The Credit Channel of Public Procurement

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Abstract

Public procurement accounts for one-third of government spending. In this paper, I document a new mechanism through which government procurement promotes firm growth: firms use procurement contracts to increase cash flow based lending. I use Portuguese administrative data from 2009 to 2019 and exploit public contests as a source of quasi-exogenous variation in the award of procurement contracts. Winning an additional 1 euro from a procurement contract increases firm credit by 7 cents at lower interest rates. This finding highlights a mechanism through which future fiscal stimulus can impact the real economy today: procurement contracts increase firms' net worth by increasing future cash flows that can be used as collateral to ease borrowing constraints and boost corporate liquidity. Consequently, this enhanced access to credit promotes higher investment and employment, with these effects being more pronounced and persistent in smaller and financially constrained firms. At the aggregate level, I empirically estimate that 1 additional euro in public procurement increases regional output by 1.3 euros with the credit channel accounting for 5% of it.

Keywords: Credit, Collateral, Corporate Finance, Fiscal Policy, Public Procurement
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1 Introduction

Public procurement is an important source of revenue for the private sector and a crucial fiscal policy tool for governments. In 2019, OECD countries allocated 12.6% of GDP to acquiring services, works, and supplies from the private sector through public procurement, which constituted 30% of total government expenditures (OECD 2021). Despite its significance, the effect of public procurement on firm growth remains underexplored, largely due to data limitations and the endogenous nature of contract awards. The key challenges are thus rooted in the collection of procurement contracts' information, their non-competitive allocation system, and the ability to link them to firms' balance sheets and financial statements. This study addresses these challenges by leveraging Portugal's detailed firm-level data and its system of competitively awarded procurement contracts, offering new insights into the relationship between public procurement and firm dynamics in a developed economy.

The literature proposes the increase in revenues as the key channel linking public procurement to firms' growth. Notwithstanding, if procurement is perceived as a secure stream of cash flows in the form of operating earnings, it can also be used as collateral to increase firms' access to credit. Therefore, not only do the increased revenues originating from procurement contracts affect firms' decisions, the ensuing cash flow based lending properties do as well. This mechanism is particularly important for small and medium enterprises in the United States (Caglio et al. 2022) and especially in countries where firms rely heavily on bank credit, as it is the case in Portugal and several other OECD countries where more than 80% of nonfinancial corporate debt is accounted for by bank loans.

In this paper, I study the effect of public procurement on corporate credit and its implications for the macroeconomy. To this end, I compile an extensive dataset from 2009 to 2019, integrating procurement contracts with crucial financial data and tax records of Portuguese nonfinancial firms. By exploiting plausibly exogenous variation in public procurement awards which are competitively attributed via public contests, I uncover the credit channel of public procurement: *firms use procurement contracts as collateral to increase their access to credit.* This finding not only highlights a novel channel through which public procurement impacts firm growth but also underscores the broader economic implications of such fiscal policies.

I estimate that winning an additional €1 from a procurement contract increases corporate credit by up to €0.07 in both drawn and undrawn credit lines. The majority of this increase is accounted for by firm guarantees including future

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procurement cash flows. This financial uplift is accompanied by a reduction in interest rates and an expansion in cash and bank deposits, suggesting that the surge in credit is predominantly supply-driven. Enhanced access to credit further promotes firm investment, which in turn catalyzes job creation and thus has real economic effects, especially for smaller firms. The credit channel of public procurement is further confirmed at the regional level where an additional €1 in public procurement is estimated to increase regional output by €1.3 with the credit channel accounting for 5% of the increase.

To enter into greater detail, the identification strategy in this study hinges on the process used to select winners in public contests. Firms compete in a setting that is analogous to a silent sealed-bid first-price auction with costs associated with submitting the only bid. In this type of auction, when submitting the bid, firms do not know with whom and how many contestants they are competing against. Thus, it is unlikely that they can anticipate the auction's outcome. This contest design yields a more accurate estimate of the effect of public procurement on corporate credit and other firm dynamics under one key identifying assumption. Winning a procurement contract via a public contest must not be systematically correlated with other firm-level characteristics. I provide empirical evidence supporting this assumption.

This study focuses on Portugal, where, since 2008, electronic registration of procurement contracts has been compulsory. I web scrape more than 1 million Portuguese procurement contracts registered online. They have information on the hiring and hired entities, the duration of the contract, its price, and its description. I use the winner's tax identification number to match contracts' information to both administrative microdata on firms' tax fillings and credit registry data. The tax data contain detailed balance-sheet and income-statement information while the credit data provide information not only on loan amounts but also on loan collateral.

I find that for each additional €1 of public procurement, firms increase total credit by €0.07. This increase is driven by credit supply as interest rates decrease by 0.3 percentage points in response to the award on impact. Moreover, I show that the majority of the increase in credit is accounted for by firm guarantees which entail future procurement cash flows. In other words, the increase in credit is being collateralized by future revenues from their anticipated sales to the government. Public procurement thus allows firms to overcome credit constraints, not only via an increase in borrowing but also via an increase in credit lines and savings in the form of cash and bank deposits.

A natural follow-up question is how firms react to the increase in the liquid-

ity and credit promoted by the awarded contracts. I find that winning a procurement contract boosts firms' employment and investment and exhibits greater magnitude and durability among enterprises that are smaller and more likely to be under financial constraints. Hence, this finding raises questions about the commonly accepted idea that revenues are the unique driver of firm dynamics' response to procurement contracting. The analysis also highlights important heterogeneities on the transmission of the procurement effects on firm credit, investment, and employment for small and big firms. While there is no statistically significant difference in the response of credit between them, smaller firms react more significantly and more persistently to procurement awards in terms of investment and staff headcount. This exercise allows me to highlight the importance of carefully designing the award of procurement contracts if the main goal of the government is to promote overall economic growth that must take into account not only relevant heterogeneities but also the result's persistency.

To further understand the aggregate effects of public procurement, I assemble a regional dataset by aggregating all procurement awards in accordance with their location and use an empirical framework that allows me to compute local fiscal multipliers and to study the impact of procurement spending on production, investment, productivity, credit, and inflation. I estimate that an additional €1 in public procurement increases regional output by €1.3 with the credit channel accounting for 5% of the increase. I also find that procurement boosts private R&D investment, highlighting its role in spurring innovation. However, this investment surge does not lead to notable improvements in total factor productivity. Meanwhile, the overall credit response among nonfinancial firms is subdued, suggesting a redirection of credit toward procurement-winning firms without a broader credit expansion in the regional economy. The slight inflationary trend observed aligns with short-term demand increases from procurement activities, indicating immediate economic stimulation without long-term persistence.

This work contributes to three separate strands of the literature. First, this paper contributes to the empirical literature documenting the firm-level effects of fiscal policy with a focus on public procurement. Most studies either focus on the U.S. (Barrot and Nanda 2020; Goldman 2020; Cox et al. 2024; Budrys 2022), on a specific sub-sector of the economy like construction or military spending (Gugler et al. 2020; Hebous and Zimmermann 2021), or on non-financial aspects of procurement like firm survival (Cappelletti et al. 2024). Some recent exceptions are the papers by Ferraz et al. (2021) and Lee (2021) who argue that procurement winning firms grow more compared to their contest's runner-ups using data across industries for Brazil and Korea, respectively. I add to this very rich

literature by studying the relationship between credit and firm growth across all industries and economic sectors and by highlighting a new transmission mechanism: firms use procurement contracts as collateral to increase their access to credit which alleviates financial frictions and has implications not only at the firm- but also at the aggregate-level.

Thus far, the increase in revenues has been proposed as the main driver of the public procurement effects on firm dynamics. This literature strand still is silent about the importance of credit with the exception of the contemporaneous work by di Giovanni et al. (2024) who provide evidence of a positive correlation between public procurement and firm credit. Relative to their work, I provide detailed evidence on the credit *supply* channel of public procurement and its implications for firm investment. Simultaneously, I can measure contract heterogeneity, tease out the importance of the credit channel and highlight policy implications of this policy tool. Moreover, while di Giovanni et al. (2024) focus on quantifying the welfare implications of different public procurement allocation systems, I focus on the direct effects of this policy and provide the first estimates for local procurement multipliers together with an estimate of the importance of the credit channel.

The second strand of the literature to which this paper makes a contribution is concerned with the effects of government spending on economic growth and on the size of fiscal multipliers. Using both macro and microdata, the empirical evidence on the effect of government spending on growth is still subject to debate (Galí et al. 2007; Ramey 2011a; Gabriel et al. 2023a,b; Briganti 2024). Given my focus on the credit channel of fiscal policy, it is important to highlight previous studies documenting a stronger and more persistent response of output to fiscal shocks when the economy is in conditions of tight credit (Ferraresi et al. 2015) or in sectors and regions with higher concentration of credit-constrained firms (Aghion et al. 2014; Juarros 2020). Other studies focusing on specific fiscal stimulus policies such as the Troubled Asset Relief Program (Duchin and Sosyura 2014) or the 2004 American Jobs Creation Act (Bird et al. 2022) also study their impact on lending outcomes. Nevertheless, both the nature of the stimulus I study and the mechanism through which the fiscal policy operates via the banking sector and impacts the regional economy in Portugal differentiate my paper from the existing literature.

Finally, this work contributes to the broader literature on how financial frictions amplify the propagation of economic shocks and, in particular, on the type of collateral that firms can use to borrow. Seminal papers in this area have emphasized the liquidation value of capital as the main factor relaxing firms' con-

straints (Kiyotaki and Moore 1997). However, recent works highlight the importance of firms' cash flows acting as collateral (Ivashina et al. 2022; Lian and Ma 2021; Drechsel 2022; Caglio et al. 2022; Gupta et al. 2022). For example, in 2004, asset based loans accounted for 39% while cash flow based loans accounted for 48% of the total volume of commercial credit by banks in Spain (Ivashina et al. 2022). In the U.S., this type of lending is also non-negligible with 23% of corporate borrowing for big publicly listed firms being backed by cash flow based collateral (Lian and Ma 2021), a percentage which is even higher when one also considers non-publicly listed firms: using FR-Y14 data from 2012 to 2019, Caglio et al. (2022) document that 28% of all loans are collateralized by account receivables. I contribute to this literature by highlighting the importance of cash flows coming from sales to the government with procurement contracts being used as collateral and thus easing firms' financial constraints. Moreover, I relate the increase in cash flow based lending with real economic effects following the firm decision on how to use the newly issued credit after winning a procurement contract and argue that, by easing financial constraints, procurement contracts can induce firm investment and employment.

My results have the greatest external validity for other countries where public procurement corresponds to a significant share of GDP and firms heavily rely on bank credit, which happens to be the case in a large sample of both developed and developing countries. Policymakers from countries with higher preponderance of procurement would be the ones most interested in these implications. The credit channel of public procurement is important because the more firms become dependent upon banks, the more economically significant it becomes. In Portugal, less than one percent of Portuguese firms have access to capital markets (Degryse et al. 2021). Thus, my results have external validity in countries where firms also heavily rely on bank credit.

Outline. The remainder of this paper is structured as follows. Section 2 introduces the dataset and describes the legal background of procurement contracting in Portugal together with some descriptive statistics on the type of awards. Section 3 tackles the key research question of the paper by studying the impact of procurement on firm dynamics such as credit, investment and employment while fleshing out the key mechanisms at play. Section 4 derives aggregate implications for the macroeconomy and Section 5 concludes.

2 Institutional Setting and Data

2.1 *Procurement Contracting in Portugal*

Public procurement is defined in this project as the acquisition of goods and services by any public entity. In Portugal, there are 4,000 such entities ranging from local and national governments to state hospitals and universities. In 2009, Portugal became among the first in the world to mandate the electronic registration of public procurement contracts. On average, these electronically published contracts account for a third of the total procurement spending and between 2% and 4% of GDP (Figure B.2). They are allocated to firms of all sizes (Table B.1) and from all industries (Table B.2). Considering the extensive time and firm coverage, this setting provides an ideal opportunity to study the dynamic effects of this mandatory registration policy at both the firm and regional levels. Additional details can be found in Appendix B.

The identification strategy in this study hinges on the decision process for choosing winners. Typically, a contest is announced by an entity seeking to hire, and firms submit a single bid. The bidding process, which includes a fee paid to a certified third-party intermediary responsible for enforcing rules and preserving anonymity, is both costly and time-consuming. The hiring entity never knows which firms are bidding, and the competing firms are unaware of their competitors. Two rule types can be set by the hiring entity: competition based on the *lowest price* or the *most economically advantageous tender*, which takes into account quality and timing. In over 99% of contracts, the price criterion accounts for more than half of the decision weight, providing a measurable factor that aids in my identification effort, as past wins do not influence the current contest's outcome.

The competitive environment's similarity to a blind, sealed-bid auction, where costs are incurred to submit a solitary bid, supports my identification strategy. In such a quasi-exogenous setting, there is no *ex-ante* predictable winner and firms remain unaware if their bid is the lowest until the result is declared. For instance, even though a big and more productive firm could have the advantage of being able to offer a lower cost for the production or provision of a service, it is not guaranteed that it would make a lower bid against a small and less productive firm that may desperately need the contract and thus might opt for slimmer margins. Additionally, without knowledge of their competitors or the number of bids, firms lack the incentive to engage in strategic pricing.

2.2 Data

I merge three different datasets: one on public procurement contracts, another on firms' balance sheets, and a third on credit information. Data on all government procurement contracts published online since 2009 are sourced from the official e-procurement website, BASE. Managed by the Portuguese Institute of Public Markets, Real Estate and Construction (IMPIC), this database offers comprehensive contract details. By matching the tax ID of nonfinancial firms bidding and winning these contracts, I link procurement information to their financial and credit records. My analysis concentrates on contracts awarded via public contests, culminating in 35,675 unique winner-year observations post-merger. Further details on data sources and the cleaning process are forthcoming, with additional information located in Appendix A.

Procurement Data. I collect information on over 1 million procurement contracts from the official e-procurement website *BASE*, covering announcements between 2009 and 2019. I scrape the contract records, for which each hiring entity is responsible for reporting. I gather the tax numbers of the winners, hiring entities, and, in some cases, bidders of public contests. Additionally, I compile contract details including the date, duration, and award value.

An essential aspect is the awarding mechanism. Two primary methods exist: *direct awards*, where a public entity selects a firm, and *public contests*, where entities announce procurement needs and firms submit bids resembling a first-price sealed bid auction. This paper focuses on the latest for identification purposes - direct awards are more predictable and thus expected than public contests. This paper focuses on public contests for identification, as direct awards tend to be more predictable. Table 1 summarizes the data: the average award size is approximately €202,170 but the median is much smaller, at around €34,762. The average contract lasts less than a year, about 297 days, with an average of between 3 and 4 contestants per bid, although contestant reporting is often incomplete.[2]

The procurement data, web scraped from the official website, accounts for more than one-third of Portugal's total public procurement expenses, as estimated by OECD (2021), amounting to about 3% of the national GDP. This substantial portion of government spending—exceeding the public investment rate of roughly 2% of GDP during the same period—means that public works in my dataset represent over half of all public investment. Notably, data coverage has improved steadily, increasing from 2% of GDP in 2012 to 4% by 2019 (Figure B.2).

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[2]For many public contests, the reporting of contestants is missing.

Table 1: Summary Statistics of Procurement Contracts

	Mean	**Std. Dev.**	**P5**	**Median**	**P95**	**Obs**
Public Contests						
Award (€)	202,170	1,312,288	500	33,762	651,600	138,561
Duration (Days)	297	354	10	183	1,095	138,561
# Contestants	3.6	4.3	1	2	12	138,561

Notes: This table presents selected summary statistics of procurement contracts for all of the public contests in my sample between 2009 and 2019.

This study is among the first to employ web-scraped data on Portuguese electronic procurement contracts, alongside those of Bonfim et al. (2024), de Sousa et al. (2023), and Caires et al. (2023). In contrast to other similar datasets of US and European procurement data, the Portuguese contracts offer distinct advantages due to their detail and granularity. Access to the winner's tax number enables direct matching with firm-level financial and credit records, bypassing name matching. The extensive coverage across all regions and industries, as shown in Table B.2, enhances the external validity of findings, particularly in comparison to sector-specific studies like those by Gugler et al. (2020) in construction and Hebous and Zimmermann (2021) in defense. Notably, the microdata represents over 33% of total government procurement, as per Portuguese national accounts, marking a significant improvement over the 13% and 16% coverage reported for Spain and the U.S. by di Giovanni et al. (2024) and Cox et al. (2024), respectively.

Given the crucial role of competition in my identification strategy, I focus solely on contracts awarded *after* public contests in my baseline analysis. This approach results in 138,561 contract-winner pairings, which, when aggregated annually and matched with firm-specific data, yield 35,675 unique winner-year observations. Additional details about the data sources and the data cleaning process are provided later, with supplementary information available in Appendix A and Table A.3.

Firm-level data. Firm-level microdata were sourced from the Portuguese Simplified Corporate Information Survey (Informação Empresarial Simplificada, IES), as provided by the Bank of Portugal's Microdata Investigation Laboratory (BPLIM) (BPLIM 2021). This dataset offers comprehensive annual balance-sheet details and profit-and-loss data for all Portuguese nonfinancial firms. Additionally, the

Credit Registry Central (CRC) at the Bank of Portugal (BPLIM 2019) supplied monthly data on credit and collateral, encompassing all loans banks extended to nonfinancial firms. Descriptive statistics for the firms in our sample are shown in Table A.2. Consistent with standard practices in economic research, the analysis includes only nonfinancial corporations. Firms with incomplete or negative book asset values, as well as those undergoing recent organizational changes like mergers or acquisitions, are excluded from the sample.

I have comprehensive data on collateral types and pledged amounts at loan issuance. If a loan is backed by multiple collateral sources, the dataset details each type and its amount. However, it's important to note that collateral values are not market-adjusted and are often capped at the loan amount when they exceed it (Degryse et al. 2021). The aggregated credit amounts are aggregated into six collateral types: (i) mortgaged real estate; (ii) unmortgaged real estate; (iii) financial assets; (iv) personal guarantees by the firm; (v) personal guarantees by the state; and (vi) an 'other' category. The first three are for asset-based lending, and the latter three for cash flow based lending. In this analysis, personal guarantees are treated like tangible collateral, although, in financial distress, they cannot be seized as tangible assets can.

cash flow based lending plays a significant role in Portugal's economy, with an average of approximately 44% of firm credit in my sample secured by personal guarantees from either the firm—such as anticipated revenues—or the state, like subsidies (Table A.2). This underscores a key insight: future revenues, including procurement contracts, can serve as collateral and enhance firms' credit access. This observation aligns with Ivashina et al. (2022) who report cash flow loans constituting 48% of Spain's commercial credit volume and Caglio et al. (2022) who document that in the U.S., 28% of loans are backed by account receivables.

3 Firm-Level Effects of Public Procurement

In this section, I estimate the impact of winning procurement contracts on firm credit. The baseline empirical framework, outlined in Section 3.2, leverages annual firm balance sheet and credit data. Findings indicate that procurement awards lead to an increase in corporate credit, a decrease in average interest rates, and an augmentation of both cash reserves and available credit lines. Considering the contracts' average duration of under a year, Section 3.3 utilizes monthly credit data to examine credit dynamics within the initial 12 months post-award, revealing significant credit effects primarily within the first six months, predominantly for credits with maturities under one year. Notably, this credit expansion

is largely supported by personal guarantees provided by the firm, indicating a reliance on cash flow based financing. Building on these findings, I delve into the broader implications at the firm level by extending the analysis to corporate investment and additional balance sheet variables and exploring heterogeneities.

3.1 Empirical Strategy

Government contracts are not randomly allocated across firms, thus estimating the correlation between winning a government contract and the subsequent growth in firm credit can lead to biased estimates. The direction of the bias will depend on the underlying data-generating process. If, for example, contracts are preferentially awarded to highly productive firms, this could result in an overestimation of the effect, as these firms are likely to have better access to credit regardless. Conversely, if government contracts displace private sector opportunities, we might underestimate their impact on credit. Additionally, if firms can foresee their chances of winning, they might adjust their business strategies in advance, potentially leading to an underestimation of the procurement effect. To address these challenges, I employ an empirical strategy that leverages the structured nature of public contests for contract awards, as detailed in subsection 2.1.

In my baseline specification, I look at the firm dynamic response to explore not only potential anticipation effects but also to evaluate the persistence of the results. I estimate the elasticity of firm credit to the actual amount won in public contests by making use of local projections à la Jordà (2005):

$$\frac{C_{i,t+h} - C_{i,t-1}}{\text{Assets}_{i,t-1}} = \beta^h \frac{\text{Award}_{i,t}}{\text{Assets}_{i,t-1}} + \psi^h \cdot \mathbf{X}_{i,t-1} + \alpha_i^h + \delta_{s,t}^h + \varepsilon_{i,t}^h \quad \forall_{h \in \{-3,\dots,2\}} \quad (1)$$

where the key dependent variable is credit growth of firm i between time $t-1$ and time $t+h$ relative to the book value of total assets in $t-1$. The key regressor $\frac{\text{Award}_{i,t}}{\text{Assets}_{i,t-1}}$ corresponds to the contractualized price of all public contests won in year t also divided by the value of total assets in the previous period. β^h corresponds to the elasticity of credit to the award value at horizon h. $\mathbf{X}_{i,t-1}$ is a control vector with one lag of the dependent and independent variables. The inclusion of lagged values of the previous awards received by the firm is important to control for a potential omitted variable bias coming from long-run effects of previous awards. This approach helps ensure my estimates are not skewed by long-term trends or prior successes.

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In the analysis, I incorporate both firm and time-industry fixed effects to control for unobserved factors. Firm-fixed effects (α_i) account for unique characteristics of individual firms, excluding those that either never win public contracts or win them consistently every year, as these 'serial winners' may not represent the broader firm population. Time-industry fixed effects ($\delta_{s,t}$) consider both common industry trends within each year and broader economic or policy influences, allowing for nuanced differences across sectors. This means that $\delta_{s,t}$ allows the common factors within a year to have different impacts across industries while addressing the possibility that the effects of government demand on firms' credit can be regime-dependent and vary across economic sectors. This dual fixed effects approach ensures a comprehensive adjustment for both firm-specific traits and wider industry or temporal influences.

To test for anticipation effects, I include horizons $h = -2$ and $h = -3$ in the estimation. To investigate potential anticipation by firms of winning contracts, I extend the analysis to include earlier time periods ($h = -2$ and $h = -3$). Given that the typical duration from contest announcement to winner selection was about four months in 2020 (IMPIC 2021), using yearly data helps minimize biases from firms potentially anticipating contract wins. Moreover, these anticipation effects are more likely to be present in directly awarded contracts, in which case firms learn whether they are going to receive the contract *before* the public announcement. Thus, focusing solely on contracts awarded through public contests, which inherently limit prior knowledge of outcomes, further reduces the risk of anticipation bias in our results.

Estimating specification (1) yields an unbiased estimate of β if the standard no-omitted variable bias assumption is satisfied over all horizons. Following the preceding discussion, one key condition needs to be met: winning a procurement contract via a public contest must not be systematically correlated with other firm-level characteristics. Figure C.1 tests whether winning a procurement award is systematically correlated with pre-determined firm characteristics. I report both unconditional correlations and correlations conditional on the fixed effects used in the baseline analysis. The unconditional estimates show significant differences between winners and other participants of public contests. However, they mostly disappear once we include the fixed effects in the analysis, with the exception of total assets which are included in the estimation. In the Appendix C.1 delves deeper into the identification strategy and presents a key robustness check where I use a local projections difference-in-differences specification following (Dube et al. 2023).

3.2 Public Procurement and Firm Credit - Annual Analysis

Figure 1 shows that for each additional Euro gained from procurement contracts, there's an immediate increase of 3.3 cents in a firm's drawn credit, which declines over the following two years. No notable anticipation effects are observed in the years preceding the award, suggesting that the impact on credit is not a result of preemptive financial behaviors.

Figure 1: Elasticity of firm credit to procurement contract awards
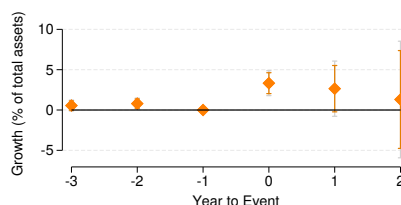
Notes: This figure displays the estimated coefficient β for each horizon h relative to the year of the award $h = 0$ from equation (1). The boxplot displays the coefficient estimate $\diamond$ and the corresponding 95% (grey) and 90% (orange) confidence bands for the response of firm credit relative to total assets in the previous year to the amount awarded after winning a procurement contract which is also normalized by the lagged value of total assets. The estimation includes firm and industry×year fixed effects. All standard errors are clustered at the firm level. Table C.2 presents the corresponding point estimates.

A preliminary calculation indicates an approximate credit growth of 2.5 percentage points one year after receiving a procurement award.[3] This estimation aligns with findings by di Giovanni et al. (2024), who identified an annual credit growth increase of roughly 5 percentage points for firms awarded procurement contracts in Spain.

At the start of the observed period, 2011-2012, the Portuguese government implemented austerity measures in response to the sovereign debt crisis and the IMF's intervention, which led to a substantial decrease in commercial bank spending. Bonfim et al. (2024) suggest that banks with greater initial exposure to companies holding procurement contracts scaled back their lending more than banks with less exposure, highlighting a potential negative aspect of procurement during financial contractions and decreased public expenditure. Conversely, my analysis reveals supporting evidence of increased spending, leading to the hy-

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[3]This figure is derived by multiplying the impact coefficient (3.3%) by the average ratio of credit to total assets for procurement firms, which is roughly 75%.

pothesis that procurement awards had a beneficial influence on corporate credit availability, ultimately fostering a rise in total production at a macroeconomic scale, further detailed in Section 4.

Credit Supply Channel. To decipher the factors behind the surge in borrowed credit by firms winning procurement contracts, I also investigate the response of the price of credit, the interest rate. If the credit rise stems primarily from firm demand, an uptick in interest rates would typically be anticipated. Conversely, a supply-driven increase by banks would likely see a dip in interest rates.

Given the lack of loan-level data, individual loan interest rates remain unobserved. Nonetheless, an inferred interest rate measure is derived by dividing the total interest outlays by prior credit amounts. Procurement victors in this study had an average interest rate of around 6.9% with the median hovering near 5.1% (Table A.2), aligning closely with figures reported by the Portuguese Central Bank.

Figure 2: Elasticity of interest rates to procurement contract awards
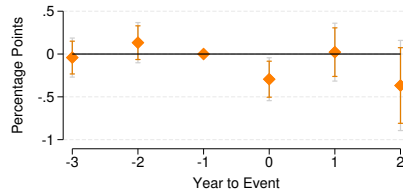
Notes: This figure displays the estimated coefficient β for each horizon h relative to the year of the award $h = 0$ from equation (1). The boxplot displays the coefficient estimate $\diamond$ and the corresponding 95% (grey) and 90% (orange) confidence bands for the implicit interest rate response to the amount awarded after winning a procurement contract which is normalized by the lagged value of total assets. The estimation includes firm and industry×year fixed effects. All standard errors are clustered at the firm level. Table C.2 presents the corresponding point estimates.

Figure 2 showcases the interest rate findings. It is observed that interest rates decrease by over 0.3 percentage points immediately after a firm is awarded a procurement contract. Such a significant reduction supports the hypothesis that the credit supply mechanism is more pronounced than the credit demand one. While this finding of lower interest rates following a specific type of government spending expansion may seem counterintuitive, there is theoretical and empirical support for such an outcome.

In demand-driven output models, the increased demand for credit from government spending shocks is balanced by a rise in credit supply due to higher

aggregate income and can lead to lower interest rates (Murphy and Walsh 2022). In tandem, some empirical studies have shown that government spending can be associated with lower interest rates at the local level (Auerbach et al. 2020) and even at the national level (Ramey 2011b; Corsetti et al. 2012; Fella et al. 2019).

Notwithstanding, the fact that winner firms face lower interest rates should not be interpreted as government spending lowering interest rates in general. Similarly to Hebous and Zimmermann (2021), there's no straightforward link between my firm-level spending shocks and the equilibrium interest rate. In my application, a government spending shock doesn't always mean an overall increase in government spending or budget deficit; it can also be interpreted as the government allocating spending differently.

In my setting, there are two possible micro-level mechanisms linking procurement awards to reduced interest rates both related to increasing the firm's perception of creditworthiness: solving banks' information asymmetry and increasing the firm's collateral. Firstly, consistent with a signaling or certification effect, public procurement could act as a government endorsement, prompting banks to broaden their credit offerings at more competitive rates to winning firms, akin to observed outcomes from other Portuguese government-backed certifications (Bonfim et al. 2023). Alternatively, the procurement contracts themselves might serve as valuable collateral, enhancing a firm's credit profile and incentivizing banks to further extend credit availability.

The data supports the involvement of both hypothesized channels in amplifying the credit supply effect. The initial hypothesis is examined in Subsection 3.5, where I compare the reactions of first-time award recipients to those firms with previous awards. I explore the second argument explicitly with monthly level data in Subsection 3 and implicitly in the following paragraphs by exploring the role of financial constraints and testing whether public procurement eases firms' borrowing constraints.

Building upon the previous results, if procurement awards lead to an increase in credit, it is likely that now firms also build precautionary savings and use their increased creditworthiness in negotiating new credit lines. The ensuing analysis, detailed in Figure 3, examines how these procurement victories influence firms' liquidity and credit line negotiations, shedding light on their strategic financial management post-award.

Figure 3a supports the notion that procurement awards lead firms to renegotiate and bolster their credit lines. Specifically, for every additional euro awarded through procurement, firms secure an extra four cents in potential credit. This potential credit encompasses the undrawn portions of credit facilities such as

Figure 3: Elasticity of credit lines and liquidity to procurement contract awards
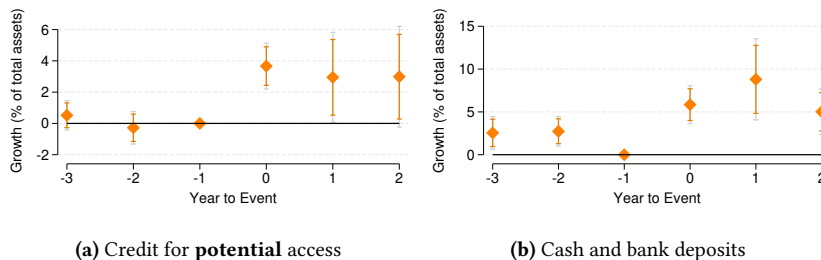
(a) Credit for **potential** access   **(b)** Cash and bank deposits

Notes: This figure displays the estimated coefficient β for each horizon h relative to the year of the award $h = 0$ from equation (1). The boxplot displays the coefficient estimate $\diamond$ and the corresponding 95% (grey) and 90% (orange) confidence bands for the response of potential credit (Fig. 3a) and cash and bank deposits (Fig. 3b) relative to total assets in the previous year to the amount awarded after winning a procurement contract which is also normalized by the lagged value of total assets. The estimation includes firm and industry×year fixed effects. All standard errors are clustered at the firm level. Table C.2 presents the corresponding point estimates.

credit cards and lines of credit, reflecting a negotiated balance between the firm and its financial partners. The observed significant uptick in potential credit, alongside a rise in utilized credit within the award year amounting to a total of seven cents, underscores that firms are proactively managing and expanding their credit potential, potentially mitigating any credit constraints they may face.
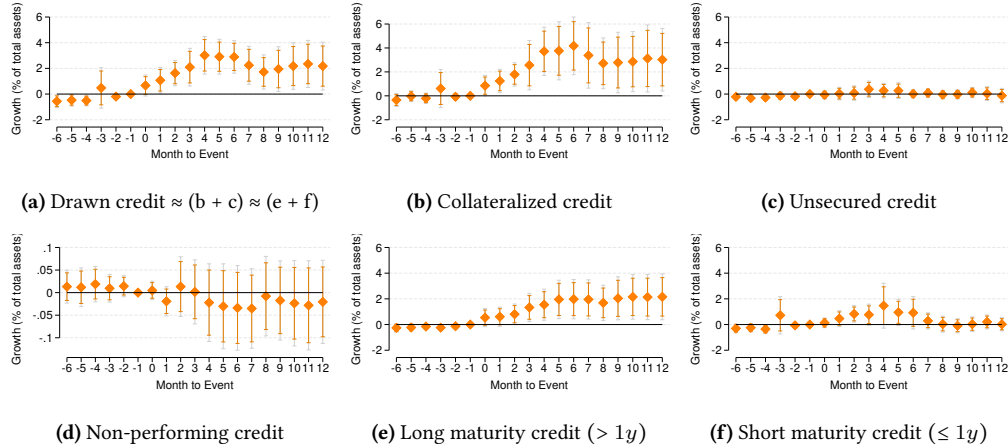
Receiving an additional euro from procurement contracts results in a nearly six-cent rise in a firm's cash and bank deposits, as shown in Figure 3b. This enduring increase in liquidity suggests that firms may be bolstering their reserves for precautionary reasons, reinforcing the concept that procurement contracts serve as a means for firms to lessen financial limitations.

3.3   Public Procurement and Firm Credit and Collateral - Monthly Analysis

Figure 4 provides a granular view of the impact of procurement awards on firm credit, offering a detailed monthly breakdown within the crucial first year following an award. This temporal lens is particularly pertinent given the average procurement contract's maturity period of less than one year, making these findings especially relevant for short-term credit dynamics.

Panel (a) highlights the immediate upshot in total credit, beginning at the award's inception, with a marked rise in the very first month. This rapid increase implies that procurement awards promptly enhance firms' credit availability, which may reflect in the swift execution of contract-related activities or

Figure 4: The effects of winning procurement awards on firm credit

(a) Drawn credit $\approx$ (b + c) $\approx$ (e + f)

(b) Collateralized credit

(c) Unsecured credit

(d) Non-performing credit

(e) Long maturity credit $(> 1y)$

(f) Short maturity credit $(\leq 1y)$

Notes: This figure displays the estimated coefficient β for each horizon h relative to the month of the award $h = 0$ from a monthly version of equation (1). The boxplot displays the coefficient estimate $\diamond$ and the corresponding 95% (grey) and 90% (orange) confidence bands for the response of each dependent variable on firm credit, as described in the sub-caption, normalized by lagged assets to the monthly amount awarded after winning a procurement contract which is also normalized by the lagged value of total assets. The estimation includes firm and industry×month fixed effects. All standard errors are clustered at the firm level.

investments. Notably, its peak response occurs approximately three months after the procurement award, which intriguingly aligns with the yearly coefficient impact of more than 3 cents per euro of procurement. This peak suggests that firms likely begin to substantially draw upon their credit lines during this period, possibly coinciding with the initiation of contract fulfillment or related investment activities. The following two panels decompose this credit response into whether it was collateralized (b) or not (c). As the latter displays a muted response, the increase in credit is almost entirely collateralized.

A discerning analysis of credit quality and maturity unfolds in panels (d), (e), and (f). The subdued movement in non-performing credit, shown in panel (d), reassures that the credit influx post-award is of sound quality. Intriguingly, while both long- and short-maturity credits depicted in panels (e) and (f) exhibit increases, it's the long-term credit that reveals a stronger response. This observation aligns with the longer-run effects of procurement contracts; besides covering the firms' needs for short-term liquidity, firms are able to resiliently expand their credit position.

Crucially, this monthly perspective allows for an investigation into potential anticipation effects. The lack of significant movement in credit variables up to three months prior to the contract signing date implies that firms are not pre-emptively altering their financial behavior based on expected outcomes of public contests. This absence of anticipation effects, coupled with the robust increase in credit post-award, reinforces the conclusions drawn from the annual analysis and underlines the validity of the procurement award as a significant determinant of credit access in the short-term. Overall, this meticulous monthly analysis reaffirms the annual findings and enriches the understanding of how procurement awards swiftly translate into financial benefits for firms.

Credit Collateral The dynamics of corporate credit in response to procurement wins may also reflect a shift in the firms' collateral base. Traditionally, physical assets were considered the cornerstone of collateral; however, recent research advocates for the significance of cash flow as a collateral component, which could rival or even surpass asset-based lending in importance (Lian and Ma 2021; Ivashina et al. 2022; Drechsel 2022; Caglio et al. 2022; di Giovanni et al. 2024). This perspective adjustment is especially relevant as the nature of collateral evolves, with firms increasingly leveraging expected cash flows, such as those from procurement contracts, to bolster creditworthiness.
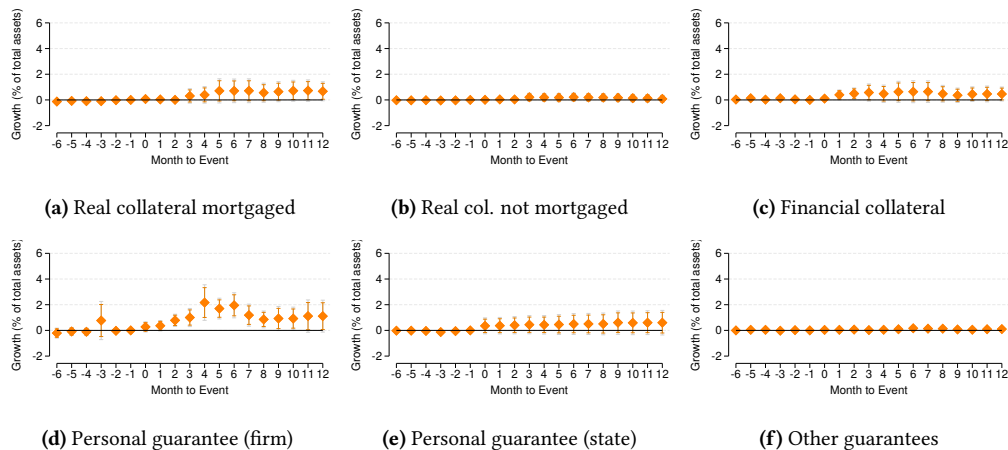
Given this context, it becomes imperative to investigate not only if procurement contracts directly contribute to the observed credit increase but also the extent to which these contracts are utilized as collateral. In this regard, my analysis probes into the collateralization of credit post-procurement victory and its implications for firm financing. Notably, the current discourse on cash flow based lending has predominantly orbited private contracts. My inquiry aims to enrich this dialogue by exploring how varying cash flow sources, particularly those stemming from public procurement, could differentially influence corporate credit dynamics. To assess this, I ask whether the contract itself is directly related to the credit increase and can be used as collateral while quantifying how much of the increase in credit is collateralized by these contracts.

In extending my analysis, I leverage the granular data obtained from web scraping procurement contracts and align it with detailed collateral information from the Portuguese credit registry. This registry specifies the collateral type and amount pledged for each loan, even when multiple collateral sources back a single loan. The granted credit can be categorized into six collateral types: (i) real estate with a mortgage; (ii) real estate without a mortgage; (iii) financial assets; (iv) personal guarantees provided by the firm; (v) personal guarantees provided by the state; and a residual (vi) other category. The delineation of these

categories allows me to differentiate between asset-based and cash flow based lending practices. The former encompasses the first three types, associated with tangible assets, while the latter comprises the remaining types, rooted in the creditworthiness and potential future earnings of the firm. This distinction is critical in understanding the nuanced ways firms leverage different collateral types to secure financing.

To investigate the influence of procurement awards on the types of collateral employed in credit acquisition, I direct my focus towards the effects of an incremental €1 from such awards. Specifically, I apply a modified version of equation (1), with the various collateral growth rates serving as dependent variables in a monthly specification. My primary interest lies in credits that are secured by personal guarantees, a category inclusive of prospective revenue streams as a form of collateral. I hypothesize that credits buttressed by personal guarantees will display a more pronounced reaction to procurement awards compared to those safeguarded by alternative collateral forms. The outcomes of this analysis are illustrated in Figure 5, which delineates the differential responses of collateral types to procurement awards.

Figure 5: The effects of winning procurement awards on firm collateral

(a) Real collateral mortgaged   **(b)** Real col. not mortgaged   **(c)** Financial collateral

(d) Personal guarantee (firm)   **(e)** Personal guarantee (state)   **(f)** Other guarantees

Notes: This figure displays the estimated coefficient β for each horizon h relative to the month of the award $h = 0$ from a monthly version of equation (1). The boxplot displays the coefficient estimate ⋄ and the corresponding 95% (grey) and 90% (orange) confidence bands for the response of each dependent variable on firm credit collateral, as described in the sub-caption, normalized by lagged assets to the monthly amount awarded after winning a procurement contract which is also normalized by the lagged value of total assets. The estimation includes firm and industry×month fixed effects. All standard errors are clustered at the firm level.

This figure shows that the majority of the increase in credit is backed by guarantees provided by the firm where the procurement contract awards in the form of future revenues are included. At month-horizon 4, firm guarantees account for more than 66% of the increase in credit. Moreover, if we take into account state guarantees, cash flow based lending explains approximately 75% of the increase in credit. All variation in the remaining types of collateral are either not statistically or economically significant with the exception for real not mortgaged collateral. The latter points out to an increase in total fixed assets that are now increasingly pledgeable which I test in Section 3.4.

This pattern aligns with previous findings for Spain, supporting the idea that firms use public procurement contracts as collateral to secure credit, as seen in di Giovanni et al. (2024). It also adds to the existing literature, predominantly US-focused, which deals with the collateralization of private revenues (Lian and Ma 2021; Drechsel 2022; Caglio et al. 2022). This evidence collectively suggests that public contracts play a pivotal role in enhancing firms' borrowing capabilities by serving as a guarantee, thereby facilitating increased access to credit.

This finding is particularly relevant for financial stability considerations, especially in the context of the post-Great Recession era characterized by stringent bank capital and collateral requirements for new loans (Degryse et al. 2021). During credit expansions fueled by elevated collateral values, like real estate booms, the economy experiences growth. However, this growth often comes at the cost of depleting the stock of information about existing projects. Collateral-based booms tend to culminate in profound economic downturns and lethargic recoveries due to the dual constraints of insufficient collateral and a dearth of project information, which necessitates time to replenish (Asriyan et al. 2022). Therefore, government procurement could serve as an additional instrument to temper financial fluctuations and mitigate the severity or even the onset of collateral-induced booms.

Two important qualifications should be acknowledged. Firstly, loans underpinned by the firm's personal guarantees may be encumbered with financial covenants. Prior studies have shown that breaches of these covenants empower banks to demand accelerated repayment and may lead to renegotiations resulting in a constricted credit supply (Roberts and Sufi 2009; Chodorow-Reich and Falato 2022). Unfortunately, due to the absence of loan-specific data, the presence and impact of such covenants cannot be ascertained in these loan agreements. Nonetheless, if financial covenants were to influence these outcomes, the results presented here would likely represent a conservative estimate of the actual effect.

Secondly, under the stipulations of Portugal's Public Procurement Code, specif-

ically in *Chapter IX*, certain procurement contracts necessitate that the awarded firms provide a guarantee of up to 5% of the contract's value. This is typically applicable for contracts valued above half a million euros. It's commonplace for public entities to accept a "garantia bancária" or bank guarantee, where the bank extends credit to the firm, secured by the anticipated cash flows from the contract itself. In the context of my analysis, such occurrences would be documented as a rise in used credit, backed by the firm's personal guarantees. Despite this nuance, the central message of this paper still holds: *procurement contracts can be used as collateral to increase firm credit.*

3.4   Public Procurement and Firm Dynamics - Annual Analysis

Building on the earlier findings that firms boost their borrowing in response to securing public procurement contracts, I delve into the broader implications at the firm level. Utilizing the empirical framework detailed in Section 3.1, this analysis extends to corporate investment and additional balance sheet variables. Figure 6 presents all the results.
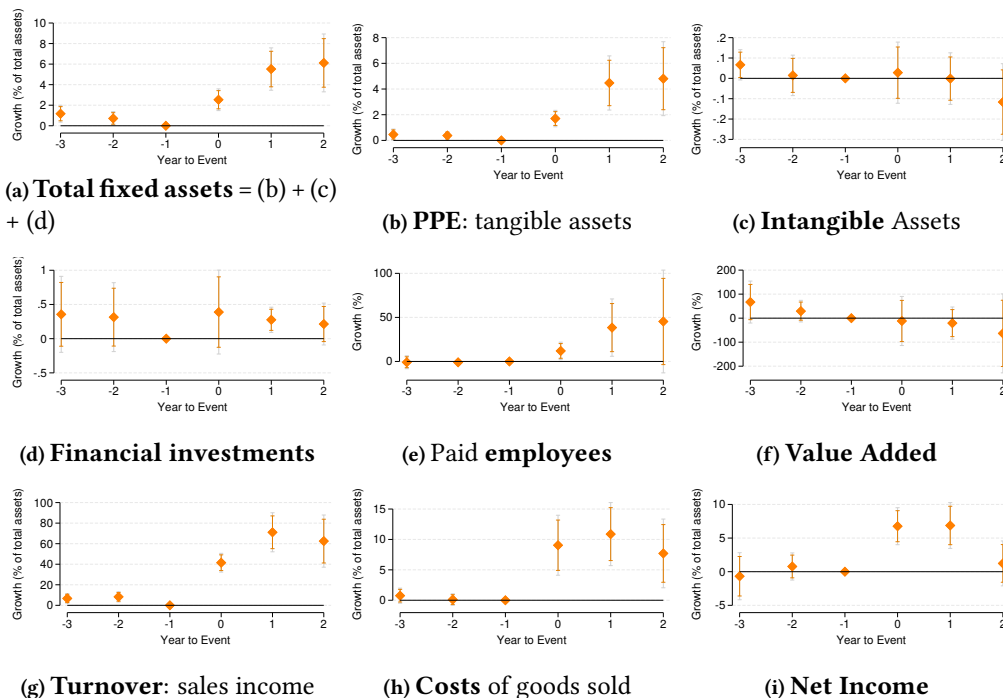
Figure 6 reveals that obtaining an additional euro through procurement contracts leads to a six cents increase in a firm's non-current assets within the first year (Figure 6a). This increase predominantly arises from enhancements in Plant, Property, and Equipment (Figure 6b) rather than from intangible assets or financial investments (Figures 6c and 6d). Notably, there are little to no significant effects in anticipation in the years leading up to the event.

This elasticity corresponds with other findings in the field, such as those reported by Hebous and Zimmermann (2021), who observed a similar increase of about 10 cents associated with procurement awards. Additionally, studies focusing on different forms of collateral, like those by Chaney et al. (2012) and Catherine et al. (2022), which examine real estate collateral, have identified an elasticity of 6 cents per dollar of increased collateral value in real estate.

Moreover, winning a procurement contract induces a persistent increase in employment that goes beyond the first year (Figure 6e). This finding aligns with recent evidence by Ferraz et al. (2021) showing the effects of winning a contract extend well beyond the average contract length of less than a year. However, the increase in employment is not coupled with a corresponding enhancement in value added (Figure 6f).

Additionally, to underscore the unexpected nature of the award, sales income responses, depicted in Figure 6g, exhibit no preceding trends and show a 70% increase in response 1-year post-award. Despite initial expectations of a direct

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Figure 6: The effects of procurement contract awards on other firm dynamics

(a) **Total fixed assets** = (b) + (c) + (d)

(b) **PPE**: tangible assets

(c) **Intangible** Assets

(d) **Financial investments**

(e) Paid **employees**

(f) **Value Added**

(g) **Turnover**: sales income

(h) **Costs** of goods sold

(i) **Net Income**

Notes: This figure displays the estimated coefficient β for each horizon h relative to the year of the award $h = 0$ from equation (1). The boxplot displays the coefficient estimate ⋄ and the corresponding 95% (grey) and 90% (orange) confidence bands for different variables to the amount awarded after winning a procurement contract which is normalized by the lagged value of total assets. Only the variables paid employees and value-added present a growth rate, all other variables have their growth scaled by lagged assets for easier interpretation. The estimation includes firm and industry×year fixed effects. All standard errors are clustered at the firm level.

correlation between a rise in public demand and sales, the observed response is less pronounced, which may be attributed to two underlying factors.

First, the variance in contract duration can distribute revenues over multiple years; even though the median contract duration is under one year, some extend to a decade − *duration effect*. Second, capacity constraints might force firms to prioritize government contracts over others, potentially diverting business to competitors and explaining the observed effects in regional gross value added at the location of spending, as detailed in Section 4 − *capacity constraint effect*. This latter issue could be alleviated if firms engage in sub-contracting to manage the excess demand.

Notwithstanding, these contracts seem to be profitable: even though firms face higher costs of goods sold (Figure 6h), net income is still positive at 5 cents for each euro of procurement award (Figure 6i).

3.5   Heterogeneous Effects

In this subsection, besides firm credit, I focus on two real variables that are of paramount importance to policymakers: investment and employment. Rather than merely quantifying their responsiveness to procurement awards, it's crucial to understand the differential effects that can optimize the policy's overall impact. This analysis underscores the necessity for meticulous planning in the allocation of procurement contracts. When the government's primary objective is to stimulate comprehensive economic growth, it's imperative to consider the nuances of heterogeneity among firms and the enduring nature of these effects.

Table 2 presents the results of estimating the baseline specification (1) for corporate credit, investment, and employment growth. It also presents estimates for a subsample analysis of small and big firms.[4] This analysis highlights important heterogeneities on the transmission of the procurement effects on firm credit, investment, and employment for small and big firms. While there is no statistically significant difference in the response of credit between them, smaller firms react more significantly and more persistently to procurement awards in terms of investment and staff headcount.

Building on the rationale that smaller firms are more likely to be bank-dependent and financially constrained (Beck et al. 2005), Table 2 reports a strong cross-sectional heterogeneity in the sensitivity of investment to the procurement award of "constrained" firms relative to the group of "unconstrained" firms. The coefficient β for small and micro firms is €7.3 compared to -€1.2 for the bigger firms 2 years after the award. The difference between these two coefficients is significant at the 1 percent level and increases with the horizon. Even though bigger firms react positively in the short run, the group of small firms is driving the aggregate response of investment.

The combination of a strong positive response of investment to a procurement award from small firms and a negligible response from large firms is in line with the work by Hebous and Zimmermann (2021). Notwithstanding, my

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[4]For the classification, I follow the Commission Recommendation 2003/361/CE and define small firms when the staff headcount is below 50 and the turnover or balance sheet total is below €10 million.

estimated elasticity for cumulative investment after one year for small firms (5 cents) is smaller than their estimate (13 cents).

The fact that small firms react strongly and more persistently to demand shocks originated from the public sector is an important one. Such a result can be rationalized with the financial accelerator hypothesis: it is expected that more constrained (smaller) firms react more to the same demand shock because they were likely sub-optimally investing in the first place (Bernanke et al. 1999). This latter point resonates to the more general literature on the sensitivity of firms' investment to fluctuations in their internal funds (Fazzari et al. 1988; Moyen 2004). Focusing on firms that do not pay dividends instead of size, this strand argues in favor of the same investment differential response between financially constrained and non-constrained firms.

Table 2: Heterogeneous effects procurement contract awards on firm investment and employment

	Drawn Credit			Investment			Employment		
	Impact	1 Year	2 Years	Impact	1 Year	2 Years	Impact	1 Year	2 Years
Panel A: Baseline Specification									
Elasticity	3.33***	2.64	1.30	2.53***	5.52***	6.12***	11.95**	38.42**	45.36
	(0.79)	(1.75)	(3.69)	(0.54)	(1.05)	(1.44)	(5.06)	(16.60)	(29.75)
Observations	10,152	7,892	5,994	24,613	14,553	10,358	24,579	14,535	10,340
Panel B: Small versus Big Firms									
Small Firms	2.36**	1.85	-3.32	2.87***	4.58***	7.31***	11.82*	30.77***	24.14***
	(1.12)	(2.34)	(3.87)	((0.70)	(1.35)	(1.84)	(6.60)	(8.01)	(7.40)
Big Firms	3.10***	3.41***	0.14	2.83***	1.26	-1.18	-7.80	-12.62	-31.69**
	(1.20)	(1.00)	(1.54)	(0.94)	(1.49)	(1.80)	(7.17)	(9.75)	(15.54)
HAC p-value	0.65	0.51	0.34	0.98	0.05	0.00	0.02	0.00	0.00
Controls	✓	✓	✓	✓	✓	✓	✓	✓	✓
FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
Observations	8,597	6,491	4,655	22,436	12,848	8,861	22,402	12,827	8,845

Notes: This table examines the effect of the procurement award on corporate credit, investment, and employees growth rates. The unit of observation is the firm-year level i, t. The sample period is 2009-2019. In Panel A, I present the baseline results for the coefficient β^h in Eq. (1) for each horizon $h = 0, 1, 2$. In Panel B, I study the differences between small and big firms defined as firms being below or above a staff headcount of 50 and the turnover or balance sheet total of €10 million. The HAC p-value presents the p-value of the difference between states using the heteroskedasticity and autocorrelation consistent test. Robust standard errors clustered at the firm level are in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% level, respectively.

Since capital and labor are complements, financial constraints can also limit firms' employment growth indirectly (Benmelech et al. 2021). Thus, it is possible that the aggregate employment response hides substantial heteronegeneity. I document heterogeneous effects on employment growth in the last 3 columns of Table 2. Financially constrained firms see their employment growing more than their unconstrained peers. This result corroborates the symmetric findings in Giroud and Mueller (2017) who argued that highly levered firms experienced larger employment losses in response to declines in local consumer demand compared to their less levered peers.

Both employment and investment responses resonate with the active policy debate on whether governments should target specific firms when allocating procurement contracts. In particular, it speaks to the recent push by the European Commission and European Parliament to use public procurement to boost small and medium enterprises' growth (Commission 2014; Parliament 2020). The main argument concerns potential efficiency gains. Nevertheless, these results highlight that, on average, small firms react more persistently and therefore it is likely that such a targeting policy will bring positive effects at the aggregate level also in terms of investment and employment. To test such a policy implication, I move to a regional analysis of the effects of public procurement in the next Section.

I document further heterogeneities in Appendix Table C.3. Most importantly, I test whether public procurement could act as a government endorsement, prompting banks to broaden their credit offerings at more competitive rates by comparing the reactions of first-time award recipients to those firms with previous awards. I find that first-time winners in my sample do enjoy significantly higher and more persistent responses in credit, employment, and investment. Thus, providing evidence of a reputation effect partially resolving a banking information asymmetry problem. I also study the differences between firms in the Construction and Medical Equipment sectors – which account for 60% of the value of public procurements in 2019 – and all remaining ones. I find that while these sectors might account for the credit response, both investment and employment display no significant and persistent differences.

4 Regional Effects of Public Procurement

So far, I have documented that public procurement serves as collateral, bolstering firms' credit positions and prompting them to escalate investment. Notably, this amplification of investment by private firms is markedly evident among

smaller enterprises. Consequently, the question arises as to whether such effects manifest on a macroeconomic scale and, if so, the extent of fiscal policy's impact on regional output. To address this, an empirical framework is introduced, elevating the analysis from the micro to the macro level by consolidating individual firm data into a regional context. The ensuing examination assesses the influence of procurement expenditures, drawing on principles from the fiscal multipliers literature.

4.1 Empirical Strategy

This section aims to bridge the micro-macro divide by evaluating whether the positive credit and investment responses at the firm level propagate to broader economic growth. Building on the methodology akin to Gabriel et al. (2023a), the analysis adopts a local projections framework:

$$\frac{\text{GVA}_{i,t+h} - \text{GVA}_{i,t-1}}{\text{GVA}_{i,t-1}} = \alpha_i + \delta_t + \beta^h \frac{\text{Proc}_{i,t}}{\text{GVA}_{i,t-1}} + \psi^h \mathbf{X}_{i,t-1} + \varepsilon_{i,t+h} \quad \forall_{h \in \{0,1,2\}} \quad (2)$$

where $\text{GVA}_{i,t}$ denotes the gross value added in region i for year t. The main variable of interest $\text{Procurement}_{i,t}$ is obtained by aggregating the procurement awards by location of spending which is obtained directly from the web-scraped data and normalized by lagged regional GVA. $\mathbf{X}_{i,t-1}$ are lagged values of both dependent and independent variable growth rates from t-2 to t-1 as standard in the literature (Stock and Watson 2018). I include both region (α_i) and time (δ_t) fixed effects to control for unobservable factors that are constant within regions and for common shocks in a given year, respectively, with the latter controlling for the aggregate macroeconomic conditions and centralized fiscal and monetary policies. To mitigate the bias that could arise from the potential correlation within regional data, standard errors are clustered at the regional level. The immediate multiplier effect, horizon 0, reflects the concept of an open economy multiplier, akin to Nakamura and Steinsson (2014). Estimates for subsequent horizons gauge the direct effect on growth within the region, deliberately excluding the cumulative change in procurement to focus on the sustained growth effect.

Figure 7a illustrates the cross-sectional variation in the main regressor — procurement spending as a percentage of gross value added (GVA)—with data averaged from 2009 to 2019. Additionally, to provide a more granular understanding, Figure 7b elucidates the total procurement spending in millions of euros, while

Figure 7c breaks down the spending per capita. These visualizations underscore the presence of substantial cross-sectional variation within Portuguese regions, which forms the empirical basis for estimating the impact of local procurement activities.

Figure 7: Cross-sectional variation in procurement spending

(a) $\frac{\text{Proc}_{i,t}}{\text{GVA}_{i,t-1}}$ (in %)

(b) Procurement (mio. €)
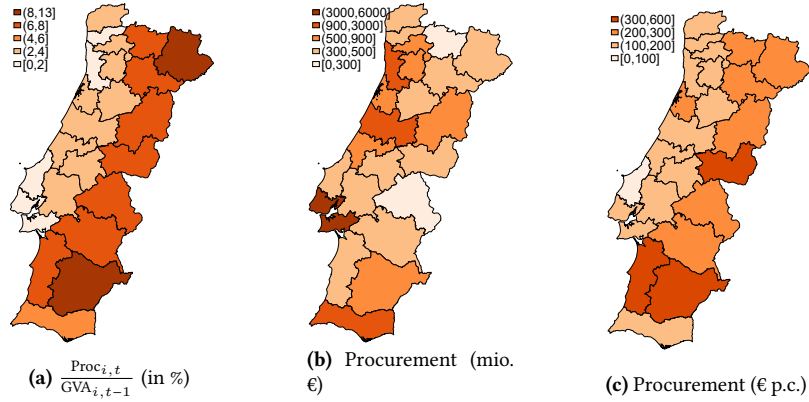
(c) Procurement (€ p.c.)

Notes: This Figure displays the cross-sectional variation across continental Nuts III Portuguese regions on the amount of procurement awarded via public contests. From left to right, maps display: (a) the main regressor in equation (2) $\frac{\text{Proc}_{i,t}}{\text{GVA}_{i,t-1}}$ averaged across the sample period 2009-2019; (b) the total amount of procurement spending in million euros; and (c) the yearly per capita value of procurement spending.

The coefficient of interest β^h can be interpreted as the open economy relative multiplier estimates for horizon $h = 0$ and captures the effect of higher procurement spending in one region relative to other regions on the region's relative output. Estimating specification (2) yields an unbiased estimate of β if the no-omitted variable bias assumption is satisfied for all horizons. The identification assumption is satisfied if the allocation of this procurement spending is unanticipated by a region and uncorrelated with its macroeconomic performance. The average duration of the contest from the announcement until the decision of the winner amounted to 4 months in 2020 (IMPIC 2021) and hence, it is unlikely that regions react one year in advance because they are unaware of the public contests.

To estimate the causal impact of credit on local procurement outcomes, it is necessary to refine specification (2) by incorporating an interaction term with firm credit, as suggested by Basso and Rachedi (2021). The modified panel regression to be estimated is as follows:

$$\Delta\text{GVA}_{i,t+h} = \beta^h\text{Proc}_{i,t} + \gamma^h\text{Proc}_{i,t} \times \Delta\text{C}_{i,t} \times 100 + \omega^h\Delta\text{C}_{i,t}$$
$$+ \alpha_i + \delta_t + \psi^h\mathbf{X}_{i,t-1} + \varepsilon_{i,t+h} \quad \forall_{h\in\{0,1,2\}} \tag{3}$$

where I simplify the notation for clearness such that for any variable Var, we have $\Delta\text{Var}_{i,t+h} = \frac{\text{Var}_{i,t+h}-\text{Var}_{i,t-1}}{\text{GVA}_{i,t-1}}$, the key regressor is also scaled by lagged GVA, and I include lags of the interacted variable as well. Importantly, I aggregate the enhanced credit amounts, post-award, according to the regional i geographical headquarters exclusively of those firms securing procurement awards into the variable $\text{C}_{i,t}$.

4.2 Results

Table 3 presents the estimated coefficients for the impact of procurement spending on regional economic output, differentiated by the immediate effect and over two subsequent years. In Panel A, we observe that a unit increase in procurement spending is associated with a statistically significant increase in the gross value added (GVA) by €1.32, suggesting a positive fiscal multiplier effect. The coefficients indicate that the impact is sustained over time but with varying magnitudes. This implies a crowding-in effect with a €1 increase in relative government production leading to a €0.32 increase in relative private sector production on impact.

In Panel B, the interaction between procurement spending and credit showcases the additional influence of the credit channel on the fiscal multiplier effect. The coefficient for procurement spending remains positive and significant across all horizons, implying that procurement has a persistent stimulatory effect on the economy.

The interaction term's positive and significant coefficients suggest that when combined with an increase in credit, procurement spending has an amplified effect on economic growth. At the initial horizon (h=0), a 1% increase in credit relative to GVA enhances the procurement multiplier by 11%. Given that the total credit from winning firms across regions is about 0.5% of GVA, the credit alone would raise the local procurement multiplier by $(0.11 \times 0.5)\%$, a 5.5% increase.[5]

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[5]I acknowledge the caveats that (i) this measure of credit might not account entirely for the cumulative response of credit and (ii) even though I documented a strong relationship between the interacted variables, the credit increase might be accounted for factors other than procurement contracting.

Table 3: The regional effects of procurement spending

	Horizon (Year)		
	(0)	(1)	(2)
Panel A: Local procurement effects			
GVA	1.32***	1.12**	1.51***
	(0.45)	(0.47)	(0.48)
Panel B: The credit channel of public procurement			
Proc	1.39***	1.27***	1.68***
	(0.42)	(0.43)	(0.45)
Proc * Credit	0.11***	0.22***	0.24***
	(0.04)	(0.06)	(0.08)
Credit	−0.37	−0.76**	−0.66
	(0.24)	(0.31)	(0.45)
Controls	✓	✓	✓
FE	✓	✓	✓
Observations	159	159	159

Notes: The unit of observation is the region-year level i, t. In Panel A, I estimate Equation (2). I present the results for the coefficient β^h for each horizon $h = 0, 1, 2$. In the first row of both panels, the coefficients can be interpreted as the response of regional production (proxied by gross value added) from period $t + h$ relative to period $t - 1$ to regional procurement spending aggregated at the spending location. I use a matched sample period from 2010 to 2016 (25 regions × 6 years) so that differences in the coefficients over the horizon can't be associated with sample changes. I winsorize the key independent variable Procurement at the percentile 95 as these results are sensitive to the inclusion of outliers. Robust standard errors clustered at the region-level are in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% level.

However, the negative coefficient on credit indicates that without the interaction with procurement spending, an increase in credit alone may not contribute to growth and might even be associated with a contraction.

This analysis relies on the aggregation of micro-level data to capture the regional-level effects, providing a link between firm-level responses to procurement contracts and the larger regional economic outcomes. However, as my coverage of total procurement spending is incomplete, these coefficient estimates should be interpreted with caution. Notwithstanding, these findings contribute to the literature by demonstrating the significant role that public procurement can play in regional economic development and the potential for credit to amplify these effects.

Understanding the procurement effects using national accounts data.
To gain deeper insights into the macroeconomic implications of local procurement, this section examines the responses of various economic indicators using national accounts data. These exercises aim to contextualize the micro-level findings within the broader economic landscape and assess the overall efficacy of procurement as a policy tool for regional development. Table 4 presents the results.

Table 4: The regional effects of procurement spending on other regional aggregates

	Horizon (Year)		
	(0)	(1)	(2)
Private Value Added	1.17***	0.76	1.02***
	(0.27)	(0.48)	(0.31)
Private Investment	3.49*	1.37	0.11
	(1.88)	(1.22)	(1.07)
Private R&D	0.63**	0.35	−0.09
	(0.28)	(0.28)	(0.27)
Private Employment	0.19**	0.35**	0.58**
	(0.09)	(0.15)	(0.24)
Private Compensation	0.50*	0.60**	0.81***
	(0.28)	(0.24)	(0.29)
Private Credit	−0.06	0.24	−0.06
	(0.24)	(0.45)	(0.32)
Price Inflation	0.04**	0.07**	0.02
	(0.02)	(0.03)	(0.03)
TFP	−0.21	−0.15	0.19
	(0.34)	(0.45)	(0.61)
Controls	✓	✓	✓
FE	✓	✓	✓
Observations	159	159	159

Notes: The unit of observation is the region-year level i, t. I estimate Equation (2) adjusting the left-hand side variable as per the first column. I present the results for the coefficient β^h for each horizon $h = 0, 1, 2$. I use a matched sample period from 2010 to 2016 (25 regions × 6 years) so that differences in the coefficients over the horizon can't be associated with sample changes. I winsorize the key independent variable Procurement at the percentile 95 as these results are sensitive to the inclusion of outliers. Robust standard errors clustered at the region level are in parentheses. *** , ** , and * denote statistical significance at the 1%, 5%, and 10% level.

The first row indicates a boost in private value added, akin to the GVA uptick from Table 3, marking a €1.2 increase on impact. Notably, private investment in RD registers a pronounced uptick, suggesting procurement's pivotal role in bolstering innovation within the private sector; however, this surge does not translate into enhanced total factor productivity as per the available TFP metrics.

In contrast, the aggregate credit response to nonfinancial firms remains subdued, hinting at a potential redistribution of credit in favor of firms that secure procurement contracts, as indicated by complementary micro-level data. This suggests that while procurement initiatives boost firms-level credit, they may not lead to a broader credit expansion in the regional economy indicating a potential redistribution of credit by banks toward procurement-winning firms. The inflationary uptrend, though modest, aligns with the short-term effects of increased demand from procurement activities. This initial surge in price levels may reflect the immediate economic stimulation but is not long-lived.

5 Conclusion

This study provides an analysis of the role public procurement contracts play in affecting corporate finance, particularly through the lens of credit enhancement. It is observed that public procurement has a discernible influence on corporate credit, with a one euro increase in procurement leading to a three-cent increase in firm credit within 4 months of the award. It is noteworthy that this increase is predominantly supported by cash flow based collateral, highlighting a shift towards reliance on projected cash flows and firm-based guarantees.

Further investigation into the firm-level impacts reveals that procurement awards are associated with increased investment and employment, suggesting an absorption of the fiscal input into substantive economic activity. The analysis quantifies this impact, finding a six-cent increase in investment for every additional euro of procurement, which may indicate a crowding-in effect of private investment as a response to public procurement initiatives.

At a regional and macroeconomic scale, the paper examines the extent to which public procurement spending influences gross value added. An increase of one euro in procurement spending correlates with a 1.3 euro increase in regional output, with the credit channel of public procurement estimated to account for 5% of that effect. These findings suggest that procurement not only stimulates direct economic activity but also acts as a catalyst for broader fiscal multiplier effects within regions with credit playing a non-negligible role.

These results show substantial cross-sectional variation which yields two key policy implications. First, the procurement contracts' design might be refined to amplify desired economic outcomes, for example by targeting smaller firms more likely to be financially constrained. Second, the ability of firms to leverage procurement contracts as a form of collateral introduces a nuanced dynamic into the credit market, potentially enhancing financial stability by diversifying the types of collateral used by firms to obtain credit.

Acknowledgements

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Supplemental Material

Appendix A   Data

This paper uses data from *Banco de Portugal*. The firm-level data was accessed via the *Banco de Portugal* Microdata Investigation Laboratory (BPLIM) and the variables' description can be found in Table A.1 and the summary statistics in Table A.2. This detailed dataset was then merged to the procurement contracts information. The application procedure to get access to this data can be found in BPLIM's website and the replication materials for the micro-level analysis can be requested at that time. The procurement contracts information that was web scraped as follows.

Table A.1: Variable Definitions

Variable Name	Definition
Panel A: Firm Characteristics	
Total Fixed Assets	Fixed tangible and intangible assets; Financial investments; Remaining non-current assets
PPE	Fixed tangible assets: Land and buildings; Basic equipment; Other fixed assets; Payments on account of fixed assets
Financial Investments	Investments in subsidiary and associated companies and other financial investments
Total Liabilities	Current liabilities: Suppliers; Obtained funding. Non-Current liabilities: Obtained funding; Post-employment benefits. Other liabilities
Liquidity	Current assets: Cash and bank deposits
Turnover	Sales of goods and products and Sales of services
Employees	Number of paid employees
Hours Worked	Number of hours worked by paid employees
Wages	Employee expenses: Salaries; Social security expenses; Insurance schemes for accidents at work and occupational diseases; Expenses with social actions; Post-employment benefits; Indemnities
Value Added	Difference between sales (turnover plus remaining income) and production costs (i.e., costs of goods sold and material consumed plus cost related to supplies and external services and indirect taxes)
Panel B: Firm Credit	
Total Credit	Total available credit that a firm can access
Drawn Credit	Effective credit that a firm used effectively
Undrawn Credit	Potential credit that a firm can access because of irrevocable commitments of the participating entities. E.g.: lines of credit; unused amounts of credit cards
Non-Performing Credit	Non-performing credit of a firm
Short-term Credit	Credit with an original maturity of less than or equal to 1 year
Long-term Credit	Credit with an original maturity of more than 1 year
Implicit Interest Rate	Ratio of Interest Expenses (t) to Effective Credit ($t-1$)
Panel C: Firm Collateral	
Real Mortgaged	Credit secured by real collateral mortgaged
Real Not Mortgaged	Credit secured by real collateral not mortgaged
Financial	Credit secured by financial collateral
Firm Guarantee	Credit secured by personal guarantee provided by firm. E.g. future cash flows
State Guarantee	Credit secured by personal guarantee granted by the state or financial institution. E.g. subsidies.
Other	Credit secured by other guarantees

Notes: This table reports the variable definitions in my sample. More detail about the variables and its construction for firm characteristics and credit variables can be found in BPLIM (2021) and BPLIM (2019) respectively. More detailed information on firm collateral information can be found here.

Table A.2: Summary statistics for each group of firms

	Procurement Firms						No Procurement Firms					
	Mean	Std. Dev.	P10	Median	P90	Obs	Mean	Std. Dev.	P10	Median	P90	Obs
Total fixed assets	8,718	186,494	3.6	155.5	3,608	35,675	846.7	45,093	0	17.5	404.0	2,773,894
Turnover	14,283	160,374	120.6	1,061	14,725	35,675	928.0	17,114	7.7	108.2	1,028	2,773,894
Liquidity	17.1%	19.8%	0.8%	9.6%	45.0%	35,674	19.3%	7%	0%	0%	1%	2,771,346
Total liabilities	10,392	155,258	49	537.5	8,193	35,675	955.4	36,111	9	88	830	2,773,894
Employees	85	525	2	12	107	35,635	8	80	1	3	13	2,773,054
Wages per worker	20.3	14.5	9.7	17.1	33.8	35,229	13.2	11.8	6.5	10.9	21.7	2,536,065
Award	561.5	2,714	8.8	74	977.3	35,675						
Total Credit	4,844	27,003	18	475	7,200	13,984	454	6,126	1	29	469	1,473,060
Effective Credit	2,284	12,863	0	196	3,623	13,984	352	4,489	0	20	374	1,473,060
Potential Credit	2,560	16,154	2	141	3,065	13,984	102	3,033	0	2	65	1,473,060
Non-performing Credit	80	1,867	0	0	0	13,984	23	992	0	0	0	1,473,060
Real Col. Mortgaged	399	5,005	0	0	250	13,984	109	1,750	0	0	66	1,473,060
Real Col not Mortgaged	177	2,962	0	0	20	13,984	34	1,623	0	0	0	1,473,060
Financial Col.	336	4,635	0	0	123	13,984	65	2,661	0	0	8	1,473,060
Personal guarantee Col.	944	5,844	0	58	1,666	13,984	154	1,595	0	6	193	1,473,060
State guarantee Col.	204	1,447	0	0	444	13,984	24	453	0	0	22	1,473,060
Other Col.	336	3,964	0	0	64	13,984	36	1,420	0	0	0	1,473,060
Implicit interest rate	6.9%	5.4%	1.4%	5.1%	18.1%	11,659	6.5%	5.3%	0.9%	4.9%	18%	1,026,881

Notes: This table presents the summary statistics for the key firm-level variables in this paper dividing them in firm-year observations when a firm won a public contest vs when a firm lose or did not participate in public contests. All economic variables are in thousand euros.

Appendix A.1 Web Scraping: Assembling the Procurement Data

The data on public procurement comes from scrapped *html* files from the link www.base.gov.pt. I scraped detailed information on public procurement contracts between 2009 and 2019. Each contract has the same structure with all the information being organized in a table similar to Figure A.1. For each contract, I can scrape the following key information: (i) the tax ID of the winner, of the hiring entity, and of bidders in public contests if any; (ii) the contract announcement and contracting dates; (iii) the contract value/award; (iv) the execution place and duration; and finally (v) whether the contract was directly awarded or awarded after a public contest.

Figure A.1: Scraped table example

Publication date	07-06-2022
Description	Concurso Público nº 1030/2022 - Aquisição de desinfetantes - Álcool e Acetona
Contracting entities	Centro Hospitalar Universitário do Porto, EPE. (CHP) (508331471)
Contracted entities	Proclinica.Eq.Pr.Clinicos, Lda (500222665)
CPVs	33690000-3
Contract date	01-06-2022
Contract value	46.116,48 €
Execution deadline	365 dias
Execution place	Portugal, Porto, Porto
Competing entities	DIMOR LUSITANA, LDA (500730741), ENZYMATIC, S.A. (510662625), ESTERIPLAS (502020776), PROCLINICA (500222665), PMH,SA (502376899), VWR INTERNATIONAL - MATERIAL DE LABORATÓRIO, SOC. UNIPESSOAL, LDA. (503842770)

The scraped files contain 1,035,232 contracts, from which 138,561 were public contests. I proceed by filtering the data. First, I drop contracts with a negative reported price. Then, auctions with more than one winner which are rare and happen when the government allows the lot to be divisible. I then aggregate these data to the firm-year level, creating a panel dataset with the total contract awards and obtaining 38,707 such observations. I merge this panel with the cleaned firm-level panel data described in section Appendix A and obtain 38,246 instances were I can match fully the information, the dropped observations are either invalid tax ids with less than 9 digits or foreign firms. Then, I keep only the private non-financial corporations, non-liquidated, with at least one paid worker

and assets above the percentile 1 given its usage as a scale variable in my main analysis, yielding a total of 35,675 observations which is the baseline procurement dataset. Table A.3 provides detailed information on the sample cleaning.

Table A.3: Public procurement contracts: sample cleaning

Step	Description	Observations
0	Web scraped contracts	1,035,232
1	Keep contests from 2009 until 2019	1,033,697
2	Keep public contests	138,561
3	Keep positive awards	137,841
4	Keep contracts with solely one winner	134,976
5	Collapse same year awards	38,707
6	Merge with Portuguese tax information	38,246
7	Keep private non-financial corporations	38,172
8	Keep only non-liquidated firms	36,931
9	Keep only firms with available information on lagged assets	36,930
10	Keep only firms with lagged total assets above p1 (€1302.67)	36,905
11	Keep only firms with at least one paid employee	35,675

Notes: This table presents detailed information on at each stage of the data cleaning process.

Some challenges about the scraping exercise are worth noting. As the hiring entity is responsible to provide the information on the e-platform, it is natural that for the initial years the coverage is worse. For example, the information on the contestants of public contests that is missing for half of the contests in my sample comes mainly in the initial years of the sample 2009-2012, exacerbating the coverage unbalance exposed in Figure B.2. Moreover, information on the award might be biased downwards as some entities display the *per unit* or *monthly* price of the contract. These outliers are likely produced by errors in data entering, where one can find contracts with *per unit* prices starting at €0.01 up to contracts in the trillion euro range that, after further individual consultation, were actually in the million euros range. Due to the latter, in the working paper version, I winsorized contracts with abnormal winning bids at percentile 1 and 99, and all results go through qualitatively.

Appendix A.2   Aggregate Data

In section 4, I use regional data. Table A.4 gives further details on their definition and source.

Table A.4: Variable Definitions: Aggregate Data

Variable Name	Definition	Source
Gross Value Added	Gross output of nonfinancial corporations less the value of intermediate goods and services consumed in production (NUTS III)	PORDATA
Private Value Added	Difference between sales (turnover plus remaining income) and production costs of all nonfinancial corporations aggregated at the Nuts III level	BPLIM (2021)
Investment	Private sector gross fixed capital formation (NUTS III)	PORDATA
CPI	Consumer Price Index (NUTS II, 2010=100)	PORDATA
GERD	Private sector expenditure on R&D (NUTS II)	Eurostat
Private Employment	Number of active workers in the private sector (NUTS III)	ARDECO
Private Compensation	Total Private Compensation (NUTS II)	ARDECO
Population	Number of inhabitants (NUTS III)	PORDATA

Notes: This table reports the variable definitions in my sample for the aggregate analysis for which I collected aggregate data.

There are two important adjustments I need to mention. First, I adjust all these variables to real terms by using the CPI series with the base year of 2015. Thus, all results should be interpreted in 2015 euros. Second, some series were only available at the NUTS2 level (corresponding to 7 Portuguese regions), I thus make the assumption that they are the same for each NUTS3 region belonging to the same NUTS2 region. For the remaining variables, I weigh them by the GVA share of the NUTS3 region for each NUTS2 region.

To construct a measure of total factor productivity, I follow Gabriel et al. (2023a). I make use of capital stock (K) estimates from Gardiner et al. (2020) which construction hinges on the perpetual inventory method using regional investment series from ARDECO and data from EU KLEMS for the national depreciation rate and national initial capital stock. TFP is then calculated as a residual with a labor share of two-thirds as is common in the literature:

$$TFP_{i,t} = exp\Big(ln(GVA_{i,t}) - 1/3 \times ln(K_{i,t}) - 2/3 \times ln(L_{i,t})\Big) \qquad (A.1)$$

where GVA is private total gross value added, K is the private capital stock adjusted to constant 2015 EUR using the GDP deflator, and L is private total hours worked. All variables are measured at the regional level i and at year t. I take the exponential of this expression to compute TFP growth rate in the exact same way as I compute it for the remaining variables, instead of taking log differences.

## Appendix B Procurement Contracting in Portugal

The current Portuguese procurement system was largely shaped by the adoption of the Public Procurement Code in 2008 - *Law number 18/2008* - by transposing the EU Directives 2004/17/CE and 2004/18/CE. In total, all more than 4,000 Portuguese public entities such as the national government, local governments, and public institutions are subject to it. Every time one such entity wants to buy a product or service, irrespective of the price stamp, they need to go through this system.

On September 11, 2009, Portugal became one of the first countries in the European Union to make *electronic* procurement mandatory following the approval of the *Law number 223/2009*, that covered the majority of the tenders. All purchases of goods and services and public works needed to be announced and enforced electronically via web platforms developed by private firms and certified by the

Centro de Gestão da Rede Informática do Governo.[1] In theory, all contracts *must* be announced online at *Portal BASE* in order to be enforced and its information is then managed by the Portuguese Institute of Public Markets, Real Estate and Construction (IMPIC).

Following 2009, Portugal was the leader in the European e-procurement scene, making information available to all citizens for more than 60% of all the contracts, and more than 90% of the major contracts. The reader can find detailed information on IMPIC's reports on Portuguese Electronic Procurement here.

Figure B.1: E-procurement rates
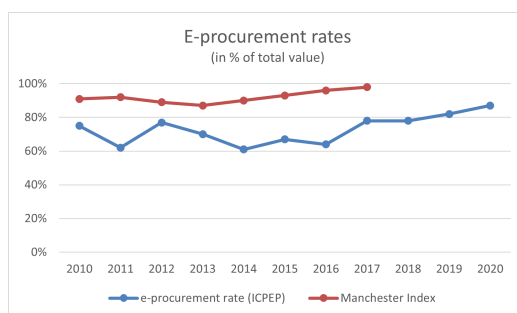
Notes: The graph plots the percentage of public procurement contracts that were available electronically (in % of total procurement value) according to IMPIC reports and, following the Manchester Index, the percentage of contracts above the EU threshold (€75,000 (goods and services) and €150,000 (works)).

The total value of procurement contracts relative to GDP in my sample has been increasing over time due to the increase in contract coverage. In 2019, I was able to scrape information on contracts worth more than 5% of GDP (Figure B.2) representing almost 50% of total procurement spending according to OECD (2021).

In Portugal, there are several types of procurement contract awards. The two main categories are the so called **direct award** in which a public hiring entity can directly choose the entity to provide the contract to and the **public contest** in which firms anonymously compete for the same contract. In 2019, 89% of all the procurement contracts were directly awarded, while 11% were awarded after

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[1]The exceptions being: i) less than €5000 contracts; ii) tenders from international conventions, work contracts, or goods donations,... iii) in-house hiring of financial or health services; iv) tenders from special sectors such as water, energy, and transportation.

Figure B.2: Data coverage (% GDP)

Procurement Data Coverage
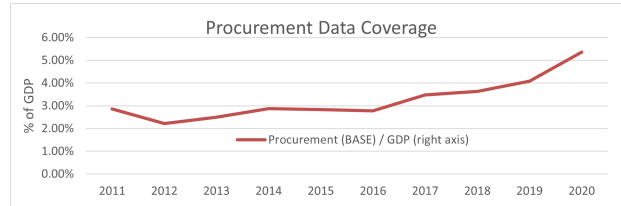
Notes: The graph plots the ratio between the total value of the public procurement awards that were web scrapped from the official Portuguese e-procurement website and the national nominal GDP (red, right axis).

a public contest. Even though the majority of the contracts is directly awarded, throughout my sample, both categories were equally important in terms of value as Figure B.3 displays.

Figure B.3: Procurement by award type

Procurement by Type of Award
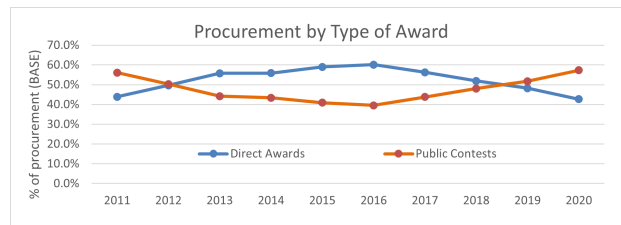
Notes: The graph plots the percentage of public procurement awards (by value) that were directly awarded (blue) or awarded after a public contest (red).

These contracts have other important characteristics that make them unique within the literature studying procurement contracting. Focusing on the year of 2019, we can decompose the contracts' value and number within different categories and highlight different characteristics.

First, in 2019, the majority of the contracts was awarded to Portuguese firms and started in the same year. 98.1% of all contracts were awarded to Portuguese firms, while 1.3% to EU firms, and the remaining 0.6% to fora-EU countries. Second, 79% of all contracts that started in 2019 were awarded in 2019, while 19% was awarded in 2018, and the remaining 2% before 2018. Third, as we can see in table Table B.1, in 2019, both the number and value of procurement contracts was almost equally distributed between small and medium enterprises (SMEs) and big firms.

Finally, it is also important to notice that, contrarily to common wisdom,

Table B.1: Who received procurement contracts in 2019?

Firm Size	Number	Value
Micro	28.3%	9.9%
Small	31.0%	21.1%
Medium	22.7%	28.6%
Big	18.1%	40.4%

Notes: This table presents statistics for the award of public procurement contracts by firm size. Micro firms have at most 10 workers and €2 million in revenues; Small firms up to 50 workers and €10 million; Medium firms up to 250 workers and €50 million in revenues; Big firms comprise all the others.

not all public works come from the construction sector. Table B.2 shows that even though the construction sector accounts for one third of total procurement spending in public works, there also other important industries such as medical equipment, business services, and petroleum products. This characteristic is fundamentally important to add external validity to this project because it allows me to generalize my findings on how procurement spending propagates to the whole Portuguese economy.

Table B.2: Which economic sectors received procurement contracts in 2019 and 2018?

CPV	Description	2019 Number	2019 Value	2018 Value
45	Construction	12.9%	42.5%	32.5%
33	Medical equipment, pharmaceuticals and personal care products	40.1%	17.7%	18.6%
09	Petroleum products, fuel, electricity and other sources of energy	2.2%	7.1%	6.2%
79	Business services: law, marketing, consulting, recruitment	12.3%	7.0%	5.9%
90	Sewage, refuse, cleaning and environmental services	4.0%	5.9%	4.2%
72	IT services: consulting, software development, and support	6.4%	4.7%	3.9%
34	Transport equipment and auxiliary products to transportation	3.9%	4.4%	2.5%
50	Repair and maintenance services	8.0%	3.9%	3.1%
71	Architectural, construction, engineering and inspection services	7.9%	3.7%	3.3%
55	Hotel, restaurant and retail trade services	2.3%	3.1%	5.1%

Notes: This table presents statistics for the award of public works by firm industry in 2019 and 2018.

Appendix C   Additional Results

Appendix C.1   Identification assumption and robustness checks

One challenge facing the main identification strategy at the firm level is that, conditional on participation on public contests, firms that win a procurement contract can be inherently different from those that do not. For example, firms might be more willing to bid for government contracts if their private sector demand is weaker. While the industry×year fixed effects and the firm fixed effects largely address such potential scenarios and unobserved heterogeneity, *a priori*, selection bias might still be a concern.

First, I look at public contests with exactly two contestants and assess whether winners and losers differ systematically from one another in order to support the validity of the key identification assumption: winning a procurement contract via a public contest is not systematically predicted by other firm-level characteristics. Table C.1 shows dramatically similar means and medians for all firm characteristics evaluated the year before the contest. Moreover, column (T-test) provides the p-value of the two sample t-test for whether the difference in each characteristic between the winner and the loser for each contest is equal to zero. If anything, only lagged assets and liabilities seem to differ, in line with Figure C.1. Hence, conditional on these characteristics the assumption holds.

Given this information, a natural starting point for my analysis would be a difference-in-differences (DiD) estimator at the contract-firm-year level. According to Goodman-Bacon (2021), a two-way fixed effects DiD estimator would not be appropriate if already-treated units act as controls given the time-varying nature of my "treatment" of winning a public contest. Hence, estimating the effect at the firm-contract level after removing already-treated firms from the control group for any other contract is a sufficient condition for unbiased estimates of the treatment effect.

A valid exercise in the setting of public contest is then the comparison between winners and contest participants that allow me to robustly address potential selection bias considerations and to make the **parallel trends assumption** more likely to hold. For roughly 10% of the baseline sample, I know which firms applied to each public contest and I can then explore the group of participants firms which lost. Nevertheless, I have no information about their bids and so, it is not possible to identify who was the runner-up for each contest. Hence, I will use the entire group of participants as the control group.

The recent DiD literature has shown that even if the parallel trends assumption holds, there can still be negative weights bias due to unclean comparisons.

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Figure C.1: Correlation between winning a public contest and predetermined firm characteristics
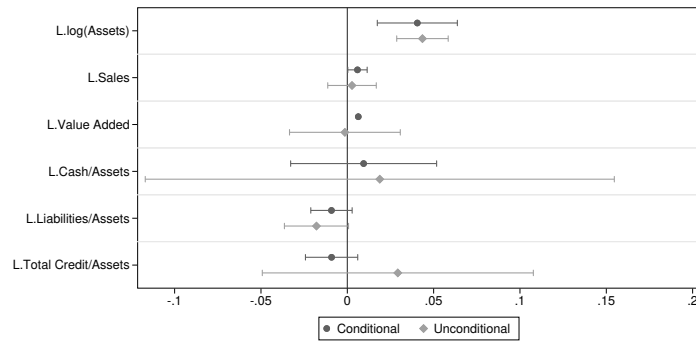
(a) Real variables
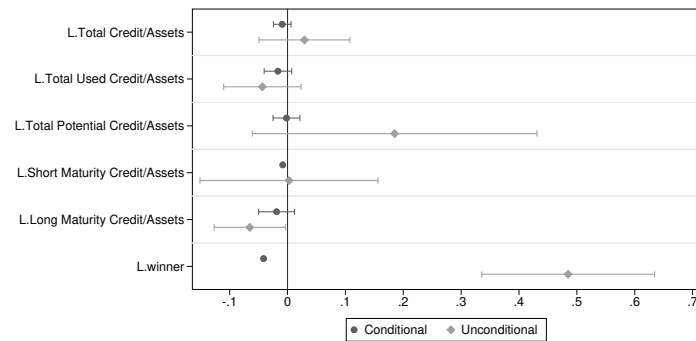
(b) Financial variables

Notes: These figures show the correlation between winning a procurement contract and firm characteristics measured at $t-1$. Regressions are not weighted. All regressions include industry $\times$ year fixed effects. The dot is the point estimate and the bar is the 95% confidence interval. All standard errors are clustered at the firm level.

Table C.1: Winners versus Losers of Public Contests

	Winners		Losers			
	Mean	Median	Mean	Median	**T-test**	**Obs**
Firm Balance Sheet						
Assets	€ 113,922	€ 2,183	€ 95,631	€ 2,250	0.18	7,486
Sales	€ 95,241	€ 2,359	€ 79,683	€ 2,366	0.14	7,486
Value Added	€ 18,266	€ 662	€ 17,585	€ 682	0.86	7,486
Employees	200	20	218	21	0.10	7,485
Firm Age	22	19	22	20	0.30	7,486
Liquidity	15.9%	8.9%	15.8%	9.0%	0.78	7,486
Total Hours Worked	344,213	35,218	370,557	36,596	0.15	7,420
Liabilities	€ 89,339	€ 1,181	€ 75,252	€ 1,225	0.22	7,486
Firm Credit Info						
Total available credit	€ 9,469	€ 548.8	€ 11,516	€ 502.8	0.43	3,101
Total used credit	€ 5,649	€ 199.2	€ 7,741	€ 174.2	0.39	3,101
Total potential credit	€ 3,819	€ 166.8	€ 3,775	€ 168.3	1.00	3,101
Short maturity credit	€ 2,457	€ 8.3	€ 3,890	€ 8.8	0.29	3,101
Long maturity credit	€ 3,191	€ 105.6	€ 3,850	€ 85.0	0.62	3,101

Notes: This table compares characteristics of firms in (thousands of euros) that either won (winners) or lost (losers) public contests for government procurement contracts. The panel is based on the *firm-level data on public contests' contracts with exactly 2 contestants* at the year before the contract award. The table reports the number of observations, mean, median, and the p-value of the two-sample t-test for whether the difference for each characteristic between the winner and the loser for each contest is equal to zero.

To avoid this, I apply a **clean control condition** conditional by only using firms that did not win any other procurement contract in my sample.

To be consistent with the baseline analysis, I use a local projections difference-in-differences (LP-DiD) strategy recently put forward by Dube et al. (2023) and estimate:

$$\frac{y_{i,t+k} - y_{i,t-1}}{y_{i,t-1}} = \beta^k \mathcal{I}(Winner_{i,t}) + \delta_t^k + \sum_{j=1}^{p} \gamma_j^k y_{i,t-j} + \epsilon_{i,t}^k \quad \text{for } k \in \{-2, ..., 2\} \quad \text{(C.1)}$$

where the dependent variable y change is of firm i at year t. $\mathcal{I}(\text{Winner}_{z,i,t})$ is an indicator variable taking the value of one if firm i is a winner at year t and zero otherwise. The key assumption is that the trend in the outcome variable for

both treatment and control firms for each contract during the pre-treatment period is similar. In other words, in the absence of treatment, the average change in the response variable would have been the same for both the treatment and control groups. I explicitly test for it by including pre-event years in the estimation horizon k. Figure C.2 reports the results.

The results are qualitatively similar to the baseline ones with credit investment, and employment growth being positively associated to winning a procurement contract. The anticipation effects are not present, with the exception for the number of employees, thus corroborating the summary statistics on the difference of firm characteristics between the winners and losers in Table C.1 which shows similarity before the event between the two groups of firms in the relevant covariates as well as the outcome measures.
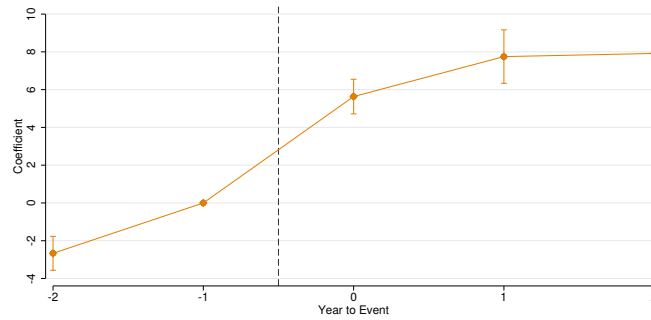
Figure C.2: Effects of public procurement using contest losers as counterfactual
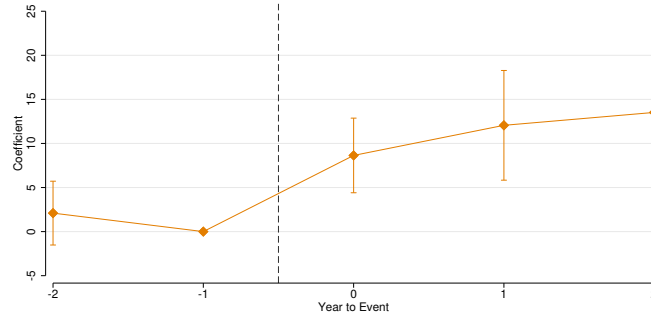
(a) Credit
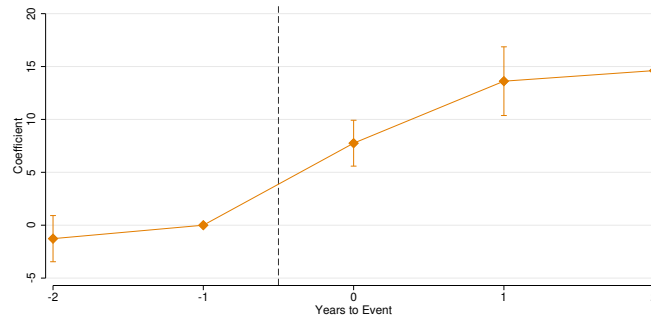
(b) Investment

(c) Employees

Notes: This figure displays the estimated coefficient β for each horizon k relative to the year of the award $k = 0$ from equation (C.1). The boxplot displays the coefficient estimate $\diamond$ and the corresponding 95% (grey) and 90% (orange) confidence bands for the response of firm credit relative to total assets in the previous year to winning a procurement contract. The estimation includes firm and industry×year fixed effects. All standard errors are clustered at the firm level.

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Appendix C.2   Supporting Tables

Table C.2: Credit, Interest Rates, and Cash responses

		Horizon	
	Impact	1 Year	2 Years
Panel A: Total Credit Response			
Total Credit	6.99***	5.59*	4.30
	(1.03)	(2.91)	(4.03)
Drawn Credit	3.33***	2.64	1.30
	(0.79)	(1.75)	(3.69)
Undrawn Credit	3.67***	2.95**	2.99*
	(0.75)	(1.47)	(1.64)
Observations	10,152	7,892	5,994
Panel B: Cash			
Cash	5.85***	8.80***	5.02**
	(1.13)	(2.42)	(1.36)
Observations	24,613	14,553	10,358
Panel C: Interest Rates			
Interest Rate	-0.29**	0.02	-0.37
	(0.13)	(0.27)	(0.41)
Observations	7,979	6,082	4,522
Controls	✓	✓	✓
FE	✓	✓	✓

Notes: This table examines the effect of the procurement award on corporate credit. The unit of observation is the firm-year level i, t. The sample period is 2009-2019. In Panel A, I present the baseline results for the coefficient β^h in Eq. (1) for each horizon $h = 0, 1, 2, 3$ corresponding to Figure 1. β^h is the *cumulative* response of credit from period $t + h$ relative to period $t - 1$ to the procurement award also normalized by lagged total assets. Standard errors are clustered at the firm level. ***, ** and * indicate statistical significance at the 1%, 5% and 10% levels, respectively.

Table C.3: Further heterogeneous effects procurement contract awards on firm investment and employment

	Drawn Credit			Investment			Employment		
	Impact	1 Year	2 Years	Impact	1 Year	2 Years	Impact	1 Year	2 Years
Panel A: Baseline Specification									
Elasticity	3.33***	2.64	1.30	2.53***	5.52***	6.12***	11.95**	38.42**	45.36
	(0.79)	(1.75)	(3.69)	(0.54)	(1.05)	(1.44)	(5.06)	(16.60)	(29.75)
Observations	10,152	7,892	5,994	24,613	14,553	10,358	24,579	14,535	10,340
Panel B: First Winners versus Other Winners									
Other Winners	2.48***	0.95	-0.76	2.57***	3.99***	3.10**	7.35*	21.63***	6.45
	(0.89)	(1.53)	(1.70)	(0.69)	(1.05)	(1.29)	(3.84)	(6.32)	(8.67)
First Winners	3.66**	15.02***	11.26***	3.30***	5.17***	21.85**	19.69	35.47***	47.03**
	(1.44)	(3.55)	(3.44)	(0.91)	(1.86)	(10.43)	(15.53)	(13.29)	(22.26)
HAC p-value	0.48	0.00	0.00	0.46	0.40	0.07	0.35	0.27	0.08
Panel C: Sectoral Analysis									
Construction and Medical Equipment	4.46***	7.57***	2.26	2.75***	3.95***	7.14***	5.66*	13.68***	13.61***
	(1.38)	(1.89)	(2.25)	(0.53)	(1.03)	(1.63)	(2.89)	(4.63)	(4.51)
Other sectors	2.07*	1.27	-3.53	2.87***	4.46***	6.58***	11.61	29.97***	18.50***
	(1.18)	(2.12)	(3.56)	(0.75)	(1.41)	(2.05)	(7.23)	(9.01)	(7.07)
HAC p-value	0.14	0.01	0.02	0.86	0.69	0.75	0.29	0.01	0.40
Controls	✓	✓	✓	✓	✓	✓	✓	✓	✓
FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
Observations	8,597	6,491	4,655	22,436	12,848	8,861	22,402	12,827	8,845

Notes: This table examines the effect of the procurement award on corporate credit, investment, and employees' growth rates. The unit of observation is the firm-year level i, t. The sample period is 2009-2019. In Panel A, I present the baseline results for the coefficient β^h in Eq. (1) for each horizon $h = 0, 1, 2$. In Panel B, I study the differences between firms that won the first contract ever in my sample versus repeated wins. In Panel C, I study the differences between firms in the Construction and Medical Equipment sectors and all remaining ones. The HAC p-value presents the p-value of the difference between states using the heteroskedasticity and autocorrelation consistent test. Robust standard errors clustered at the firm level are in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% level, respectively.