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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 the amount of cash-flow based lending. I use Portuguese administrative data over 2009-2019 and exploit public contests as a source of quasi-exogenous variation in the award of procurement contracts. Winning an additional €1 from a procurement contract increases firm credit by €0.05 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 an additional €1 in public procurement increases regional output by €1.8 with the credit channel accounting for 10% of it.

JEL Classification: E62, G38, H32, H57, H81

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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 spent 12.6% of GDP on the purchase of services, works and supplies from the private sector via public procurement contracting accounting for 30% of total government expenditures (OECD 2021). Nonetheless, empirical evidence on the impact of public procurement spending on firm growth is still scarce. This important policy tool is surprisingly understudied due to the lack of data and because its attribution is usually endogenous. The key challenges are thus rooted in the collection of procurement contracts information, the ability to link it to firms' balance sheets and financial statements data, and their non-competitive allocation system. In this paper, I overcome them by focusing on Portugal, a developed economy with detailed information on firms and competitively awarded procurement contracts.

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 (Figure B.2).

In this paper, I study the effect of public procurement on corporate credit and its implications for the macroeconomy. To this end, I assemble a comprehensive dataset spanning from 2009 to 2019 that combines procurement contracts with key financial indicators and tax-fillings for non-financial Portuguese 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.

I estimate that winning an additional €1 from a contract increases corporate credit by up to €0.05. Approximately 80% of this increase is accounted for by firm guarantees which entail future procurement cash-flows. Simultaneously, winning firms incur lower interest rates and increase the amount of credit lines and savings in the form of cash and bank deposits implying that the overall credit response is driven by credit supply. Enhanced access to credit further promotes firm investment and as a consequence, employment. These effects are more pronounced and persistent for smaller, younger, and financially constrained firms.

To explore the macro-level effects of procurement, I aggregate firm-level awards by region by using the winning firms' headquarters location. Awarding more contracts to a region promotes relatively more private investment in R&D and productivity which translates into positive

long-run effects on gross value added. An additional €1 in public procurement increases regional output by €1.8 with the credit channel accounting for 10% of the increase. The regional output re-sponse is driven by an increase in private investment and consumption. Finally, I show that there are heterogeneous effects: regional output increases more when regions receiving the contracts (i) have a higher share of smaller firms; (ii) receive a higher share of longer maturity contracts; and (iii) receive investment- rather than consumption-oriented procurement awards.

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 to submitting the only bid. Importantly, in this format of auction, when submitting the bid, firms do not know with whom and how many con-testants they are competing against and 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. Empirical evidence supports this assumption: descriptive statistics show no statistical differences between winners and the runner-ups participating in public contests.

Besides identification, another key challenge is the availability of data. I focus on Portugal where the electronic registration of procurement contracts is mandatory since 2008, and I build a novel extensive dataset. 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 provided by the Bank of Portugal (BPLIM 2021). 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. This information is essential to addressing this paper's central question of whether public procurement affects corporate credit.

I find that for each additional €1 of public procurement, firms increase credit by €0.05. This increase is driven by credit supply as interest rates decrease by up to 0.5 percentage points in response to the award. 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. The credit supply channel of public procurement is further corroborated by the relatively stronger response experienced by smaller, younger, and financially constrained firms. 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.

The importance of firms' private revenues acting as collateral has been subject of discussion in previous studies (e.g. Ivashina et al. (2021), Lian and Ma (2021), Drechsel (2022), Caglio et al.(2022)). The credit effects of public procurement, however, present a series of unique puzzles that differ markedly from those in private credit markets. I highlight the uniqueness of public revenues by showing their relatively bigger importance: for the same amount, public procurement revenues are more pledgeable than private cash-flows (di Giovanni et al. 2022).

A natural follow-up question is how firms react to the increase in liquidity and credit promoted by the newly awarded contract. I find that winning a procurement contract boosts firms' employment and investment. For the latter, I uncover that the interaction between the award and the increase in credit is statistically more important than the value of the awards itself in explaining the increase in total fixed assets. Hence, this finding raises questions about the commonly accepted idea that revenues are the unique driver of firm dynamics' response to procurement contracting.

To further understand the aggregate effects of public procurement, I assemble a regional dataset by aggregating all procurement awards in accordance with the location of firms' headquarters. I develop an empirical framework that allows me to compute local fiscal multipliers and to study the impact of procurement spending on production, investment, productivity and inflation. The key identification assumption underlying this exercise is that public procurement spending is not allocated to a specific region because that region happens to be doing poorly relative to the other regions. The objective design of the awarding contest reassures that this assumption holds as a firm is not more likely to win because of its headquarters' location but rather, because it is placing the lowest bid.

I find that an additional €1 in procurement spending leads to a 1-year cumulative increase in regional output by €1.8 with approximately 10% of this increase being accounted by the credit channel. The regional output response is being driven by an increase in private investment and consumption while net exports actually decrease. In particular, I find that private investment in R&D and total factor productivity react positively in the short-run, which further reinforces the growth in regional output in the medium-run. Moreover, I find that procurement is an effective fiscal policy tool for the purposes of promoting employment and real labor compensation in the medium run without affecting inflation nor increasing aggregate borrowing. In accordance to the micro level evidence, I also find that this policy's effect is more pronounced when governments allocate longer procurement contracts to smaller and financially constrained firms.

Finally, I study two types of spillover effects and conclude that regions where the procurement is booked have stronger spillover effects than regions where the money is spent. These results have one important implication for the literature on local fiscal multipliers. By using aggregate series on government spending that only consider the location of spending and ignores where the actual money flows to, the estimation of spillover effects and therefore also the real impact of fiscal policy might be underestimated.

Related literature and contribution. 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. 2022) or on a specific sub-sector of the economy like construction or military spending (Gugler et al. 2020; Hebous and Zimmermann 2021). 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. (2022) 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. I also explore the data granularity of my dataset which allows me to test for firm heterogeneity and understand the role of financial frictions. 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. (2022) 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 micro data, the empirical evidence on the effect of government spending on growth is still subject to debate (Galí et al. 2007; Ramey 2011; Gabriel et al. 2022a,b). 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 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 out-comes. 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' constraints (Kiyotaki and Moore 1997; Bernanke et al. 1999; Brunnermeier and Sannikov 2014). However, recent works highlight the importance of firms' cash flows acting as collateral (Ivashina et al. 2021; 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. 2021). 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.

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 credit and fleshes out the key mechanism at play. Section 4 investigates the effects of procurement and credit on firm dynamics such as investment and employment. Section 5 derives aggregate implications for the macroeconomy. Section 6 presents the policy implications of this work while Section 7 concludes.

2. Data and Institutional Setting

2.1 Procurement Contracting in Portugal

Institutional setting. Public procurement is defined here as the acquisition of goods and services by any public entity. In Portugal, there are 4,000 such entities from local and national governments to state hospitals or universities. In 2009, Portugal became one of the first countries in the world to make the electronic registration of public procurement contracts mandatory. Such electronically published contracts account, on average, for a third of the total spending in procurement and between 2% and 4% of GDP (Figure C.8). They are allocated to firms of all sizes (Table C.3) and from all industries (Table C.4). Given the extensive time and

firm coverage, this setting is then ideal to study the dynamic effects of such a policy at both the firm and regional level. Additional details can be found in Section A3.

The quasi-exogenous design of public contests. The identification strategy in this study hinges on the winner decision process of public contests. Typically, contests are announced by a potential hiring entity and participating firms are allowed to bid only once. The application process itself can be costly in terms of time and money; applicants pay a participation fee to a certified third-party ruler whose main function is to apply the contest's rules while ensuring anonymity. At no point in time will the hiring entity know which firms are applying, and firms do not know against whom they are competing. There are two rules that can be chosen by the hiring entity: firms will either compete on the lowest price or on the most economically advantageous tender that also considers the quality and timing of the proposed project. For more than 99% of contracts the price criteria weighs more than 50% and thus, this quantifiable criteria also helps my identification quest as previous wins are not taken into account when deciding the winner of a given public contest.

The fact that firms compete in a setting that bears close resemblance to a silent sealed bid auction with costs attached to submitting the only bid is helpful for my identification strategy. In such a quasi-exogenous setting, there is no ex-ante predictable winner and firms do not know whether their bid was the lowest until the announcement. For example, 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 clear that it would make a lower bid against a small and less productive firm that need the contract merely to 'survive' and thus might be more willing to charge a lower markup. Moreover, firms do not have an incentive to strategically price their bid given that they do not know against whom and how many bidders they are competing against.

2.2 Data

I merge three administrative datasets on public procurement contracts firms' balance sheets and credit information. I collect data from the official e-procurement website BASE, which has information on all government procurement contracts published online since 2009. The database is managed by the Portuguese Institute of Public Markets, Real Estate and Construction (IMPIC) and includes detailed information about the public procurement contracts. By using the tax number of the nonfinancial firms applying for and being awarded such contracts, I am able to match the procurement data to balance sheet and credit information. Given my focus on contracts that are awarded after a public contest, after having merged all three datasets, I have a total of 34,490 winner-year observations. More information on the data sources and data cleaning procedure follows and complementary information can be found in Appendix A1.

Procurement Data. I collect information on over 1 million procurement contracts that were announced on the official e-procurement website BASE between 2009 and 2019. First, I

scrape records of these contracts off BASE's website in which each contract's hiring entity is responsible to provide the information. For each contract, I obtain the tax number of the winner, of the hiring entity, and of bidders in some public contests. Moreover, I collect several contract characteristics such as: the contract date, duration, and its award value.

One important characteristic refers to how the contract is awarded. There are two important mechanisms: direct awards, where the public entity attributes a procurement contract to a firm of its choosing, and public contests, where the public entity announces the intent of acquiring some good or service and firms compete for the contract on the basis of a first-price sealed bid auction setting. From Table 1, we can see that there are some differences between these two mechanisms used to award contracts. Awards attributed after a public competition are, on average, of higher value and higher duration.

The procurement data that was web scrapped from the official website accounts, on average, for more than one third of the total public procurement expenses estimated by OECD (2021) and corresponds to roughly 3% of Portugal's national GDP. This is a non-negligible amount of government spending and investment for Portugal whose public investment for the same time period has been around 2% of GDP. Another way to put this is that in my sample total public works account for more than 50% of public investment. More importantly, the data coverage consistently improves throughout the sample from 2% of GDP in 2012 to 4% in 2019 (Figure C.8).

To the best of my knowledge, this is the first paper using web-scraped data on Portuguese e-procurement contracts. In contrast to other similar datasets of U.S. and European procurement data, the detail and granularity of the Portuguese contracts have several advantages. First, the information on the winner's tax number allows me to directly match contracts with firm level balance sheets and credit data, instead of relying on name matching. Second, the broad coverage of public contests across all country locations and industries, as presented in Table C.4, allows me to add external validity to the literature focusing on specific economic sectors such as the papers by Gugler et al. (2020) and Hebous and Zimmermann (2021) who focus on the construction and defense sectors respectively. Finally, the micro data accounts for more than 33% of total government procurement as measured in Portuguese national accounts over the later years in the sample which is a significant improvement in the literature where, in comparison, di Giovanni et al. (2022) report a 13% coverage for Spain and Cox et al. (2022) a 16% coverage for the United States.

Given the importance of competition to my identification strategy, my baselines analysis retains only those contracts that are awarded after a public contest. This strategy yields 138,578 winner-contract observations that after aggregating at the yearly level and matching with the firm level information turn into 34,490 winner-year observations. More information on the data sources and data cleaning procedure follows and complementary information can be found in Appendix A1 and Table A.2.

Firm level data. Microdata on firms was obtained using the Portuguese Simplified Corporate Information Survey (Informação Empresarial Simplificada, IES), provided by the Bank of Portugal's Microdata Investigation Laboratory (BPLIM 2021). This dataset contains detailed balance-sheet data, as well as annual profit and loss data on all Portuguese nonfinancial firms. Information about credit and collateral was provided by the Credit Registry Central (Central de Registo de Crédito - CRC) of Bank of Portugal (BPLIM 2019). The latter data cover all aggregated loans granted by banks to nonfinancial firms. Table B.2 presents descriptive statistics for all the firms in the sample.

Following the standard practice in the literature, I only include firms that are categorized as nonfinancial corporations in the sample. I exclude firms that have missing or negative values of book assets. I remove firms that experienced recent significant changes in their organizational structure such as mergers and acquisitions. To control for the effects of outliers, all variables are winsorized at the 1st and 99th percentiles.

Importantly, I have aggregate information about the type of collateral and the amount pledged at issuance: if a single loan is backed up by several sources of collateral, their respective types and amounts are reported. It must, however, be noted that the collateral value is not marked to market, and is often truncated to be equal to the loan if the value of collateral exceeds the loan amount (Degryse et al. 2021). Therefore, I can map the amount of aggregated granted credit into six different types of collateral: (i) real mortgaged; (ii) real not mortgaged; (iii) financial; (iv) personal guarantees provided by the firm; (v) personal guarantees provided by the state; and a residual (vi) other category. The first three categories correspond to asset based lending activities, while the three later categories to cash flow based lending activities. For the purposes of this paper, personal guarantees have the same role as real and financial assets in the sense that they can act as collateral, and thus they are addressed as such. However, it is important to note that in case of financial distress, banks can't seize these guarantees as they can with collateralized assets.

The existence of cash flow based lending in Portugal is economically relevant. The average firm in my sample has approximately 51% of credit being collateralized by personal guarantees provided either by the firm (e.g. future stream of revenues from both public or private contracts) or by the state (e.g. subsidies). In particular, procurement winning firms have a higher share of about 62% (Table B.2), already hinting to the main result of this paper - procurement contracts can be used as collateral and lead to an increase in firm's access to credit. These values are in line with the recent work by Ivashina et al. (2021) who document a 48% share of cash flow loans on the total volume of commercial credit by banks in Spain. It also resonates with the work by Caglio et al. (2022) who document that in the United States, among all firms, 28% of all loans are collateralized by account receivables and only 24% by fixed assets and real estate.

2.3 Life Cycle of Procurement Firms

I now study the evolution of firm growth and finance over the life cycle of firms to add additional insight to the previous summary statistics. In particular, I am interested in highlighting differences between firms which have won at least one procurement contract in my sample and firms which have not, including non-participants. I thus estimate the following specification for each group of firms:

$$y_{i,t} = \sum_{a \in A} \gamma_a D_{i,t}^a + \alpha_i + \alpha_t + \varepsilon_{i,t} \quad (1)$$

where y is the variable of interest, $D_{i,t}^a$ is a dichotomic variable equal to 1 if firm i belongs to age group a at period t , α_i and α_t denote firm and year fixed effects. The set A includes 6 age groups: age 0-3, age 4-7, ..., age 16-19, and age greater or equal to 20.

The results are presented in panels (a)-(d) of Figure 1 in which I plot the predicted variables from regression (1) for four variables on firm growth: annual sales and assets growth; and on firm finance: total credit over assets (leverage) and interest rates (proxied by dividing total interest expenses by credit). For presentation purposes, the coefficients estimated in the regression are scaled using the unconditional mean of the omitted group, which corresponds to the oldest firms (20+).

The first two panels present the results for firm growth. Both total sales and total assets growth rates are higher for procurement winning firms across the entire age distribution. Nevertheless, the difference tends to decrease as age increases. Panels (c) and (d) present the results for firm finance. Not only do procurement firms grow faster, their leverage tends to be higher across the entire age distribution. However, despite being more levered, these procurement firms enjoy lower interest rates. As with the growth variables, these differences decrease with age.

3. Public Procurement and Firm Credit

I am interested in estimating the impact of winning a procurement contract on firm credit. Because government contracts are not randomly allocated across firms observing the correlation between winning an award and the growth rate of credit would produce biased estimations. The direction of the bias will depend on the underlying data generating process. For instance, if contracts are awarded to the most productive firms, then my estimates would be over-estimated as productivity tends to be positively correlated with access to credit. Alternatively, if private sector contracts are crowded-out by government contracts due to capacity constraints, then we might underestimate the effects of public procurement on firm credit. Moreover, if participant firms can anticipate whether they win a procurement contract, it would be expected that the effect is underestimated as firms would anticipate their business decisions. To overcome these estimation concerns, I propose an empirical strategy that exploits the design of the public contests underlying the procurement contracts award as emphasized in subsection 2.1.

3.1 Empirical Strategy

I now present the methodology to investigate the impact of winning a public procurement contract via a public contest on corporate credit at the firm level. 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):

$$\frac{C_{i,t+h} - C_{i,t-1}}{Assets_{i,t-1}} = \beta^h \frac{Award_{i,t}}{Assets_{i,t-1}} + \psi^h \cdot X_{i,t-1} + \alpha_i^h + \delta_{s,t}^h + \varepsilon_{i,t}^h \quad \forall h \in \{-3, \dots, 3\} \quad (2)$$

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 $\frac{Award_{i,t}}{Assets_{i,t-1}}$ corresponds to the contractualized price of the public contest divided also by the value of total assets in the previous period. Given the adjustment, β^h directly gives us the elasticity of credit to the award value at horizon h . $X_{i,t-1}$ is a control vector with one lag of the dependent and independent variables and other important firm characteristics such as lagged firm assets and liabilities. 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.

I add firm and time×industry fixed effects. The firm-fixed effects, α_i , capture firm-specific unobserved characteristics and exclude firms that never won a public contest or firms that won at least one contest every year of the sample period. The latter is especially important to disregard firms which are serial winners of procurement contracts and might be inherently different from the average firm in my sample. All industry specific and time invariant unobserved heterogeneous effects, such as the market structure, are nested within the set of firm fixed effects. Furthermore, not only the time×industry fixed effects capture all factors that are common across firms within an industry in a given year, such as industry-wide demand and supply shocks, they also control for global factors such as monetary policy or relevant legislative changes. 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.

To test for anticipation effects, I include horizons $h = -2$ and $h = -3$ in the estimation. Using annual data should alleviate this concern because the average duration of the contest from the announcement until the decision of the winner amounted to 4 months in 2020 (IMPIC 2021). 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 official winning announcement date. Therefore, only focusing on procurement contracts that are awarded after a public contest should also mitigate this concern.

Estimating specification (2) yields an unbiased estimate of β if the standard no-omitted variable bias assumption is satisfied over all horizons. Following the preceding discussion in

Section 2.1, 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 B.3 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 correlations show significant differences between winners and other participants of public contests. However, they mostly disappear once we include the fixed effects in the analysis. Three characteristics remain unbalanced, firm size measured by $\log(\text{Assets})$, liabilities over assets ratio and having won previous contracts. These are included as controls in the above baseline specification.

3.2 Results

Figure 2 suggests that winning one more Euro via procurement contracting increases total available credit by 4 cents after one year. The pre-event years display no significant anticipation effects. Moreover, the effect is persistent even three years after the event.

In a back of the envelope calculation, this implies a credit growth of approximately 3 percentage points one year after the award. My estimate is thus quantitatively consistent with the existing evidence from di Giovanni et al. (2022) who showed that firms winning a procurement contract in Spain increase credit growth by 5.5 percentage points annually.

In the beginning of my sample, during 2011 and 2012, austerity measures were enforced by the Portuguese government following the sovereign debt crisis intervention by the IMF and commercial banks significantly reduced spending. Bonfim et al. (2022) argue that banks with higher pre-austerity exposure to firms with procurement contracts reduced lending to all firms significantly more than banks with lower exposure to these firms. They argue for a “dark-side” of procurement in times of financial distress and reduced government spending. I find complementary evidence for increases in spending and conjecture that there was a positive effect of procurement awards on corporate credit that spilled over to an increase in overall production at the aggregate level, as I point out in Section 5.

Credit Supply Channel. In order to understand what is driving the increase in the quantity of credit that procurement winning firms borrow, I also investigate the response of the price of credit, the interest rate. On the one hand, if the increase in credit is mostly driven by the firm demand, then we would expect to see an increase in the interest rate. On the other hand, if the increase is driven by the bank supply, we would expect the interest rates to drop.

Given that I do not observe loan-level data, I do not have information on the interest rate a firm pays for each individual loan. However, I compute a proxy for interest rate by dividing total interest expenses by lagged credit. The average interest rate paid by procurement winners in my sample was about 7.4% and the median close to 5% (Table B.2) which is roughly in line with official data from the Portuguese Central Bank. Figure 3 presents the results.

I find that after receiving a procurement award worth 100% of total assets, interest rates fall persistently up to 0.5 percentage points. This result lends support to the idea that the credit supply channel dominates the credit demand one. There are several arguments to be made in response to the question as to why that might be the case.

On the one hand, if public procurement is perceived as some sort of government certification, it is likely that banks will expand the credit supply for these firms and lend at lower rates in line with the effect of other government certification programs (Bonfim et al. 2022). On the other hand, it might be that these procurement contracts can be used as collateral increasing the overall credit worthiness of the firm, and thus promoting credit supply to expand more.

I test the first reasoning in subsection 3.3 by evaluating whether new winners have differentiated responses to the same award value than their peers who already were awarded at least one contract in the past. I explore the second argument now.

Credit Collateral. One complementary interpretation is that even though the firms' credit demand did not increase more than the banks' credit supply, the amount of firms' pledgeable assets and cash-flows increased in response to winning a procurement contract thus increasing the firm's credit worthiness. 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.

Over the last years, there has been a shift from the traditional idea that only physical assets can be used as collateral (Kiyotaki and Moore 1997; Bernanke et al. 1999). Recent research has highlighted that the importance of cash-flow based lending is, at least, as important as asset-based lending (Ivashina et al. 2021; Lian and Ma 2021; Drechsel 2022; Caglio et al. 2022). As the literature on cash-flow based lending evolves and focuses mostly on private contracts, it is important to understand how different sources of cash-flows might have different implications on the dynamics of corporate credit. In the particular case of public demand, only the work by di Giovanni et al. (2022) tried to emphasize the relation between procurement contracts and credit.

I go one step further and capitalize on the detailed data on procurement contracts that I web scrape together with the Portuguese credit registry data. The latter offers information about the type of collateral and the amount pledged at the time of issuance: if a single loan is backed up by several sources of collateral, their respective types and amounts are reported. Therefore, I can map the amount of granted credit into six different types of collateral: (i) real mortgaged; (ii) real not mortgaged; (iii) financial; (iv) personal guarantees provided by the firm; (v) personal guarantees provided by the state; and a residual (vi) other category. The first three categories correspond to asset based lending activities, while the three later categories to cash flow based lending activities.

I thus estimate the effect of an additional €1 of procurement award on the type of collateral used to increase credit. To be precise, I estimate specification (2) using growth rates of the 6 different types of collateral as dependent variables. I am mostly interested in credit backed by personal guarantees since this category includes future revenues as collateral. If procurement contract act as collateral for credit, credit using personal guarantees are expected to respond more to procurement awarding than credit backed by other types of collateral. Table 2 presents the results.

This table 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 year-horizon 1 firm guarantees account for more than 50% 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 will be testing in Section 4.

This set of results thus provides indirect evidence that firms indeed borrow against public procurement contracts and highlight a direct link between these contracts and credit provision. It reinforces the conclusions of a recent study of the Spanish context (di Giovanni et al. 2022) and complements the literature on this topic mainly focusing on the pledgeability of private revenues in the US (Lian and Ma 2021; Drechsel 2022; Caglio et al. 2022).

This finding is particularly relevant for financial stability considerations as my sample covers the post Great Recession period when banks faced higher capital requirements on lending and increased collateral requirements on newly issued loans (Degryse et al. 2021). We know that during credit booms driven by high collateral values (e.g., real estate booms), economic activity expands but the economy's stock of information on existing projects becomes depleted. As a result, collateral-driven booms end in deep crises and slow recoveries: when booms end, investment is constrained both by the lack of collateral and by the lack of information on existing projects, which takes time to rebuild (Asriyan et al. 2022). Procurement spending thus might be one extra tool that governments can use in order to smooth financial cycles and reduce the probability of collateral-driven booms or its deep consequences afterwards.

It is important to note that there are two caveats worth mentioning at this point. First, loans backed by personal guarantees provided by the firm might be subject to financial covenants. It is studied that covenant breaches give banks the right to accelerate the repayment of a loan and banks might use this threat in the renegotiation after to tighten credit supply (Roberts and Sufi 2009; Chodorow-Reich and Falato 2022). I do not have access to loan-level information and thus, I can't confirm whether covenants are present in these loan agreements. Notwithstanding, even if that is the case, one would expect the results in Table 2 to be a lower bound of the unbiased estimate.

Second, according to the Portuguese Public Procurement Code, Chapter IX, some procurement contracts require winning firms to have a guarantee up to 5% of the contract value, for example, contracts priced above half a million euros. There, it is common for the public entity to accept a “*garantia bancária*” where the bank provides a credit to the firm backed by the future cash-flow of the contract itself. In my setting, this will be registered as an increase in effectively used credit backed by personal guarantees provided by the firm. In order to test whether this increase in collateral is being driven only by this “automatic” effect, I am currently developing a textual analysis tool explained in Section A1.1.1 that will allow me to isolate procurement contracts with mandatory guarantees and the exact amount requested. Notwithstanding, the central message of this paper still holds: procurement contracts are used as collateral to increase firm credit.

The role of financial constraints. It is important to consider the potential role of financial constraints in shaping the credit channel of public procurement. As a first step, it is important to understand whether firms react to public procurement by easing potential borrowing constraints. Following on 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 credit worthiness by negotiating new credit lines.

Figure 4 displays the firm-level responses of negotiated credit lines and firm liquidity, measured by the sum of cash and bank deposits, to a procurement award.

Figure 4a provides evidence that following the award, firms (re-)negotiated their credit position and increased their credit lines. In particular, for each additional €1 of procurement, firms negotiated 2 more cents of potential credit. The variable potential credit includes unused amounts of credit cards, lines of credit, and any other credit facilities likely to be converted into effective debts. It is thus an equilibrium variable that results from a negotiation between the firm and its lending partners. The statistically significant difference in its response together with the increase in used credit provides evidence that during the same year of the award these firms (re-)negotiated their credit position, hence easing potential credit constraints.

An additional €1 of procurement also leads to the increase of cash and bank deposits of 5 cents (Figure 4b). The positive response of liquidity can be rationalized by a precautionary motive and adds substance to the idea that firms are using procurement contracts to diminish financial constraints. Further discussion on the role of financial constraints and how to identify them follows in subsection 3.3.

Public and Private Revenues. One important question that follows is whether winning a procurement contract increases firm credit more so than it would than selling to the private sector. I follow di Giovanni et al. (2022), and estimate specification (2) with total revenue growth as an extra control on the right hand side, measured between t and $t + 1$ because sales carried out by the firm in t will manifest in the data in $t + 1$. If the pledgeability of both

types of revenues was similar, the effect of the procurement award value should become null once we control for total sales growth. Table 3 presents the results.

In column (1), I investigate the effect of winning a public contest on corporate credit without any controls or fixed effects. I find that winning an additional €1 in procurement contracting increases firm credit by €0.08. Nevertheless, this coefficient confounds the endogenous relationship between procurement and corporate credit. To address this concern, I augment my model with firm and year×industry fixed effects (column (2)). This specification only exploits within-firm variation, comparing changes in credit of the same firm over time. I find that the positive effect persists. The estimation of the 1-year horizon baseline specification of equation 2 is displayed column (3). This includes a vector of lagged control variables besides the fixed effects. The point estimate remains positive albeit smaller and corresponds to the baseline estimates.

Column (4) displays the key result of this exercise. I find that the award value remains positive and significant when controlling for total sales growth. Even though total sales partly explain the increase in credit, the procurement award explains almost four times as much.

Further Results. Figure B.4 in Appendix presents further results on the intensive margin of the award. The first figure shows that the maturity of the majority of the newly issued credit is above 1 year. This is a very interesting result in the light of the average and median duration of the analyzed procurement contests which are less than 1 year (Table 1). This might indicate that there are potential reputational gains incurred by winning firms being able to issue credit with longer maturities. Moreover, as the non-performing loans of the winning firms (qualitatively) decrease, winning a contract might be important to ameliorate the credit position of the firm by increasing its creditworthiness, reinforcing the financial stability considerations of public procurement.

3.3 Heterogeneous effects on firm credit

In this subsection, I focus on the heterogeneous effects of winning a procurement contract. More important than quantifying the elasticity of credit to the procurement award value, policymakers care about differential effects of a specific policy. Thus, I investigate whether specific firms or contract characteristics are driving the results. Given the discussion in the previous section, I am particularly interested in assessing whether financially constrained firms react differently than their non-constrained peers.

The literature uses several approaches to measure financial constraints based on firm characteristics. For example, young and small firms may face frictions in obtaining external financing because they have less well-established financial market relationships, are subject to greater asymmetric information, and have more uncertain returns. Therefore, young firm age (e.g. Cloyne et al. (2022)) and small firm size (e.g. Gertler and Gilchrist (1994)) are common proxies of financial constraints. Another approach uses firm characteristics that may

be induced by financial constraints. For example, Cunha and Pollet (2020) document that financially constrained firms accumulate more cash, likely for precautionary reasons. Buera and Karmakar (2022) explore the same Portuguese administrative data and uses the share of short-term debt to total debt and leverage as proxies for constraints. Either way, firms typically classified as constrained do not actually behave as if they were constrained: they have no trouble raising debt when their demand for debt increases exogenously (Farre-Mensa and Ljungqvist 2016).

I can go one step further into the identification of financial constraints by using detailed credit information. In my setting, credit allowances are changing over time, this provides me with a time-varying and firm-specific measure to assess a firm's financial constraints. Moreover, I can observe whether firms have available credit lines. By merging this information, I can create a binary and time-varying measure for identifying whether a firm is financially constrained. It should be noted, however, that while credit information offers a far more detailed notion of a firm being constrained compared to standard financial ratios such as leverage or liquidity, it is still a proxy. Therefore, I carefully document results using the array of the above approaches to measuring financial constraints.

For this exercise, I focus on horizon 1 of equation (2) and include an interaction term for each specific firm-level characteristic as follows:

$$\begin{aligned} \frac{C_{i,t+1} - C_{i,t-1}}{\text{Assets}_{i,t-1}} = & \alpha_i + \delta_{s,t} + \mathcal{I}_{i,t-1} \left[\beta^{\mathcal{I}} \frac{\text{Award}_{i,t}}{\text{Assets}_{i,t-1}} + \psi^{\mathcal{I}} \cdot \mathbf{X}_{i,t-1} \right] \\ & + (1 - \mathcal{I}_{i,t-1}) \left[\beta^{(1-\mathcal{I})} \frac{\text{Award}_{i,t}}{\text{Assets}_{i,t-1}} + \psi^{(1-\mathcal{I})} \cdot \mathbf{X}_{i,t-1} \right] + \varepsilon_{i,t} \end{aligned} \quad (3)$$

where the key independent variable and the vector of control variables $\mathbf{X}_{i,t-1}$ are interacted with a state indicator variable $\mathcal{I}_{i,t-1}$ that takes the value of 1 if a firm i at year $t - 1$ belongs to a specific group of firms. Table 4 presents the results corresponding to equation (3) where controls include lagged firm assets, firm liabilities, and past awards. Standard errors are clustered at the firm level.

This set of results highlight important heterogeneities on the transmission of the credit channel of public procurement. Younger, smaller, and financially constrained firms react more to winning a procurement contract than their peers. The different credit responses can be rationalized by the fact that smaller and younger firms are typically more bank dependent which is especially true for Portugal where more than 80% of nonfinancial corporations debt is accounted by bank loans (Figure B.2).

Notwithstanding, these results are important for most economies. Most countries' small and medium enterprises mostly borrow against collateral whose value is inextricably tied to firm operations and not against typical collateral like fixed assets and real estate. This observation

also holds for the so-called “market based economies”. For example, bank credit is the most important source of credit to small and medium enterprises in the United States (Caglio et al. 2022).

Other potential drivers of the credit channel of public procurement like the reputation effect of new winners or high dependence of procurement on total revenues do not seem to matter, with one exception. The maturity of the award shapes the credit channel. Longer contracts are associated with a stronger increase in credit. A complementary explanation is that the longer the maturity of the contract, the lower the firms’ cash-flow uncertainty, and the stronger the reaction will be (Buera and Karmakar 2022). Longer contracts can thus solve cash-flow uncertainty faced by firms allowing their CEOs to anticipate and promote stronger changes when conducting their business.

Heterogeneous dynamic effects. Table 5 explores one potential heterogeneity within the previous set and displays the computed elasticities with respect to the firm size. The key rationale is that smaller firms are more likely to be bank dependent and financially constrained (Beck et al. 2005). 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.

When dividing our sample of winning firms by size, I classify small (big) firms as I did previously: when their book value of total assets is below (above) the median value in the whole sample. Table 5 presents evidence in favor of statistically significant differences in the response of small relative to big firms to the procurement contract award. Small firms increase credit more than big firms in response to a same-sized procurement award. Furthermore, their response is characterized by its persistence: up to horizon-year 3, smaller firms show a statistically significant increase in credit contrarily to their bigger peers.

This setting parallels the one in Banerjee and Duflo (2014) where they test for the existence of credit constraints using variation in a directed lending program. By winning these procurement contracts, both constrained (small) and unconstrained (big) winners may be willing to absorb all the credit that they can get because it may be cheaper than other sources of credit. By studying the dynamic responses of each group, I uncover an important fact: even though in the short-run both groups of firms increase their credit position, small firms drive the persistence of the results which resonates with the financial accelerator hypothesis (Bernanke et al. 1996).

3.4 Identification assumption and robustness checks

3.4.1 Discussion of identification assumptions.

This section provides additional tests that further 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.

First, I look at public contests with exactly two contestants and assess whether winners and losers differ systematically from one another. Table 6 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 B.3. Hence, conditional on these characteristics the assumption holds.

Second, I try to generalize the previous exercise for all contracts with more than 2 contestants and show that, in a regression form analysis, no firm characteristics determined before the public contest should have predictive power for its outcome once I control for the contest fixed effects.

I thus test for the conditional random assignment by running the following regression for each contest z and participant j :

$$\text{Winner}_{z,j,t} = \gamma \mathbf{X}_{j,t-1} + \kappa_z + \delta_{s,t} + \varepsilon_{z,j,t} \quad (4)$$

where $\text{Winner}_{z,j,t}$ is the dummy variable of winning the auction conditional on participation. $\mathbf{X}_{j,t-1}$ is the set of firm level baseline controls measured at the period before the contest takes place with contest fixed effects and industry×year fixed effects. This randomization test is similar to Lee (2021) and Ferraz et al. (2021) who show evidence in favor of no correlation between the allocation of contracts within a procurement auction and firm characteristics in Korea and Brazil, respectively.

Table B.1 provides omnibus p-values for the joint significance of the firms' characteristics. No firm level baseline control (other than assets and liabilities) is significant, and the p-values are much larger than 0.10, which confirms that the allocation of contracts within a contest is not correlated with observable characteristics conditional on those two and the contest fixed effects. Such a result confirms the exploratory analysis in Table 6.

3.4.2 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 for some readers that were not convinced by the evidence presented in Section 3.4.1.

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 runner-ups that allow me to robustly address potential selection bias considerations. For roughly 10% of the baseline sample, I know which firms applied to each public contest and I can then use the group of losers to create the winner's counterfactual. Nevertheless, I have no information about their bids and so, it is not possible to identify who was the runner-up for each contest.

To identify the closest firm from the pool of participants of each contract, I use the nearest-neighbor matching proposed by Abadie and Imbens (2011). I match the winning firm with the losing firm that is most similar in terms of observed characteristics, conditional on the fact that such losing firm did not win any other procurement contract around a 3-year time window. Formally, suppose firm i with covariate vector x_i wins the public contest. The nearest neighbor matching algorithm selects the firm j from the pool of losing firms with covariates x_j that has the smallest Mahalanobis distance.

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. (2022) and estimate:

$$\frac{y_{z,i,t+k} - y_{z,i,t-1}}{y_{z,i,t-1}} = \beta^k \mathcal{I}(\text{Winner}_{z,i,t}) + \alpha_z^k + \delta_t^k + \sum_{j=1}^p \gamma_j^k y_{z,i,t-j} + \epsilon_{z,i,t}^k \quad \text{for } k \in \{-3, \dots, 3\} \quad (5)$$

where the dependent variable y change is normalized by lagged total assets of firm i participating in contest z at year t . $\mathcal{I}(\text{Winner}_{z,i,t})$ is an indicator variable taking the value of one if firm i won contest z 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 5 reports the results.

The results are qualitatively similar to the baseline ones with credit growth being positively associated to winning a procurement contract. In addition, the interest rate differential displays a negative response. The anticipation effects are not present thus corroborating the summary statistics on the difference of firm characteristics between the winners and losers (Table 6) that shows similarity before the event between the two groups of firms in the relevant covariates as well as the outcome measures.

4. Public Procurement and Firm Dynamics

The previous results show that firms increase their credit in response to the award of a public procurement contract. To have a comprehensive view of the real firm level implications of winning procurement contracts, I employ the empirical strategies outlined in Section 3 on

corporate investment and other balance sheet firm-level variables. I ask how the increase in available credit affects firms' investment and measure how much of the increase in firm investment can be attributed to the credit channel of public procurement. And finally, I study the heterogeneous effects of this fiscal policy.

4.1 Results

Table 7 presents the results corresponding to specification (2) on the growth rate of total fixed assets (investment) weighted by lagged total assets for a one-year horizon after the event $h = 1$. Besides displaying different specifications, I investigate whether the increase in credit growth is as important as the value of the award itself to promote corporate investment. To do so, I focus on $h = 1$ and extend model (2) to include a credit growth control but also its interaction with the award value.

In column (1), I investigate the effect of winning a public contest on corporate investment without any controls or fixed effects. I find that winning an additional €1 in procurement contracting increases firm investment by €0.26. Next, I augment my model with firm and year \times industry fixed effects (column (2)). I find that the positive effect persists. The estimation of the 1-year horizon baseline specification of equation 2 is displayed column (3). This includes a vector of lagged control variables besides the fixed effects. I find that winning an additional €1 in procurement contracting increases firm investment by €0.07.

Column (4) displays the key result of this exercise. I find that the award value coefficient loses its significance when controlling for credit growth and the interaction term. This can be interpreted as evidence in favor of the importance of credit to explain the increase in corporate investment. I thus complement previous studies on the effects of public procurement by highlighting the importance of its credit channel. The increase in credit is as important as the increase in procurement revenues when one wants to explain the corporate investment response.

Dynamic results. I now estimate the elasticity of firm investment to the procurement award value to measure the impact of one more euro in procurement spending at the firm level. Figure 6 suggests that winning one more Euro via procurement contracting increases firm's non-current assets by 7 cents one year after the event. The majority of this response stems from an increase in Plant, Property, and Equipment (PPE) and not from financial investments. The pre-event years display no anticipation effects. Moreover, the effect is persistent and cumulatively increases to 12 cents three years after the event.

The elasticity is in line with other estimates such as the one also for procurement awards of about 10 cents by Hebous and Zimmermann (2021) but also for other types of collateral, such as the studies by Chaney et al. (2012) and Catherine et al. (2022) that focus on real estate collateral and find an elasticity of 6 cents for each dollar of increased real estate collateral value.

Again, it is important to note that loans backed by personal guarantees provided by the firm might be subject to financial covenants. Covenant breaches might negatively affect firm investment (Adler 2020). I do not have access to loan-level information and thus, I can't confirm whether covenants are present in these loan agreements. Notwithstanding, even if that is the case, one would expect the results in Figure 6 to be a lower bound.

Further results. Figure 7 presents further results on the intensive margin of the award. To further highlight the unanticipation of the award, Figure 7a displays the sales income response that shows no pre-trends and displays a €0.60 increase to €1 unexpected award one year after the award. Ex-ante one would expect a 1 to 1 relation between an increase in public demand and registered sales, nevertheless there are two potential reasons for why the response is smaller.

First, the duration of the contract might spread out revenues throughout the years. Even though the median duration of the contracts is less than one year, some contracts last as long as 10 years. Second, if a firm is capacity-constrained and receives a new purchase order from the government, it might end up neglecting other orders that end up being fulfilled by its competitors - the origin of potential spillover effects analyzed in Section 5. This latter effect might be counteracted if the firm sub-contracts which seems plausible. According to Figure 7d, firms do increase their total liabilities in response to winning a procurement contract.

Moreover, winning a procurement contract induces a persistent increase in employment that goes beyond the first year (Figure 7e). This finding is in line with recent evidence by Ferraz et al. (2021) that also find effects of winning a contract well beyond the average contract length of a year, even though their results dye out 3 years after the event. Moreover, Figure 7f shows that not only firms hire more people but also increase their employees compensation. The later resonates with previous findings by Cantore and Freund (2021) and Gabriel et al. (2022a) that argue for a redistribution effect of fiscal policy expansions from capital owners to workers using macro-level data for the U.S. and for Europe.

4.2 Heterogeneous Effects

As pointed out in a different context by Kaplan and Zingales (1997), it is unclear a priori that the sensitivity of investment to (procurement) collateral value should be increasing with the extent of financial constraints. This remains ultimately an empirical question, which I answer using three different ex ante measures of financial constraints defined in Section 3.2.

In this sub-section, I thus focus on two real variables that are of paramount importance to policymakers: investment and employment. More important that quantifying the elasticity of investment to the procurement award value, policymakers care about potential differential effects that might maximize the aggregate response to a specific policy. Table 8 presents the results of estimating specification (3) for corporate investment and employment growth.

This set of results highlight important heterogeneities on the transmission of the credit channel of public procurement to firm investment. Younger, smaller, and financially

constrained firms also react more to winning a procurement contract than their peers in terms of investment.

Other microfoundations do not seem to drive the corporate investment response, thus pointing out to the importance of the credit channel. I now focus on the latter by exploring the dynamic heterogeneous effects by firm size up to horizon year 3, not only on investment but now also on employment.

Table 9 reports a strong cross-sectional heterogeneity in the response of investment to winning a procurement contract. The sensitivity of investment to the procurement award is larger in the group of “constrained” firms relative to the group of “unconstrained” firms. For instance, the coefficient β for firms in the bottom of the size distribution is €0.09 compared to €0.01 for the firms in the top. The difference between these two coefficients is significant at the 10 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. This coincides with the previous results for credit in Table 4.

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 Hebus and Zimmermann (2021). Notwithstanding, my estimated elasticity for cumulative investment after one year for small firms (9 cents) is smaller than their estimate (13 cents). I believe the main reason for this is that they focus on procurement contracts in the defense industry so, doing an industry level analysis might be important to explain potential differences.

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.

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 heterogeneity. I document heterogeneous effects on employment growth in the last 4 columns of Table 9. Financially constrained firms see their employment growing more than their unconstrained peers. This result corroborates the 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 has concerned 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.

5. Regional Effects of Public Procurement

So far, I have documented that public procurement acts as collateral to enlarge the credit position of firms and that these same firms react by increasing investment. This crowding-in of private sector investment is especially pronounced among small firms and hence, it is not clear whether the effect is present at the aggregate level and what is the aggregate effect of the fiscal policy on regional output. Thus, this section develops an empirical framework to bridge such a gap by aggregating the micro-level data at the regional level to perform a regional analysis.

First, I aim at understanding the effect of unexpectedly attributing procurement spending to a specific region within the country in the tradition of the (local) fiscal multipliers literature. Then, I investigate the effects of the policy on investment, employment, and GDP components. Next, I study spillover effects by performing different types of aggregation exercises in order to understand whether firms not directly associated to winning procurement contracts also benefit from the policy or not. Finally, I investigate three potential sources of heterogeneity that can be driving the results: the share of small firms; the average contract maturity; and the purpose of the spending consumption or investment.

5.1 Empirical Strategy

In order to understand whether these firm level effects are translated into macroeconomic effects, I proceed to estimate the impact of procurement spending on cumulative economic growth. Following previous work by Gabriel et al. (2022a), the empirical specification is based on a local projections approach:

$$\frac{GVA_{i,t+h} - GVA_{i,t-1}}{GVA_{i,t-1}} = \alpha_i + \delta_t + \beta^h \frac{Proc_{i,t}}{GVA_{i,t-1}} + \psi^h \mathbf{X}_{i,t-1} + \varepsilon_{i,t+h} \quad \forall h \in \{0,1,2,3\} \quad (6)$$

where $GVA_{i,t}$ is the gross value added in region i and year t . $\mathbf{X}_{i,t-1}$ 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 and time fixed effects. Region fixed effects capture time-invariant differences in gross value added growth and other heterogeneity across regions, they correspond to 25 NUTS3 regions. Year fixed effects control for the aggregate macroeconomic conditions and centralized fiscal and monetary policies. I account for the overlapping nature of the observations in the regression by clustering the standard errors by

region. For horizon 0, the estimate can be perceived as an open economy relative multiplier in the spirit of Nakamura and Steinsson (2014). However, as I will not take into account the cumulative change in procurement, longer horizon estimates represent solely the cumulative increase in regional growth.

Given that revenues from these contracts will be registered and accounted where the winning firm has its headquarters, the main variable of interest Procurement is obtained by aggregating the procurement awards by firm headquarters' location. Procurement spending is then normalized by the lagged regional GVA, so the stimulus is expressed in percent of initial GVA. To be consistent, both the gross value added and procurement variables are aggregated at the regional level using firms' headquarters location.

Figure 8a displays the cross-sectional variation of the main regressor averaged across 2009-2019. For completeness, Figure 8 also displays the total amount of Procurement in million euros and the amount of Procurement spending per capita in euros, Figures 8b and 8c respectively. The key takeaway is that there is sufficient cross-sectional variation that I can use to estimate the effects of local procurement using Portuguese data.

The coefficient of interest β^h thus gives us 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.

To estimate the causal effect of credit on the local procurement effects I need to update specification (6) with an interaction term with firm credit, following Basso and Rachedi (2021). I thus aggregate the amount of credit by firm headquarters' location that increased between t and $t+1$, after the award. Because I want to understand what is the effect of increased credit due to the award I focus on the cash-flow-collateralized credit, the type for which I am more certain to be linked to the procurement award, exclusively from the procurement winning firms. Then, I estimate the following panel regression:

$$\begin{aligned} \Delta GVA_{i,t+h} = & \beta^h \text{Proc}_{i,t} + \gamma^h \text{Proc}_{i,t} \times \Delta C_{i,t} + \omega^h \Delta C_{i,t} \\ & + \alpha_i + \delta_t + \psi^h \mathbf{X}_{i,t-1} + \varepsilon_{i,t+h} \quad \forall h \in \{0,1,2,3\} \end{aligned} \quad (7)$$

where I simplify the notation for clearness such that for any variable Var , we have $\Delta \text{Var}_{i,t+h} = \frac{\text{Var}_{i,t+h} - \text{Var}_{i,t-1}}{\text{GVA}_{i,t-1}}$ and the key regressor is also scaled by lagged GVA.

Discussion of identifying assumption. Estimating specification (6) yields an unbiased estimate of β if the no-omitted variable bias assumption is satisfied for all horizons. To put it in words, the identifying assumption is that public institutions do not allocate more procurement spending to a specific region where the winning firm's HQ is located because that region is doing poorly relative to other regions. Intuitively, this assumption could be violated if there is a strong local bias in the hiring decisions; I estimate that more than 50% of the contracts are sourced locally.

Nevertheless, the identification assumption is likely to be satisfied if the allocation of this procurement spending is unanticipated by a region and uncorrelated with its macroeconomic performance. In other words, what matters is the unanticipated location of the winning firm's headquarters, where the good or service is produced, that will determine the regional allocation of the award, and not where the money is spent. Building on the competitive contest setting, the discussion in Section 2.1 and the evidence provided in Section 3.4.1, I believe that this is a minor concern because *ex ante* it is not clear whether a local firm is more likely to win a public contest.

5.2 Results

Table 10 provides the estimates of the coefficient β_h up to horizon year 3. It displays a positive impact of procurement spending on regional economic activity as measured by the gross value added. An increase in procurement spending by €1 leads to a cumulative increase in gross value added by €2.4 three years after the procurement spending takes place. This implies a crowding-in effect with a €1 increase in relative government production leading to a €1.4 increase in relative private sector production.

Panel B investigates whether the credit channel of public procurement is also present at the regional level. I take the total amount of credit collateralized by personal guarantees provided by the winning firm and aggregate it by the firm headquarters' location. I then interact its growth rate from $t - 1$ to t with the amount of procurement awarded in year t with both measures being relative to GVA in $t - 1$. At horizon-year 0, increasing the amount of cash-flow-collateralized credit of procurement winning firms by 1% of GVA raises the local procurement multiplier by, approximately, 20%, from 1.39 up to 1.67. Given that the average increase of cash-flow collateralized credit from winning firms across regions is about 0.5% of GVA and by making the assumption that the change is fully accounted by procurement contracts' awards, I can estimate that across all horizons the credit channel accounts for, approximately, 10% of the procurement multiplier.

5.3 Understanding the procurement effects using national accounts data

In order to shed more light on what is underlying the macroeconomic effects local procurement, I perform two exercises. First, I explore responses of other economic variables using macroeconomic data from national accounts. Then, I decompose the effects on GDP into the response of each one of GDP's components.

The first row of Table 11 displays the GDP response which is closely in line with the GVA response, also yielding an increase of €2.4 at horizon year 3. We can observe a significant response of private sector expenditure on R&D that is accompanied by a positive response of total factor productivity. Taken together, these responses point out to the role of public procurement as fomenting innovation in the private sector. Moreover, as R&D expenditures are associated to intangible capital, regions responding the most to procurement spending

more likely have the most constrained firms who are more likely to hold more cash in response to the award according to (Falato et al. 2022).

Contrarily to the firm-level responses, employment does not significantly increase on aggregate in the short run. A potential explanation rests on the reallocation of labor towards procurement winning firms. Nevertheless, the effect cumulatively increases and becomes significant at horizon year 3. Paired with this labor response, total real labour compensation also increases in the long run. Regional credit does not significantly increase, which might indicate that commercial banks are reallocating more credit towards procurement winning firms instead of expanding credit for all firms. Finally, I do not find evidence in favor of inflationary effects of this policy.

In order to shed some more light on the aggregate production response, I estimate the response of each GDP component. National accounts data presented in Table A.3 allow me to have estimates for gross domestic product, government spending, gross fixed capital investment, imports, and exports at the regional level. Using that information, I can estimate a residual category that will roughly correspond to private consumption. Then, for each GDP component, GDP_c, I estimate the following specification:

$$\frac{\text{GDP}_{c,i,t+h} - \text{GDP}_{c,i,t-1}}{\text{GDP}_{i,t-1}} = \alpha_i + \delta_t + \beta^h \frac{\text{Proc}_{i,t}^{\text{Agg}}}{\text{GDP}_{i,t-1}} + \psi^h \mathbf{X}_{i,t-1} + \varepsilon_{i,t+h} \quad \text{for Agg} \in \{\text{HQ}; \text{LOC}\} \quad (8)$$

I now use two different ways of aggregation. Not only I aggregate the procurement spending by firm's headquarter location (HQ), I now also aggregate by the spending location (LOC). The latter exercise aggregates the spending where the spending is registered. About 50% of the procurement spending via public contests is awarded to local firms so there is no perfect overlap of these two measures. Moreover, it allow us to understand whether the impact of the policy is also relevant for the location that receives the good or service. Table 12 presents the results.

In the first four columns, the aggregate effect on gross domestic production is being driven by investment and consumption. Interestingly, net exports actually decrease, which might be a consequence of public procurement crowding out exports together with increasing the need for imported products so that procurement firms can produce. In the last four columns, the effect is being driven mainly by private consumption while government spending reacts negatively.

Personally, the most striking result of this exercise is the different investment response. In the literature, there are opposing views on what is the effect of government spending on investment. Some papers document a reduction in capital expenditures to local spending shocks (Cohen et al. 2011; Kim and Nguyen 2020), while others argue for a positive effect (Gabriel et al. 2022a). My results highlight the importance of considering the actual spending location when deriving such implications. If one wants to evaluate the impact of government spending on investment, one should consider where the money is being booked instead of

where the money is being spent. This last observation is then enough motivation investigate the existence of spillover effects.

5.4 Spillover Effects

In the previous sections, not only did I show that firms increase private investment and production in response to a procurement contract award, but also that these responses aggregate into significant effects at the regional level. An important follow-up policy question associated to the size of the effect of procurement spending is whether there were indirect effects associated to this fiscal policy expansion. In other words, are non-procurement firms benefiting from procurement spending?

There are two reasons that explain why non-procurement firms might benefit from government procurement policy. First, contracts can be sub-contracted to assist in fulfilling the winning firm's contract or capture the demand that procurement firms could not fulfill after receiving an award. Second, they could benefit from the outcome of specific procurement contracts. Take the example of building a new road in a region that can impact a non-procurement firm by reducing costs of transportation or even increasing productivity.

One important observation from my data departs from Figure 9 where I plot the total amount of procurement aggregated by headquarter location (a) and by spending location (b) together with their difference (c), all in million euros at 2015 prices. Figure 9c shows that out of the 25 regions, only 5 are "net importers" of procurement spending, that is flowing mainly from the interior to these 5 coastal regions. It might not be surprising per se given that these 5 regions have the highest quantity of enterprises, nevertheless it hints to the necessity of taking the regional dimension into account when estimating spillovers from fiscal policy.

With that in mind I run two specifications to test for spillover effects based on Equation (6). First, I exclude winning firms when computing the aggregated series to test whether non-procurement firms located in the winning firms' headquarters benefit from the contract itself. Then, I extend the previous exercise by changing the aggregation method for the procurement regional series: instead of aggregating in accordance with winning firms' headquarters, I aggregate the series by the location of the spending, information collected by the web-scraping exercise.

Table 13 presents the results. In the first four columns, I display the coefficient estimate β for each horizon h for the first exercise where I take out the winning firms' GVA when aggregating the series. They are statistically significant and positive. One year after the change, an additional €1 of public procurement spending generated €0.75 in gross value added in the same region originated from firms other than the public contest winners.

The last 4 columns in Table 13 present the results for the second exercise where I aggregate the procurement awards by the actual spending location. The results are positive but not statistically significant. These results have one strong implication for the literature on local fiscal multipliers because researchers usually compute these spillover effects on aggregate

series that capture where the money is spent, but not where the money is actually booked. If the pattern presented in Table 13 is common and spillover effects exist only in the location where money is booked, estimating spillover effects using subnational data might underestimate the real impact of this type of fiscal policy.

5.5 What drives the macroeconomic effects of local procurement?

In this subsection, I focus on the heterogeneous effects of procurement spending at the regional level. It is just as important to understand the differential effects of this type of fiscal policy as it is to quantify the macroeconomic effects of local procurement spending. Thus, I seek to determine whether different regional characteristics are of greater importance than others in shaping the output response to an increase in procurement spending. To do so, I extend equation (6) to include an interaction term and estimate state-dependent effects as follows:

$$\begin{aligned} \frac{GVA_{i,t+h} - GVA_{i,t-1}}{GVA_{i,t-1}} = & \alpha_i + \delta_t + \mathcal{I}_{i,t} \left[\beta^{h,(I)} \frac{Proc_{i,t}}{GVA_{i,t-1}} + \psi^{h,(I)} \mathbf{X}_{i,t-1} \right] \\ & + (1 - \mathcal{I}_{i,t}) \left[\beta^{h,(1-I)} \frac{Proc_{i,t}}{GVA_{i,t-1}} + \psi^{h,(1-I)} \mathbf{X}_{i,t-1} \right] + \varepsilon_{i,t+h} \end{aligned} \quad (9)$$

where the key independent variable $Proc_{i,t}$ is the sum of competitive procurement contracts won by firms whose headquarter is located in region i in period t and together with the vector of control variables $\mathbf{X}_{i,t-1}$ is interacted with a state indicator variable $\mathcal{I}_{i,t}$ that takes the value of 1 if a region i at year t belongs to the bottom 50% of a specific distribution.

To be precise, I evaluate three distinct dimensions. First, I classify region-year observations by the share of procurement spending that is awarded to small firms: the variable $\mathcal{I}_{i,t}$ takes the value of 1 if the share is above median. Second, I look at contracts' maturities: when aggregating the procurement spending at the regional-year level I also compute the average duration of the contract. Then, I allocate a region-year observation to the longer maturity group if the average contract length is above median for all regions which is approximately 300 days. Finally, I characterize each procurement contract as either government consumption or government investment by making use of textual analysis tools as explained in Section A1.1.1 and allocate a region-year observation to the government investment group if the ratio of investment to consumption is above median. Table 14 displays the results.

In Panel A, I recall the baseline estimation to facilitate comparison. Panel B corroborates the micro-level findings with regions with a higher share of small firms reacting more than regions with a higher share of big firms. Such a finding can be interpreted in light of the financial accelerator framework (Bernanke et al. 1996; Juarros 2020). Small firms face a higher credit spread in equilibrium which responds more to changes in firms' balance sheets. The procurement award improves firms' net worth and relaxes borrowing constraints as per evidence displayed in Section 3. This boosts borrowing, investment and production as per Section 4. Which in turn endogenously amplifies the effect on regional growth.

Panel C shows that while shorter maturity contracts are characterized by stronger effects in the short-run, the longer maturity contracts display more persistent effects. From the point of view of the firm, longer maturity contracts are helpful in reducing cash-flow uncertainty which allow firms to invest more. As a consequence we see stronger and more persistent effects of longer maturity contracts.

Finally, Panel D provides evidence that the goal of government spending matters. When there is more investment-oriented procurement spending in a region, the impact of the fiscal expansion is bigger. This can be rationalized together with the responses on productivity and private investment in R&D from Table 11, which can lead to a second output expansion over the medium-run (Antolin-Diaz and Surico 2022). Another way to rationalize this difference is based on the observation that investment-oriented procurement is more strongly associated with the tradables sector whereas consumption-oriented procurement is more strongly associated with the non-tradables sector. Models for the spending multiplier in a multi-sector economy predict that there is substantial heterogeneity in the multiplier depending on which sector the government buys, with the tradables sector being associated with higher multipliers (Bouakez et al. 2020; Proebsting 2022).

6. Discussion

Policy implications. The main goal of public procurement is to deliver goods and services necessary to accomplish the government's mandate to provide public goods in a timely and efficient manner. However, this policy tool is often used to promote economic growth. Policymakers designing its awards process should take into account the heterogeneous effects of public procurement operating via the credit channel. Governments and public institutions promote more growth when they allocate longer and investment-oriented procurement contracts to smaller and financially constrained firms.

The credit channel of public procurement is also relevant when considering the broader goal of financial stability. My sample covers the post Great Recession, a period characterized by bank lending being delimited by higher capital requirements and increased collateral requirements on the issuance of new loans (Degryse et al. 2021). Given that procurement contracts can be used as collateral, firms are increasing their credit position at lower rates without pledging real assets such as real estate, increasing their cash position, and thus reducing their credit risk.⁴ Moreover, given its considerable size, procurement spending is possibly a policy tool available to governments to smooth out the deleterious effects of financial cycles.

External validity. 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. Figure B.1 displays the amount of procurement spending in percentage of GDP and total government expenditures for a sample of OECD countries where Portugal is one of the

countries with lowest shares. 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. Figure B.2 presents the share of nonfinancial corporate debt accounted by bank loans, a pattern which only reinforces this relationship. Like Portugal, most OECD countries display a share higher than 80% in contrast to countries such as Mexico and the United States where firms can finance themselves outside the banking sector. Notwithstanding, even in the U.S., small and medium enterprises are highly dependent of bank credit (Caglio et al. 2022).

7. Conclusion

This paper has investigated the collateral role of public procurement contracts and their effect on corporate credit, investment, employment, and output at both the micro and macro-level.

First, I documented that winning a procurement contract fundamentally shapes corporate credit. I show that an additional €1 in procurement increases credit by €0.04 one year after the event. I further show that more than 80% of this increase in credit is cash-flow based and that the majority of it is accounted for by firm guarantees of future cash-flows. Second, I focused on the real effects of public procurement, showing that corporate investment and employment respond positively to unanticipated procurement awards. I quantified that an additional €1 in procurement increases corporate investment by €0.07. Such a result hints to the potential crowding-in of private investment in response to an increase in government spending and motivates the aggregate level analysis.

I further estimated the direct effect of this type of government spending on regional production. I quantify that an increase in procurement spending by €1 leads to an increase in gross value added of €1.8. Moreover, I found that procurement is an effective fiscal policy tool for the purposes of promoting investment, productivity, private consumption and employment without affecting inflation, nor in increasing borrowing at the aggregate level.

My results show substantial cross-sectional variation. Smaller and financially constrained firms are the ones driving the result at both the micro and macro level. This yields two key policy implications. First, governments promote more economic growth and thus obtain the biggest “policy returns” when they allocate longer procurement contracts to smaller firms. Second, because public procurement contracts can be used as collateral, firms can increase their credit position without pledging real assets such as real estate while reducing their credit risk. Hence, governments can use public procurement as a policy tool to promote financial stability.

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Tables

Table 1: Summary Statistics of Procurement Contracts

| | Mean | Std. Dev. | P5 | Median | P95 | Obs |
|--|---------|-----------|-----|--------|-----------|---------|
| Public Contests | | | | | | |
| Award (€) | 291,031 | 1,473,640 | 634 | 73,279 | 1,027,066 | 138,578 |
| Duration (Days) | 348 | 402 | 28 | 257 | 1,095 | 138,578 |
| # Contestants | 4 | 5.1 | 1 | 1 | 15 | 138,578 |
| Public Contests ($n > 1$) | | | | | | |
| Award (€) | 296,911 | 1,518,677 | 967 | 78,052 | 1,009,989 | 65,202 |
| Duration (Days) | 353 | 384 | 26 | 245 | 1,095 | 65,202 |
| # Contestants | 7.6 | 5.8 | 2 | 6 | 19 | 65,202 |
| Direct Awards | | | | | | |
| Award (€) | 35,897 | 425,979 | 154 | 9,700 | 94,030 | 896,654 |
| Duration (Days) | 181 | 256 | 1 | 60 | 730 | 896,654 |
| # Contestants | 0.4 | 1.4 | 0 | 0 | 3 | 896,654 |

Notes: This table presents selected summary statistics of procurement contracts characteristics divided by the awarding mechanism type. The first panel displays information for all of the public contests in my sample. The second panel focuses on public contests in which I can scrape information of at least 1 other competitor. And finally, the last panel displays the summary statistics for directly awarded contracts.

Table 2: Decomposition of the credit effect into collateral type

| | Horizon | | | |
|---|------------------|-------------------|-------------------|-------------------|
| | Impact | 1 Year | 2 Years | 3 Years |
| Panel A: Baseline Credit Response | | | | |
| Credit | 1.48** (0.62) | 3.97*** (1.32) | 4.59** (2.29) | 2.47** (1.19) |
| Panel B: Type of Credit Collateral | | | | |
| Real Mortgaged | -0.03 (0.09) | -0.01 (0.20) | 0.11 (0.27) | -0.03 (0.22) |
| Real Not Mortgaged | 0.04 (0.14) | 0.14 (0.23) | 0.10 (0.17) | 0.43** (0.21) |
| Firm Guarantee | 0.87* (0.47) | 2.05** (0.88) | 2.20* (1.15) | 2.58*** (0.95) |
| State Guarantee | 0.27* (0.16) | 0.89*** (0.23) | 1.29*** (0.39) | 1.01** (0.47) |
| Financial | -0.04 (0.17) | 0.26 (0.27) | -0.37 (0.28) | -0.67 (0.59) |
| Residual | -0.17 (0.24) | -0.36 (0.32) | -0.63 (0.45) | -0.12 (0.15) |
| Controls | ✓ | ✓ | ✓ | ✓ |
| FE | ✓ | ✓ | ✓ | ✓ |
| Observations | 35,555 | 27,229 | 19,973 | 13,367 |

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. (2) for each horizon $h = 0, 1, 2, 3$ corresponding to Figure 2. 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 the dependent variable was winsorized at percentiles 1 and 99. ***, ** and * indicate statistical significance at the 1%, 5% and 10% levels, respectively. The corresponding IRFs are in Appendix Figure B.5.

Table 3: Differential impact of earnings from procurement on corporate credit

| | Credit growth | | | |
|---------------------|-------------------|-------------------|-------------------|-------------------|
| | (1) | (2) | (3) | (4) |
| Award | 7.96*** (0.95) | 5.76*** (0.86) | 3.97*** (1.32) | 2.70** (1.17) |
| Sales Growth | | | | 0.70*** (0.25) |
| Controls | - | - | ✓ | ✓ |
| FE | - | ✓ | ✓ | ✓ |
| Observations | 60,704 | 50,898 | 27,229 | 27,229 |

Notes: This table examines the effect of the public procurement contract award on corporate credit. The unit of observation is the firm-year level. It reports the results of estimating specification (2) for $h = 1$ with different sets of control variables: column (1) without any control variable; column (2) only with firm and industry×year fixed effects; column (3) was obtained by estimating the baseline specification (2); and column (4) by adding an additional control variable of total sales growth from t to $t + 1$. The outcome variable is the growth rate of credit granted to firm i weighted by lagged total assets from year $t - 1$ to year $t + 1$. The key independent variable is the total contractualized amount of procurement contracts in period t received by firm i also scaled by lagged assets. Standard errors are clustered at the firm level and the dependent variable was winsorized at percentiles 1 and 99. ***, ** and * indicate statistical significance at the 1%, 5% and 10% levels, respectively.

Table 4: Severity of procurement effect on corporate credit by firms' characteristics

| | Credit Growth | | | | | | | | |
|---------------------------------------|-------------------|-------------------|-------------------|-------------------|-------------------|-------------------|-----------------|-------------------|-------------------|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
| Award | 5.47*** (1.97) | 4.12*** (1.33) | 2.34*** (0.74) | 4.01*** (1.56) | 4.58*** (1.56) | 2.16*** (0.82) | 8.95 (6.41) | 5.03*** (1.67) | 3.65*** (1.35) |
| Panel A: Financial Constraints | | | | | | | | | |
| ×Old | -3.75** (1.80) | | | | | | | | |
| ×Big | | -1.66** (0.81) | | | | | | | |
| ×No Credit Lines | | | 1.00*** (0.28) | | | | | | |
| ×High Liquidity | | | | -0.20 (1.56) | | | | | |
| ×High Leverage | | | | | -1.23 (1.39) | | | | |
| Panel B: Other Characteristics | | | | | | | | | |
| ×Long Contract | | | | | | 1.79** (0.83) | | | |
| ×High Proc Rev | | | | | | | -4.65 (6.46) | | |
| ×High Prod | | | | | | | | -1.86 (1.33) | |
| ×New Winner | | | | | | | | | 0.43 (0.79) |
| Controls | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| FE | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Observations | 27,229 | 27,229 | 27,229 | 27,229 | 27,228 | 27,229 | 27,229 | 26,620 | 27,229 |

Notes: This table examines the effect of the procurement award on corporate credit at the firm level. It reports the results of estimating specification (3). The outcome variable is the growth rate of credit granted to firm i . The key independent variable $Winner_{i,t}$ is a dummy variable taking value 1 if the firm won a competitive procurement contract in period t . Old is a dummy equal to 1 if firm's age is above median. Large is a dummy equal to 1 if firm's assets are above median. Long contract is a dummy equal to 1 if the average contract maturity that firm i received in period t is above median. Fin Constrain is a dummy equal to 1 if a firm does not have potential available credit. High Proc Rev is a dummy equal to 1 if a firm has an above median share of procurement awards over total revenues. High liquidity is a dummy equal to 1 if a firm as an above median ratio of cash and deposits over total assets. High leverage is a dummy equal to 1 if a firm has an above median ration of liabilities over total assets. High Prod is a dummy equal to 1 if a firm has an above median productivity measured by ... Standard errors are clustered at the firm level and dependent variable was winsorized at percentiles 1 and 99. ***, ** and * indicate statistical significance at the 1%, 5% and 10% levels, respectively.

Table 5: Heterogeneous elasticities of firm credit to procurement contract award

| | Credit Growth | | | |
|--|------------------|-------------------|-------------------|------------------|
| | Impact | 1 Year | 2 Years | 3 Years |
| Panel A: Baseline Specification | | | | |
| Award | 1.48** (0.62) | 3.97*** (1.32) | 4.59** (2.29) | 2.47** (1.19) |
| Panel B: Small versus Big Firms | | | | |
| Small Firms | 2.05** (0.98) | 5.47*** (1.97) | 8.46*** (3.20) | 5.24** (2.63) |
| Big Firms | 0.57** (0.24) | 1.72* (0.95) | 0.72 (0.82) | - 0.30 (0.68) |
| HAC p-value | 0.08 | 0.04 | 0.01 | 0.02 |
| Controls | ✓ | ✓ | ✓ | ✓ |
| FE | ✓ | ✓ | ✓ | ✓ |
| Observations | 35,555 | 27,229 | 19,973 | 13,367 |

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. (3) for each horizon $h = 0, 1, 2, 3$ corresponding to Figure 2. β^h is the *cumulative* response of credit from period $t + h$ relative to period $t - 1$. In Panel B, I study the differences between small and big firms defined as firms being below or above the median in terms of total assets across the sample. I present the baseline results for the coefficient β^h in Eq. (3) for each horizon $h = 0, 1, 2, 3$. 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.

Table 6: Winners versus Losers of Public Contests

| | Winners | | Losers | | T-test | Obs |
|---------------------------|-----------|---------|-----------|---------|--------|-------|
| | Mean | Median | Mean | Median | | |
| Firm Balance Sheet | | | | | | |
| Assets | € 240,000 | € 4,466 | € 198,000 | € 4,172 | 0.08 | 6,136 |
| Sales | € 199,000 | € 4,790 | € 156,000 | € 4,127 | 0.19 | 6,136 |
| Value Added | € 35,800 | € 1,208 | € 34,300 | € 1,129 | 0.74 | 6,136 |
| Employees | 312 | 31 | 328 | 30 | 0.54 | 6,134 |
| Firm Age | 24 | 20 | 23 | 20 | 0.52 | 6,136 |
| Liquidity | 13.8% | 6.6% | 13.8% | 6.9% | 0.67 | 6,136 |
| Total Hours Worked | 552,628 | 54,208 | 575,205 | 51,072 | 0.92 | 6,094 |
| Liabilities | € 188,000 | € 2,503 | € 159,000 | € 2,320 | 0.10 | 6,136 |
| Firm Credit Info | | | | | | |
| Total available credit | € 12,800 | € 807.7 | € 16,100 | € 696.7 | 0.42 | 4,200 |
| Total used credit | € 7,649 | € 296.1 | € 11,100 | € 256.2 | 0.36 | 4,200 |
| Total potential credit | € 5,118 | € 295.1 | € 5,065 | € 246.5 | 0.96 | 4,200 |
| Short maturity credit | € 3,452 | € 42.1 | € 5,492 | € 43.8 | 0.35 | 4,200 |
| Long maturity credit | € 4,196 | € 138.0 | € 5,582 | € 118.8 | 0.47 | 4,200 |

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 number of observations, mean, median, and the p-value of the two-sample t-test for whether the difference on each characteristic between the winner and the loser for each contest is equal to zero. Firm level variables are not winsorized.

Table 7: Elasticity of firm investment to procurement contract awards and the role of credit

| | Firm Investment | | | |
|----------------------|-----------------|----------|---------|---------|
| | (1) | (2) | (3) | (4) |
| Award | 26.23*** | 23.04*** | 7.39*** | 1.83 |
| | (0.95) | (0.86) | (1.32) | (1.17) |
| Credit Growth | | | | 0.25 |
| | | | | (0.16) |
| Interaction | | | | 0.76*** |
| | | | | (0.25) |
| Controls | - | - | ✓ | ✓ |
| FE | - | ✓ | ✓ | ✓ |
| Observations | 71,136 | 59,146 | 30,487 | 27,229 |

Notes: This table examines the effect of the procurement award on corporate investment. The unit of observation is the firm-year level. It reports the results of estimating specification (2) for $h = 1$ with different sets of control variables: column (1) without any control variable; column (2) only with firm and industry×year fixed effects; column (3) was obtained by estimating the baseline specification (2); and column (4) by adding an additional control variable of credit growth from t to $t + 1$ and its interaction with the award value. The outcome variable is the growth rate of firm i 's investment weighted by lagged total assets from year $t - 1$ to year $t + 1$. The key independent variable is the total contractualized amount of procurement contracts in period t received by firm i also scaled by lagged assets. Standard errors are clustered at the firm level and the dependent variable was winsorized at percentiles 1 and 99. ***, ** and * indicate statistical significance at the 1%, 5% and 10% levels, respectively.

Table 8: Severity of procurement effect on corporate investment by firms' characteristics

| | Firm Investment | | | | | | | | |
|--|--------------------|--------------------|-------------------|-------------------|--------------------|-------------------|--------------------|-------------------|-------------------|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
| Award | 10.78*** (2.67) | 8.84*** (1.90) | 7.16*** (1.49) | 7.66*** (2.03) | 12.97** (19.10) | 8.75*** (3.27) | 41.10*** (3.02) | 9.46*** (1.97) | 6.35*** (2.35) |
| Panel A: Financial Constraints | | | | | | | | | |
| ×Old | -5.06*** (2.60) | | | | | | | | |
| ×Big | | -3.82*** (1.34) | | | | | | | |
| ×No Credit Lines | | | 0.59 (0.73) | | | | | | |
| ×High Liquidity | | | | -0.24 (3.28) | | | | | |
| ×High Leverage | | | | | -7.33*** (2.74) | | | | |
| Panel B: Other Microfoundations | | | | | | | | | |
| ×Long Contract | | | | | | 6.30 (4.56) | | | |
| ×High Proc Rev | | | | | | | -31.43 (19.07) | | |
| ×High Prod | | | | | | | | -0.23 (2.51) | |
| ×New Winner | | | | | | | | | 1.20 (0.82) |
| Controls | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| FE | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Observations | 30,487 | 30,487 | 30,487 | 28,046 | 30,486 | 30,487 | 30,487 | 29,479 | 30,487 |

Notes: This table examines the effect of the procurement award on corporate investment at the firm level. It reports the results of estimating specification (3). The outcome variable is the growth rate of credit granted to firm i . The key independent variable the value of the firm's competitive procurement contract in period t . Old is a dummy equal to 1 if firm's age is above median. Large is a dummy equal to 1 if firm's assets is above median. Long contract is a dummy equal to 1 if the average contract maturity that firm i received in period t is above median. Fin Constrain is a dummy equal to 1 if a firm does not have potential available credit. High Proc Rev is a dummy equal to 1 if a firm has an above median share of procurement awards over total revenues. High liquidity is a dummy equal to 1 if a firm as an above median ratio of cash and deposits over total assets. High leverage is a dummy equal to 1 if a firm has an above median ration of liabilities over total assets. High Prod is a dummy equal to 1 if a firm has an above median productivity. Standard errors are clustered at the firm level and dependent variable was winsorized at percentiles 1 and 99. ***, ** and * indicate statistical significance at the 1%, 5% and 10% levels, respectively.

Table 9: Heterogeneous effects procurement contract awards on firm investment and employment

| | Investment | | | | Employment | | | |
|--|-------------------|-------------------|-------------------|--------------------|--------------------|--------------------|---------------------|--------------------|
| | Impact | 1 Year | 2 Years | 3 Years | Impact | 1 Year | 2 Years | 3 Years |
| Panel A: Baseline Specification | | | | | | | | |
| Elasticity | 3.52*** (0.82) | 7.39*** (1.32) | 9.51*** (2.50) | 12.32*** (4.40) | 19.42*** (4.93) | 31.10*** (7.89) | 30.44*** (10.19) | 30.88** (12.63) |
| Panel B: Small versus Big Firms | | | | | | | | |
| Small Firms | 4.34*** (1.83) | 9.22** (4.69) | 10.46** (5.20) | 11.17* (6.66) | 22.12*** (8.75) | 35.27** (17.71) | 36.45* (19.33) | 36.98* (20.67) |
| Big Firms | 1.50* (0.89) | 1.23 (1.38) | 0.50 (1.47) | - 0.05 (1.96) | 3.94*** (1.76) | 4.50* (2.44) | 2.27 (2.31) | 1.80 (1.12) |
| HAC p-value | 0.09 | 0.04 | 0.01 | 0.00 | 0.05 | 0.02 | 0.01 | 0.01 |
| Controls | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| FE | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Observations | 38,819 | 30,487 | 25,803 | 19,964 | 38,819 | 30,487 | 25,803 | 19,964 |

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. (2) for each horizon $h = 0, 1, 2, 3$ of either investment in non-current assets (first 4 columns) corresponding to Figure 6, employment (last 4 columns) from period $t + h$ relative to period $t - 1$. In Panel B, I study the differences between small and big firms defined as firms being below or above the median in terms of total assets across the sample. I present the baseline results for the coefficient β^h in Eq. (3) for each horizon $h = 0, 1, 2, 3$. 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.

Table 10: The regional effects of procurement spending

| | Horizon (Year) | | | |
|--|-------------------|-------------------|-------------------|-------------------|
| | (0) | (1) | (2) | (3) |
| Panel A: Local procurement effects | | | | |
| Proc | 1.76*** (0.46) | 1.75*** (0.53) | 2.02*** (0.51) | 2.40*** (0.66) |
| Panel B: The credit channel of public procurement | | | | |
| Proc | 1.39*** (0.35) | 1.51*** (0.44) | 1.77*** (0.49) | 2.12*** (0.62) |
| Proc × Credit | 0.28** (0.13) | 0.32** (0.15) | 0.30** (0.15) | 0.28* (0.16) |
| Controls | ✓ | ✓ | ✓ | ✓ |
| FE | ✓ | ✓ | ✓ | ✓ |
| Observations | 150 | 150 | 150 | 150 |

Notes: The unit of observation is the region-year level i, t . In Panel A, I estimate Equation (6). I present the results for the coefficient β^h for each horizon $h = 0, 1, 2, 3$. β^h 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 headquarter location. In Panel B, I estimate Equation (7) and present the estimates for the coefficients β^h and γ^h . I use a matched sample period from 2010 to 2016 (25 regions \times 6 years) so that changes in the estimate can't be associated to sample changes. Robust standard errors clustered at the region-level are in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% level for the typical null hypothesis that $\beta^h = 0$, however colored cells indicate whether the coefficient is statistically significantly different from one at the 10% level.

Table 11: The regional effects of procurement spending on other regional aggregates

| | Horizon (Year) | | | |
|------------------------|------------------|------------------|-------------------|-------------------|
| | (0) | (1) | (2) | (3) |
| GDP | 1.50** (0.73) | 1.42 (1.10) | 1.89 (1.49) | 2.43** (1.22) |
| Private R&D | 4.42** (1.88) | 3.51** (1.55) | 6.43*** (2.36) | 6.05*** (2.07) |
| TFP | 3.40 (2.84) | 6.58* (3.56) | 5.44*** (1.96) | 4.43 (3.46) |
| Employment | 0.13 (0.54) | 0.41 (0.90) | 1.04 (1.18) | 2.04* (1.19) |
| Compensation | 1.24 (0.79) | 1.69 (1.22) | 2.42 (1.57) | 3.86*** (1.21) |
| Credit | 0.24 (0.52) | 0.18 (0.45) | 0.29 (0.48) | 0.35 (0.32) |
| CPI | -0.01 (0.09) | 0.06 (0.13) | -0.07 (0.14) | -0.28 (0.17) |
| Controls | ✓ | ✓ | ✓ | ✓ |
| FE | ✓ | ✓ | ✓ | ✓ |
| Observations | 150 | 150 | 150 | 150 |

Notes: I estimate Equation (6). The unit of observation is the region-year level i, t . β^h can be interpreted as the response of different dependent variables from period $t+h$ relative to period $t-1$ to regional procurement spending aggregated at the headquarter location. Dependent variables are: regional gross domestic product (GDP), private sector expenditure on R&D; total factor productivity (TFP); number of active workers (Employment); total labour compensation; total granted credit (Credit); consumer price index (CPI). I use a matched sample period from 2010 to 2016 (25 regions \times 6 years) so that changes in the estimate can't be associated to sample changes. Robust standard errors clustered at the region-level are in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% level.

Table 12: Decomposition of the regional effects of procurement spending

| Horizon (Year) | HQ Aggregation | | | | Location Aggregation | | | |
|----------------------|-------------------|-------------------|-------------------|-------------------|----------------------|--------------------|--------------------|-------------------|
| | (0) | (1) | (2) | (3) | (0) | (1) | (2) | (3) |
| GDP | 1.50** (0.73) | 1.42 (1.10) | 1.89 (1.49) | 2.43** (1.22) | 1.23 (0.79) | 1.29** (0.58) | 1.78** (0.75) | 1.83*** (0.55) |
| Consumption | 1.64*** (0.61) | 1.47*** (0.52) | 1.91*** (0.78) | 1.94*** (0.91) | 0.89 (0.98) | 0.77 (1.18) | 3.14*** (0.88) | 2.94*** (0.77) |
| Gov. Spending | -0.09 (0.21) | -0.03 (0.34) | -0.04 (0.36) | 0.32 (0.41) | -0.29*** (0.09) | -0.47*** (0.16) | -0.47*** (0.16) | -0.33** (0.16) |
| Investment | 1.01** (0.50) | 1.21** (0.62) | 1.28** (0.56) | 1.68*** (0.65) | 0.03 (0.37) | 0.19 (0.44) | -0.95 (0.59) | -0.72 (0.61) |
| Net Exports | -1.06** (0.50) | -1.23 (0.89) | -1.26 (1.03) | -1.51* (0.90) | 0.60 (0.70) | 0.79 (0.85) | 0.06 (0.72) | -0.67 (0.64) |
| Controls | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| FE | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Observations | 150 | 150 | 150 | 150 | 150 | 150 | 150 | 150 |

Notes: I estimate Equation (6). The unit of observation is the region-year level i, t . I present the baseline results for the coefficient β^h for each horizon $h = 0, 1, 2, 3$. β^h can be interpreted as the response of regional gross domestic product and its components from period $t + h$ relative to period $t - 1$ to regional procurement spending aggregated at the headquarter location (first 4 columns) or at the spending location (last 4 columns). I use a matched sample period from 2010 to 2016 (25 regions \times 6 years) so that changes in the estimate can't be associated to sample changes. Robust standard errors clustered at the region-level are in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% level.

Table 13: The spillover effects of public procurement spending

| Horizon (Year) | HQ Spillover | | | | Location Spillover | | | |
|------------------|------------------|------------------|-----------------|----------------|--------------------|---------------|---------------|---------------|
| | (0) | (1) | (2) | (3) | (0) | (1) | (2) | (3) |
| Spillover | 0.68*** (0.2) | 0.75*** (0.3) | 0.85** (0.4) | 0.72* (0.4) | 0.25 (0.3) | 0.38 (0.3) | 0.47 (0.4) | 0.40 (0.5) |
| Controls | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| FE | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Observations | 150 | 150 | 150 | 150 | 150 | 150 | 150 | 150 |

Notes: I estimate Equation (6). The unit of observation is the region-year level i, t . I present the baseline results for the coefficient β^h for each horizon $h = 0, 1, 2, 3$. β^h can be interpreted as the response of regional production (proxied by GVA excluding the winning firms' GVA) from period $t + h$ relative to period $t - 1$ to an increase in procurement spending when aggregated by the winning firms' headquarter location (first 4 columns) and when aggregated by the spending location (last 4 columns). The sample period is 2010-2016 and rectangularized (25 regions \times 6 years). Robust standard errors clustered at the region-level are in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% level.

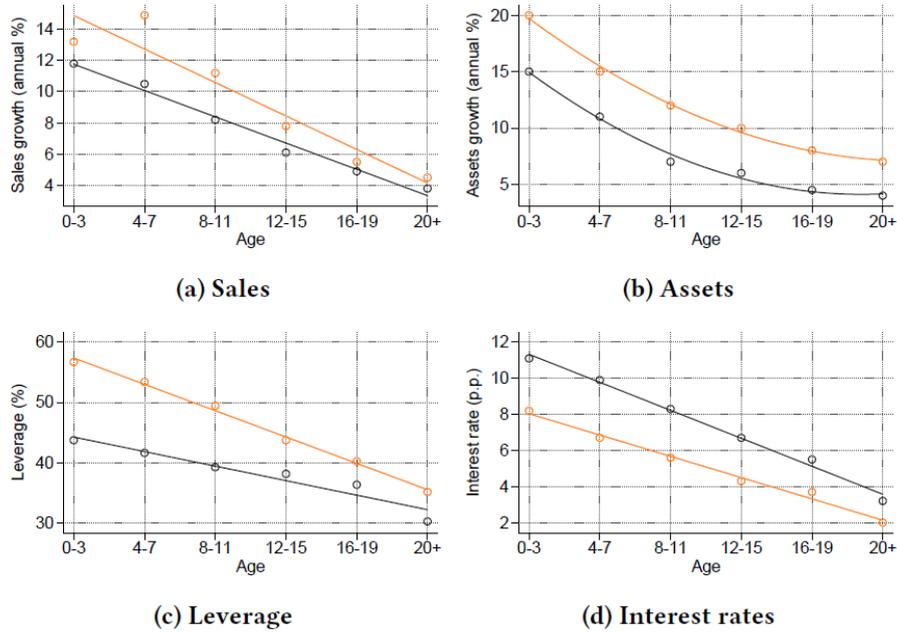
Table 14: GVA State Dependent Effects of Procurement Spending

| | GVA | | | |
|--|-------------------|-------------------|-------------------|-------------------|
| | Impact | 1 Year | 2 Years | 3 Years |
| Panel A: Baseline Specification | | | | |
| Proc | 1.76*** (0.46) | 1.75*** (0.53) | 2.02*** (0.51) | 2.40*** (0.66) |
| # Obs | 150 | 150 | 150 | 150 |
| Panel B: Small versus Big Firms | | | | |
| ×Small | 1.85*** (0.56) | 1.78*** (0.58) | 2.23*** (0.83) | 2.67*** (0.88) |
| ×Big | 1.50** (0.70) | 0.82 (0.75) | 0.51 (0.62) | 0.47 (0.49) |
| HAC Test | 0.80 | 0.43 | 0.09 | 0.05 |
| Panel C: Long versus Short Contract Maturity | | | | |
| ×Long | 0.54 (0.54) | 0.76 (0.51) | 1.80** (0.71) | 2.45*** (0.83) |
| ×Short | 1.54** (0.73) | 1.67*** (0.63) | 1.24** (0.50) | 0.97 (0.67) |
| HAC Test | 0.37 | 0.25 | 0.52 | 0.22 |
| Panel D: Government Investment vs Consumption | | | | |
| ×Investment | 1.80*** (0.62) | 1.36*** (0.47) | 1.78*** (0.64) | 2.15*** (0.76) |
| ×Consumption | -0.50 (1.37) | 0.14 (0.89) | 0.96 (1.66) | 1.11 (1.77) |
| HAC Test | 0.05 | 0.09 | 0.37 | 0.49 |

Notes: I estimate Equation (6). The unit of observation is the region-year level i, t . I present the baseline results for the coefficient β^h for each horizon $h = 0, 1, 2, 3$. In Panel A, β^h can be interpreted as the response of regional production from period $t+h$ relative to period $t-1$ to an increase in procurement spending when aggregated by the winning firms' headquarter location. In Panel B, I test whether regions differ when the share of procurement spending allocated to small firms is above median. In Panel C, I show state dependencies for the contract maturities. More precisely, I define that a region is in a state characterized by longer maturities when the average contract length in a particular year is above the median. Panel D shows results comparing the effects when procurement spending is allocated to the government investment versus consumption. The HAC Test presents the p-value of the difference between states using the heteroskedastic autocorrelated adjusted test. The sample period is 2010-2016 and rectangularized (25 regions \times 6 years). Robust standard errors clustered at the region-level are in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% level.

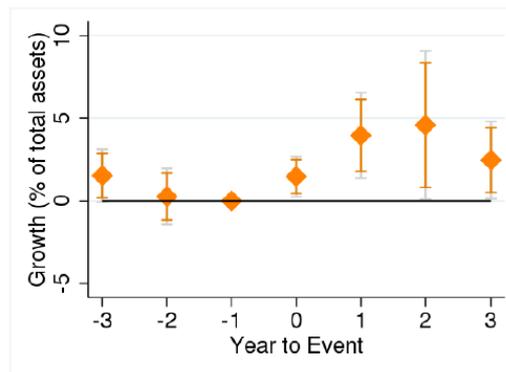
Figures

Figure 1: Firm finance and growth over the life cycle



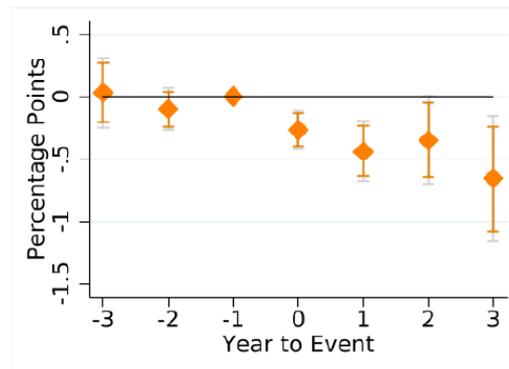
Notes: This figure plots the predicted values from regression (1). For each sub-figure, I plot the results for **procurement** firms in orange and **non-procurement** firms in gray. Sales accounts for total sales of goods, products and services. Assets are defined as total fixed tangible and intangible assets, and financial investments. Leverage is the ratio between effectively used credit and total assets. Interest rate is computed by dividing interest expenses by lagged effective credit.

Figure 2: Elasticity of firm credit to procurement contract awards



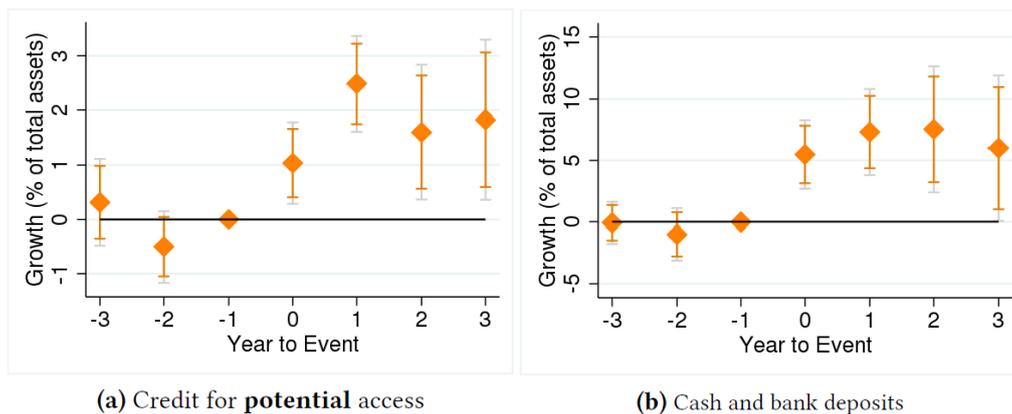
Notes: This figure displays the estimated coefficient β for each horizon h relative to the year of the award $h = 0$ from equation (2). The boxplot displays the coefficient estimate \diamond and the corresponding 95% (grey) and 90% (orange) confidence bands for the response of firm credit relative to 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 \times year fixed effects. All standard errors are clustered at the firm level.

Figure 3: 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 (2). 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 \times year fixed effects. All standard errors are clustered at the firm level.

Figure 4: Elasticity of credit lines and liquidity to procurement contract awards

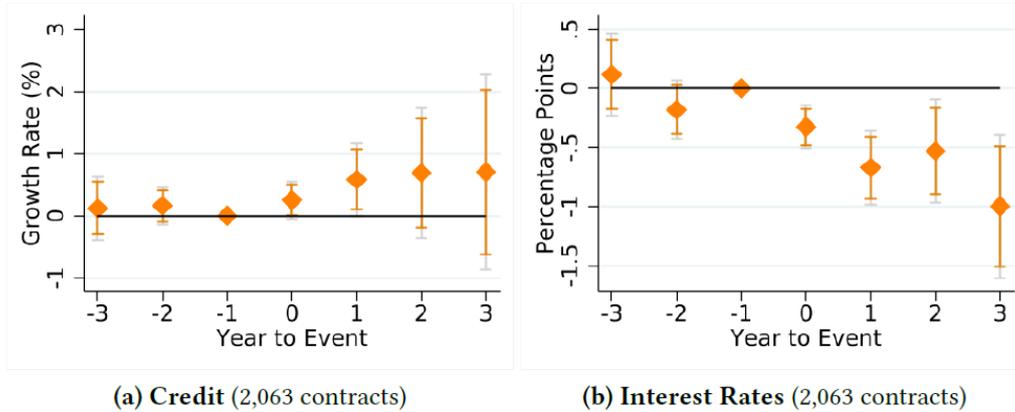


(a) Credit for **potential** access

(b) Cash and bank deposits

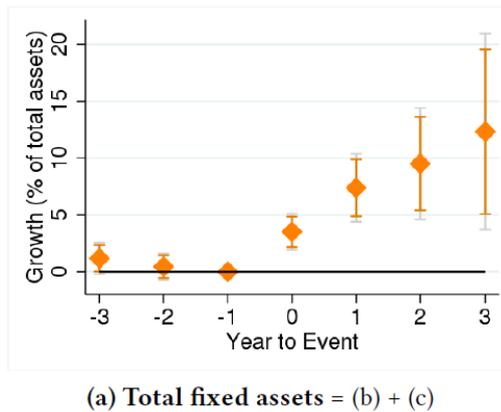
Notes: This figure displays the estimated coefficient β for each horizon h relative to the year of the award $h = 0$ from equation (2). The boxplot displays the coefficient estimate \diamond and the corresponding 95% (grey) and 90% (orange) confidence bands for the response of potential credit (Fig. 4a) and cash and bank deposits (Fig. 4b) 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 \times year fixed effects. All standard errors are clustered at the firm level.

Figure 5: Effects of public procurement using contest losers as counterfactual



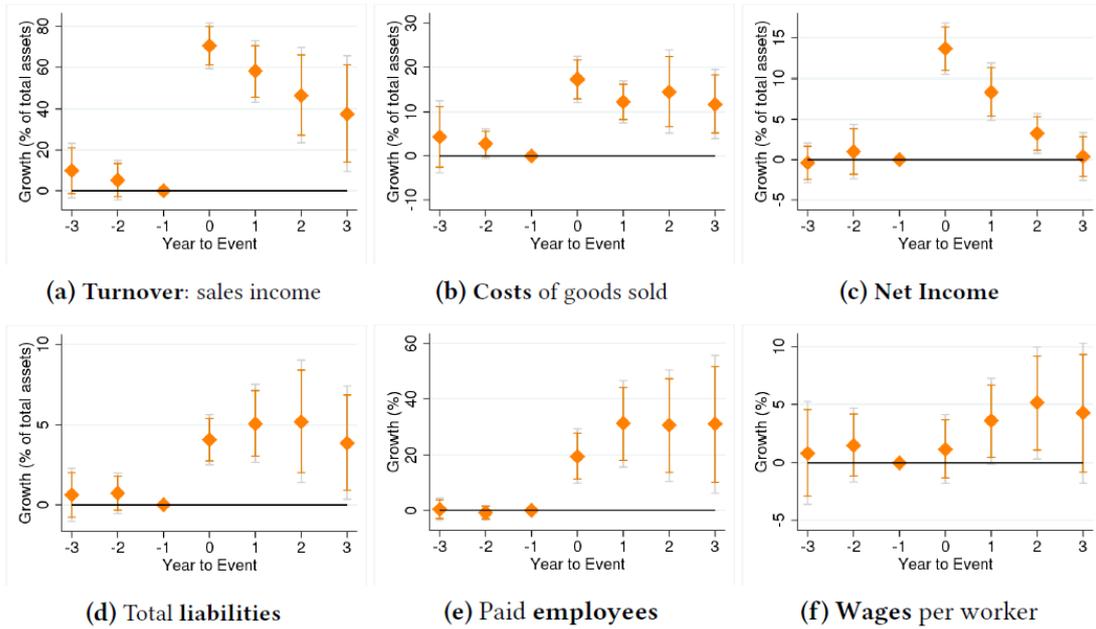
Notes: This figure displays the estimated coefficient β for each horizon k relative to the year of the award $k = 0$ from equation (5). The boxplot displays the coefficient estimate \diamond and the corresponding 95% (grey) and 90% (orange) confidence bands for the response of firm credit relative to total assets in the previous year to winning a procurement contract. The estimation includes firm and industry \times year fixed effects. All standard errors are clustered at the firm level.

Figure 6: Elasticity of firm investment to procurement contract awards



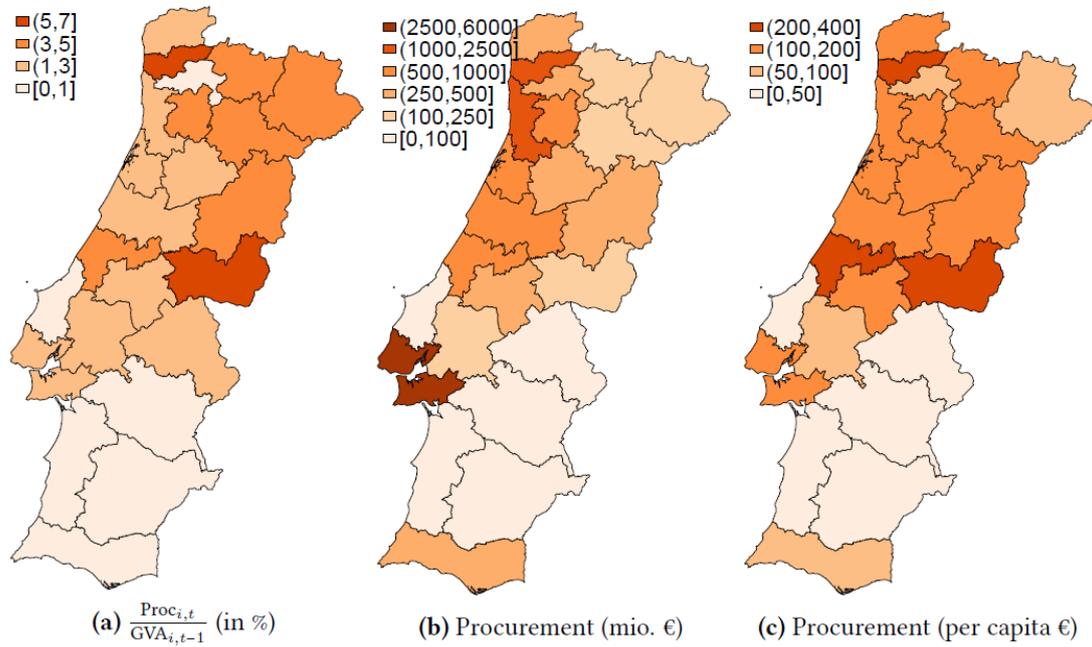
Notes: This figure displays the estimated coefficient β for each horizon h relative to the year of the shock $h = 0$ from equation (2). The boxplot displays the coefficient estimate \diamond and the corresponding 95% (grey) and 90% (orange) confidence bands for the response of each dependent variable on firm investment, as described in the sub-caption, 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 \times year fixed effects. All standard errors are clustered at the firm level.

Figure 7: The effects of procurement contract awards on other firm dynamics



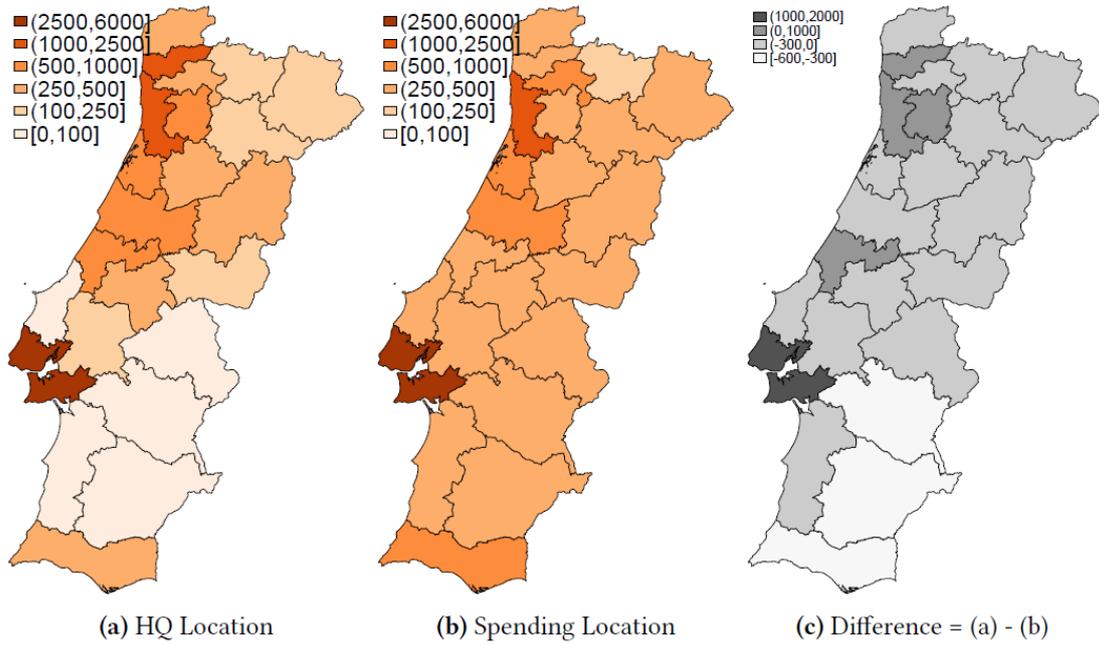
Notes: This figure displays the estimated coefficient β for each horizon h relative to the year of the award $h = 0$ from equation (2). The boxplot displays the coefficient estimate \diamond and the corresponding 95% (grey) and 90% (orange) confidence bands for the response of total sales (Fig. 7a), costs of goods sold 7b, net income 7c, liabilities (Fig. 7d), employees (Fig. 7e), and wages (Fig. 7f) in the previous year 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 \times year fixed effects. All standard errors are clustered at the firm level.

Figure 8: Cross-sectional variation in procurement spending



Notes: This Figure displays 3 maps the cross-sectional variation across continental Nuts III Portuguese regions on the amount of procurement obtained via public contests. From left to right, maps display: (a) the main regressor in equation (6) $\frac{Proc_{i,t}}{GVA_{i,t-1}}$ 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. Darker colors represent more spending.

Figure 9: Differences in procurement spending by aggregation method



Notes: This Figure displays 3 maps the cross-sectional variation across continental Nuts III Portuguese regions on the amount of procurement obtained via public contests in million Euros. From left to right, the maps display: (a) total procurement value with aggregation using the headquarter location; (b) total procurement value with aggregation using the spending location; and (c) their difference. Darker colors represent more spending.

Appendix

A1 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.1.1 This detailed dataset was then merged to the procurement contracts information that were web scraped as follows.

A1.1 Web Scraping: Assembling the Procurement Data

The data on public procurement comes from scrapped html files from 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. Moreover, some contracts also have the actual contract signed by both parties with detailed information about the good or service being provided as well as more information on the payment structure, more details follow in section A1.1.1.

The scraped files contain 1,035,232 contracts, from which 138,578 were public contests. I proceed by filtering the data. First, I drop 720 contracts with a negative reported price. Then, 2,865 auctions with more than one winner. Auctions with more than one winner 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 44,919 such observations. I merge this panel with the cleaned firm level panel data described in section A1 and obtain 38,431 instances where 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 34,490 observations which is the baseline procurement dataset. Table A.2 provides detailed information on the sample cleaning.

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 C.8. Moreover, information on the award might be biased downwards as some entities display the per unit or monthly price of the contract. Due to the latter, I winsorize contracts with abnormal winning

bids at percentile 1 and 99. 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.

A1.1.1 Optical character recognition tools: Collecting information from written contracts

During the web scraping exercise, I also collected 37,768 pdf (60 GB) files referring to contracts celebrated after public contests with detailed information on the good or serviced being acquired by the public entity as well as the payment method and timeline.

After downloading these contracts, I used an optical character recognition software, Tesseract, to read all pdf files and perform text recognition on them. In the end, I obtained for each pdf file, the text in a format that can then be analyzed using Python textual analysis tools.

Once this process ended, I used Python textual analysis tools to classify each contract according to its main goal of either government investment or government consumption. To do that, I assembled a list of words, dictionaries, that would characterize each government spending type. For now, I am deeming a contract as investment-oriented if words like “building, investment, requalification, construction, infrastructure, road, research and development” are in higher proportion.

A1.2 Aggregate Data

In section 5, I also use regional data to complement the analysis. Table A.3 gives further details on their definition, computation, and source.

There are two important adjustments I need to mention. First, I adjust all these variables to real terms by using the GDP deflator 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 variables such as CPI and GDP deflator are the same for each NUTS3 region belonging to the same NUTS2 region. For all other stock variables such as GERD or total labor compensation, I weight them by the GDP share that a NUTS3 region accounts for each NUTS2 region.

Contrary to the remaining variables private consumption and total factor productivity (TFP) measures are constructed by me. GDP is composed by private consumption, government spending, investment, and net exports. I have information on all variables except consumption with caveats of investment being proxied by gross fixed capital formation and government spending being proxied by the gross value added of the non-market sector (Gabriel et al. 2022a). Hence, I can construct a residual measure that can be interpreted as mostly accounting for private consumption. To make sure this series are sensible I aggregate them at the country level and obtain close proportions to national accounts data: consumption accounted for 70% of GDP, government spending (25%), investment (8%), and net exports (-6%).

To construct a measure of total factor productivity, I follow Gabriel et al. (2022a). 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:

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

where $GV A$ 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.

C 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. 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](#).

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 C.8) 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 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 C.9 displays.

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 C.3, 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, not all public works come from the construction sector. Table C.4 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.

Tables

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 |
| Effective Credit | Credit that a firm used effectively |
| Potential Credit | 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: Public procurement contracts: sample cleaning

| Step | Description | Observations |
|------|---|--------------|
| 0 | Web scraped contracts | 1,035,232 |
| 1 | Keep public contests | 138,578 |
| 2 | Keep positive awards | 137,858 |
| 3 | Keep contracts with solely one winner | 134,993 |
| 4 | Collapse same year awards | 44,919 |
| 5 | Merge with Portuguese tax information | 38,431 |
| 6 | Keep private non-financial corporations | 37,980 |
| 7 | Keep only non-liquidated firms | 37,906 |
| 8 | Keep only firms with lagged total assets above p1 (€827.28) | 37,829 |
| 9 | Keep only firms with available information on lagged assets | 36,575 |
| 10 | Keep only firms with at least one paid employee | 34,490 |

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

Table A.3: Variable Definitions: Aggregate Data

| Variable Name | Definition | Source |
|---------------------|---|----------------|
| GDP | Final value of the goods and services produced within (NUTS III) | PORDATA |
| Gross Value Added | Gross output of nonfinancial corporations less the value of intermediate goods and services consumed in production (NUTS III) | PORDATA |
| Investment | Private sector gross fixed capital formation (NUTS III) | PORDATA |
| Exports | Value of total exports (NUTS III) | PORDATA |
| Imports | Value of total imports (NUTS III) | PORDATA |
| Government Spending | Proxied by gross value added of the non-market sector, check Gabriel et al. (2022a) for more details (NUTS III) | Gabriel et al. |
| Consumption | Residual obtained via the formula: GDP - Investment - Net Exports - Government Spending (NUTS III) | PORDATA |
| GDP Deflator | GDP Deflator (NUTS II) | Eurostat |
| CPI | Consumer Price Index (NUTS II, 2010=100) | PORDATA |
| GERD | Private sector expenditure on R&D (NUTS II) | Eurostat |
| Employment | Number of active workers (NUTS III) | ARDECO |
| Compensation | Total 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.

Table B.1: Conditional Random Assignment Test within Public Contests

| Dep. var: $Winner_{z,j,t}$ | T-Statistic |
|-----------------------------|-------------|
| Assets | 1.71* |
| Sales | 0.84 |
| Value Added | 0.56 |
| Employees | 0.02 |
| Liquidity | 0.36 |
| Total Hours | 1.01 |
| Liabilities | -1.42 |
| Total Credit | -0.60 |
| FE | ✓ |
| R-squared | 0.5789 |
| Observations | 33,940 |
| Lagged Firm Characteristics | |
| F-statistic | 2.3 |
| P-value | 0.21 |

Notes: This table examines the effect of the procurement award on corporate credit at the firm level. It reports the results of estimating specification (4). The key dependent variable $Winner_{z,j,t}$ is a dummy variable taking value 1 if the firm won a *competitive* procurement contract in period t . Under the null hypothesis of random assignment, the variables determined before the contest should not have any predictive power, conditional on the auction fixed effects. The reported p-value is from the test of the joint significance for all independent variables which I can't reject. Standard errors are clustered at the firm level. ***, ** and * indicate statistical significance at the 1%, 5% and 10% levels, respectively.

Table B.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 | 14,100 | 248,000 | 11 | 287 | 6,053 | 34,490 | 837 | 45,000 | 0 | 14 | 392 | 3,049,057 |
| Turnover | 21,600 | 208,000 | 237 | 1,927 | 3,391 | 34,490 | 963 | 17,700 | 15 | 115 | 1,059 | 3,049,057 |
| Liquidity | 14.7% | 17.4% | 0.6% | 7.7% | 39.5% | 34,490 | 19.7% | 30.0% | 0.5% | 9.4% | 57.1% | 3,049,057 |
| Total liabilities | 16,100 | 205,000 | 111 | 1,052 | 13,200 | 34,490 | 954 | 36,100 | 9 | 86 | 817 | 3,049,057 |
| Employees | 120 | 577 | 3 | 20 | 169 | 34,490 | 9 | 87 | 1 | 3 | 13 | 3,048,990 |
| Wages per worker | 21.8 | 16.3 | 9.8 | 17.9 | 37.3 | 34,490 | 12.8 | 11.7 | 5.8 | 10.6 | 21.3 | 3,048,990 |
| Award | 405 | 791 | 15 | 50 | 1,015 | 34,490 | | | | | | |
| Total Credit | 4,401 | 26,300 | 21 | 475 | 7,018 | 27,236 | 472 | 6,381 | 2 | 31 | 477 | 1,659,673 |
| Effective Credit | 2,137 | 12,400 | 1 | 208 | 3,607 | 27,236 | 359 | 4,496 | 0 | 23 | 382 | 1,659,673 |
| Potential Credit | 2,264 | 15,600 | 3 | 137 | 2,821 | 27,236 | 112 | 3,165 | 0 | 2 | 68 | 1,659,673 |
| Non-performing Credit | 46 | 1,268 | 0 | 0 | 0.3 | 27,236 | 18 | 8,741 | 0 | 0 | 0.2 | 1,659,673 |
| Real Col. Mortgaged | 344 | 4,919 | 0 | 0 | 250 | 27,236 | 106 | 1,754 | 0 | 0 | 63 | 1,659,673 |
| Real Col not Mortgaged | 160 | 2,877 | 0 | 0 | 23 | 27,236 | 32 | 1,542 | 0 | 0 | 3 | 1,659,673 |
| Financial Col. | 308 | 4,332 | 0 | 0 | 138 | 27,236 | 62 | 2,469 | 0 | 0 | 12 | 1,659,673 |
| Personal guarantee Col. | 865 | 5,268 | 0 | 70 | 1,620 | 27,236 | 153 | 1,569 | 0 | 8 | 190 | 1,659,673 |
| State guarantee Col. | 182 | 1,155 | 0 | 0 | 416 | 27,236 | 23 | 600 | 0 | 0 | 20 | 1,659,673 |
| Other Col. | 307 | 3,545 | 0 | 0 | 78 | 27,236 | 36 | 1,361 | 0 | 0 | 0 | 1,659,673 |
| Implicit interest rate | 7.4% | 7.4% | 1.5% | 4.9% | 20.6% | 21,623 | 6.6% | 6.4% | 1.0% | 4.8% | 13.9% | 1,227,784 |

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. Variables are not winsorized.

Table C.3: 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.

Table C.4: Which economic sectors received procurement contracts in 2019 and 2018?

| CPV | Description | 2019 | | 2018 |
|-----|---|--------|-------|-------|
| | | Number | Value | Value |
| 45 | Construction | 12.9% | 42.5% | 32.5% |
| 33 | Medical equipment, pharmaceuticals and personal care products | 40.1% | 17.7% | 18.6% |
| 9 | 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.

Figures

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) |

Notes: This figure presents part of the information

Figure B.1: Public procurement expenditures statistics from OECD (2021)

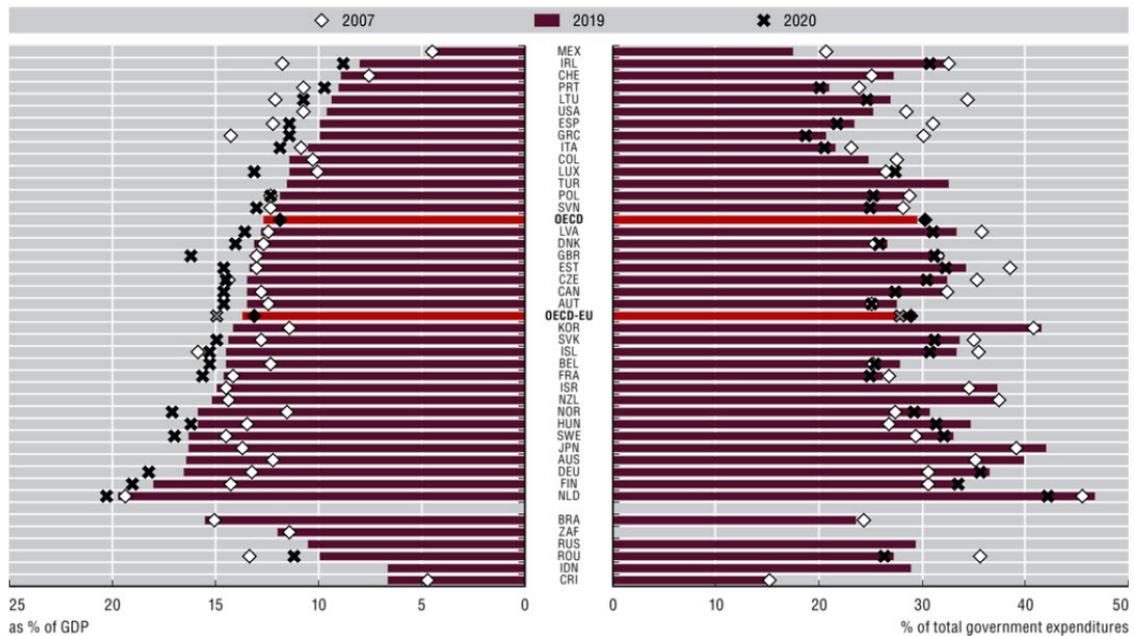


Figure B.2: Share of nonfinancial corporations debt accounted by bank loans

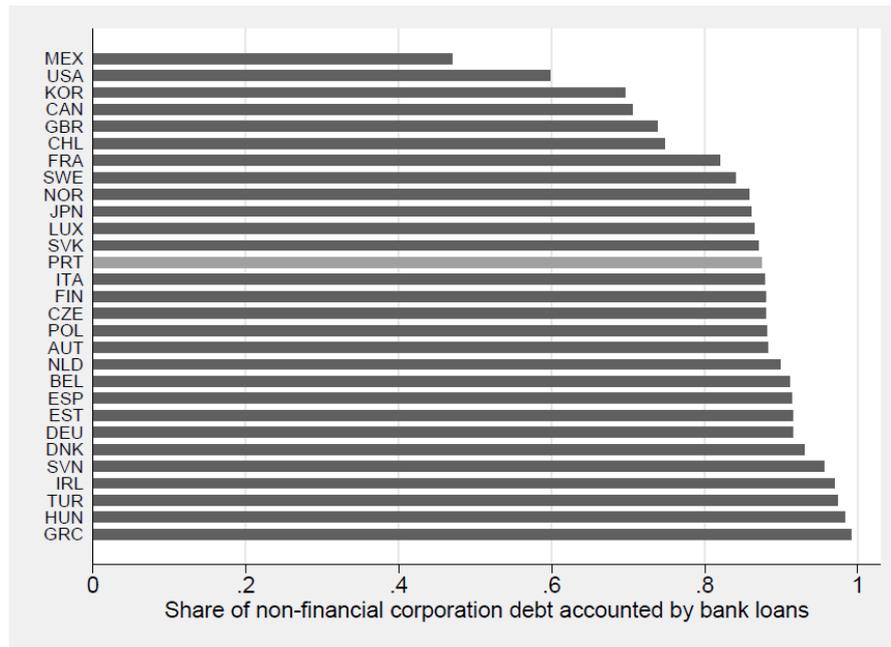
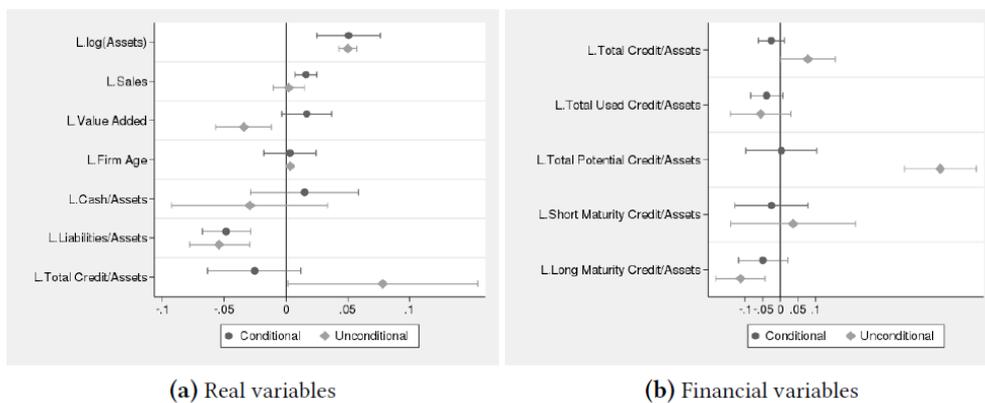
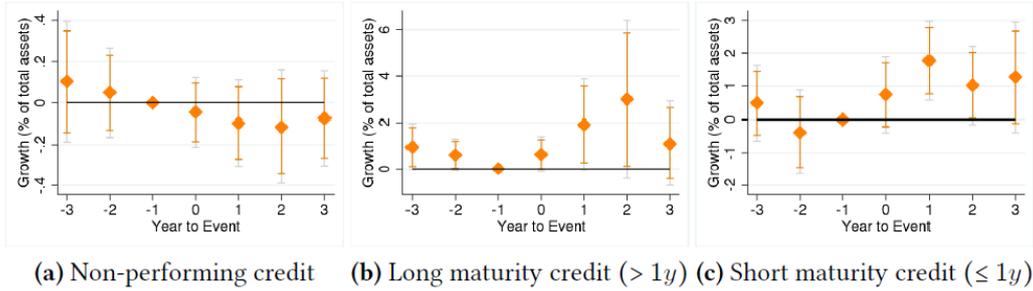


Figure B.3: Correlation between winning a public contest and predetermined firm characteristics



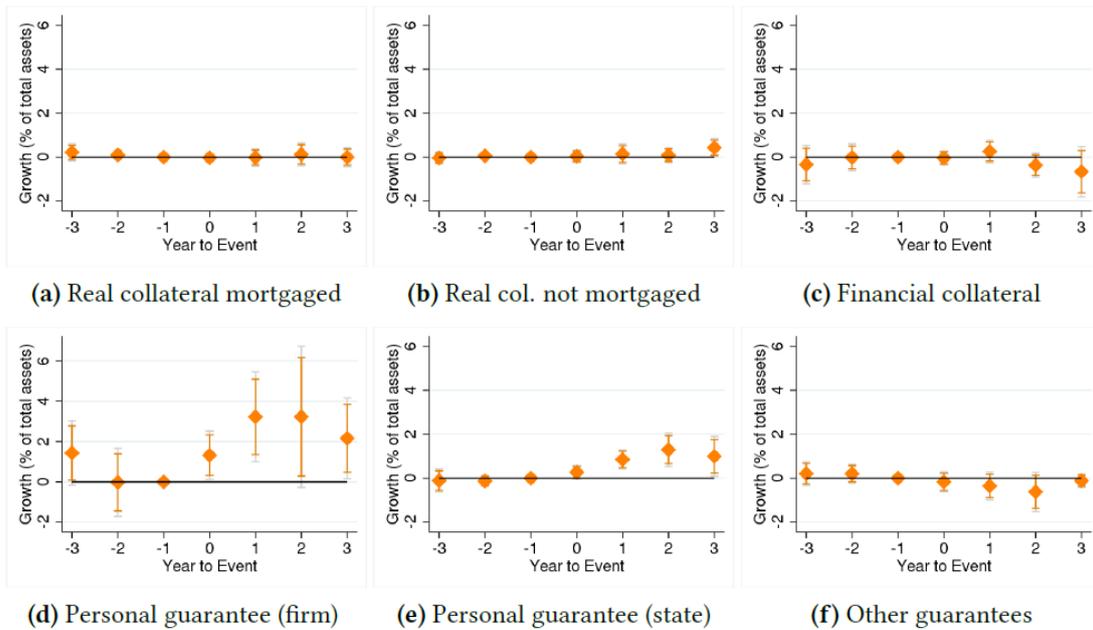
Notes: These figures show the correlation between winning a procurement contract and firm characteristics measured at $t - 1$. Regressions are not weighted and variables winsorized at the percentiles 1 and 99. All regressions include industry \times year fixed effects. The dot is the point estimate and the bar is the 95% confidence interval. The estimation includes firm and industry \times year fixed effects. All standard errors are clustered at the firm level.

Figure B.4: The effects of winning procurement awards on other firm credit dynamics



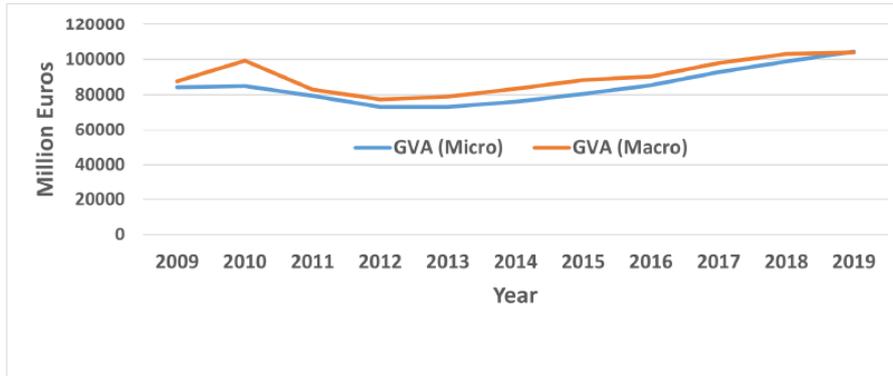
Notes: This figure displays the estimated coefficient β for each horizon h relative to the year of the award $h = 0$ from equation (2). The boxplot displays the coefficient estimate \diamond and the corresponding 95% (grey) and 90% (orange) confidence bands for the response of non-performing credit and the amount of issued credit of long and short maturity in the previous year normalized by lagged assets 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 \times year fixed effects. All standard errors are clustered at the firm level.

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



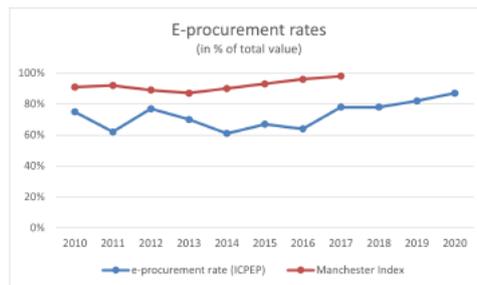
Notes: This figure displays the estimated coefficient β for each horizon h relative to the year of the award $h = 0$ from equation (2). The boxplot displays the coefficient estimate \diamond and the corresponding 95% (grey) and 90% (orange) confidence bands for the response of each dependent variable on firm credit collateral, as described in the sub-caption, in the previous year normalized by lagged assets 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 \times year fixed effects. All standard errors are clustered at the firm level.

Figure B.6: Gross value added aggregation



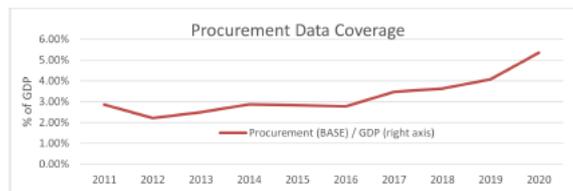
Notes: This figure compares two series for gross value added (GVA). GVA (Micro) is computed as the sum of sales minus production costs for each firm i in Portugal. GVA (Macro) is computed as the difference between output and intermediate consumption from national accounts data.

Figure C.7: E-procurement rates



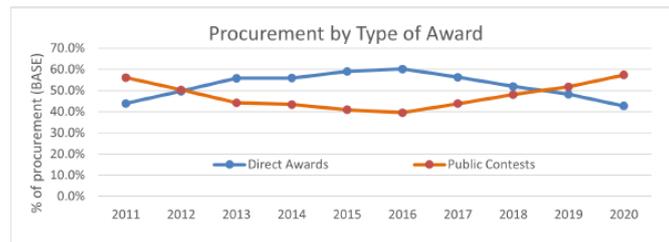
Notes: The graph plots the percentage of public procurement contracts that were available electronically (in % of total procurement value) according to IMPIC reports and, following the Manchester Index, the percentage of contracts above the EU threshold (€75,000 (goods and services) and €150,000 (works)).

Figure C.8: Data coverage (% GDP)



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).

Figure C.9: Procurement by award type



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

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