p-values of the coefficients are reported in parentheses.

IV Causal Analysis

The survival analysis is based on repeated snapshots of extensive firm-level information and provides a compelling starting point for studying the dynamic impact of public demand on firm survival. The key finding is a strong association between public revenues and survival, both at the extensive and intensive margin. This section aims to complement these findings with a causal identification of the effect of being awarded a public contract on survival.

IV.1 Identification Concerns

The results of the analyses proposed in SectionIII suffer from a few potential sources of endogeneity, all related to firms' decision to participate in the private and/or in the public market.

We list three examples: (i) 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. (ii) favoritism in winning contracts. Following the analysis in Akcigit et al. (2018), we know that connected firms are more likely to be awarded a contract, irrespective of their productivity, and according to the descriptive and exploratory evidence, survive longer. If the degree of firms' political connection evolves over time, omitting this information yields upward-biased estimates for β_1 in equation (2). (iii) participation decisions 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, and they may decide to engage in public auctions and to bid aggressively (see, e.g., Board, 2007 and Calveras et al., 2004). Such "bidding for resurrection" effect might downward-bias β_1.

We cannot rule out the above concerns—as well as a number of similar ones—in a non-experimental context unless assuming that participation decisions and procurement contracts were allocated randomly among firms, which would produce an unexpected mix of public demand shocks. In this ideal experimental scenario, we might simply confront the survival rates for procurement versus not procurement firms at both the extensive and intensive margins. A feasible alternative strategy involves tagging specific projects as unexpectedly assigned to winners exploiting the regulatory setting and the auction design. To this aim, we focus on the subset of contracts awarded via open auctions for which we observe both winning and losing bidders and the complete distribution of bids. The *analysis* sample of construction contracts[17] is relevant in terms of the overall Italian procurement. Throughout the 11-years period covered by our analysis, approximately 40% of procurement firms were awarded at least one construction contract, representing around 60% of the cumulative 2.48 trillion public procurement spending from OpenAnac. Equipped with bid-level data, we can compare, auction by auction, winners and losers *at the margin* (the runners-up and the third-ranked)—i.e., firms that had an equivalent ex-ante winning probability but ended up not being awarded the contract quasi-randomly. In order to quantify the effect of contract winning, we employ a RD analysis, whose main elements are tailored according to the Italian regulatory framework.

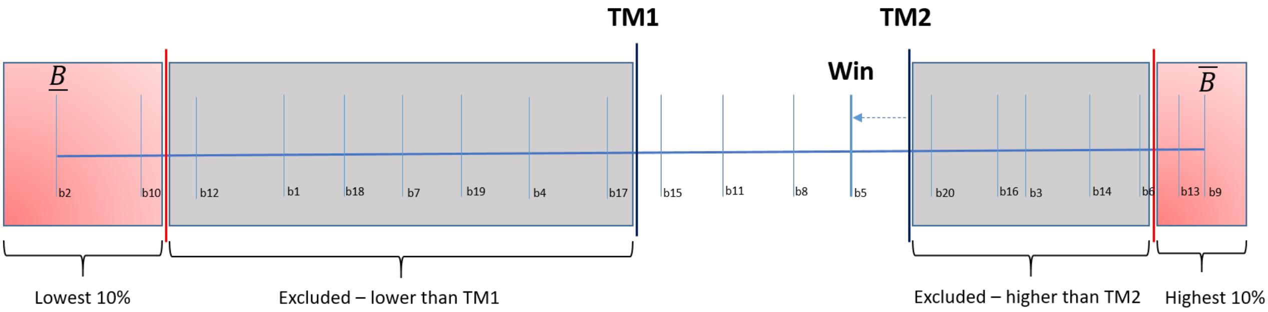
IV.2 Institutional background: Construction firms and Auction Mechanism

From 2012 to 2017, Italian contracting authorities were required to select procurement contractors through sealed bid auction contests (first-price auctions or FPAs), which in turn could feature the automatic exclusion of anomalous bids via an algorithm (average-bid auctions or ABAs) or award the lowest bid. 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. In addition to FPAs and ABAs, contracting authorities can use scoring rule auctions to select the most economically advantageous offer. According to an auction-specific rule, up to 100 points are assigned to weigh dimensions other than price. In the rest of this analysis, given that the firm obtaining the highest score awards the contract, scoring rule auctions are treated as FPAs.

The idea underlying the introduction of the ABAs is that, in the context of auctions with several offers and a high number of participants, some bids are "too good to be true"—i.e., can be associated with underbidding or poor quality bidders—and therefore contracting authorities would be better off by awarding more expensive bidders. The algorithm underlying the ABA

[17]See SectionII for a description of how the analysis sample is built and its main features.

Figure 2: Visual Representation of the ABA Mechanism



Notes: 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 ($\mp$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).

procedure essentially eliminates all offers above a mechanically calculated threshold close to the average bid and awards to the highest rebate in the interval. Figure (2) offers a visual representation of the ABA mechanism in a fictional 20-bid auction. In an ABA, the winner is determined as follows: (i) bids are ranked from the lowest to the highest discount (in the example, the highest and lowest two); (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 rebate strictly lower than TM2.[18] 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 European threshold.[19]

IV.3 Identification Strategy: RD with a "runner-up cutoff"

In order to ensure the identification of the public demand effect, we exploit the quasi-random allocation of a contract to firms in the vicinity of the winning bid. Conditioning on the auction mechanism in place, we compare the outcomes of winning and losing bidders, under the assumption that—except for the fact that the former have been awarded a public contract—the two groups are ex-ante identical (Cattaneo et al., 2020). To do that, we deviate from the classical RD setting with an exogenously imposed cutoff whose value informs the treatment status because, in our setting, every auction provides a different cutoff, depending on the value of the winning and runner-up bids. Despite that, and provided that there are no observable variables that influence the treatment probability,[20] units with values of the running variable at or just below the cutoff can be used as a control group for treated units with values just above to estimate the (local) treatment effects on the outcomes of interest.

The cutoff In a *sharp RD*, individuals with positive values of the running variable are treated, i.e., if $X_i > 0$. In typical settings, it is equivalent to say that the probability of treatment

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

[19] Note that ABA rules changed slightly in May 2017. The details of these changes are discussed in Section IV.5. Therefore, in Panel (6) of Table (7) we test whether our results are robust to the exclusion of 2017.

[20] This includes all endogenous firms' decisions aimed at sorting above the threshold.

$(Pr(D_i))$ is one whenever the forcing variable B_i exceeds some cutoff level $B^\star$—i.e., $Pr(D_i = 1|B_i > B^\star) = 1$, where $X_i = B_i - B^\star$. In the context of procurement auctions, though, there is no "fixed" cutoff like $B^\star$ to be used in the definition of the running variable for all contests pooled together, as long as the discount of the winning bids differs depending on the bid distribution, the contract amount, the local market conditions, and so on. Hence, we propose to use an auction-level cutoff ($B_a^\star$) with the same characteristics as the one above, namely:

$$Pr(D_{i,a} = 1|B_{i,a} > B_a^\star) = 1, \tag{3}$$

and change the definition of the running variable accordingly ($X_i = B_{i,a} - B_a^\star$).[21] We identify the auction-level cutoff by exploiting the institutional features presented in Section IV.2 and our bid-level dataset. More specifically, for each auction, we can rank the bids and pinpoint the winning, runner-up, and higher-order bids ($B_a^1, B_a^2, .., B_a^N$). Consider the case of FPAs: conditional on the observed bid distribution up to the runner-up's, any discount exceeding B_a^2 would have won the contest—in formulas:

$$Pr(D_{i,a} = 1|B_{i,a} > B_a^2) = 1, \tag{4}$$

and an immediate comparison between (3) and (4) reveals that a straightforward choice of cutoff is $B_a^\star = B_a^2$.

ABA is a peculiar form of lowest price auctions. In fact, once excluded the tails and computed the trimmed averages, all bids in the TM2-TM1 interval are treated as in a plain lowest price auction, and the winner is the one offering the largest discount (see Figure 2). 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 ($B_{a,TM}^1, B_{a,TM}^2, .., B_{a,TM}^N$) and define the cutoff as $B_a^\star = B_{a,TM}^2$. Note that, in defining such cutoff, we are implicitly modifying the definition in (3) to reflect the fact that a winning firm should overbid the runner-up discount, but not exceed TM2—in formulas, $Pr(D_{i,a} = 1|B_{i,a} > B_{a,TM}^2 \forall B_{i,a} < TM2) = 1$.[22] 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 excluded bid—in a robustness check, we define the cutoff using the nearest bid.

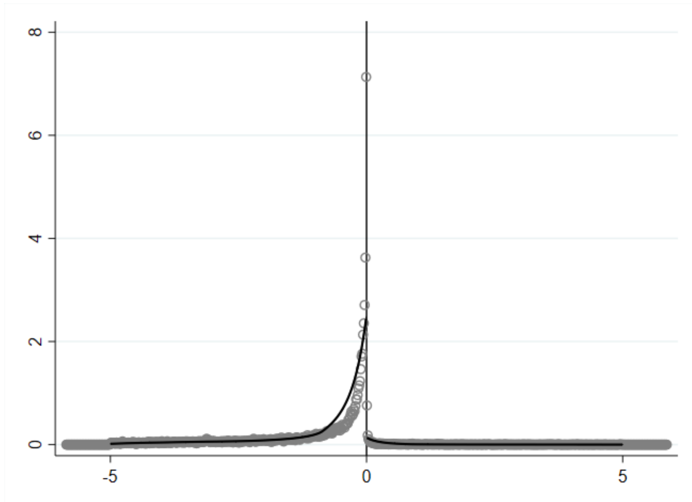
The running variable By construction, the running variable takes up the following values: $X_i = 0$ for the runner-up, $X_i > 0$ for the winners, and $X_i < 0$ for all losing bidders. For both ABAs and FPAs, the distribution of X_i has a mass point at zero, as shown in Figure (3). While usually a sign of manipulation in RD analyses, this mass point is not going to be worrisome for manipulation concerns in our setting, as we fix the cutoff *ex post*. We observe one point per auction (A) to the right of the cutoff representing the winning bids (i.e., positive scores), A points fixed at zero, and $N_l = \sum_{i=1}^{A} N_i$ to the left (i.e., negative scores), representing losing bidders other than the runner-up.

[21] Cattaneo et al. (2016) present a class of RD models with multiple cutoffs very near to the one that we propose, and discuss three common applications in the empirical literature—namely on running variables informed by vote shares, population, or test scores. In a recent contribution, Bertanha (2020) discusses the same class of models, identifying their shortcomings and proposing a generalized estimator for the ATE in such cases.

[22] We stress that our interest is in the ex post analysis of bid distribution, hence we can safely condition our analysis to 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.

The implementation In the spirit of Gugler et al. (2020), we argue that the comparison between the winner and the runner-up plus the third-ranked bidder provides a valid counterfactual to estimate the effect of winning an 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 much similar to the winners not only in terms of bid distance but also in terms of the underlying characteristics.In the Appendix D, we show that the firms above and below the cutoff are no longer similar if we keep a wider spectrum of bids;[23] nonetheless, the main estimation results hold even keeping the full sample. 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.

Figure 3: Distribution of X_i — Pooled Sample



Notes: Density of the running variable $X_i = B_{i,a} - B^{\star}_a$. $B^{\star}_a$ is fixed at the auction-specific runner-up bid value. On the right of the cutoff we report the winning bids and their distance from the runner-up bid. On the left of the cutoff, we report the losing bids (up to the third-closest to the winning bid) and their distance from the runner-up bid.

The model Once defined the cutoff and the running variable, we can perform a sharp-RD by pooling the auction-specific scores. The regression model reads

$$Y_{i,a} = \alpha + \tau D_{i,a} + f_l(B_{i,a} - c_a) + D_{i,a} f_r(B_{i,a} - c_a) + \epsilon_{i,(t),a}, \quad D_{i,a} = \mathbb{I}[B_{i,a} \geq c_a] \tag{5}$$

where $Y_{i,a}$ is the outcome of interest—e.g., in the baseline analysis it is an indicator for survival ahead of the award ($Surv^{t+m}_{i,t}$ and $m = [12, 24, 36, 48]$ months). More specifically, we look at the probability of being alive after 12, 24, 36 and 48 months of participating in the auction a—note that we suppress the t subscript because each auction is run in a specific point in time. $f_k(B_{i,a} - B^{\star}_a), \quad k \in \{l, r\}$ is a second-degree polynomial function, which we allow to vary on both sides of the cutoff. $B_{i,a}$ 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, and τ is the estimand treatment effect.

[23]Note that this is true not only in terms of discontinuities at the threshold but also when running a joint test on differences.

Testing the assumptions The first important assumption on which the RD relies is that agents cannot manipulate the assignment around the cutoff. Therefore, the main potential confounding factor to the causal interpretation of model (5) is the possibility that bidders change their score strategically and be assigned to their preferred treatment condition (McCrary, 2008). We believe that 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 the Italian procurement of construction context, which features, on average, 28 bids in competitive contests (see Table 3). A potential concern, though, is constituted by collusive behaviors by cartel members that may manipulate their bids—and their ranking—even in the proximity of the cutoff. In Section IV.5, we further discuss the ranking manipulation problem, and we provide suggestive evidence that the results do not suffer from the risk of collusion.

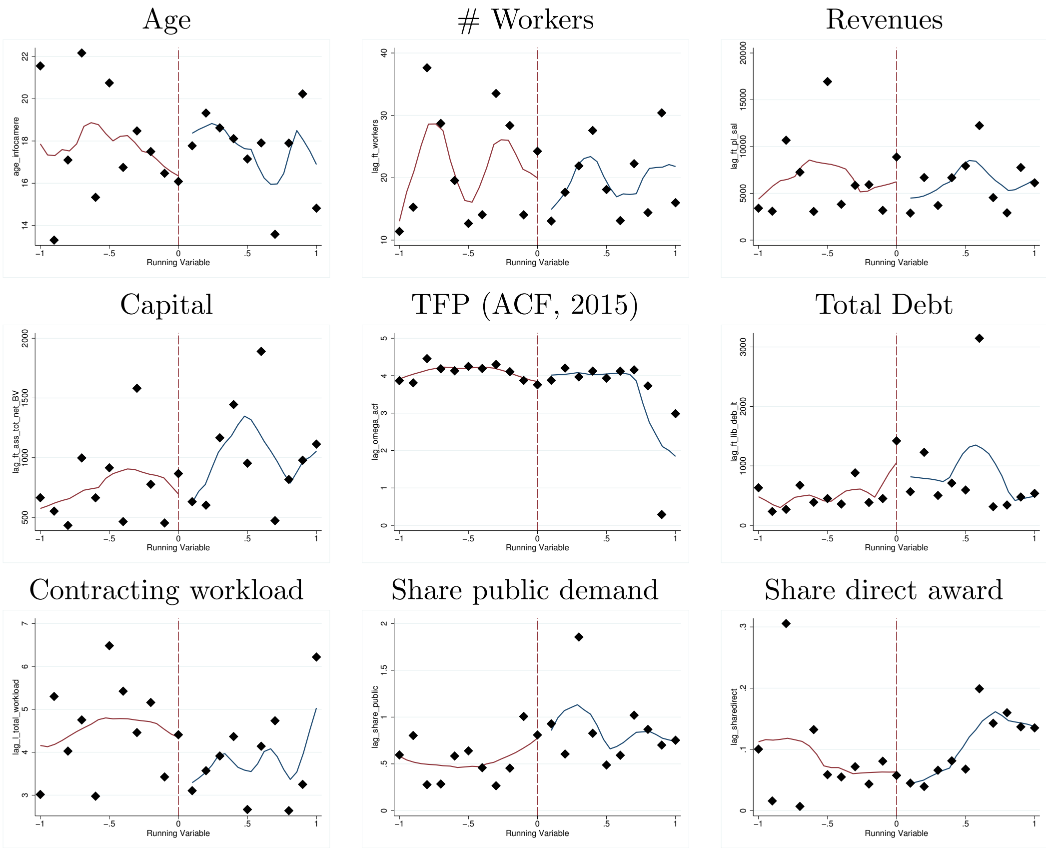
The second key element in a RD is the randomization assumption, namely that the regression functions $E\left[Y_i\left(0\right)|X_i = x\right]$ and $E\left[Y_i\left(1\right)|X_i = x\right]$ are continuous in x at $B^{\star}_{a}$. In other words, treated and untreated firms are supposed to be *ex ante* identical, differing only by treatment status, and in the absence of it, they would exhibit the same dynamics of outcome variables. In other words, any difference between the average response of treated and control units around the cutoff can be 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 (4)—complented by those in Figure (D1) in Appendix D— where we plot the mean values of several characteristics the year prior to the auction, in 0.1 p.p. bins.[24]

We test the continuity of firms' characteristics that correlate with the probability of winning: revenues, capital, total factor productivity, total debt, number of workers, and age. In addition, we build some measures that control for firms' behavior in public procurement. This exercise allows us to reduce the risk of capacity constraints, collusion, corruption, and firms' connection concerns biasing our results, as argued in Section IV.1. First, continuous firm features in the vicinity of the cutoff exclude the presence of shill bidders created by the cartel to manipulate the allocation, particularly in the case of ABAs. A shill bidder is a firm created only for this illegal purpose and tat closes down afterward, and it is hardly comparable with established "real" firms. Second, *contracting workload* is the number of active procurement contracts in the year prior to the award. It proxies the costs of participation in the auction, as those with a higher backlog might be less likely to participate in a given auction, all else equal (Kawai et al., 2020). Given that we find no sign of discontinuity at the cutoff, we can rule out the concerns on possible collusion. Third, *Share public demand* controls for firms that rely more on public procurement and therefore are more likely to win, either because of experience or because of political connections. Finally, firms politically connected or prone to corruption or in healthy relational contracts with buyers may be more likely to receive direct awards, i.e., contracts are awarded without open procedures. We capture this effect through the *Share direct award* variable. Overall, the plots confirm that there is no statistically significant difference between winners and losers at the cutoff on any of the measures considered—not even for the runners-up whose average value is summarized in the bin centered at $X_i = 0$.

As we consider several covariates, some discontinuities could be statistically significant (or close to) by chance. Therefore, we perform an auxiliary exercise following Lee and Lemieux (2010) and estimate a seemingly unrelated regression, where each regression represents one of the nine covariates considered above plus value-added and short-term debt. We regress a

[24]Note that—although we run the RD analysis keeping the distance within five p.p. from the cutoff—-we plot here such figures within one p.p. from the cutoff to show better visually what is happening around the threshold.

Figure 4: Firm Characteristics': Winners and Marginal Losers at $t-1$



Notes: This figure reports different plots for firm' characteristics—for bidders up to rank three—to validate the RD strategy and check for the continuity at the cutoff to show that firms are statistically similar prior the event. Observation are at the auction participant level and we restrict our attention to the winner, runner-up and the third-closest bid, as we later do in the empirical analysis. We report participants in auctions for which we were able to retrieve the placed bid, as described in text. Variable are at the year level. Points represent bin average of the covariate calculated at 0.1 p.p. bids.

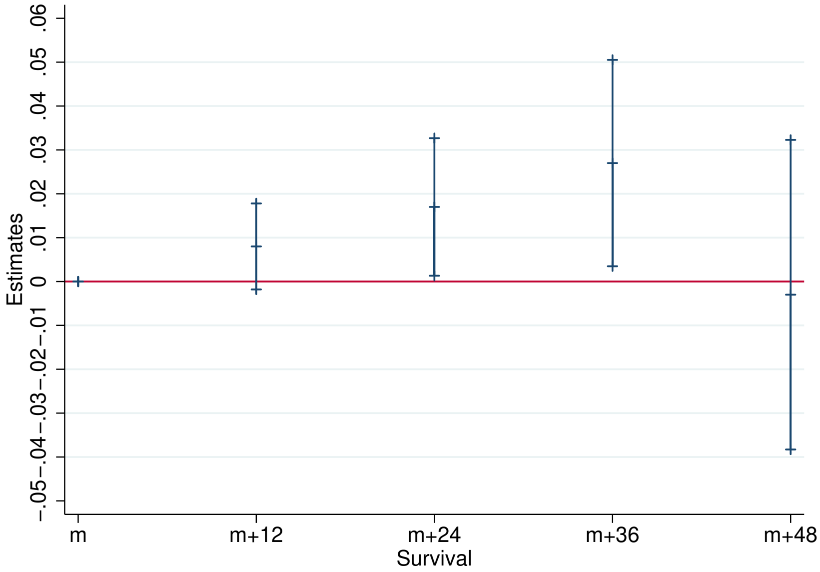
binary indicator equal to one if the observation is treated, i.e., if it lies above the threshold, on each of the above-reported covariates. We then perform a χ^2-test for the coefficients of such indicator being jointly equal to zero. We cannot reject the null, which provides further evidence of the observable characteristics being continuous at the cutoff. Altogether, our empirical design supports a causal interpretation of the RD results, which we present in the next subsection.

IV.4 Main Results

We begin by showing the visual relationship between the increase in the probability of survival and the contract award event. In Figure (5), we plot the estimated increase in the probability of staying in business 12, 24, 36, and 48 months ahead of the award. We observe an increasing effect over time until the third year, which tends to dissipate in the longer run. Part of the effect mechanically reflects the contract duration. However, as the median contract in the RD sample lasts approximately eight months—229 days, as shown in Table (3)— the boost to survival goes well beyond it. Hence, public awards positively affect the survival probability of the winning bidders in the medium run.

We report the baseline results on four survival outcomes in Table (6). The estimated parameters are local in the sense that we select winners and second-closest bidders (i.e., third-ranked) with bids that are no more than five p.p. of rebate on the reserve price away from the runner-up

Figure 5: RD Estimates of Firm Survival



Notes: We plot the coefficients and the standard errors of the baseline RD regression (Table 6, Panel 1) for the survival of the firm after m months from the participation in a specific auction a. The coefficients are bias-corrected and the computed standard errors are robust.

threshold. We show the results of our preferred model specification in the first panel:[25] the estimates show that a contract has a positive effect on the probability of surviving both 24 and 36 months after the award date. Survival increases by 1.7 and 2.7 p.p. from baseline values of 98.0 and 96.1 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 3.9% to 1.2%.[26]

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 (Panel 2) or an alternative non-parametric specification of the triangular kernel (i.e., Epanechnikov, Panel 3). In both cases, we obtain very similar results, both qualitatively and quantitatively. We also change the window of scores around the threshold kept in the analysis: In Panels 4 and 5, we restrict and extend the window by 4 p.p. on both sides of the cutoff, respectively. The more we zoom in 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 ±1-p.p. window, the results on +36 months increase in magnitude, indicating a stronger survival boost. When we expand the window to ±9 points around the cutoff, or when we do not impose any window in the running variable space, the estimates are quantitatively

[25]We employ a triangular kernel and a second-order degree polynomial. The chosen kernel gives more weight to observation close to the cutoff. Moreover, we opt for a second-order polynomial as it 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).

[26]We employ heteroskedasticity-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, 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 virtually unchanged results.

Table 6: RD Regressions: Baseline and Functional Form Robustness Checks

	Polynomial	Kernel	Discontinuity m+12	m+24	m+36	m+48
(1) Within 5 points	Quadratic	Triangular	**0.008**	**0.017**	**0.027**	**-0.003**
			(0.005)	(0.008)	(0.012)	(0.018)
			0.993	0.980	0.961	0.943
			2,445	*2,445*	*2,445*	*2,140*
(2) Within 5 points	Linear	Triangular	**0.007**	**0.015**	**0.024**	**-0.004**
			(0.005)	(0.008)	(0.012)	(0.017)
			0.993	0.980	0.961	0.943
			2,445	*2,445*	*2,445*	*2,140*
(3) Within 5 points	Quadratic	Epanechnikov	**0.008**	**0.016**	**0.026**	**-0.004**
			(0.005)	(0.008)	(0.012)	(0.018)
			0.993	0.980	0.961	0.943
			2,445	*2,445*	*2,445*	*2,140*
(4) Within 1 point	Quadratic	Triangular	**0.008**	**0.016**	**0.045**	**0.020**
			(0.003)	(0.009)	(0.012)	(0.020)
			0.994	0.984	0.970	0.954
			2,081	*2,081*	*2,081*	*1,849*
(5) Within 9 points	Quadratic	Triangular	**0.006**	**0.013**	**0.020**	**-0.007**
			(0.005)	(0.008)	(0.012)	(0.017)
			0.993	0.979	0.958	0.939
			2,594	*2,594*	*2,594*	*2,257*
(6) All points (optimal bandwidth)	Quadratic	Triangular	**0.007**	**0.017**	**0.030**	**0.004**
			(0.005)	(0.009)	(0.012)	(0.019)
			0.991	0.975	0.950	0.932
			2,792	*2,792*	*2,792*	*2,432*

Notes: The RD coefficients (first row of each panel, in bold) are bias-corrected, 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). "Within X point" reports whether we restrict the sample to X p.p. of rebate before and after the cutoff. Panel (1) reports the baseline model.

and qualitatively comparable. The inclusion of less comparable firms discussed in Section IV.3 seems not to affect our estimates but only their validity. We then keep our preferred window specification to five p.p. to maximize the trade-off between the locality of estimates and their validity.

IV.5 Robustness Checks

We show that the effect of public demand on firm survival is positive; in particular, winning a public contract lowers the exit rate significantly in the short- and medium-run—i.e., at least until one and a half years after the median contract expires —and dissipates after that. We have ruled out the possibility that the RD results are driven by functional form definition, but we must address a few concerns about the model specification and the identifying assumptions. In particular, there exists the possibility that the estimated parameters are driven by i) *manipulation* of bid ranking—it is the case for bid-rigging or cartels among bidders, and ii) *regulation features* on awarding procedures.

We begin by examining whether and to what extent the baseline results suffer from manipulation concerns. The RD identification strategy is based on the assumption that firms behave competitively; more specifically, we must assume that firms do not agree on manipulating their ranking strategically around the threshold because bidders' rank is key to selecting treated and control bidders.[27] The presence of cartels in our sample of auctions could be an issue depending on the interplay between a bid-rigging strategy and the award mechanism. 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 some empirical exercises to corroborate the validity of the identification assumptions. All of them suggest that our findings are robust against manipulation concerns.

The existence of a cartel is more likely guaranteed when "the cake is shared", that is, if all members are awarded a contract at some point in time. As a result, we would expect all cartel members to win at least one contract every year. In Panel b of Table (7), we repeat the 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, we employ the entire OpenAnac data to make this firm selection independent on the RD sample of contracts. By excluding the "winning losers", we exclude auctions potentially awarded to cartel members from the sample and keep bidders that participate in likely competitive contests only. By implementing this exercise, we also partially address the risk of contamination in the definition of the control group: Although ideally, we would like the runners-up and third-ranked bidders to be never awarded a contract, these have multiple awards opportunities over time. Hence, the longer the time frame ahead of the event, the higher the chances of being "treated" and contaminated. The results are stronger, more significant, and robust despite the halved sample size. Panel (3) discloses a similar message. When we instead of ruling out contaminated controls, we include as a covariate of the RD the number of awards in all the years preceding that of the survival outcome under analysis; we are controlling for the intensity of the contamination, but the results are virtually unaltered.

The second exercise that we propose is inspired by the results of Decarolis et al. (2016) and Chassang et al. (2020), considered jointly. The former discuss how the risk of collusive behavior in Italian public procurement auctions is particularly relevant for ABAs, as they provide strong incentives to manipulate the bid distribution. The paper discusses how bid coordination can be achieved either by a single firm controlling multiple bids or by different firms within a cartel. 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, which the authors refer to as "shills."[28] Alternatively, a bidder may also seek to coordinate with other companies to form a bidding ring and pilot TM2 (see Section IV.2): For the strategy to work, cartel members must participate in a sufficient number. In contrast, non-coordinating firms do not have incentives to participate jointly. Although we excluded auctions with an *ex-ante* higher risk of collusion, 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 (3): The medium-term effects are bigger in magnitude and more persistent despite the much-restricted sample, with strongly significant estimates up to m+48. According to the extensive collusion detection literature, a signal of bid-rigging in FPAs would be the variance of all bids (Abrantes-Metz et al., 2006), not necessarily around the threshold.[29] To corroborate these results on the FPA sample, we propose in Appendix A a frontier collusion detection tool based on Chassang et al. (2020): We do not find any evidence of collusion in the FPAs contained in the analysis sample. This is also understandable as long as cartel members had the possibility to participate in ABAs, where bid-rigging was easier.

[27]If the manipulation only occurs among losing bidders, though, this would not undermine the correct identification as clarified in equation (4).

[28]We rule out the risk of shilling by showing that winners and the closest losers have indistinguishable features at the cutoff, which would be unlikely for firms formed *ad hoc*.

[29]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.

In the fifth panel, we exclude the 2017 auctions from the sample. In fact, the rules of ABA changed slightly in 2017 and from May onward, before the buyer opens the sealed bids, a random draw among five criteria to assess an offer as anomalously low (i.e., the one in place beforehand plus four new approaches) have been introduced, and not all alternative criteria are coherent with our definition of local average treatment effect.[30] Hence, we test the sensitivity of the RD analysis to the exclusion of the entire calendar year.[31] Furthermore, as mentioned in Section II.3, we test the results by excluding both 2016 and 2017 since contracts in the analysis sample appear to be bigger than the population in these two years.[32] Finally, in Panel (6), we use an alternative definition of the runner-up for the ABAs. The ABA mechanism, irrespective of the TM1-TM2 interval definition, yields 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. We define the cutoff using the nearest bid instead of the baseline ABA's runner-up bid. This further specification does not show a different pattern in the results. All in all, the additional exercises confirm the robustness of the baseline findings against the major risks for the validity of the RD.

In an additional exercise, we test our model on alternative survival outcomes using the details of our survival data. Since we know the date of firm exit, if any, we can compute the interval in days between award and closure. The drawback of this result is that we can only focus on exiting firms, as we cannot calculate the number of days for firms still in the market at the end of the firm registry panel, i.e., December 31, 2020. The sample of firms and contracts decreases significantly: from 9037 to 1070 firms and 1226 to 725 contracts. Nevertheless, our RD estimates are very significant and strong: winning future exiters increase their average time on the market by 262 days compared to marginal losing future exiters.

[30]The full details are presented by Conley and Decarolis (2016).

[31]Note that 2017 is always excluded for the $Surv^{m+48}$ result because the data from CADS and Infocamere are only available up to December 2020. Accordingly, we can observe firm-level outcomes four years in advance only up to 2016.

[32]Results are available upon request.

Table 7: RD Regressions: Robustness Checks

	Discontinuity			
	m+12	m+24	m+36	m+48
(1) Baseline	**0.008**	**0.017**	**0.027**	**-0.003**
	(0.005)	(0.008)	(0.012)	(0.018)
	0.993	0.980	0.961	0.943
	2,445	*2,445*	*2,445*	*2,140*
(2) No contamination	**0.017**	**0.019**	**0.041**	**-0.002**
	(0.009)	(0.012)	(0.018)	(0.020)
	0.986	0.970	0.949	0.932
	1,206	*1,206*	*1,206*	*1,055*
(3) Controlling for contamination	**0.008**	**0.020**	**0.034**	**0.005**
	(0.005)	(0.008)	(0.012)	(0.018)
	0.993	0.980	0.961	0.943
	2,445	*2,445*	*2,445*	*2,140*
(4) Only FPA	**0.000**	**0.009**	**0.045**	**0.082**
	(0.000)	(0.008)	(0.031)	(0.036)
	0.993	0.980	0.961	0.943
	306	*306*	*306*	*246*
(5) Without 2017	**0.008**	**0.015**	**0.017**	**-0.003**
	(0.005)	(0.008)	(0.012)	(0.018)
	0.993	0.981	0.963	0.943
	2,140	*2,140*	*2,140*	*2,140*
(6) Alternative runner-up	**0.005**	**0.014**	**0.020**	**0.000**
	(0.005)	(0.008)	(0.011)	(0.014)
	0.991	0.976	0.953	0.935
	2,865	*2,865*	*2,865*	*2,498*

Notes: The coefficients (first row of each panel, in bold) are bias-corrected, the robust standard errors are in parentheses (second row). We report the mean of the corresponding dependent variable (third row), as well as the number of observations used for the estimation (forth row). The data is at the auction-bidder level. For all regressions, we use triangular kernel, second-order polynomial, and consider observations within 5 p.p. around the threshold. The Panel (1) report the result for the baseline—Table (6), Panel a.

V Discussion

The RD strategy proves to be appealing to identify a public demand shock to firms. Altogether, our results imply a strong, positive reaction of survival probabilities to public contract awards. In this section we discuss different mechanisms behind our results and their consistency with the general patterns described in Section III. In Table (8), we replicate the RD estimation on alternative outcomes in order to investigate the variables that are affected by public demand shocks and that possibly affect survival odds. Specifically, by linking performance and survival, we aim to check whether the selection process bolstered by public demand has any dynamic effect on procurement firms' performance.

Productivity We start with the analysis of TFP, as it is both a strong predictor for survival, and as indicator of future business performance. The first panel of Table (8) reports the estimated parameters for TFP 1, 2, 3, and 4 years after the award. Indeed, unlike for survival, we only observe the productivity once a year since we measure it from balance sheet data. Essentially, we are comparing the future values of TFP of firms treated at t with their control counterpart; crucially, we can only do that for firms in the market, hence the estimated difference takes into account both the evolution of TFP and the survival rate in both groups. Results suggest that there is no detectable dynamic effect of public demand on TPF; if any, the only slightly significant parameter (Ω^{ACF}_{t+3}) is negative.

Table 8: RD Regressions: Additional Outcomes

	Ω^{ACF}_{t+1}	Ω^{ACF}_{t+2}	Ω^{ACF}_{t+3}	Ω^{ACF}_{t+4}
(1) Productivity ACF	**-0.338**	**-0.449**	**-0.323**	**-0.199**
	(0.240)	(0.267)	(0.235)	(0.300)
	3.782	3.743	3.723	3.740
	2,047	*1,701*	*1,393*	*1,066*
	$\left(\frac{Y_{t+1}}{L_{t+1}}\right)$	$\left(\frac{Y_{t+2}}{L_{t+2}}\right)$	$\left(\frac{Y_{t+3}}{L_{t+3}}\right)$	$\left(\frac{Y_{t+4}}{L_{t+4}}\right)$
(2) Labor Productivity	**-16.877**	**-6.826**	**2.359**	**-7.210**
	(21.121)	(4.654)	(6.084)	(7.373)
	61.048	60.510	60.688	61.930
	2,091	*1,766*	*1,444*	*1,105*
	t+1	t+2	t+3	t+4
(3) Share of public demand	**0.002**	**0.104**	**0.073**	**0.097**
	(0.039)	(0.043)	(0.048)	(0.058)
	0.551	0.540	0.525	0.567
	1,559	*1,286*	*1,058*	*810*
	t+1	t+2	t+3	t+4
(4) Share of direct awards	**-0.020**	**-0.019**	**-0.016**	**-0.077**
	(0.015)	(0.018)	(0.022)	(0.020)
	0.073	0.092	0.124	0.139
	1,831	*1,749*	*1,647*	*1,361*

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). The data is at the bid level. For all regressions, we use triangular kernel, second-order polynomial, and consider observations within 5 p.p. around the cutoff. TFP is estimated à la Ackerberg et al. (2015). t stands for the year in which the auction was run.

The RD parameter only partially reflects the dynamics of productivity following a public demand shock. In fact, we have shown that treated and control groups face, because of the treatment, rather different survival prospects. Hence, the estimation on the difference in lead variables captures both the dynamics per group, and the difference in survival rates that influences within-group averages through selection. We further investigate these two channels by breaking down the change in the estimated treatment effect on productivity, that is

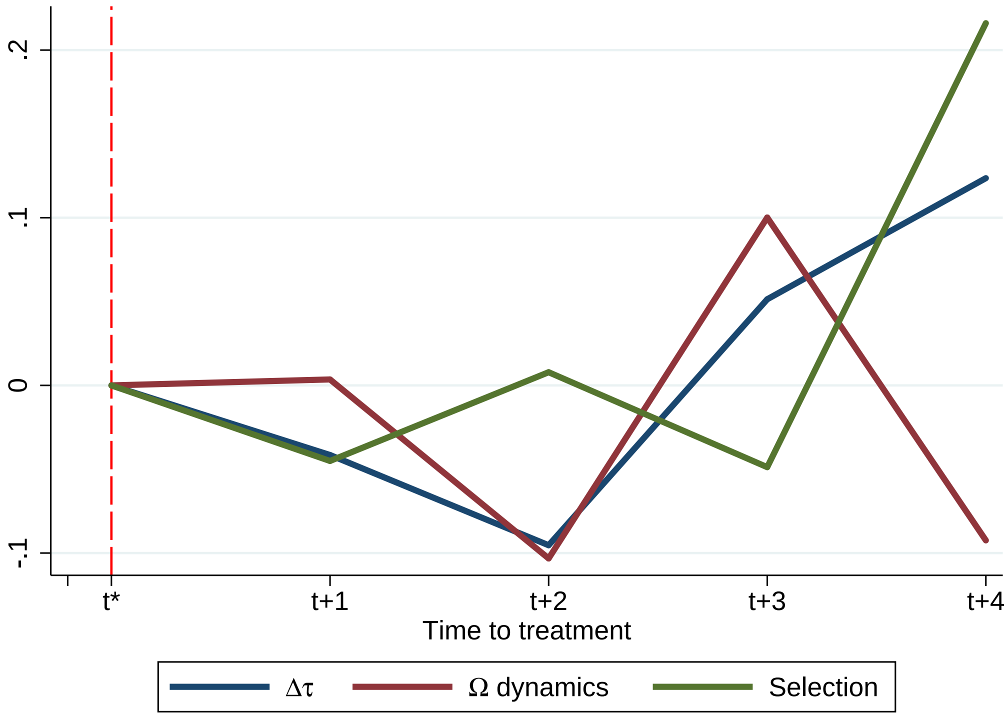
$$\Delta\tau_t = \tau_{t+1} - \tau_t = \left[\mathbb{E}(\Omega^T_{t+1}) - \mathbb{E}(\Omega^C_{t+1})\right] - \left[\mathbb{E}(\Omega^T_t) - \mathbb{E}(\Omega^C_t)\right], \tag{6}$$

in two components: one reflecting the actual differentials in TFP, the other accounting for the selection of survivors within groups. More specifically, the empirical counterpart of equation (6) reads:

$$\Delta\tau_t = \left[\left(\frac{\sum_{i=1}^{N^{S,T}_{t+1}} \Omega_{i,t+1}}{N^{S,T}_{t+1}} - \frac{\sum_{i=1}^{N^{S,C}_{t+1}} \Omega_{i,t+1}}{N^{S,C}_{t+1}}\right) - \left(\frac{\sum_{i=1}^{N^{S,T}_{t+1}} \Omega_{i,t} + \sum_{j=1}^{N^{E,T}_t} \Omega_{j,t}}{N^{S,T}_{t+1} + N^{E,T}_t} - \frac{\sum_{j=1}^{N^{S,C}_{t+1}} \Omega_{j,t+1} + \sum_{j=1}^{N^{E,C}_t} \Omega_{j,t}}{N^{S,C}_{t+1} + N^{E,C}_t}\right)\right], \tag{7}$$

where T and C denote treatment and control group, respectively, and we exploit the fact that, for each group G, the number of firms at time t amounts to the sum of firms present in

Figure 6: $\Delta\tau$ decomposition



Notes: $\Delta\tau$ (blue line) and its components, as specified in equation (8). Ω *dynamics* (maroon line) reflects the changes in productivity between—survived—treated and control firms, *selection* (green line, negative) captures the difference in productivity between treated and control exiters. $t^\star$ is the contract awarding year.

the following period plus the exiters ($N_t^G = N_{t+1}^{S,G} + N_t^{E,G}$). By rearranging, we obtain[33]

$$\Delta\tau_t = \underbrace{\left(\bar{Z}_{t+1}^{S,T} - \bar{Z}_t^{S,T} w_t^{S,T}\right) - \left(\bar{Z}_{t+1}^{S,C} - \bar{Z}_t^{S,C} w_t^{S,C}\right)}_{\Omega\, dynamics} - \underbrace{\left(\bar{Z}_t^{E,T} w_t^{E,T} - \bar{Z}_t^{E,C} w_t^{E,C}\right)}_{selection} \tag{8}$$

In Figure (6) we plot $\Delta\tau$ and its components. The figure highlights two facts: first, in line with the RD estimates, the variation in τ is rather limited; second, the dynamics of $\Delta\tau$ is initially driven by Ω, while the *selection* channel's importance grows with time—on average, it is the main driver accounting for the 85% of the variation in $\Delta\tau$.

The results are replicated using labor productivity, defined as the total sales divided by the average number of workers (i.e., average output per worker).[34] Irrespective of the definition, lead productivity does not appear to be affected by demand shocks. This finding introduces the much broader issue of resource misallocation in government contracting. In Appendix B, we also provide a regression-based exercise to corroborate this specific finding. The high-dimensional fixed-effect model exploits the wealth of our data in the spirit of the Section III.2 to see whether

[33]For expositional convenience, we define $\bar{Z}_t^{V,G} = \frac{\sum_{i=1}^{N_t^{V,G}} \Omega_{i,t}}{N_t^{V,G}}$ as the average productivity across groups, and $w_t^{S,G} = \frac{N_{t+1}^{S,G}}{N_{t+1}^{S,G}+N_t^{E,-G}}$ is the relative weight of stayers and exiters, for $V \in [S,E]$ (stayers and exiters, respectively) and $G \in [T,C]$.

[34]This is the productivity measure employed by, e.g., Akcigit et al. (2018).

the association of TFP dynamics and public demand metrics—the latter defined in multiple ways—is confirmed in the full sample of firms and contracts. Findings are correlational in nature but draw a very similar picture: Public procurement helps firms stay in the market longer, but conditional on survival, does not necessarily support the most efficient firms with growth potential.

Public market participation There are several explanations for why firms with sluggish productivity are necessarily forced to exit. Winning a public contract may have medium- and long-term consequences on business models. It is the case for firms that, once awarded a contract, e.g. establish relational contacts with public administrations and steadily increase their participation in the public market (Calzolari and Spagnolo, 2009). On the other side of the spectrum there might be firms that, once awarded the contract, exploit the easier access to credit to invest and compete in the private market - i.e., they *decrease* their participation. We explore here the former mechanism.

Government contracting may support inefficient firms and protect them from competition in the private market (Akcigit et al., 2018). This is true if productivity dynamics are truly correlated with future firm efficiency, especially in the private market. At a first glimpse, these results might suggest that firms survive longer because they become more dependent on public contracts. The richness of the OpenAnac data allows us to test whether this is the case. To explore the subsequent behavior in the latter market, we use future performance metric in procurement as alternative firm outcomes. We test the above effects by looking at the evolution of two variables after the "event": first, the share of public demand, which captures the engagement with the public sector and how important it is in running the business; second, the share of direct awards received, which reflects the intensity of relational contracting with the public sector. In this setting the contamination with other awards is of particular concern as the firm outcomes measure the future performance in public procurement. To control for this dynamic contamination, following the robustness check "Controlling for contamination" presented in Table (7), we add as covariates to the RD indicator(s) of award counts in the year(s) prior to $t + k$. For instance, adding this control allows us to estimate the performance of winners at $t + 2$ in the auction a with respect to marginal losers with similar performance in the procurement market other than the auction a at t and $t + 1$. Panels (3) and (4) of Table (8) report these RD estimates: the importance of the public revenues become important to the firm business until $t + 3$ while we do not find any significant difference between the treated and control firms in terms of direct awards besides $t + 4$ where the coefficient enters with a negative sign. All in all, in order to stay in the market, the winner seems to be more dependent on public contracting, although there is no evidence on the establishment of relation contracts with buyer down the road.

VI Conclusions

Governments have a whole arsenal of powerful demand-side solutions to intervene in economic processes. Through various fiscal measures, they can support the development of local firms, sectors, or regions. Several empirical and theoretical contributions on fiscal policy have shown the effectiveness of subsidies (Cerqua and Pellegrini, 2014; Criscuolo et al., 2019; Cingano et al., 2021) and place-based policies (Becker et al., 2010; Kline and Moretti, 2014). Although these public interventions are widespread, they are directly designed to have a lasting impact on the economy. This paper contributes to the literature that shows that the government can also instrumentally make use of its purchasing power to intervene in the economy indirectly and significantly affect firm performance. We focus primarily on the survival rate—an aspect not addressed in the literature, which instead focuses primarily on business performance *conditioning* on survival.

To explain these results, we examine the impact of public demand on other firm dynamics by replicating our RD for different performance measures. The TFP is unaffected by demand shocks (and, if anything, shows a negative effect). This result suggests that public procurement helps firms stay in the market longer, but it does not necessarily support the most efficient firms with growth potential. To understand why inefficient firms in the market are not necessarily forced to exit, we rely on the evidence showing that public contracting revenues protect them from competing with more efficient firms in the private market (Akcigit et al., 2018). Our findings confirm that "procurement firms" survive also because they become more dependent on government contracts: the future size of orders and the performance of firms in the public market improve.

The results of this study support the notion that government contracting is an effective industrial policy measure to enhance firm performance and suggest that the government does not necessarily intervene in the economy to address a market failure but rather uses contracts as an indirect tool to pursue other goals. Is this policy intentional or unintentional, however? The fact that governments have traditionally pursued restrictive purchasing policies to procure domestically and favor local actors explicitly may suggest that the former is more plausible. At the same time, however, the government could choose to target those firms that make this policy more rewarding, which does not appear to be the case based on our findings and in light of the existing literature. Future research could shed more light on this line of inquiry.

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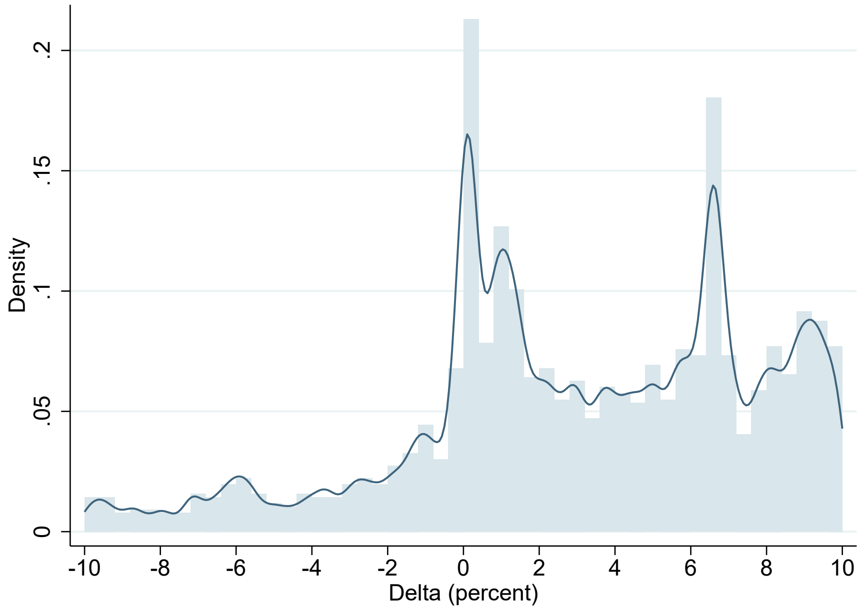
A Appendix: Is There Evidence of Collusion in our FPAs?

Figure (A1) replicates the visual test for collusion proposed by Chassang et al. (2020). Whenever a cartel is participating 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 ideas is that colluding firms rig the planned-to-be-losing bids, but they do so far away from the designated winning bid. This creates a suspicious drop in the density of the bid-to-bid distance around the 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.[35] A fair and competitive auction will show increasing bid density as this difference approaches zero, while a corrupted auction will exhibit missing mass near $\Delta = 0$. Unlike the results from Chassang et al. (2020) on Japanese construction auctions, we observe no missing bids near $\Delta = 0$—suggesting that the behavior in our sample of FPAs is not the same as in collusive 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 that are greater than 90% 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. In our data, the distribution has heavier tails and we believe this has two implications. First, there would be less of an 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 the cartel enforce a "no bid mass near zero" rule, since the incentive to deviate is lower. Figure (A2) further examines the density falloff with a window three times larger than Chassang et al. (2020)'s.

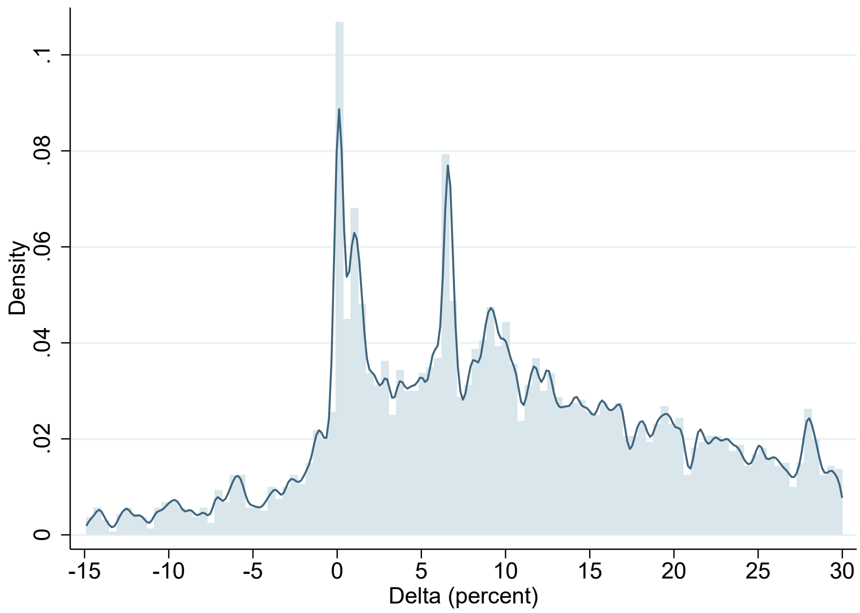
[35] For the winning bid, the difference is from the second-lowest, this creating negative values of Δ'.

Figure A1: The distribution of bid difference Δ from the winning bid.



Notes: This figure reports the distribution of bid difference Δ from the winning bid. According to Chassang et al. (2020), the density at $\Delta = 0$ indicates that collusion is likely not present in these auctions. Density estimation parameters match those of the original paper, with a smoothing width of 0.75%.

Figure A2: Figure (A1) reproduced with a broader window.



Notes: This figure reports the distribution of bid difference Δ from the winning bid. According to Chassang et al. (2020), the density at $\Delta = 0$ indicates that collusion is likely not present in these auctions. Density estimation parameters match those of the original paper, with a smoothing width of 0.75%. Note that the distribution, even at this relatively wide observation window, does not rapidly taper.

B Appendix: Exploratory Productivity Analysis

We pool the panel data of limited companies merged with the universe of available firm-government matches to study how firms see their future productivity affected when exposed to public demand. This exercise complements the RD analysis on productivity outcomes in Section V. In this Appendix, we explore the same relationship by executing high-dimensional fixed-effects linear regressions at the firm-year level based on the following equation

$$\Omega_{i,j}^{t+k} = \alpha + \beta \; PubD_{i,t} + \gamma \; PrivD_{i,t} + \kappa_i + \lambda_{t,j} + \epsilon_{i,t,j}, \tag{9}$$

where $\Omega_{i,j}^{t+k}$ is the productivity of firm i in industry j in year $k = [1, 2]$ after receiving procurement revenues at t.[36] The regressor $PubD_{i,t}$ refers to a general index of public demand, for which we consider four alternative versions. First, for the extensive margin, a dummy variable *Awardee* indicates that the firm obtained at least one public project at t. Second, for the intensive margin of the award dimension, *# Awards* indicates the number of public contracts received. Third, *Public Revenues* report the sum of the values of all government contracts won by the firm. Finally, *Public/Total Revenues* stands for the ratio of *Public Revenues* over total revenues. All intensive margin variables measuring procurement value are sparse, with many zeros for those firms without procurement contracts. We are interested in the estimation of the parameter β, that is the effect of each of the public demand specifications on firm lead productivity, separately.

Different layers of endogeneity affect the relationship between firm productivity and public demand, as discussed in Section IV. This exploratory exercise, as the survival analysis in Section III, can isolate some of them in a large sample of firms-procurement matches. First, we start with including the firm's private market exposition and, accordingly, $PrivD_{i,t}$ represents the cumulated private sales for a firm i in the year t. The variable is computed by subtracting *Public Revenues* from total revenues. Lacking to control for the private source of demand in this setting yields serious omitted variable bias as private sales are correlated with both the firm's productivity and the public counterpart as private contracts are more likely assigned to the most productive firms. If this is the case, firms who receive positive (negative) productivity shocks, everything else equal, will have a better (worse) chance of getting private revenues. This argument introduces bias in our model sourced by the mutual crowd-out effect of one market from the other, mostly due to the firm's capacity constraints in the short-run (i.e., one year), implying that whether and how much firm sells to the private and public sector are not orthogonal decisions. These mechanics would lead us to underestimate (overestimate) the effect of public demand on productivity.

Second, we are interested in studying the implication for the same firm when differently exposed to public sales besides controlling for the firm's private demand and the crowd-out mechanics. The richness of our data allows us to account for time-invariant heterogeneity among firms and the mix of intensive and extensive margin shifts in the public procurement revenues within each firm over time by using firm-fixed effects κ_i, which also nest all unobserved sub-industry- and geographical-specific effects. [37] Third, we consider that government spending can have differential impacts on firms across sub-industries, depending on the business cycle's stance. We can address this concern in our micro-founded analysis by employing industry-year-

[36]We use labor productivity in this exercise as the fixed-effect model explains $> .99$ of ACF productivity leading to overfitting and misleading coefficients.

[37]Despite age being a relevant driver of firm survival, the firm-fixed-effect model makes firm age collinear with the fixed effects as age mechanically increases by one every year for every firm.

fixed effects $\lambda_{t,j}$. Including a fixed effect for each industry-period in the sample captures 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. Holding the industry fixed also allows a neater interpretation of productivity, which is computed starting with the firm's industry affiliation.

Table B1: Lead Productivity on Public Demand

	Ω_i^{t+1}				Ω_i^{t+2}			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Awardee	-45.0				24.8			
	(36.4)				(57.4)			
# Awards		-0.65				-0.64		
		(0.59)				(0.80)		
Public Revenues (000,000)			-27.6				-218.9	
			(212.1)				(314.2)	
Public Revenues (000,000, Squared)			2.96*				0.60	
			(1.46)				(1.68)	
Public/ Total Revenues				-50.5				20.6
				(39.7)				(81.8)
Private Revenues (000,000)	37.7	37.1	91.4		10.5	9.25	-88.8	
	(34.2)	(33.7)	(115.0)		(35.0)	(34.7)	(157.4)	
Private Revenues (000,000, Squared)	0.32	0.31	-1.38		0.026	0.021	1.15	
	(0.45)	(0.44)	(1.56)		(0.37)	(0.36)	(2.22)	
# Firms-Years	4117089	4117089	4117089	4085517	3406172	3406172	3406172	3376615
Adj R-Sq	0.10	0.10	0.10	0.10	0.12	0.12	0.12	0.12
Mean Y	99.32	99.32	99.32	99.61	106.94	106.94	106.94	107.46
S.D. Y	33831.53	33831.53	33831.53	33960.87	37082.32	37082.32	37082.32	37243.58

* $p < .1$, ** $p < .05$, *** $p < .01$

Notes: OLS regressions. The observation is a firm-year. The regressand is the firm's labor producitity one year lead (columns 1-4) or two years lead (columns 5-8). Our sample starts in 2007. Given we have procurement and firm-level information until 2019, observations in 2018 and 2017 are dropped from the analysis to assess the actual firm's productivty in the year t+1 and t+2, respectively. *Awardee* indicates that the firm win at least a public contract in the year t. *# Awards* reports the firm's total number of public awards in the year t. *Public Revenues* indicates the total value of public contracts awarded by a firm in the year t, if any. *Private Revenues* indicates the total source of revenues raised in the private market in the year t. All specifications include firm and industry-year fixed effects. Standard errors are clustered at the firm and year level.

We report the estimates of equation (9) for each specification of $PubD_{i,t}$ in Table (B1). The unit of observation across all specifications is the firm-year pair. Every model includes firm- and industry-time fixed effects. Private revenues (and their squared value, to explore possible non-linearities) are in levels and included in every specification. Columns (1) to (4) refer to productivity one year lead, while columns (5) to (8) report the productivity two years lead. The pooled sample includes 4,085,517 or 3,376,615 observations (and 664,696 and 575,406 firms) depending on the lead of the dependent variable for nine years. Since procurement firms could

Table B2: Lead Productivity on Public Demand - No Contamination

	Ω_i^{t+1}				Ω_i^{t+2}			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Awardee	-47.6 (38.8)				149.9 (118.9)			
# Awards		-6.32 (7.78)				62.7 (51.1)		
Public Revenues (000,000)			219.8 (180.3)				-36.9 (409.6)	
Public Revenues (000,000, Squared)			2.25 (4.33)				-5.92 (8.66)	
Public/ Total Revenues				-68.7 (56.3)				302.4 (289.9)
Private Revenues (000,000)	30.7 (72.6)	31.7 (72.5)	155.9 (184.3)		-53.6 (98.2)	-55.1 (98.5)	-168.5 (372.4)	
Private Revenues (000,000, Squared)	0.41 (0.94)	0.41 (0.94)	-3.08 (4.13)		-0.64 (1.23)	-0.65 (1.24)	4.20 (8.93)	
# Firms-Years	3890226	3890226	3890226	3870720	3128801	3128801	3128801	3112333
Adj R-Sq	0.09	0.09	0.09	0.09	0.11	0.11	0.11	0.11
Mean Y	98.00	98.00	98.00	98.38	105.95	105.95	105.95	106.46
S.D. Y	34752.97	34752.97	34752.97	34839.71	38630.28	38630.28	38630.28	38731.68

* $p < .1$, ** $p < .05$, *** $p < .01$

Notes: We replicate the linear regressions from Table (B1) excluding those firms awarding at least one contract at t+1 (columns 1 to 4) and t+2 also (columns 5 to 8).

participate in and win multiple auctions over time and within the year, these firms typically appear more than once in the procurement database and have different incentives to participate in procurement auctions. Moreover, all firms in the market may have different time-specific incentives to bid in procurement tenders during economic downturns or expansions. We two-way cluster the standard errors at the firm and year level to account for this and other serial correlations within and across firms.

With this setup, columns (1) and (5) report the extensive margin analysis coefficients. Being a procurement firm (versus not awarding any public contract in the year t) is not associated in a statistically significant way with future productivity. The effect flips from negative to positive, and their magnitude is negligible compared to the standard deviation of the regressands. In columns (2) and (6), we explore the intensive margin effect of awarding one additional public contract. The impact on productivity in the lead one and two years is small and not statistically significant. Then, we explore the intensive margin effect of public sources of revenues in columns (3) and (7). Private revenues have no significant impact on productivity. The same applies to public demand, although the sign of the effect is consistently negative: €1 of revenues from public contracts seems to have different and opposite implications on future firm productivity than €1 of sales to private clients. In columns (4) and (8), we regress survival on public sales normalized by total yearly revenues. We exclude private revenues from this specification to avoid multicollinearity. Again, public revenues do not boost a firm's productivity down the road. The major takeaways from this analysis are that public sales are not associated in a meaningful way with productivity for a firm staying in business, respectively, in one and two years lead.

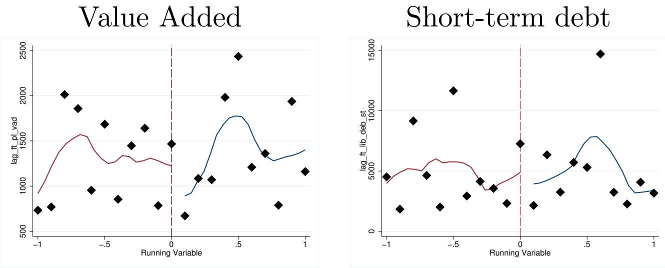
To exclude confounding future drivers of firm productivity due to demand at t, in Table (B2), we perform the same regressions of Table (B1) and exclude those firms awarding at least one public contract at t+1 (for lead one productivity) and also t+2 (for lead two productivity). This selection shrinks the sample to 649,419 firms and 3,870,720 firms-year (columns 1 to 4) and 550,901 firms and 3,128,801 firms-year (columns 5 to 8). In this fashion, the comparison between procurement and non-procurement firms is less contaminated by future events and can be more directly associated with events occurring at t—in the same spirit of the contamination check in Section IV.5. This analysis displays qualitatively similar results and highlights again a non-effect of public demand for firm-level productivity whatsoever.

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 e 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).

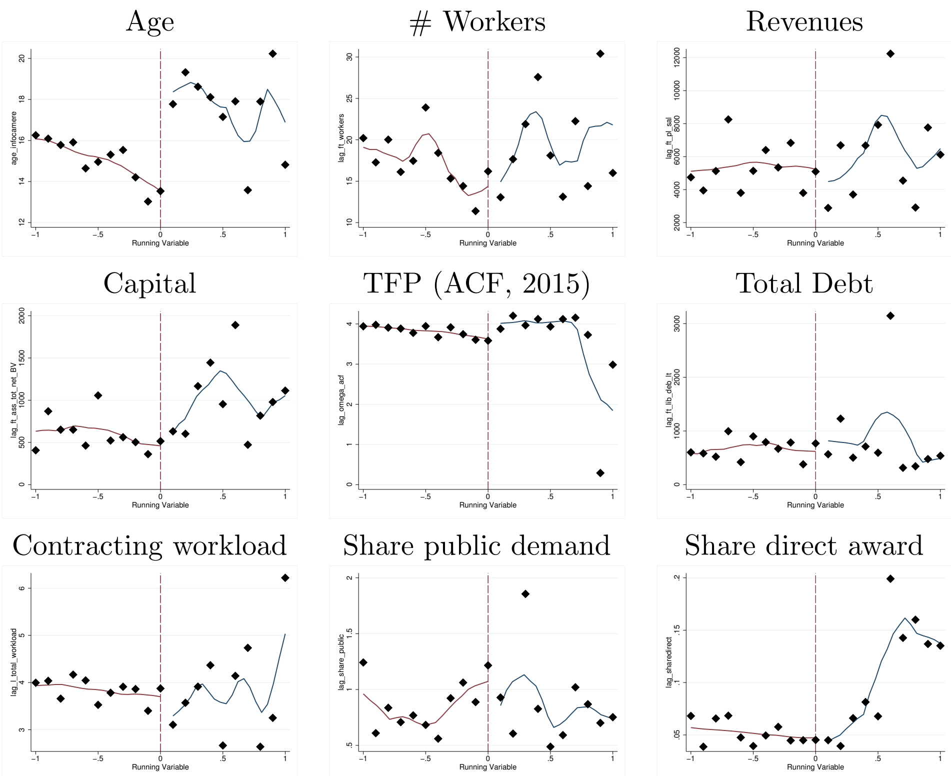
D Appendix: Additional Figures

Figure D1: Firm Characteristics': Winners and Marginal Losers at $t-1$ (up to order 3)



Notes: This figure reports additional plots for firm' characteristics for bidders up to rank 3 to validate the RD strategy and check for the continuity at the cutoff to show that firms are statistically similar prior the event (at $t-1$). Observation are at the auction participant level and we restrict our attention to the winner, runner-up and the third-closest bid, as we later do in the empirical analysis. We report participants in auctions for which we were able to retrieve the placed bid, as described in text. Variable are at the year level. Points represent bin average of the covariate calculated at 0.1 p.p. bids. We report 95% confidence interval as well.

Figure D2: Firm Characteristics': Winners and Marginal Losers at $t-1$ (all bidders)



Notes: This figure reports different plots for firm' characteristics for all bidders to validate the RD strategy and check for the continuity at the cutoff to show that firms are statistically similar prior the event. Observation are at the auction participant level and we restrict our attention to the winner, runner-up and the third-closest bid, as we later do in the empirical analysis. We report participants in auctions for which we were able to retrieve the placed bid, as described in text. Variable are at the year level. Points represent bin average of the covariate calculated at 0.1 p.p. bids. We report 95% confidence interval as well.

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