Fiscal Policy and Credit Supply: The Procurement Channel*

Diana Bonfim† Miguel A. Ferreira‡ Francisco Queiró§
Sujiao (Emma) Zhao¶

November 30, 2023

Abstract

We measure how cuts to public procurement propagate through the banking system in a financial crisis. During the European sovereign debt crisis, the Portuguese government cut procurement spending by 4.3% of GDP. We find that this cut saddled banks with non-performing loans from government contractors, which led to a reduction in credit supply to other firms. The credit supply shock, in turn, caused firm output to decline. In a general equilibrium model, our firm-level estimates imply an aggregate elasticity of credit supply with respect to aggregate demand of 1.6 and a credit-driven fiscal multiplier of 0.6.

JEL classification: G01, G20, G31, H57
Keywords: Credit supply, Financial crises, Fiscal policy, Aggregate demand, Public procurement, Fiscal multipliers, Bank-sovereign loop

---

*We thank Rui Albuquerque, Manuel Amador, Murillo Campello, Gabriel Chodorow-Reich, Hans Degryse, Tim Eisert, Felipe Iachan, Miguel H. Ferreira, Filipe Gomes, Helene Hall, Nicholas Kozeniauskas, Jean-Stéphane Mésonnier, Karsten Müller, Orkun Saka, Dominik Supera, and Andrei Shleifer; participants at the Barcelona Summer Forum 2021, Cambridge-Nova Workshop, CEBRA 2021, Day-Ahead Workshop on Financial Regulation, DebtCon5 at the European University Institute, European Finance Association 2021, Lubrafin 2021, Lubramacro 2022, PEJ 2022, SFA 2022, Swiss Winter Conference on Financial Intermediation 2022, and 4th Conference on Contemporary Issues in Banking; and seminar participants at Banco de España, Banco de Portugal, Bank of Greece, Banque de France, BIS, Ca' Foscari, Chicago Fed University of Venice, Corporate Finance Webinar, CSEF University of Naples Federico II, Humboldt University, Rotterdam School of Management, University of Bristol, University of Georgia, University of Luxembourg, University of Minho, University of Sussex, and Warwick Business School for helpful comments. These are our views and do not necessarily reflect those of Banco of Portugal, the ECB, or the Eurosystem. Bonfim, Ferreira, and Queiró acknowledge financial support from the Fundação para a Ciência e Tecnologia (Grant PTDC/EGE-OGE/4714/2021).

†Banco de Portugal, ECB and Católica Lisbon. Email: dbonfim@bportugal.pt.
‡Nova School of Business and Economics, ECGI and CEPR. Email: miguel.ferreira@novasbe.pt.
§Nova School of Business and Economics. Email: francisco.queiro@novasbe.pt.
¶Banco de Portugal and Faculdade de Economia do Porto. Email: szhao@bportugal.pt.

1 Introduction

A central amplification mechanism in the 2010-2011 European sovereign debt crisis was the feedback between sovereign and bank distress operating through bank holdings of domestic sovereign debt, whose value plunged as sovereign yields rose (Acharya, Dreschsler and Schnabl, 2014; Gennaioli, Martin and Rossi, 2014; Bocola, 2016). Yet, even though sovereign yields had largely normalized by the end of 2012, except for Greece, credit and economic activity in crisis-hit countries remained depressed for several years afterward (Figure 1). At the same time, there is evidence that the multipliers associated with fiscal consolidation in Europe during the crisis may have been unusually large (Blanchard and Leigh, 2013; House, Proebsting and Tesar, 2020).

This paper identifies a mechanism that can help explain the slow recovery and the large multipliers. In addition to sovereign debt holdings, sovereigns, and banks are linked indirectly through bank lending to firms with public procurement contracts. When governments cut procurement spending, default risk increases for firms that lose contracts, which affects the balance sheets of banks that lend to these firms. Weaker banks, in turn, tighten credit supply, depressing real activity and amplifying fiscal multipliers.

In Greece, Ireland, Italy, Portugal, and Spain, the countries at the epicenter of the crisis, public procurement was cut by 1.7% to 7.2% of GDP (Figure 2) as governments strove to restore access to capital markets. These cuts were a major component of consolidation efforts in these countries, accounting for 57% to 98% of the reductions in primary budget deficits achieved in the same period. We study the case of Portugal, where we are able to merge administrative data on the universe of public procurement contracts, bank-firm lending relationships, firm financial statements, and bank supervisory data.

We first show that the distress induced by these procurement cuts was large enough to affect the banking system. At the onset of the crisis, public procurement contracts in our matched data accounted for 18% of sales for the firms that held them, henceforth called government contractors, and these firms accounted for 33% of value added in the corporate sector. Bank lending to government contractors amounted to 17% of total corporate lending, 75% of total bank equity, and 90% of domestic sovereign debt bank holdings. Both in terms of their ability to absorb losses and in comparison to sovereign debt, banks were significantly exposed to public procurement.

As the crisis hit, the government cut procurement by 4.3% of GDP. Government contractors subsequently experienced steep declines in output, and the resulting distress spilled over into bank balance sheets. Non-performing loans (NPLs) from contractors increased six-fold in the following years, an amount equivalent to 13% of precrisis bank equity. For comparison, losses in the market value of precrisis domestic sovereign debt bank holdings attained a similar peak of 14% of bank equity in early 2012. But while the drop in sovereign yields from 2012 onwards quickly reversed these losses and led to large gains in the market value of sovereign bonds (Acharya et al., 2019), NPLs from contractors grew steadily until 2015, pressuring bank balance sheets for much longer.

To estimate the effect of these procurement cuts on credit supply, we exploit variation across banks in exposure to the cuts via their credit portfolios. We regress credit growth on exposure at the bank-firm level, restricting the sample to non-contractors to focus on the effects of the cuts operating through the banking system. Our exposure measure can be interpreted as a bank-weighted drop in aggregate demand driven by procurement spending, with the weights given by precrisis bank lending shares. To the extent that shocks to procurement propagate similarly to other shocks to government and private spending (Chodorow-Reich, Nenov and Simsek, 2021; Guren et al., 2021; Wolf, 2023), our specification yields a micro-level estimate of the elasticity of credit supply with respect to aggregate demand shocks in general.

Our estimate for this elasticity at the bank-firm level is 2.5. The identifying assumption we make is that, conditional on observables, procurement exposure was uncorrelated with other determinants of credit growth. We evaluate this assumption in three ways. First, we examine trends in credit growth before the procurement cuts, and find that they were uncorrelated with exposure.

Second, we exploit the shift-share structure of our exposure measure. Following Goldsmith-Pinkham, Sorkin and Swift (2020), we view identification as coming from the exogeneity of contractor credit shares, and we decompose our effect into a weighted-average of contractor-specific estimates obtained by instrumenting exposure with the credit share of each contractor. The weights indicate that our results are predominantly driven by a subset of contractors in the construction sector. Identification therefore hinges largely on the exogeneity of these contractors' credit shares. Consistent with this assumption, credit growth before the cuts was uncorrelated with the weighted average credit share to construction contractors. The same holds for other sectors.

Third, we test whether our estimates remain stable as we add a series of controls for possible confounders. The results are unchanged when we control for exposure to the construction sector as a whole. The same is true, more generally, when we control for exposure to other shocks to the quality of bank loan portfolios, using a shift-share predictor of NPL growth for non-contractors based on precrisis bank exposures by sector. Our estimates are slightly stronger when we control for firm-level credit demand in a within-firm specification estimated on the sample of firms with at least two banking relationships (Khwaja and Mian, 2008). This suggests our baseline specification is conservative.[1] The results are also unaffected when we control for bank-specific credit demand shocks induced by bank specialization, as measured by shift-share predictors of credit growth based on precrisis financing and collateral types, sectors, or locations.

[1]Our baseline approach allows us to include firms with only one banking relationship and to use the same specification in bank-firm and firm-level analyses.

In models of financial frictions through bank balance sheets, the effect of a shock to asset quality on bank net worth, and hence on credit supply, depends on bank leverage (Gertler and Kiyotaki, 2010). Consistent with this prediction, we estimate that the elasticity of credit supply decreases by about 0.3 for each percentage point of precrisis bank equity as a fraction of total assets. We also estimate a smaller elasticity for banks that were recapitalized during the crisis.

Next, we evaluate whether firms were able to replace the credit lost from more exposed banks with credit from other banks. Our estimate of the firm-level elasticity of credit supply is 1.4, which corresponds to 58% of its bank-firm level counterpart. This suggests that firms were able to substitute 42% of the credit they lost. In line with previous studies (Bentolila, Jansen and Jiménez, 2018), the level of substitution was lower for firms with only one bank relationship before the crisis (25%) than for firms with multiple relationships (59%). The high prevalence of firms with multiple relationships in Portugal (Kosekova et al., 2023) can help explain the large degree of substitution we find on average.

We then turn to real effects. Using the same empirical design, we estimate an elasticity of firm value added of 0.6. Dividing this elasticity by the credit supply elasticity, we obtain an elasticity of value added to credit supply of 0.4, somewhat larger but not far from the estimates reported in other studies (Cingano, Manaresi and Sette, 2016; Huber, 2018). We also find that firms borrowing from more exposed banks experienced substantial declines in sales, assets, and employment growth after the crisis.

Finally, we draw aggregate implications from our firm-level results using the general equilibrium model of credit supply shocks developed by Herreño (2023). To a first-order, the aggregate elasticities of credit supply and output in the model can be expressed as the product of two terms: the corresponding firm-level elasticity and a general equilibrium term capturing opposing effects. Reallocation across firms dampens the shock, while imperfect substitution across goods lowers aggregate labor demand, amplifying the effect. The model abstracts from the role of bank net worth, and does not incorporate second-round effects of the credit supply shock on aggregate demand, which might feed back into credit supply, triggering a loop (Gertler and Kiyotaki, 2010). It also abstracts from amplification through bank deposit competition (Herreño, 2023) and agglomeration spillovers (Huber, 2018, 2023). Our estimates can thus be seen as a lower bound.

Under our baseline calibration, the model yields an elasticity of credit supply of 1.6, and an elasticity of output of 0.6, indicating a modest amount of general equilibrium amplification. These estimates imply that the credit supply shock induced by the procurement cuts played a significant role in the protracted economic recovery in Portugal, accounting for 62% of the drop in credit and 31% of the drop in output in the 2011-2015 period.

The aggregate output elasticity can be interpreted as a credit-driven fiscal multiplier, which captures the effects of the procurement cuts operating through the banking system. Blanchard and Leigh (2013) find that fiscal multipliers in the European crisis were larger than anticipated by forecasters by about one in 2010-2011 and 0.4 in 2011-2013. To the extent that our mechanism was unanticipated, it can help explain these larger multipliers.[2]

Our paper contributes to four main strands of the literature. First, we add to the literature on the role of financial intermediaries in macroeconomic fluctuations. Financial accelerator models driven by bank net worth feature two-way effects between credit supply and the real economy (Gertler and Kiyotaki, 2010; Gertler and Karadi, 2011). Micro-level empirical studies have focused on the effect flowing from credit supply to the real economy (Chodorow-Reich, 2014; Huber, 2018). We estimate the effect in the opposite

[2]Nominal interest rates close to the zero lower bound (Christiano, Eichenbaum and Rebelo, 2011) and economic slack (Auerbach and Gorodnichenko, 2012) are complementary explanations for the large multipliers.

direction, from the real economy to credit supply, in the context of a financial crisis.

While the shock in our setting is a drop in government spending, the effect of a demand shock in typical macro models is the same whether it originates in the private or public sector (Chodorow-Reich, Nenov and Simsek, 2021; Guren et al., 2021; Wolf, 2023). Under such demand equivalence, our findings characterize the response of credit supply to aggregate demand shocks in general. We also provide evidence on how this response varies with bank leverage. This makes our estimated elasticities useful both in policy analysis and in quantitatively disciplining macro models (Nakamura and Steinsson, 2018).

Second, the literature on the links between sovereign and bank distress has focused on bank holdings of sovereign debt as the central mechanism linking sovereigns and banks, as formalized in the models of Gennaioli, Martin and Rossi (2014), Acharya, Dreschsler and Schnabl (2014) and Bocola (2016). The effect of sovereign exposure on credit supply and firm output was negative in the early stages of the European crisis, when spreads were high (Gennaioli, Martin and Rossi, 2018; Acharya et al., 2018; Bottero, Lenzu and Mezzanotti, 2020), but reverted to zero when spreads fell after 2012 (Altavilla, Pagano and Simonelli, 2017). We highlight a different source of bank exposure to the sovereign, operating through firms with procurement contracts, and we find that its effects were not just quantitatively important but also significantly more persistent.[3]

Third, we add to the literature on fiscal multipliers. It is well known that multipliers increase in the presence of credit constraints, which make current consumption more dependent on current income (Mankiw, 2000). A number of papers study the interaction between fiscal policy and credit constraints, including Eggertsson and Krugman (2012), Kaplan and Violante (2014), and Brinca et al. (2016). In these studies, fiscal policy and

[3] Huber (2018) also finds that credit supply shocks can have persistent effects on output, significantly outlasting the financial stress that caused them. Another factor that may have contributed to the slow economic recovery is zombie lending (Acharya et al., 2019).

credit constraints interact but are independently determined. We show that fiscal contractions can increase credit constraints via their effect on bank balance sheets, giving rise to a credit-driven multiplier. Our work is related to Auerbach, Gorodnichenko and Murphy (2020), who find that increases in defense spending can lower interest rates on consumer loans across U.S. cities. They conjecture that part of the effect may operate through improvements in the balance sheets of local contractors and their lenders.[4] We provide direct evidence of this mechanism in the case of a spending cut and quantify its impact on credit and output.

In addition, a growing literature exploits cross-sectional research designs at the local level to draw implications for national multipliers (Shoag, 2010; Cohen, Coval and Malloy, 2011; Chodorow-Reich et al., 2012; Nakamura and Steinsson, 2014). Pinardon-Touati (2023) takes this approach to the firm level, showing that debt-financed government spending crowds out private borrowing, and that this crowding out lowers fiscal multipliers. In the same vein, we do not estimate an overall multiplier but offer causal evidence on a specific mechanism at the firm level, which can be used to discipline models of the overall multiplier.

Finally, our paper is related to the literature on the links between public procurement and economic performance. Procurement and its regulation are important drivers of the quality and efficiency of public services (Hart, Shleifer and Vishny, 1997; Bosio et al., 2022). Winning procurement contracts spurs firm growth (Ferraz, Finan and Szerman, 2015; di Giovanni et al., 2022; Hvide and Meling, 2023) and facilitates access to credit through the use of contract revenues as collateral (Gabriel, 2022). However, procurement is also associated with corruption (Porter and Zona, 1993), favoritism (Burgess et al., 2015), and waste (Bandiera, Prat and Valletti, 2009). We contribute to this literature by showing that public procurement creates a link between governments and the

[4]In contemporaneous work, and consistent with that view, Goldman, Iyer and Nanda (2022) find that increases in defense spending also lower non-performing loans and increase lending at the county level.

financial system that may lead to fragility in times of crisis.

2 Data

2.1 Public procurement

Measuring public procurement is challenging. Two approaches are commonly employed: a macro-level approach based on System of National Accounts (SNA) data, and a micro-level approach based on individual contract data (Kutlina-Dimitrova, 2018).

In the macro approach, public procurement is the sum of government gross fixed capital formation, intermediate consumption, and social transfers in kind via market producers. The Organisation for Economic Co-operation and Development (OECD) publishes data on public procurement for its member countries based on this definition. An important advantage of the SNA-based approach is the availability and consistency of data across countries. On the flip side, it excludes non-government public entities, such as state-owned enterprises, and includes some non-procurement expenditures, potentially overstating the amount of procurement (OECD, 2011). We use SNA data to characterize the evolution of public procurement during the European sovereign debt crisis.[5]

At the micro level, many countries make data on individual procurement contracts publicly available. In the European Union (EU), all contracts above a legally prescribed threshold must be published in the *Official Journal of the European Union*, and data on these contracts are made available online through the Tenders Electronic Daily (TED) database.[6] In addition, most EU countries also publish their own contract databases,

[5]We obtain the data from: https://stats.oecd.org/Index.aspx?DataSetCode=SNA_TABLE12#

[6]In 2010, the threshold was €4.8 million for works, and either 125,000 or €193,000 for supplies and services, depending on whether the buyer was the central government or another entity. These thresholds are periodically updated, typically every two years.

often employing lower thresholds than TED.

The contract data tend to yield aggregate procurement amounts substantially lower than those obtained from SNA data. For example, data from TED accounted on average for 22% of SNA-based public procurement in 2008 across EU countries (OECD, 2011).[7] Despite the more limited coverage, a key advantage of contract-based data is that it can be linked to the firms providing products and services to the government, which is essential for our purposes.

We obtain micro-level data on public procurement contracts in Portugal from BASE, a web portal managed by the *Instituto dos Mercados Públicos, do Imobiliário e da Construção* (IMPIC). All public procurement contracts in non-exempt sectors must be communicated to this portal by law without a minimum threshold, and this communication is a precondition for contracts to become legally binding.[8] Data are available starting in 2009 and include information about the amount, date, and duration of the contracts, as well as the identification of contractors and awarding entities, including tax identifiers. One limitation of BASE is that it only includes comprehensive coverage of open tenders, the procedure typically adopted for the largest contracts, from 2011 onward. To overcome this, we complement BASE with data from TED, which we obtain through the web portal Opentender.eu, taking care to avoid any duplication.

Our combined data set accounts for 44% of SNA-based public procurement expendi-

[7]These differences could be driven by several factors. First, as mentioned above, the SNA-based measure includes some non-procurement expenditures, and contract databases do not include contracts below the publication thresholds. Second, awarded amounts in contract databases typically exclude value added taxes (VAT), while SNA-based public procurement includes VAT; the average standard VAT rate across EU countries in 2010 was 21%. Third, contracts in some sectors are often exempted from publication. The main categories exempted from publication in TED under EU directive 2014/24 are real estate, media services, legal services, financial services, public transport, R&D, defense, and security contracts. Finally, SNA data are based on actual expenditures for each year, regardless of when contracts were awarded, whereas contract databases report contract awards, which may or may not be disbursed in the year they were made.

[8]Public procurement in Portugal is governed by the *Código dos Contratos Públicos* enacted in 2008, which exempts from publication the same sectors as EU directive 2014/24 (see footnote 7).

ture in Portugal in 2010, well above the 22% average covered by TED in 2008 across EU countries (OECD, 2011). More importantly, Figure D.1 of the online Appendix shows that our data can fully account for the drop in procurement expenditure during the crisis and, therefore, for the exposure of firms to these cuts, which is our focus in this paper.

Table C.1 in the online Appendix presents summary statistics for our contract data in 2010, the year before the procurement cuts we study. The median contract is worth €12,132, and the 10th percentile is €523. This illustrates how well the data cover small contracts, given the absence of a reporting threshold in BASE. At the same time, large contracts generate considerable skewness in the distribution: the mean contract is worth €132,217, above the 90th percentile of €95,950. The vast majority of contracts (93%) take the form of outright awards, but these only account for 26% of contracting volume. The 7% of contracts awarded through open, negotiated, and restricted tenders tend to be much larger, and account for the remaining 74% of volume. In terms of buyers, central and local government represent about two thirds and one third of contract volume, respectively.

When it comes to the type of goods or services purchased, construction accounts for the largest share of contract volume (55%), which reflects the large role of infrastructure projects. We address the role of construction as a possible confounder of our results below. The remainder is distributed across a wide range of goods and services, including health and social work services (9%), energy (5%), and sewage services (3%).

2.2 Loan, bank and firm data

Using firm tax identifiers, we merge our contract data with loan, bank, and firm data from three administrative data sets managed by Banco de Portugal. Quarterly loan-level data from 2009 to 2015 come from *Central Credit Responsibilities*, a database covering all credit exposures above €50 in Portugal. We collect quarterly bank characteristics from statistical data reported to Banco de Portugal (Monetary and Financial Statistics).[9] And we draw annual firm characteristics from the Central Balance Sheet Database, which includes detailed financial statements for all non-financial firms operating in Portugal. We use data on value added, sales, employment, total assets, two-digit sectors, and headquarter locations (municipalities). In our regressions, we winsorize all variables except procurement exposure at the 2.5% and 97.5% levels. We do not winsorize exposure so that we can decompose our estimates following the method developed by Goldsmith-Pinkham, Sorkin and Swift (2020), but we show that our results are unchanged when we do. Table A.1 in the Appendix provides definitions for the variables we use.

3 Procurement Cuts and the Banking System

Prior to the crisis, differences in sovereign yields across euro area countries were negligible, but the IMF/EU bailout of Greece in May 2010 set off a rise in yields in Ireland, Italy, Portugal, and Spain relative to those in Germany, as shown in Figure 1a. This rise in yields brought all four countries under severe financial pressure, and eventually all but Italy received bailouts of their own. Ireland followed Greece in November 2010, Portugal was next in May 2011 and Spain in June 2012.

In order to restore access to capital markets and meet the bailout terms, these countries turned to aggressive fiscal consolidation efforts, which included the large cuts to public procurement shown in Figure 2.[10,11] In Portugal, the government cut procure-

[9] We assign parent group leverage ratios, obtained from annual reports, to foreign branches as branches are not independent legal entities and their leverage ratios have limited economic meaning.

[10] Table C.2 in the online Appendix shows that these large procurements were not isolated events. We identify 16 episodes in 15 OECD countries where procurement was cut by at least 10% between 1995 and 2018. The average cut amounted to 20%, or 2.8% of GDP. Half of these 16 episodes overlapped with systemic banking crises. This evidence is suggestive of the broader relevance of the mechanism we study.

[11] In Ireland the procurement cuts started in 2009, before the rise in sovereign yields, as a response to

ment by 4.3% of precrisis GDP between 2010 and 2014, with almost 90% of the cut taking place between 2010 and 2012. This compares with an even stronger cut of 7.2% in Greece, similar cuts of 4.0% and 3.6% in Spain and Ireland, and a milder cut of 1.7% in Italy, relative to their respective precrisis spending peaks. These cuts account for the bulk of the reductions in primary budget deficits in these countries in the same period: 71% in Portugal, 65% in Greece, 57% in Spain, 98% in Ireland, and 76% in Italy.[12] In contrast, public procurement in Germany remained on a stable upward trend throughout the crisis.[13]

Table C.3 in the online Appendix shows that public procurement represented an important source of demand for the private sector in Portugal before the crisis. In 2009-2010, the procurement contracts in our data amounted to 18% of the sales of the firms that held them, on average, and to 57% of sales at the 90th percentile. Although these firms represented only 5% of all firms, they accounted for 33% of value added and 26% of employment in the corporate sector. We focus on this set of firms that held contracts in 2009 or 2010, which we refer to as government contractors, as those most likely to be affected by the cuts.

Government contractors held a substantial amount of credit from the banking system at the onset of the crisis, accounting for 17% of corporate lending (Table C.3). To put this figure in perspective, credit to contractors corresponded to 75% of bank equity and 90% of domestic sovereign debt bank holdings, including both bonds and loans. Here and throughout the paper, we measure precrisis exposures and bank characteristics in 2010Q1, before the Greek bailout in May 2010 that triggered the rise in sovereign yields.

the 2008 government bailout of the banking sector. This indicates that our mechanism can potentially be triggered by either sovereign or bank distress, hinting at a possible negative feedback loop between procurement cuts and the deterioration of bank balance sheets.

[12] We exclude capital transfers in Greece, which include a large bank bailout in 2013, from the deficit. Including these transfers makes the deficit reduction smaller and the contribution of the procurement cut larger.

[13] Precrisis spending peaks were fairly similar across countries: 12.2% of GDP in Italy, 12.7% in Ireland, 13.4% in Portugal, 13.9% in Spain, 15.3% in Germany and 15.5% in Greece.

Figures 3a and 3b show that contractors were severely affected by the cuts to procurement. Between 2010 and 2015, the value added of these firms dropped by 28%, versus 17% for other firms, and this decline seems to have led to a substantial deterioration in their ability to repay their loans. Contractor NPLs grew six-fold by 2015, while those for other firms only doubled.[14] Before the crisis, both value added and NPLs for the two sets of firms exhibited similar trends.

Moreover, the postcrisis decline in value added and increase in NPLs were stronger for the firms supplying the government with products and services that suffered above-median procurement cuts, as one would expect if the shock to contractors was caused by the cuts to public procurement.[15] In fact, we estimate an elasticity of contractor value added to procurement demand of 0.92 (the negative of the coefficient in column 1 of Table C.4), which implies that firms were essentially unable to substitute for the lost revenue over the 2011-2015 period. We also estimate that a procurement cut of one percent of sales increased contractor NPL ratios by 0.13 percentage points (column 2 of Table C.4).

Figure 4 shows that the growth in troubled loans from government contractors in turn had a material effect on banks. Between 2010 and 2015, NPLs from these firms increased by an amount equivalent to 13% of precrisis total bank equity. We see this as a lower bound for the impact of the procurement cuts on bank balance sheets, since it only includes firms directly exposed to procurement contracts. The cuts could also have im-

[14]The credit register database reports credit overdue for at least 90 days at the bank-firm level, but NPLs are only available at the bank level. We use the overdue credit data and follow Banco de Portugal (2016) to define firm-level NPLs. If a firm has overdue credit from a bank, we define as non-performing the full exposure of the firm to that bank. Once a firm has had no overdue credit from a bank for one year, we no longer consider the exposure to that bank as non-performing. In Banco de Portugal (2016), an exposure is also defined as non-performing if "the debtor is assessed as unlikely to pay its obligations in full without realization of collateral". We cannot observe this assessment, but we find that aggregate NPLs computed with our definition equal 96% of aggregate NPLs reported at the bank level; we scale our NPL measure by this factor.

[15]We use the 8-digit Common Procurement Vocabulary (CPV) codes reported in contracts to identify products and calculate product-level contract cuts. When a firm supplies multiple products, we take the average cut weighted by firm-level contract amounts in 2010.

pacted other firms through supply chain relationships with contractors.

For comparison, we estimate that the decrease in the market value of precrisis bank holdings of domestic sovereign debt attained a maximum of 14% of precrisis bank equity in early 2012. An alternative measure of the impact of the rise in sovereign debt risk on banks is the temporary equity buffer mandated by the European Banking Authority (EBA) in late 2011 to face potential sovereign debt losses. This also amounted to 14% of precrisis bank equity.[16] These numbers suggest that the shock to banks through the procurement channel we document was of the same order of magnitude as the shock through the sovereign debt channel that has been the focus of the literature on the sovereign-bank nexus (Acharya, Dreschsler and Schnabl, 2014; Gennaioli, Martin and Rossi, 2014; Brunnermeier et al., 2016; Farhi and Tirole, 2018).

Figure 4 also shows that the impact of the sovereign debt shock on banks was relatively short-lived. Sovereign yields dropped sharply after European Central Bank (ECB) President Mario Draghi's famous "whatever it takes" speech in July 2012, effectively defusing the sovereign-debt driven loop (Figure 1). This drop erased any losses and eventually generated large gains in the market value of domestic sovereign debt holdings (Acharya et al., 2019). In contrast, the contractor NPL shock persisted well beyond the acute phase of the crisis, as did the procurement cuts shown in Figure 2.

[16] The results of the EBA 2011 capital exercise that mandated this buffer can be found at https://www.eba.europa.eu/risk-analysis-and-data/eu-capital-exercise/final-results.

4 Effect on credit supply

4.1 Methodology

4.1.1 Bank exposure to procurement cuts

To study the effect of the cuts to public procurement on credit supply, we start by defining bank exposure to these cuts. Our definition takes into account both how exposed a bank was to government contractors and how exposed contractors were to the cuts. We measure the former through the bank's share of credit to contractors, and the latter through the share of procurement cuts in firm sales:

$$Procurement\ Exposure_b = \kappa \sum_{i}^{n} \frac{Credit_{ib}}{Credit_b} \times \frac{Procurement\ Cut_i}{Sales_i}, \tag{1}$$

Banks and firms are indexed by b and i. Credit is measured in 2010Q1, sales as the 2009-2010 average, and procurement cuts are defined as the change in average procurement between the 2009-2010 and 2011-2015 periods:[17]

$$Procurement\ Cut_i = \frac{1}{2} \sum_{t=2009}^{2010} Procurement_{it} - \frac{1}{5} \sum_{t=2011}^{2015} Procurement_{it}. \tag{2}$$

Our procurement exposure measure can be interpreted as a bank-weighted drop in aggregate demand, where the drop is driven by the procurement cuts and the weights by credit shares. One caveat is that, although we include credit to all firms in the shares,

[17]We assign zeros to contract increases, since their effect on credit quality is unlikely to be symmetric to that of contract cuts, and we cap contract cuts at 100% of 2009-2010 sales. We also exclude from the calculation a small set of Public-Private Partnerships (PPPs) that would otherwise be incorrectly classified as large cuts. There are four PPPs in our data, all awarded in 2009 and 2010. Each corresponds to the construction and operation of a hospital for a period of 30 years. The payment schedules consisted of roughly constant annual payments over the contract life cycle (payment schedules can be found in the last pages of each contract here: https://www.utap.gov.pt/PPP_saude.htm).

we only account for the effect of the cuts on final goods producers. This implies that our measure understates the shock to aggregate demand. Ideally, we would either (1) only include credit to final goods producers in the shares, or (2) allocate each contract cut across the firms involved in the supply chain, not just to the contractor performing the final sale, and scale the cuts by value added instead of sales. Unfortunately, neither is possible with our data because we do not observe supply chains. To approximate the correct magnitude of the shock, we instead scale exposure in equation (1) by a factor κ such that the sample mean of exposure equals the aggregate ratio of procurement cuts to value added.

4.1.2 Empirical strategy

We estimate the effect of exposure to procurement cuts on credit supply at the bank-firm level by exploiting within sector-municipality variation in credit growth across banks with different levels of precrisis exposure. Our dependent variable is the log of cumulative growth in credit granted by bank b to firm i between 2010Q4 and 2015Q4:

$$g_{ib} = \log\left(\sum_{t=2011Q1}^{2015Q4} \frac{Credit_{ibt}}{Credit_{ib2010Q4}}\right), \tag{3}$$

and our regression equation is

$$g_{ib} = \beta Procurement\ Exposure_b + \gamma_{jm} + \lambda_1 X_b + \lambda_2 Z_i + \epsilon_{ib}. \tag{4}$$

The coefficient of interest is β. Since exposure represents a *drop* in demand, β is the negative of the elasticity of credit supply with respect to procurement demand. γ_{jm} denotes sector by municipality fixed effects, to control for credit demand. X_b and Z_i are sets of precrisis bank and firm controls measured in 2010Q1. X_b includes bank exposure to domestic sovereign debt over equity, the log of total bank assets, and bank leverage; Z_i comprises the log of total firm assets, return on assets, leverage, and the current ratio. To test for preexisting differential trends, and to examine the effect of procurement exposure over time, we also estimate regressions with g_{ib} defined as cumulative credit growth up to $T \in [2009Q1, 2015Q3]$.

This specification enables us to account for relationship and firm exit in a straightforward manner: the dependent variable is defined as long as credit is positive at any point between 2011Q1 and 2015Q4. Since the underlying credit data are monthly, a relationship must only survive until the end of January 2011 to be included in the sample. Given that the procurement cuts were implemented starting in 2011, it is unlikely that our specification suffers from survivor bias.

We cluster standard errors at the bank level in all regressions. Since we have a small number of clusters (13 banks), we implement the Imbens and Kolesár (2016) "LZ2" correction to our standard errors, and we use a t-distribution with the degrees of freedom suggested by Bell and McCaffrey (2002) (BM) to compute confidence intervals.

4.1.3 Sample

We restrict our sample to banking groups (which we refer to as banks) with at least 1% of the corporate credit market in 2010Q1, thus excluding very small banks, mostly foreign branches that tend to operate in niche markets and extend small amounts of credit. There are 13 banks in Portugal that meet this requirement, and together they accounted for 95% of corporate credit in 2010Q1. Figure D.2 in the online Appendix plots the distribution of procurement exposure across banks in our sample.

We also restrict the sample to firms that existed in 2009 and 2010, and to non-contractors, i.e., firms without public procurement contracts in 2009-2010. We impose the latter restriction so that our estimates capture only effects operating through the banking system, not the direct effects of procurement exposure on contractors. At the bank-firm level, we further restrict the sample to lending relationships that existed in 2009 and 2010, and we exclude relationships of less than €25,000 of credit in 2010Q4, the reporting threshold set by the ECB for AnaCredit.[18] These small relationships, which represented 1.2% of corporate credit in 2010Q4, behaved differently than the rest of the sample, as we show in section 4.2.4. We include these relationships when we aggregate credit at the firm level.

Table 1 reports summary statistics for our sample at the bank-firm and firm levels, and Table 2 compares banks with above and below median procurement exposure. The two groups of banks are relatively balanced across a range of variables, including those in our baseline set of controls and others that we introduce in robustness tests below. Figure D.3 in the online Appendix shows the aggregate evolution of credit for banks in the two groups throughout our sample period, without conditioning on controls. Both groups followed similar paths before the procurement cuts. After the cuts, credit from the high exposure group suffered a steeper drop than credit from the low exposure group, in line with our proposed mechanism.

4.2 Results

Our baseline estimate for β, reported in column 1 of Table 3, is -2.460, with a 95% confidence interval of (-4.514,-0.406). This implies a drop of approximately $1 - e^{-2.460\times 0.085} = 19$ percentage points in credit growth evaluated at the mean of exposure in the sample. Figure 5a shows the corresponding binned scatter plot, which suggests the effect is approximately linear in exposure.

[18]AnaCredit is the euro area loan database that the ECB relies on for monetary policy, financial stability, and economic research and statistics (Israel et al., 2017).

Our identifying assumption is that, in the absence of government procurement cuts and conditional on controls, credit growth would have followed similar trends across banks with different levels of procurement exposure. To test for differential preexisting trends, Figure 5b presents point estimates and 95% confidence intervals for β from estimating equation (4) for cumulative credit growth up to $T \in [2009Q1, 2015Q4]$. The rightmost point in the figure corresponds to our baseline estimate for the overall effect. Consistent with our assumption, procurement exposure was unrelated to changes in credit growth before the cuts. After the cuts, procurement exposure led to a sizeable and persistent decline in credit growth. In line with the evidence on NPLs in Figure 4, the effect strengthens over time.

Column 2 of Table (3) examines the effect of exposure along the extensive margin, replacing credit growth with an indicator for whether a lending relationship survived until 2015Q4. We find evidence that procurement exposure lowered the probability of survival – the coefficient on exposure is -1.522. Evaluated at the mean of exposure, this represents 20% of the unconditional probability of survival. Columns 3-5 report robustness checks that we discuss below.

4.2.1 Decomposing exposure

Our exposure measure has a shift-share structure, with the shares given by bank credit exposures and the shifters by procurement cuts. Goldsmith-Pinkham, Sorkin and Swift (2020) show that exogeneity of the shares, conditional on controls, is a sufficient condition for identification in such designs. They propose a method to identify the key sources of variation underlying an exposure measure like ours, in terms of sensitivity of the results to violations of exogeneity. In particular, they show that our estimator $\hat{\beta}$ can be expressed as $\sum_i \hat{\alpha}_i \hat{\beta}_i$, where $\hat{\beta}_i$ is the estimate obtained by instrumenting procurement exposure with the credit share of firm i, and $\hat{\alpha}_i$ is the corresponding Rotemberg weight, as termed by Goldsmith-Pinkham, Sorkin and Swift (2020). This weight is a function of firm i's procurement cut and credit shares, and captures how sensitive $\hat{\beta}$ is to misspecification in $\hat{\beta}_i$ driven by endogeneity in firm i's credit relationships.[19]

We employ this method to dissect the identifying variation in our design. We first calculate $\hat{\beta}_i$ and $\hat{\alpha}_i$ for each contractor, and then aggregate them to the sector level. Table 4 lists the top five sectors by $\hat{\alpha}_j = \sum_i \hat{\alpha}_i$, the weight of sector j. Our results are predominantly determined by exposure to construction firms, which account for 84% of the weight. The other sectors in the top five are administrative services, water and waste management, consulting, and wholesale and retail trade, each one representing less than 4% of the weight. Moreover, within construction, the top 5% of contractors by $\hat{\alpha}_i$ account for 81% of the sector's weight, and weights in the remaining top sectors are also highly concentrated. The Table also reports $\hat{\beta}_j = \frac{\sum_j \hat{\alpha}_i \hat{\beta}_i}{\sum_j \hat{\alpha}_i}$, the weighted average $\hat{\beta}_i$ in each sector. We find that the $\hat{\beta}_j$ for each of the top three sectors, which together represent 92% of the weight, are close to our overall $\hat{\beta}$, while those for the remaining two sectors are higher.

The decomposition shows that the validity of our design hinges to a large extent on the exogeneity of the credit shares of a subset of construction contractors. To evaluate the plausibility of this assumption, we test for pre-trends in credit growth as a function of the $\hat{\alpha}_i$-weighted average credit share to the construction sector, as suggested by Goldsmith-Pinkham, Sorkin and Swift (2020). We do this by replicating Figure 5b with the weighted credit share replacing procurement exposure in equation (4). Figure D.5a in the online Appendix plots the resulting estimates. Like procurement exposure, the $\hat{\alpha}_i$-weighted average credit share was unrelated to changes in credit growth before the procurement cuts, and led to a visible decline in credit growth after the crisis, although our estimates are significantly noisier. The remaining plots in Figure D.5 show that the same pattern holds for

[19] Let c_i denote firm i's procurement cut, Z_i the vector of firm i's credit shares across banks, and $P^{\perp}$ the vector of procurement exposure across banks, residualized on the controls in equation (4). Then $\hat{\alpha}_i = \frac{c_i Z_i' P^{\perp}}{\sum_i c_i Z_i' P^{\perp}}$.

the other top five sectors and across all sectors. The plot for all sectors unsurprisingly resembles the one for the construction sector.

4.2.2 Robustness

One concern with our specification is that procurement exposure may be correlated with exposure to sectors that performed poorly during the crisis for reasons unrelated to the procurement shock. If exposure to these sectors also impacted credit supply, this would confound our estimates. In particular, and given its predominant role, our results could be driven by exposure to the construction sector, rather than to public procurement. In fact, overall exposure to construction was somewhat higher for banks with high procurement exposure, as shown in Table 2, legitimizing this concern.

To evaluate this possibility, we start by re-estimating our baseline regression including exposure to the construction sector in the set of bank controls. The coefficient on procurement exposure, reported in column 4 of Table 3, is similar to our baseline estimate reported in column 1. Taking a more general approach, we construct a shift-share predictor of NPL growth for non-contractors. The shares are precrisis bank credit exposures by sector and the shifters are leave-one-out national changes in NPLs, as a share of precrisis credit, in each sector between 2010Q1 and 2015Q4.[20] This captures the expected change in bank NPL ratios driven by non-contractors. Column 5 reports the results controlling for this variable. The coefficient on procurement exposure is again very close to our baseline estimate. This is consistent with the fact that high and low exposure banks are balanced in terms of this additional control (Table 2).

Another factor that may have affected credit supply in this period is that several banks

[20] Let j index sectors, $-b$ denote the set of all banks except b and $-C$ the set of non-contractors. Let $\overline{x}^{[T_1,T_2]}$ denote the mean of x over the period between T_1 and T_2. We define bank b's predicted NPL growth for non-contractors as $\sum_j \frac{Credit_{b,j,-C,2010Q1}}{Credit_{b,2010Q1}} \times \frac{\overline{NPL}^{[2011Q1,2015Q4]}_{-b,j,-C} - \overline{NPL}^{[2009Q1,2010Q4]}_{-b,j,-C}}{\overline{Credit}^{[2009Q1,2010Q4]}_{-b,j,-C}}$.

were recapitalized in 2010-2013, through both government and private capital injections. These recapitalizations were driven by the need to comply with the stricter capital requirements imposed externally by the EBA and by the terms in Portugal's bailout (Augusto and Félix, 2014), and affected six out of the 13 banks in the sample. Column 6 shows that controlling for whether a bank was recapitalized has little effect on the results.

An additional concern is that the results could be driven by credit demand, not supply, within sector-municipality cells. For example, non-contractors may be connected to contractors through supply chains and be negatively affected by the cuts through such connections. These firms may also be more likely to borrow from more exposed banks, biasing our coefficients. We address this concern by estimating an alternative specification with firm fixed effects, which absorb firm-level credit demand (Khwaja and Mian, 2008). This requires restricting the sample to firms with at least two lending relationships in 2010. Columns 1 and 2 of Table 5 present coefficients from our baseline and within-firm specifications estimated in this sample. The coefficients in the two specifications are similar and, if anything, larger in the within-firm specification. This supports the validity of our design and suggests that our baseline estimates are conservative.

The within-firm strategy assumes that credit demand is not bank-specific. Ivashina, Laeven and Moral-Benito (2022) and Paravisini, Rappoport and Schnabl (2023) show that bank specialization can invalidate that assumption. We test if bank-specific credit demand shocks can explain our findings by constructing predictors of credit growth for non-contractors as a function of precrisis bank specialization along several dimensions.

First, the credit register data include exposure-level information on the type of financing (e.g., term loans, credit lines, factoring, and leasing), and also on the type of collateral involved (e.g., real, financial, personal guarantees, and government guarantees). We use the type of financing and collateral information to construct two shift-share predictors of credit growth where the shares are bank exposures by financing or collateral type, and the shifters are the leave-one-out national growth rates in credit for each financing or collateral type between 2010Q1 and 2015Q4.[21] Second, we construct two analogous predictors of credit growth as a function of precrisis bank exposures to sectors and to municipalities. Table 2 shows that high and low exposure banks had similar values for these variables. We add them to our baseline specification in columns 3-6 of Table 5, and find that our results are robust to their inclusion.

Table C.5 in the online Appendix presents several additional robustness checks. Panel A considers variations in the definition of procurement exposure. Column 1 shows that the results hold when we replace procurement cuts in equation (1) with predictors for the national growth of NPLs by product, in line with our hypothesized causal chain.[22] The results also hold when we replace the cuts with precrisis procurement levels in column 2, when we do not assign zeros to procurement increases (column 3), or when we winsorize exposure at the 2.5th and 97.5th percentiles, in line with the other variables in the regression (column 4).

Panel B examines variations in the sample. Column 1 indicates that the results are slightly stronger for firms that had a single banking relationship before the crisis. In column 2, the coefficient on exposure is similar to our baseline estimate when we drop firms operating in sectors with above-median exposure to procurement cuts (e.g., construction), which may have been affected by competitive spillovers from the cuts. In column 3, the results are slightly stronger when we estimate the regression on the sample of contractors, although the standard errors are larger given the smaller sample size. Finally, column 4 shows that the results are unchanged when we weight observations by log credit in 2010Q4.

[21]Let k index financing or collateral types, $-b$ denote the set of all banks except b and $-C$ the set of non-contractors. Let $\overline{x}^{[T_1,T_2]}$ denote the mean of x over the period between T_1 and T_2. We define bank b's predicted credit growth for non-contractors as $\sum_k \frac{Credit_{b,k,-C,2010Q1}}{Credit_{b,2010Q1}} \times \frac{\overline{Credit}^{[2011Q1,2015Q4]}_{-b,k,-C} - \overline{Credit}^{[2009Q1,2010Q4]}_{-b,k,-C}}{\overline{Credit}^{[2009Q1,2010Q4]}_{-b,k,-C}}$.

[22]As in Figure 3, we use 8-digit CPV codes to identify products. When a firm supplies more than one product, we take the average cut weighted by firm-level contract amounts in 2010.

4.2.3 Interaction with bank leverage and recapitalizations

Our baseline coefficient represents an average effect across banks, but the effect of a shock to asset quality on bank net worth, and thus on credit supply, depends on bank leverage ratios (Gertler and Kiyotaki, 2010). Banks that were either better capitalized before the crisis or recapitalized during the crisis were better able to absorb losses without cutting lending, and therefore credit supply at these banks may have been less affected by exposure to the procurement cuts.

Table 6 tests this hypothesis. Column 1 reports results from adding an interaction between procurement exposure and precrisis bank leverage to our baseline regression. The sample consists of only 13 banks, so our estimates are necessarily imprecise. Recalling that bank leverage is defined as the ratio of equity to assets, the coefficient on the interaction term is positive, as expected. Our point estimate indicates that the effect of exposure decreases in magnitude by about 0.3 for each percentage point of leverage.

Six of the banks in our sample were recapitalized in 2010-2013 to meet the stricter capital requirements imposed during the crisis, as explained above. In column 2, we interact exposure with an indicator for whether the bank was recapitalized. The effect of exposure on recapitalized banks was only about a third of the effect on other banks. The capital injections received by these banks were significant, ranging from 2.0% to 7.7% of precrisis bank equity. Columns 3 and 4 combine the effects of leverage and recapitalizations by adding these capital injections to precrisis equity and assets when calculating leverage ratios. In column 3 we add only government recapitalizations. The coefficient on the interaction term is slightly larger than in column 1, and the standard error remains constant. In column 4 we include both public and private recapitalizations. This specification yields an interaction coefficient similar to the one in column 3 but with a lower standard error. These results are consistent with the mechanism we propose, and they shed light on how its effect may vary with the health of the banking system.

4.2.4 Heterogeneity

An important question is whether the procurement shock affected the composition of bank credit portfolios, namely whether exposure caused banks to differentially reduce credit supply to ex-ante worse borrowers. For example, Balloch (2023) finds that the liberalization of bond markets in Japan shifted the composition of bank credit towards riskier firms.

We do not find evidence of such differential effects. Columns 1 and 2 of Table 7 split the sample by credit risk in 2010 using data from SIAC, a credit assessment system developed by Banco de Portugal to provide individual credit risk ratings to firms. We estimate very similar coefficients for high and low risk firms. Columns 3 and 4 split the sample by firm size in 2010 (below and above median assets). Our point estimates suggest a slightly larger effect on large firms, if anything, but the difference is not statistically significant. The results are the same when we split firms by employment or value added. Lastly, columns 5 and 6 split the sample by firm age, and we again find no evidence of heterogeneous effects. Taken together, these results suggest banks cut credit independently of risk, perhaps under pressure to deleverage quickly.

We also observe no significant heterogeneity as a function of the size of the lending relationship, with one exception (Figure D.4 in the online Appendix). When we estimate equation (4) for the set of relationships below €25,000, which we excluded from the sample in Section 4.1.3, we find no evidence of an effect, although our estimate is noisy. When we split our regression sample by relationship size quintiles, in contrast, the estimated effects are all close to our baseline coefficient from column 1 of Table 3. One possibility is that banks ignored very small relationships in their efforts to deleverage, given their immaterial impact (1.2% of corporate credit in 2010Q4).

5 Effect on firms

5.1 Credit

To evaluate the impact of the credit supply shock at the firm level, we need to ask to what extent firms were able to substitute credit from more exposed banks for credit from less exposed banks. To do so we must first aggregate the bank level variables in X_b in equation (4) at the firm level. We do this by averaging across the banks that lend to each firm, weighting by each bank's share of credit.

We estimate firm-level regressions using our baseline specification from equation (4), replacing bank-level variables with firm-level variables and clustering standard errors at the level of the firm's main bank by loan size. Our estimate of β for firm-level cumulative credit growth, reported in Column 1 of Table 8, equals -1.431, with a 95% confidence interval of (-2.223,-0.640). This corresponds to 58% of our bank-firm-level estimate, and implies that firms were able to substitute 42% of the credit they lost from more exposed banks. The magnitude of the effect is still substantial, however, as it implies an 11 percentage points reduction in credit growth evaluated at the mean of exposure in the sample.

Figure 6a plots coefficients and 95% confidence intervals from estimating (4) for cumulative credit growth up to each quarter in [2009Q1, 2015Q4]. The rightmost point in the figure corresponds to our baseline estimate for the overall effect. As in our bank-firm results, credit growth for firms borrowing from banks with different levels of procurement exposure followed similar trends before the procurement cuts. After the cuts, firms borrowing from more exposed banks experienced a decline in credit growth, which again strengthened over time.

The amount of substitution across banks in the literature varies substantially. Khwaja and Mian (2008) find no substitution in Pakistan except for large firms. In Italy, Cingano, Manaresi and Sette (2016) also find no substitution in the 2007-2008 financial crisis, but Bottero, Lenzu and Mezzanotti (2020) find about 50% substitution in the European sovereign debt crisis. Bentolila, Jansen and Jiménez (2018) estimate substitution of around two-thirds in the 2007-2008 financial crisis in Spain. One factor that has been found to affect the degree of substitution is whether firms had more than one lending relationship before the shock. For example, Bentolila, Jansen and Jiménez (2018) find even higher substitution (80%) when they restrict the sample to firms with multiple lending relationships. This also holds in our data: we estimate substitution of only 25% for single relationship firms (column 1 of Panel B of Tables C.5 and C.7 in the online Appendix). The relatively large amount of substitution we find on average may be partly driven by the high prevalence of multiple lending relationships in Portugal, in line with the rest of Southern Europe (Kosekova et al., 2023).

Tables C.6 and C.7 in the online Appendix present firm-level tests analogous to those in Tables 3, 5 and C.5. We find that our firm-level credit results are equally robust.

5.2 Real outcomes

We next focus on the real effects of the procurement-driven shock to credit supply. We take the firm-level analog of equation (4) that we used for firm credit and estimate it for log cumulative growth in other firm outcomes, with t now indexing years rather than quarters since our firm outcomes are observed annually.

Our main focus is on the effect on firm value added, which we map into an effect on aggregate output below. Column 2 of Table 8 reports the effect of procurement exposure on cumulative value added growth between 2010 and 2015. Our point estimate is -0.563, with a 95% confidence interval of (-0.963,-0.162). Evaluated at the mean of exposure in our sample, this corresponds to a drop of 4.6 percentage points in value added growth.

Figure 6b plots the coefficients and 95% confidence intervals from estimating regressions for cumulative value added growth between 2010 and $T \in [2008, 2015]$.[23] In line with the effect on credit growth, there are no significant preexisting differential trends. After the cuts, firms borrowing from more exposed banks experienced a persistent decline in value added growth relative to firms borrowing from less exposed banks.

Tables C.8 and C.9 in the online Appendix replicate the robustness tests in Tables C.6 and C.7 for value added, and we again find that the results are robust. Columns 3 to 5 of Table 8 report results for sales, assets, and employment growth, which are similar to the results for value added growth.

One way of comparing our findings with those from other studies of credit supply shocks is to compute the implied elasticity of real outcomes with respect to credit supply, which can be obtained by taking the ratio of the corresponding estimated effects.[24] Studies that report effects on both credit volume and real outcomes include Cingano, Manaresi and Sette (2016), Huber (2018), Bentolila, Jansen and Jiménez (2018), and Bottero, Lenzu and Mezzanotti (2020). Cingano, Manaresi and Sette (2016) and Huber (2018) report results for value added, and their estimates imply elasticities with respect to credit supply of 0.26 and 0.30, respectively.[25] Our estimates yield a somewhat larger elasticity of

[23] Since firm outcomes are available for earlier years, we extend our sample period to start in 2008 to offer a better sense of precrisis trends at the annual frequency.

[24] These elasticities should be interpreted with care, since they may be driven by changes in credit terms such as interest rates, not just by credit volume.

[25] In Cingano, Manaresi and Sette (2016), the elasticity is reported in column 4 of Table 11. In Huber (2018), the estimate for credit is -0.205 (column 3 of Table 4), and the one for value added equals -0.061 (column 2 of Table 7).

0.39. All four papers report results for employment, and the elasticities range from 0.18 to 0.52.[26] The implied elasticity for employment in our setting is 0.29, close to the mean of the four studies.

6 Aggregate implications

What do our results imply for the aggregate elasticity of credit supply with respect to aggregate demand? And to what extent can the credit supply shock induced by the procurement cuts account for the aggregate decline in lending and output in the aftermath of the crisis? Our findings identify the effects of the shock on firms borrowing from exposed banks relative to firms borrowing from unexposed banks, but those borrowing from unexposed banks may themselves have been affected by the shock through general equilibrium effects.

We incorporate these effects into our analysis by using our estimates to calibrate Herreño (2023)'s general equilibrium model of credit supply shocks. In Herreño (2023), as in the model of Chodorow-Reich (2014), there are opposing effects on firms borrowing from unexposed banks. First, some output is reallocated from firms that borrowed from exposed banks, and became financially constrained as a result, to unconstrained firms, attenuating the aggregate effect of the shock. This is driven by a decrease in the relative price of output for unconstrained firms and by a decline in the equilibrium wage, as constrained firms raise prices and reduce their labor demand. Second, the output reduction for constrained firms reduces demand for the output of unconstrained firms, through complementarities across goods. This causes labor demand at unconstrained

[26] In Cingano, Manaresi and Sette (2016), the elasticity is reported in column 1 of Table 11. In Huber (2018), the estimate for credit is -0.205 (column 3 of Table 4), and the one for employment is -0.053 (column 3 of Table 6). In Bentolila, Jansen and Jiménez (2018), the elasticity is reported in column 1 of Table 6. In Bottero, Lenzu and Mezzanotti (2020), the estimate for credit is -0.181 (column 1 of Table 7), and the one for employment is -0.047 (column 4 of Table 7).

firms to fall, amplifying the shock. An attractive feature of the model is that it yields simple closed-form approximations linking the aggregate effects of credit supply shocks to the cross-sectional elasticities we estimate.[27]

6.1 Model

We offer only a very brief description of the model and the key expressions we use, and refer the reader to Herreño (2023) for details. The model is static, and the economy is composed of a unit mass of firms operating under monopolistic competition, a discrete number of banks and non-bank funding sources (e.g. bonds, shareholder loans), and a representative household. The number of non-bank lenders is set to one.

Firms produce output by performing a continuum of tasks using labor supplied by households, and must finance their wage bill. Borrowing choices are task-specific. For each task, firms choose either a particular bank or non-bank funding, based on idiosyncratic preferences. This implies that bank credit is imperfectly substitutable across banks, with elasticity of substitution θ, and with respect to non-bank funding, with elasticity of substitution φ. The elasticity of substitution in the goods market is η, the Frisch elasticity of labor supply is $1/\phi$, and the elasticity of labor supply to individual firms is α. A finite α introduces frictions in the reallocation of labor across firms.

All bank and non-bank lending rates are initially set at R. In Herreño (2023), the credit supply shock consists of one bank's rate increasing to Re^u, for a small and positive u. We develop a simple extension where bank exposure to the shock is heterogeneous, to fit our empirical setting.[28] We instead assume that the rates of all banks increase to $Re^{p_b u}$, where

[27] Herreño (2023) also develops an extension featuring additional general equilibrium amplification through competition for bank deposits. The approximations we employ correspond to the model without this channel, and can therefore be seen as a lower bound.

[28] See section A of the online Appendix for all derivations.

$p_b \geq 0$ is the procurement exposure of bank b, and write firm i's exposure as $p_i = \sum_b \nu_{ib} p_b$, the average exposure of the banks it borrows from, weighted by credit shares ν_{ib}.

We focus on first-order approximations to the aggregate effects of the shock, which yield simple and intuitive expressions. As we show in section B of the online Appendix, second-order approximations analogous to those presented in Herreño (2023) lead to very similar results. To a first-order, the elasticity of aggregate credit supply with respect to aggregate exposure $E(p_i)$ is:

$$\varepsilon_{credit} \approx \beta_{credit} \left(\frac{1 + \frac{1}{\phi} + \varphi \frac{1-\bar{s}}{\bar{s}}}{\eta \frac{1+\alpha}{\eta+\alpha} + \varphi \frac{1-\bar{s}}{\bar{s}}} \right), \tag{5}$$

where β_{credit} denotes the firm-level elasticity of credit supply with respect to exposure, the model counterpart of the coefficient on exposure in our firm-level credit regression, and $\bar{s}$ is the mean share of funding provided by banks across firms before the shock.

The β_{credit} term is the partial equilibrium response. The second term reflects general equilibrium forces. A more flexible product market (higher η) or labor market (higher α) weaken the effect, by increasing the extent of reallocation of demand and labor across firms. A more elastic labor supply curve (higher $1/\phi$), on the other hand, amplifies the response of employment to the drop in labor demand caused by the shock, and thus of the wage bill that must be financed. The $\varphi \frac{1-\bar{s}}{\bar{s}}$ term governs substitution away from bank credit and towards non-bank funding. Higher substitution reduces the extent of labor reallocation and the drop in employment, and pushes the effect of the shock on credit towards the partial equilibrium response. Substitution is determined by β_{credit} and by β_{output}, the firm-level elasticity of output with respect to exposure:

$$\varphi \frac{1-\bar{s}}{\bar{s}} = \frac{\alpha\eta}{\eta+\alpha} \left(\frac{\beta_{credit}}{\beta_{output}} - \frac{1+\alpha}{\alpha} \right) \tag{6}$$

Intuitively, at the firm level, substitution towards non-bank funding magnifies the effect of the shock on bank credit, while dampening the effect on output. The ratio of the two elasticities thus pins down the extent of substitution.

A first-order approximation to the elasticity of aggregate output with respect to aggregate exposure, in turn, yields:

$$\varepsilon_{output} \approx \beta_{output} \frac{\eta + \alpha}{\phi \alpha \eta} \tag{7}$$

As with credit, the effect on output is the product of the partial equilibrium elasticity and a general equilibrium term. The comparative statics with respect to η, α and $1/\phi$ are also similar to those for credit. Higher η or α increase reallocation, dampening the response. Higher $1/\phi$ amplifies the drop in employment and output, strengthening the effect.

Recalling that our definition of procurement exposure in equation (1) takes the form of an aggregate demand drop, ε_{credit} and ε_{output} represent first-order approximations to the negative of the elasticities of aggregate credit supply and output with respect to aggregate demand.

6.2 Calibration

We set $1/\phi = 0.75$ and $\eta = 4$ following Herreño (2023). The Frisch elasticity is consistent with micro evidence (Chetty et al., 2011), although well below the values typically used in macro models. The value for η is also in line with the evidence in the literature (Broda and Weinstein, 2006).

The elasticity α measures the extent of labor market frictions to the reallocation of workers across firms, which may be particularly high in Portugal. Job and worker flows are much lower than in the United States, possibly because employment protection in Portugal is among the most stringent in the OECD (Blanchard and Portugal, 2001; OECD, 2013). We account for this by estimating α using data from Portugal.

We follow the standard approach developed by Manning (2003), who shows that, in the steady-state of the canonical Burdett and Mortensen (1998) model of labor market search frictions, $\alpha = -2\alpha_S$, where α_S is the elasticity of separations with respect to wages. We estimate α_S by regressing an indicator for job separations on the log of wages at the worker level, using data from Quadros de Pessoal (QP), an administrative employer-employee matched dataset covering the universe of workers in Portugal, for the 2011-2015 period. The elasticity is obtained by dividing the coefficient on log wages by the mean separation rate. We perform the estimation within narrow cells of workers defined by year, region, professional category,[29] educational attainment, age, and gender. Table C.10 in the online Appendix shows that our estimate of α remains stable as we vary the set of controls defining these cells. We set $\alpha = 0.862$ in our calibration, the value estimated within the narrowest worker cells and reported in column 6. This is below the value of one employed by Herreño (2023), based on estimates from Webber (2015) for the United States, and consistent with the presence of stronger frictions in Portugal.

Finally, we set β_{output} and β_{credit} equal to our point estimates of the corresponding elasticities, from columns 1 and 2 of Table 8, respectively.

6.3 Results

Our calibration of equations (5), (6) and (7) implies an aggregate credit supply elasticity ε_{credit} equal to -1.604, with a 95% confidence interval of (-2.578,-0.631).[30] This compares with our firm-level estimate of -1.431 for β_{credit}, indicating a modest amount of general equilibrium amplification. Multiplying ε_{credit} by the size of shock, $E(p_i)$, yields a

[29] These are standardized categories from collective bargaining agreements that correspond to similar jobs in terms of tasks, hours, and necessary skills or experience.

[30] We calculate the standard error of our estimate using equations (5) and (6), and the "LZ2"-corrected covariance matrix of β_{output} and β_{credit} (Imbens and Kolesár, 2016). We then compute the confidence interval using a t-distribution with the Bell and McCaffrey (2002) degrees of freedom for the β_{credit} and β_{output} coefficients, which are the same.

contraction in aggregate credit of 13.3%, which amounts to 62% of the average drop in real corporate credit in Portugal in the 2011-2015 period relative to 2010Q4.

Our estimate for ε_{output} equals -0.595, with a 95% confidence interval of (-1.019,-0.172). This is only marginally larger than its firm-level counterpart $\beta_{output} = -0.563$. Multiplying by $E(p_i)$ implies that the credit supply shock led to a drop of 4.9% in output, which represents 31% of the average drop in real corporate value added in Portugal in the 2011-2015 period relative to 2010.

Since ε_{output} is an elasticity with respect to an aggregate demand drop, its negative can also be interpreted as a fiscal multiplier operating through credit supply, under no monetary policy response. We stress that this is not the total multiplier associated with the procurement cuts, only the effect operating through bank credit supply. In addition, it is estimated in the context of a financial crisis. As our results on bank leverage in Table 6 indicate, its magnitude likely depends on the health of the financial system.

Table 9 examines the sensitivity of the results to the choice of parameters. A more flexible product market, with $\eta = 10$, has little impact on the results: ε_{credit} and ε_{output} fall in magnitude to -1.457 and -0.532, respectively. A more flexible labor market, with $\alpha = 3$,[31] lowers the magnitude of ε_{credit} to -1.255, and has a large impact on ε_{output}, which falls by more than half to -0.246. Offsetting the effect on ε_{output} requires either a rigid product market or a more elastic aggregate labor supply curve. Letting $\eta = 1.5$ yields $\varepsilon_{credit} = -1.665$ and $\varepsilon_{output} = -0.422$. With $1/\phi = 2$, in line with the values typically used in macro models, the values rise to $\varepsilon_{credit} = -1.665$ and $\varepsilon_{output} = -0.656$. In the last row of the table, we use the baseline values employed by Chodorow-Reich (2014) in calibrating his model of credit supply shocks ($1/\phi = 2$, $\eta = 6.5$, $\alpha = 2$). Relative to our baseline,

[31] There is a wide range of estimates for α in the literature. Sokolova and Sorensen (2021) survey existing studies and report a median estimate of 1.7, with substantial variation as a function of methodology and setting. Chodorow-Reich (2014) considers a range between 1 and 3 in his calibration, and we use his upper bound to define a flexible labor market.

these parameters imply a similar effect on credit ($\varepsilon_{credit} = -1.691$) and a stronger effect on output ($\varepsilon_{output} = -0.736$).

In sum, the aggregate elasticity of credit supply is relatively robust to the structural characteristics of labor and product markets, and not far from the corresponding firm-level elasticity. In the case of output, the model needs one of three features to produce an aggregate response that approximates the cross-sectional one: a rigid labor market, a rigid product market, or an elastic aggregate labor supply curve. The first two create barriers to reallocation, while the third causes the effect of the shock to translate into a meaningful drop in labor supply.

7 Conclusion

This paper shows that the link between governments and banks through firms with public procurement contracts can amplify bank distress during a financial crisis, with large effects on credit supply and output. To the extent that shocks to public procurement propagate similarly to other shocks to public and private spending, we provide a micro-level estimate of the elasticity of credit supply with respect to aggregate demand shocks in general.

In a general equilibrium model of credit supply shocks, we find that the aggregate elasticity of credit supply is similar to the one we estimate in the cross-section of firms. We see this result as a lower bound. Namely, we do not account for the effect of credit supply on aggregate demand, which could trigger a feedback loop. Future research might incorporate our estimates into richer frameworks featuring this and other sources of amplification, such as the role of bank leverage.

In addition, research on this mechanism in other countries and periods could shed light on how broadly it operates and in which settings. A key question of interest, in our view, is how negative aggregate demand shocks affect credit supply in a non-crisis context. Another question is whether negative and positive demand shocks generate symmetric effects.

References

Acharya, Viral, Itamar Dreschsler, and Philipp Schnabl. 2014. "A Pyrrhic Victory? Bank Bailouts and Sovereign Credit Risk." *Journal of Finance*, 69(6): 2689–2739.

Acharya, Viral, Tim Eisert, Christian Eufinger, and Christian Hirsch. 2018. "Real Effects of the Sovereign Debt Crisis in Europe: Evidence from Syndicated Loans." *Review of Financial Studies*, 31(8): 2855–2896.

Acharya, Viral, Tim Eisert, Christian Eufinger, and Christian Hirsch. 2019. "Whatever It Takes: The Real Effects of Unconventional Monetary Policy." *Review of Financial Studies*, 32(9): 3366–3411.

Altavilla, Carlo, Marco Pagano, and Saverio Simonelli. 2017. "Bank Exposures and Sovereign Stress Transmission." *Review of Finance*, 21(6): 2103–2139.

Auerbach, Alan, and Yuriy Gorodnichenko. 2012. "Measuring the Output Responses to Fiscal Policy." *American Economic Journal: Economic Policy*, 4(2): 1–27.

Auerbach, Alan, Yuriy Gorodnichenko, and Daniel Murphy. 2020. "Effects of Fiscal Policy on Credit Markets." *AEA Papers and Proceedings*, 110: 119–124.

Augusto, Francisco, and Sónia Félix. 2014. "The Impact of Bank Recapitalization on Firms' Access to Credit: Evidence from Portugal." Banco de Portugal Financial Stability Paper.

Balloch, Cynthia. 2023. "Inflows and Spillovers: Tracing the Impact of Bond Market Liberalization." Columbia University Working Paper.

Banco de Portugal. 2016. "Financial Stability Report." Banco de Portugal.

Bandiera, Oriana, Andrea Prat, and Tommaso Valletti. 2009. "Active and Passive Waste in Government Spending: Evidence from a Policy Experiment." *American Economic Review*, 99(4): 1278–1308.

Beers, David T., and Jamshid Mavalwalla. 2017. "Database of Sovereign Defaults, 2017." Bank of Canada Technical Report 101.

Bell, Robert, and Daniel McCaffrey. 2002. "Bias Reduction in Standard Errors for Linear Regression with Multi-stage Samples." *Survey Methodology*, 28(2): 169–182.

Bentolila, Samuel, Marcel Jansen, and Gabriel Jiménez. 2018. "When Credit Dries Up: Job Losses in the Great Recession." *Journal of the European Economic Association*, 16(3): 650–695.

Blanchard, Olivier, and Daniel Leigh. 2013. "Growth Forecast Errors and Fiscal Multipliers." *American Economic Review*, 103(3): 117–120.

Blanchard, Olivier, and Pedro Portugal. 2001. "What Hides behind an Unemployment Rate: Comparing Portuguese and U.S. Labor Markets." *American Economic Review*, 91(1): 187–207.

Bocola, Luigi. 2016. "The Pass-Through of Sovereign Risk." *Journal of Political Economy*, 124(4): 879–926.

Bosio, Erica, Simeon Djankov, Edward Glaeser, and Andrei Shleifer. 2022. "Public Procurement in Law and Practice." *American Economic Review*, 112(4): 1091–1117.

Bottero, Margherita, Simone Lenzu, and Filippo Mezzanotti. 2020. "Sovereign Debt Exposure and the Bank Lending Channel: Impact on Credit Supply and the Real Economy." *Journal of International Economics*, 126: 1–26.

Brinca, Pedro, Hans Holter, Per Krusell, and Laurence Malafry. 2016. "Fiscal Multipliers in the 21st Century." *Journal of Monetary Economics*, 77: 53–69.

Broda, Christian, and David E. Weinstein. 2006. "Globalization and the Gains From Variety." *Quarterly Journal of Economics*, 121(2): 541–585.

Brunnermeier, Markus, Luis Garicano, Philip Lane, Marco Pagano, Ricardo Reis, Tano Santos, David Thesmar, Stijn Van Nieuwerburgh, and Dimitri Vayanos. 2016. "The Sovereign-Bank Diabolic Loop and ESBies." *American Economic Review*, 106(5): 508–512.

Burdett, Kenneth, and Dale T. Mortensen. 1998. "Wage Differentials, Employer Size, and Unemployment." *International Economic Review*, 39(2): 257–273.

Burgess, Robin, Remi Jedwab, Edward Miguel, Ameet Morjaria, and Gerard Padró i Miquel. 2015. "The Value of Democracy: Evidence from Road Building in Kenya." *American Economic Review*, 105(6): 1817–1851.

Chetty, Raj, Adam Guren, Day Manoli, and Andrea Weber. 2011. "Are Micro and Macro Labor Supply Elasticities Consistent? A Review of Evidence on the Intensive and Extensive Margins." *American Economic Review*, 101(3): 471–475.

Chodorow-Reich, Gabriel. 2014. "The Employment Effects of Credit Market Disruptions: Firm-Level Evidence from the 2008-09 Financial Crisis." *Quarterly Journal of Economics*, 129(1): 1–59.

Chodorow-Reich, Gabriel, Laura Feiveson, Zachary Liscow, and William Gui Woolston. 2012. "Does State Fiscal Relief During Recessions Increase Employment? Evidence from the American Recovery and Reinvestment Act." *American Economic Journal: Economic Policy*, 4(3): 118–145.

Chodorow-Reich, Gabriel, Plamen T. Nenov, and Alp Simsek. 2021. "Stock Market Wealth and the Real Economy: A Local Labor Market Approach." *American Economic Review*, 111(5): 1613–1657.

Christiano, Lawrence, Martin Eichenbaum, and Sergio Rebelo. 2011. "When Is the Government Spending Multiplier Large?" *Journal of Political Economy*, 119(1): 78–121.

Cingano, Federico, Francesco Manaresi, and Enrico Sette. 2016. "Does Credit Crunch Investment Down? New Evidence on the Real Effects of the Bank-Lending Channel." *Review of Financial Studies*, 29(10): 2737–2773.

Cohen, Lauren, Joshua Coval, and Christopher Malloy. 2011. "Do Powerful Politicians Cause Corporate Downsizing?" *Journal of Political Economy*, 119(6): 1015–1060.

di Giovanni, Julian, Manuel García-Santana, Priit Jeenas, Enrique Moral-Benito, and Josep Pijoan-Mas. 2022. "Government Procurement and Access to Credit: Firm Dynamics and Aggregate Implications." CREI Working Paper.

Eggertsson, Gauti, and Paul Krugman. 2012. "Debt, Deleveraging, and the Liquidity Trap: A Fisher-Minsky-Koo Approach." *Quarterly Journal of Economics*, 127(3): 1469–1513.

Farhi, Emmanuel, and Jean Tirole. 2018. "Deadly Embrace: Sovereign and Financial Balance Sheets Doom Loops." *Review of Economic Studies*, 85(3): 1781–1823.

Ferraz, Claudio, Frederico Finan, and Dimitri Szerman. 2015. "Procuring Firm Growth: The Effects of Government Purchases on Firm Dynamics." National Bureau of Economic Research Working Paper 21219.

Gabriel, Ricardo. 2022. "The Credit Channel of Public Procurement." University of Bonn Working Paper.

Gennaioli, Nicola, Alberto Martin, and Stefano Rossi. 2014. "Sovereign Default, Domestic Banks, and Financial Institutions." *Journal of Finance*, 69(2): 819–866.

Gennaioli, Nicola, Alberto Martin, and Stefano Rossi. 2018. "Banks, Government Bonds, and Default: What Do the Data Say?" *Journal of Monetary Economics*, 98: 98–113.

Gertler, Mark, and Nobuhiro Kiyotaki. 2010. "Financial Intermediation and Credit Policy in Business Cycle Analysis." In *Handbook of Monetary Economics*. Vol. 3, 547–599. Elsevier.

Gertler, Mark, and Peter Karadi. 2011. "A Model of Unconventional Monetary Policy." *Journal of Monetary Economics*, 58(1): 17–34.

Goldman, Jim, Rajkamal Iyer, and Ramana Nanda. 2022. "Amplifying the Fiscal Multiplier: The Role of Banks." Imperial College Working Paper.

Goldsmith-Pinkham, Paul, Isaac Sorkin, and Henry Swift. 2020. "Bartik Instruments: What, When, Why, and How." *American Economic Review*, 110(8): 2586–2624.

Guren, Adam, Alisdair McKay, Emi Nakamura, and Jón Steinsson. 2021. "What Do We Learn from Cross-Regional Empirical Estimates in Macroeconomics?" *NBER Macroeconomics Annual*, 35: 175–223.

Hart, Oliver, Andrei Shleifer, and Robert W. Vishny. 1997. "The Proper Scope of Government: Theory and an Application to Prisons." *Quarterly Journal of Economics*, 112(4): 1127–1161.

Herreño, Juan. 2023. "The Aggregate Effects of Bank Lending Cuts." Columbia University Working Paper.

House, Christopher, Christian Proebsting, and Linda Tesar. 2020. "Austerity in the Aftermath of the Great Recession." *Journal of Monetary Economics*, 115: 37–63.

Huber, Kilian. 2018. "Disentangling the Effects of a Banking Crisis: Evidence from German Firms and Counties." *American Economic Review*, 108(3): 868–898.

Huber, Kilian. 2023. "Estimating General Equilibrium Spillovers of Large-Scale Shocks." *Review of Financial Studies*, 36(4): 1548–1584.

Hvide, Hans, and Tom Meling. 2023. "Do Temporary Demand Shocks Have Long-Term Effects for Startups?" *Review of Financial Studies*, 36(1): 317–350.

IGCP. 2018. "Government Debt and Cash Management Annual Report." IGCP.

Imbens, Guido, and Michal Kolesár. 2016. "Robust Standard Errors in Small Samples: Some Practical Advice." *Review of Economics and Statistics*, 98(4): 701–712.

Israel, Jean Marc, Violetta Damia, Riccardo Bonci, and Gibran Watfe. 2017. "The Analytical Credit Dataset. A Magnifying Glass for Analysing Credit in the Euro Area." ECB

Occasional Paper 187.

Ivashina, Victoria, Luc Laeven, and Enrique Moral-Benito. 2022. "Loan Types and the Bank Lending Channel." *Journal of Monetary Economics*, 126: 171–187.

Kaplan, Greg, and Giovanni Violante. 2014. "A Model of the Consumption Response to Fiscal Stimulus Payments." *Econometrica*, 82(4): 1199–1239.

Khwaja, Asim Ijaz, and Atif Mian. 2008. "Tracing the Impact of Bank Liquidity Shocks: Evidence from an Emerging Market." *American Economic Review*, 98(4): 1413–42.

Kosekova, Kamelia, Angela Maddaloni, Melina Papoutsi, and Fabiano Schivardi. 2023. "Firm-Bank Relationships: A Cross-Country Comparison." ECB Working Paper.

Kutlina-Dimitrova, Zornitsa. 2018. "Government Procurement: Data, Trends and Protectionist Tendencies." DG TRADE Chief Economist Notes 2018-3.

Laeven, Luc, and Fabian Valencia. 2020. "Systemic Banking Crises Database II." *IMF Economic Review*, 68(2): 307–361.

Mankiw, Gregory. 2000. "The Savers–Spenders Theory of Fiscal Policy." *American Economic Review*, 90(2): 120–125.

Manning, Alan. 2003. *Monopsony in motion: Imperfect competition in labor markets.* Princeton University Press.

Nakamura, Emi, and Jón Steinsson. 2014. "Fiscal Stimulus in a Monetary Union: Evidence from US Regions." *American Economic Review*, 104(3): 753–792.

Nakamura, Emi, and Jón Steinsson. 2018. "Identification in Macroeconomics." *Journal of Economic Perspectives*, 32(3): 59–86.

OECD. 2011. *Government at a Glance 2011.* OECD Publishing.

OECD. 2013. *OECD Employment Outlook 2013.* OECD Publishing.

OECD. 2016. "Irish GDP up by 26.3% in 2015?" Statistics and Data Directorate Note.

Paravisini, Daniel, Veronica Rappoport, and Philipp Schnabl. 2023. "Specialization in Bank Lending: Evidence from Exporting Firms." *Journal of Finance*, 78(4): 2049–2085.

Pinardon-Touati, Noemie. 2023. "The Crowding Out Effect of Local Government Debt: Micro- and Macro-Estimates." HEC Paris Working Paper.

Porter, Robert, and J. Douglas Zona. 1993. "Detection of Bid Rigging in Procurement Auctions." *Journal of Political Economy*, 101(3): 518–538.

Shoag, Daniel. 2010. "The Impact of Government Spending Shocks: Evidence on the Multiplier from State Pension Plan Returns." Harvard University Working Paper.

Sokolova, Anna, and Todd Sorensen. 2021. "Monopsony in Labor Markets: A Meta-Analysis." *ILR Review*, 74(1): 27–55.

Webber, Douglas. 2015. "Firm Market Power and the Earnings Distribution." *Labour Economics*, 35(C): 123–134.

Wolf, Christian K. 2023. "The Missing Intercept: A Demand Equivalence Approach." *American Economic Review*, 113(8): 2232–2269.

Table 1: Sample summary statistics

Panel A: Bank-Firm Matched Sample					
	Mean	SD	P10	Median	P90
Bank-Firm Variables					
Total Credit (€ thousand)	262.87	446.14	31.82	94.94	651.44
Bank Variables					
Procurement Exposure	0.08	0.02	0.05	0.09	0.11
Sovereign Debt Exposure	0.64	0.59	0.11	0.40	1.72
Total Assets (€ billion)	64.23	38.68	17.57	60.43	112.46
Leverage	0.08	0.03	0.05	0.08	0.11
Liquidity	0.04	0.01	0.02	0.04	0.06
Foreign Bank	0.19	0.39	0.00	0.00	1.00
Credit/Assets	0.69	0.06	0.65	0.66	0.76
NPL/Total Credit	0.10	0.06	0.03	0.09	0.16
Observations	76,289				
Panel B: Firm-Level Sample					
	Mean	SD	P10	Median	P90
Firm Variables					
Value Added (€ thousand)	312.88	479.61	32.92	137.23	758.30
Sales (€ thousand)	1,272.50	2,185.07	102.25	451.96	3,179.89
Assets (€ thousand)	1,392.01	2,456.17	126.52	499.00	3,406.39
Cash (€ thousand)	0.08	0.15	0.00	0.03	0.21
Employment	12.60	16.65	2.00	7.00	30.00
Return on Assets	-0.02	5.03	-0.08	0.05	0.16
Leverage	0.40	0.41	0.11	0.35	0.71
Current Ratio	0.64	0.37	0.21	0.70	0.97
Bank Variables					
Procurement Exposure	0.08	0.02	0.05	0.09	0.11
Sovereign Debt Exposure	0.62	0.52	0.11	0.42	1.62
Total Assets (€ billion)	62.65	34.16	17.57	60.43	111.25
Leverage	0.08	0.02	0.05	0.08	0.10
Liquidity	0.04	0.01	0.03	0.04	0.06
Foreign Bank	0.17	0.31	0.00	0.00	0.79
Credit/Assets	0.69	0.05	0.64	0.67	0.76
NPL/Total Credit	0.10	0.05	0.04	0.09	0.16
Observations	50,346				

This table reports mean, standard deviation (SD), 10th-percentile (P10), median and 90th-percentile for the bank-firm matched sample (Panel A) and firm-level sample (Panel B). Bank-level variables are measured in 2010Q1 and firm-level variables in 2010. In Panel B, bank variables are aggregated by firm using the credit shares of each bank as weights. The sample consists of banks with at least 1% of the corporate credit market, and firms without public procurement contracts (non-contractors) in 2009-2010. Variable definitions are provided in Table A.1 in the Appendix.

Table 2: Balancing of bank covariates

	Procurement exposure			
	Below median	Above median	Difference	SE
Sovereign Debt Exposure	0.706	0.635	0.071	(0.351)
Total Assets (log)	9.944	10.229	-0.284	(0.671)
Leverage	0.067	0.067	-0.001	(0.019)
Liquidity	0.041	0.032	0.009	(0.010)
Foreign Bank	0.500	0.286	0.214	(0.290)
Credit/Assets	0.738	0.750	-0.013	(0.062)
NPL/Total Credit	0.051	0.104	-0.053	(0.033)
Predicted Growth in Other NPLs	0.084	0.079	0.005	(0.008)
Construction Exposure	0.156	0.227	-0.071	(0.033)
Recapitalized	0.333	0.571	-0.238	(0.292)
Predicted Growth in Other Credit				
By Financing Type	-0.120	-0.112	-0.008	(0.033)
By Collateral Type	0.000	0.006	-0.005	(0.038)
By Sector	-0.114	-0.124	0.011	(0.025)
By Location	-0.102	-0.097	-0.005	(0.011)

This table compares precrisis (2010Q1) covariates of banks with below and above median procurement exposure. The table report means, the difference in means and the standard error of the difference in means for each group of banks. The sample consists of banks with at least 1% of the corporate credit market. Variable definitions are provided Table A.1 in the Appendix.

Table 3: Effect of procurement exposure on bank-firm level credit

			Controls for other credit supply shocks		
	Baseline (1)	Survival (2)	Construction exposure (3)	Predicted growth in other NPLs (4)	Recapita-lizations (5)
Procurement Exposure	-2.460	-1.522	-2.578	-2.597	-2.557
	(0.682)	(0.766)	(0.672)	(0.821)	(0.759)
BM degrees of freedom	3.3	3.3	3.4	3.3	3.2
Observations	76,289	76,289	76,289	76,289	76,289
Adjusted R^2	0.067	0.102	0.068	0.069	0.067

This table presents estimates from credit regressions using bank-firm matched data. The dependent variable is the log cumulative growth in credit between 2010Q4 and 2015Q4. Procurement exposure is the fraction of credit to government contractors in the bank's loan portfolio in 2010Q1, weighted by the share of contract cuts in firm sales. All regressions control for precrisis sovereign debt exposure, total assets, and leverage at the bank level, as well as for precrisis log total assets, return on assets, leverage, and the current ratio at the firm level. In column 2 the dependent variable is an indicator for whether a relationship survived until 2015Q4. Column 3 adds the share of credit to the construction sector in 2010Q1 to the set of bank controls. Column 4 adds a shift-share predictor of NPL growth for non-contractors during the crisis, in which the shares are bank exposures by sector in 2010Q1 and the shifters are the leave-one-out national changes in NPLs as a share of precrisis credit in each sector between 2010Q1 and 2015Q4. Column 5 adds an indicator for whether a bank was recapitalized. The sample consists of banks with at least 1% of the corporate credit market, firms without public procurement contracts (non-contractors) in 2009-2010, and lending relationships that existed in 2009 and 2010 and were above €25,000 in 2010Q4. Standard errors in parentheses are clustered at the bank level using the "LZ2" bias-reduction modification of Imbens and Kolesár (2016). The BM degrees of freedom row reports the degrees of freedom suggested by Bell and McCaffrey (2002) to compute t-distribution confidence intervals for the coefficient on procurement exposure.

Table 4: Decomposing the effect of exposure

	$\hat{\alpha}_j$	Top 5% share	$\hat{\beta}_j$
Construction	0.845	0.811	-2.04
Administrative services	0.039	0.738	-2.82
Water and waste management	0.032	0.957	-2.50
Consulting	0.029	0.546	-6.86
Wholesale and retail trade	0.018	0.467	-3.61

This table lists the top five sectors by the sum of Rotemberg weights ($\hat{\alpha}_j$), calculating following the decomposition proposed by Goldsmith-Pinkham, Sorkin and Swift (2020), as explained in the text. The second column displays the fraction of weights within each sector accounted for by the top 5% of contractors by weight. The third column reports the weighted average coefficient on exposure obtained from the decomposition for each sector.

Table 5: Alternative controls for credit demand

	Firms with multiple relationships		Controls for predicted growth in other credit			
	Baseline (1)	Within firm (2)	Financing type (3)	Collateral type (4)	Sector (5)	Location (6)
Procurement Exposure	-2.306	-2.593	-2.444	-2.420	-2.196	-2.412
	(0.898)	(0.810)	(0.727)	(0.521)	(0.689)	(0.746)
BM degrees of freedom	3.3	5.1	3.2	3.3	3.3	3.7
Observations	41,138	41,138	76,289	76,289	76,289	76,289
Adjusted R^2	0.099	0.297	0.068	0.070	0.070	0.067

This table presents estimates from credit regressions using bank-firm matched data. The dependent variable is the log cumulative growth in credit between 2010Q4 and 2015Q4. Procurement exposure is the fraction of credit to government contractors in the bank's loan portfolio in 2010Q1, weighted by the share of contract cuts in firm sales. All regressions control for precrisis sovereign debt exposure, total assets, and leverage at the bank level, as well as for precrisis log total assets, return on assets, leverage, and the current ratio at the firm level. Columns 1 and 2 restrict the sample to firms with at least two lending relationships, and column 2 includes firm fixed effects. Column 3 adds a shift-share predictor of credit growth for non-contractors during the crisis, where the shares are bank exposures by financing type in 2010Q1 and the shifters are the leave-one-out national credit growth rates for each financing type between 2010Q1 and 2015Q4. Columns 4, 5 and 6 add analogous predictors of credit growth based on precrisis exposures to credit collateral types, sectors and municipalities respectively. The sample consists of banks with at least 1% of the corporate credit market, firms without public procurement contracts (non-contractors) in 2009-2010, and lending relationships that existed in 2009 and 2010 and were above €25,000 in 2010Q4. Standard errors in parentheses are clustered at the bank level using the "LZ2" bias-reduction modification of Imbens and Kolesár (2016). The BM degrees of freedom row reports the degrees of freedom suggested by Bell and McCaffrey (2002) to compute t-distribution confidence intervals for the coefficient on procurement exposure.

Table 6: Interaction with bank leverage and recapitalizations

	Leverage (1)	Recapita-lizations (2)	Leverage with public injections (3)	Leverage with all injections (4)
Procurement Exposure	-4.276	-4.515	-4.668	-4.716
	(0.990)	(0.852)	(0.819)	(0.660)
Leverage	-0.366		-0.403	-0.439
	(0.274)		(0.300)	(0.259)
Procurement Exposure × Leverage	28.180		33.843	34.640
	(18.643)		(18.537)	(15.157)
Recapitalized		-0.028		
		(0.006)		
Procurement Exposure × Recapitalized		3.121		
		(0.535)		
BM df for interaction	3.8	2.9	4.1	4.7
Observations	76,289	72,648	76,289	76,289
Adjusted R^2	0.068	0.069	0.068	0.069

This table presents estimates from credit regressions using bank-firm matched data. The dependent variable is the log cumulative growth in credit between 2010Q4 and 2015Q4. Procurement exposure is the fraction of credit to government contractors in the bank's loan portfolio in 2010Q1, weighted by the share of contract cuts in firm sales. All regressions control for precrisis sovereign debt exposure, total assets, and leverage at the bank level, as well as for precrisis log total assets, return on assets, leverage, and the current ratio at the firm level. In column 1, bank leverage is the ratio of bank equity to total assets. In column 2, recapitalized is an indicator for whether a bank was recapitalized in the 2010-2013 period. In column 3, bank leverage includes the capital injections banks received from the government in 2010-2013. In column 4, leverage additionally includes private capital injections in the same period. The sample consists of banks with at least 1% of the corporate credit market, firms without public procurement contracts (non-contractors) in 2009-2010, and lending relationships that existed in 2009 and 2010 and were above €25,000 in 2010Q4. Standard errors in parentheses are clustered at the bank level using the "LZ2" bias-reduction modification of Imbens and Kolesár (2016). The BM degrees of freedom row reports the degrees of freedom suggested by Bell and McCaffrey (2002) to compute t-distribution confidence intervals for the coefficient on procurement exposure.

Table 7: Heterogeneous effects

	Credit Risk		Firm size		Firm age	
	Low	High	Small	Large	Young	Old
	(1)	(2)	(3)	(4)	(5)	(6)
Procurement Exposure	-2.435	-2.587	-2.277	-2.735	-2.500	-2.450
	(0.805)	(0.644)	(0.511)	(0.866)	(0.573)	(0.797)
BM degrees of freedom	3.4	3.7	3.6	3.4	3.5	3.5
Observations	40,913	33,400	41,050	33,413	42,596	31,864
Adjusted R^2	0.077	0.087	0.080	0.076	0.083	0.077

This table presents estimates from credit regressions using bank-firm matched data. The dependent variable is the log cumulative growth in credit between 2010Q4 and 2015Q4. Procurement exposure is the fraction of credit to government contractors in the bank's loan portfolio in 2010Q1, weighted by the share of contract cuts in firm sales. All regressions control for precrisis sovereign debt exposure, total assets, and leverage at the bank level, as well as for precrisis log total assets, return on assets, leverage, and the current ratio at the firm level. Split samples are defined using median values in 2010. Credit risk is the probability of default from SIAC, a credit assessment system developed by Banco de Portugal to provide individual credit risk ratings to enterprises. Firm size equals total assets. Firm age is calculated from the date of incorporation. The sample consists of banks with at least 1% of the corporate credit market, firms without public procurement contracts (non-contractors) in 2009-2010, and lending relationships that existed in 2009 and 2010 and were above €25,000 in 2010Q4. Standard errors in parentheses are clustered at the bank level using the "LZ2" bias-reduction modification of Imbens and Kolesár (2016). The BM degrees of freedom row reports the degrees of freedom suggested by Bell and McCaffrey (2002) to compute t-distribution confidence intervals for the coefficient on procurement exposure.

Table 8: Effect of procurement exposure on firm-level outcomes

	Credit (1)	Value added (2)	Sales (3)	Assets (4)	Employment (5)
Procurement Exposure	-1.431	-0.563	-0.623	-0.317	-0.410
	(0.293)	(0.148)	(0.218)	(0.075)	(0.134)
BM degrees of freedom	4.3	4.3	4.3	4.3	4.3
Observations	50,346	50,346	50,346	50,346	50,346
Adjusted R^2	0.087	0.277	0.239	0.224	0.172

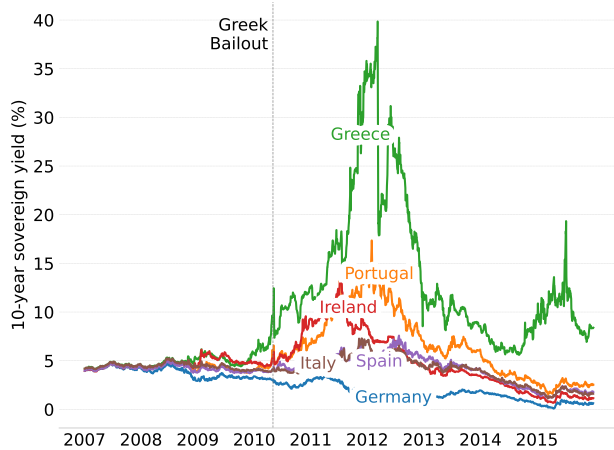
This table presents estimates from regressions for firm-level outcomes. The dependent variable is the log cumulative growth in each outcome between 2010 and 2015 (2010Q4 and 2015Q4 for credit). Procurement exposure is the fraction of credit to government contractors in the bank's loan portfolio in 2010Q1, weighted by the share of contract cuts in firm sales. All regressions control for precrisis sovereign debt exposure, total assets, and leverage at the bank level, as well as for precrisis log total assets, return on assets, leverage, and the current ratio at the firm level. Procurement exposure and bank controls are aggregated to the firm level using the credit shares of each bank as weights. The sample consists of banks with at least 1% of the corporate credit market, and firms without public procurement contracts (non-contractors) in 2009-2010. Standard errors in parentheses are clustered at the level of the main bank by loan size, using the "LZ2" bias-reduction modification of Imbens and Kolesár (2016). The BM degrees of freedom row reports the degrees of freedom suggested by Bell and McCaffrey (2002) to compute t-distribution confidence intervals for the coefficient on procurement exposure.

Table 9: Aggregate elasticities of credit and output

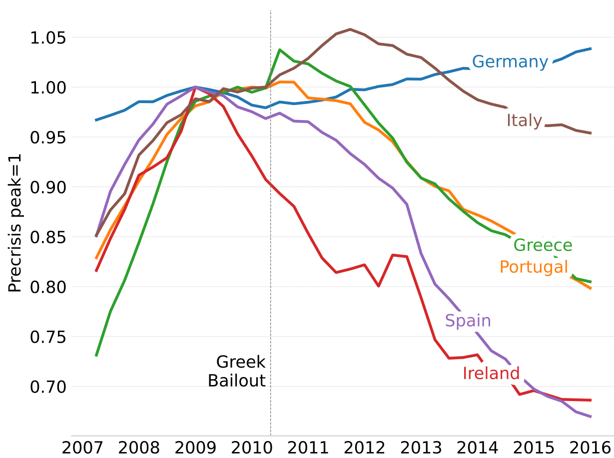
	$1/\phi$	η	α	ε_{credit}	ε_{output}
Baseline	0.75	4.00	0.86	-1.604	-0.595
Flexible product market	0.75	10.00	0.86	-1.457	-0.532
Flexible labor market	0.75	4.00	3.00	-1.255	-0.246
Flexible labor, rigid product	0.75	1.50	3.00	-1.665	-0.422
Flexible labor, high Frisch elasticity	2.00	4.00	3.00	-1.665	-0.656
Chodorow-Reich (2014)	2.00	6.50	2.00	-1.691	-0.736

This table presents results from our baseline calibration and sensitivity tests. The first three columns report the parameters used in each calibration. The last two columns present the resulting estimates of the aggregate elasticities of credit and output.

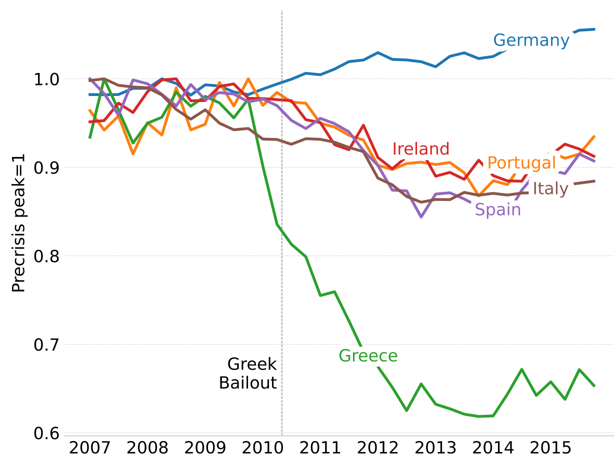
Figure 1: The European sovereign debt crisis



(a) Sovereign yields



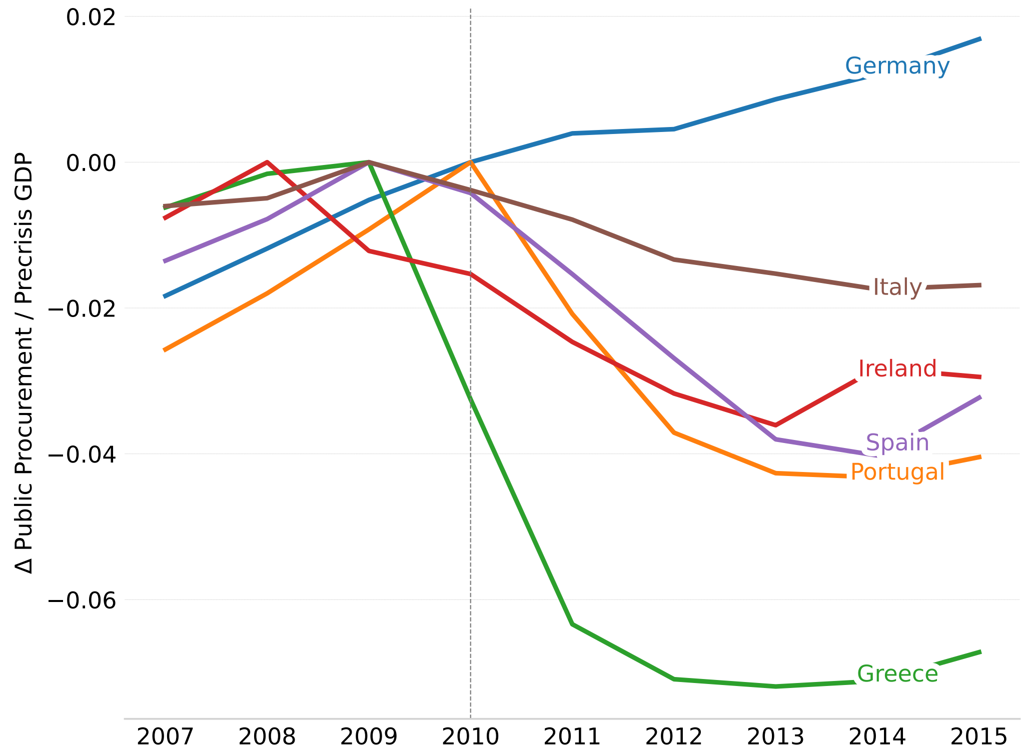
(b) Credit to private non-financial sector



(c) Household disposable income

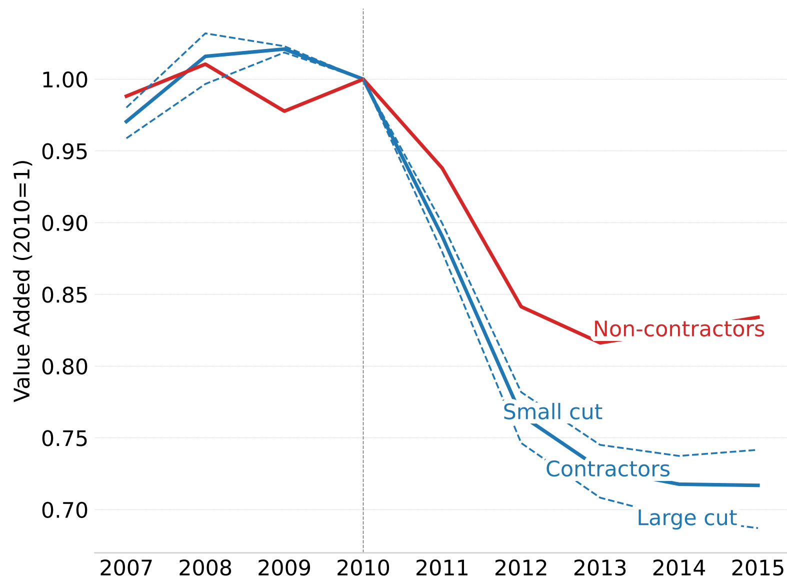
This figure plots the evolution of 10-year sovereign yields, credit to the domestic private non-financial sector, and real household disposable income per capita for the set of countries at the center of the European sovereign debt crisis and for Germany. Sovereign yield data are from Refinitiv. Credit is from the ECB except for credit from monetary and financial institutions in Greece, for which we use data from the Bank of Greece that corrects for a series break in June 2010. Household income data are from the OECD. We present household income rather than GDP to exclude the effect of multinational corporations domiciled in Ireland for tax reasons (see OECD, 2016, for a discussion of this issue).

Figure 2: Public procurement in the crisis

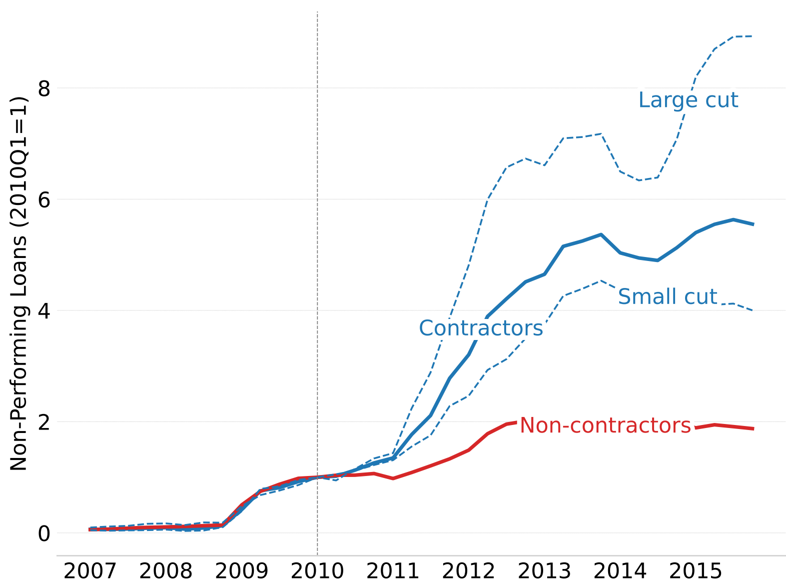


This figure plots the change in real public procurement spending relative to its precrisis peak, as a fraction of precrisis GDP, for crisis-hit countries and Germany. Data are from the OECD.

Figure 3: Impact of procurement cuts on firms



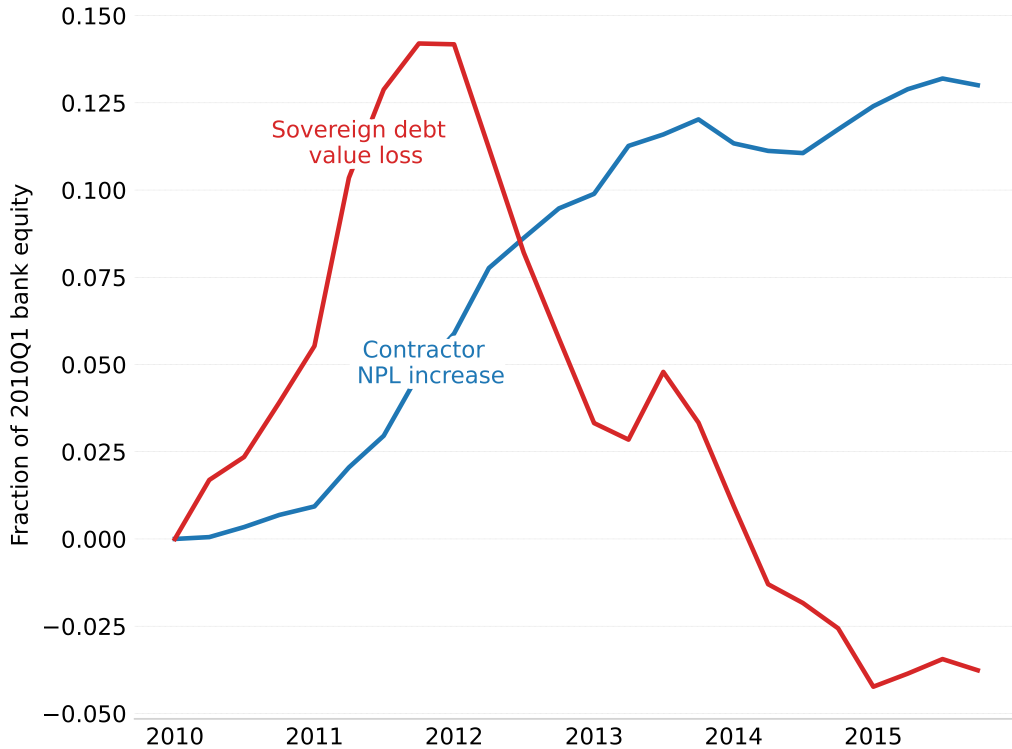
(a) Value added



(b) Non-performing loans

This figure presents the evolution of value added in Panel (a) and NPLs in Panel (b) for firms with public procurement contracts in 2010 (contractors) versus firms without such contracts in 2010 (non-contractors). The dashed lines further separate government contractors into those supplying products and services that suffered above and below median procurement cuts. Product classifications are based on the Common Procurement Vocabulary (CPV) codes reported in each contract. When a firm supplies more than one product or service, we use the average cut weighted by firm-level contract amounts in 2010.

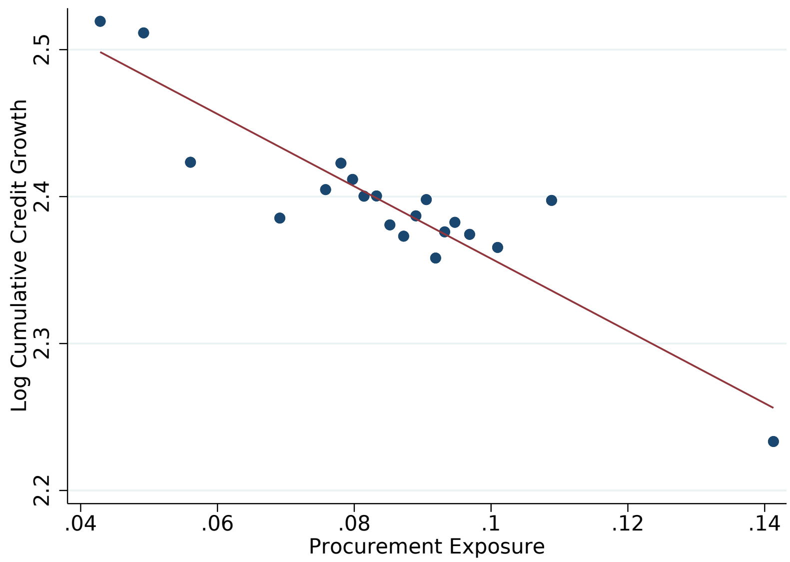
Figure 4: Impact of the procurement and sovereign debt shocks on NPLs



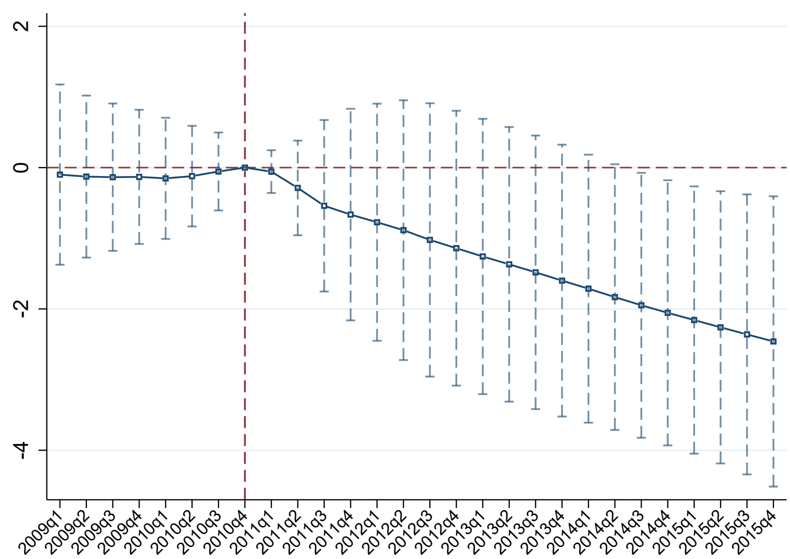
This figure plots the increase in NPLs from firms with public procurement contracts in 2010 (contractors), along with the loss in the market value of bank domestic sovereign debt holdings, in the period between 2010Q1 and 2015Q4. Both series are plotted as a fraction of total bank equity in 2010Q1. Our estimate for the change in the aggregate market value of domestic sovereign debt is based on data on debt holdings from Banco de Portugal's Monetary and Financial Statistics, the average residual maturity from the EBA's 2011 stress test data, sovereign yield data from Refinitiv and the average interest rate on outstanding debt in 2010 reported by IGCP (2018).

Figure 5: Effect of procurement exposure on credit at the bank-firm level

(a) Credit growth 2010Q4-2015Q4 vs. procurement exposure



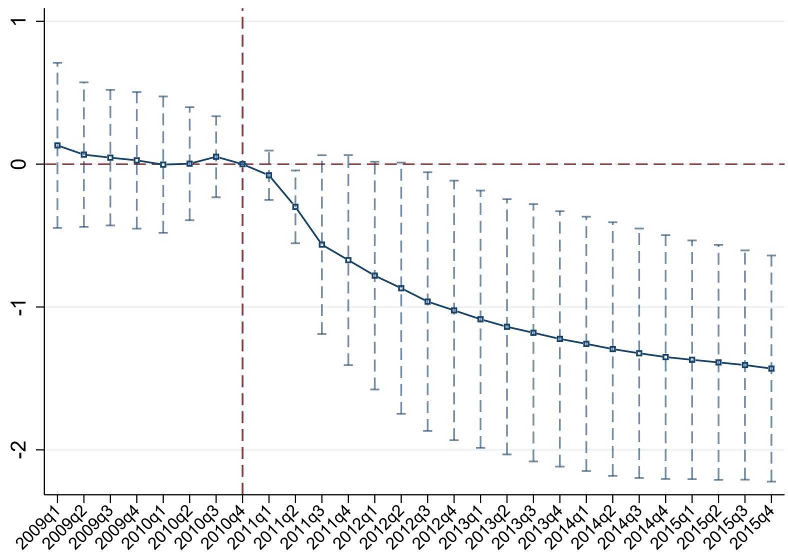
(b) Effect over time



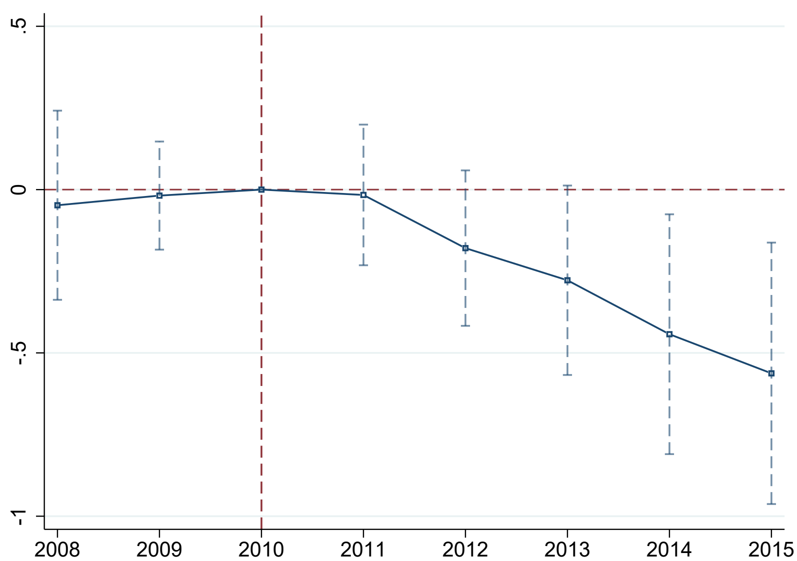
Panel (a) presents a binned scatter plot of the log of cumulative credit growth between 2010Q4 and 2015Q4 vs. procurement exposure at the bank-firm level. Panel (b) shows point estimates and 95% confidence intervals for the bank-firm level effect of procurement exposure on log cumulative credit growth between 2010Q4 and each quarter between 2009Q1 and 2015Q4. Standard errors are clustered at the bank level using the "LZ2" bias-reduction modification of Imbens and Kolesár (2016), and confidence intervals are calculated using a t-distribution with the degrees of freedom suggested by Bell and McCaffrey (2002).

Figure 6: Effect of procurement exposure at the firm level

(a) Credit



(b) Value added



Panel (a) shows point estimates and 95% confidence intervals for the firm-level effect of procurement exposure on log cumulative credit growth between 2010Q4 and each quarter between 2009Q1 and 2015Q4. Panel (b) presents estimates for log cumulative value added growth between 2010 and each year between 2008 and 2015. Standard errors are clustered at the level of the main bank by loan size, using the "LZ2" bias-reduction modification of Imbens and Kolesár (2016), and confidence intervals are calculated using a t-distribution with the degrees of freedom suggested by Bell and McCaffrey (2002).

Appendix

Table A.1: Variable definitions

Bank-Firm Variables	
Total credit	Firm's total credit outstanding in each bank
Bank Variables	
Procurement exposure	Credit to firms with public procurement contracts in 2010, weighted by the share of contract cuts in firm sales, as a fraction of total credit (see equation (1))
Sovereign debt exposure	Bank exposure to domestic sovereign debt, including bonds and loans, as a fraction of total bank equity
Total assets	Book value of total bank assets
Leverage	Ratio of bank equity to total assets (we use the regulatory definition of bank leverage, under which a higher leverage ratio corresponds to a better capitalized bank)
Liquidity	Ratio of cash and marketable securities to total assets
Foreign bank	Indicator that takes the value of one if a majority of the bank's equity is owned by a foreign bank
Credit/Assets	Ratio of corporate credit to total assets
NPL/Total credit	Ratio of non-performing to total corporate loans
Construction exposure	Credit to construction firms as a fraction of total corporate credit
Predicted growth in other NPLs	Shift-share predictor of NPL growth for non-contractors, i.e., firms without public procurement contracts in 2010, where the shares are precrisis bank exposures by sector and the shifters are leave-one-out national changes in NPLs, as a share of precrisis credit, in each sector between 2010Q1 and 2015Q4
Recapitalized	Indicator that takes the value of one if the bank received a public or private injection in 2010-2015 (all recapitalizations in this period ocurred between 2010 and 2013)
Predicted growth in other credit by financing type	Shift-share predictor of credit growth for non-contractors, i.e., firms without public procurement contracts in 2010, where the shares are bank exposures by financing type and the shifters are the leave-one-out national growth rates in credit for each financing type between 2010Q1 and 2015Q4
Predicted growth in other credit by collateral type	Shift-share predictor of credit growth for non-contractors, i.e., firms without public procurement contracts in 2010, where the shares are bank exposures by collateral type and the shifters are the leave-one-out national growth rates in credit for each collateral type between 2010Q1 and 2015Q4
Predicted growth in other credit by sector	Shift-share predictor of credit growth for non-contractors, i.e., firms without public procurement contracts in 2010, where the shares are bank exposures by sector and the shifters are the leave-one-out national growth rates in credit for each sector between 2010Q1 and 2015Q4
Predicted growth in other credit by location	Shift-share predictor of credit growth for non-contractors, i.e., firms without public procurement contracts in 2010, where the shares are bank exposures by municipality and the shifters are the leave-one-out national growth rates in credit for each municipality between 2010Q1 and 2015Q4
Firm Variables	
Sales	Total sales
Value added	Difference between sales (turnover plus remaining income) and intermediate input costs (i.e., costs of goods sold and material consumed plus cost related to supplies and external services and indirect taxes)
Assets	Book value of total assets
Cash	Ratio of cash and short-term investments of total assets
Employment	Number of employees
Return on assets	Ratio of earnings before interest, taxes, depreciation and amortization (EBITDA) to total assets
Leverage	Ratio of total debt to total assets
Current ratio	Ratio of current assets to total assets

Online Appendix (for online publication)

A Herreño (2023) with heterogeneous bank exposure

We follow the model in Sections 1-3 in Herreño (2023), except that we modify Herreno's Assumption 2 such that the shock affects the lending rates of all banks as a function of their exposure to the procurement cuts, instead of just one bank. Specifically, we assume that bank lending terms are disrupted from R to $Re^{p_b u}$, where $p_b \geq 0$ is the procurement exposure of bank b, for a positive and sufficiently small u.

This section derives the expressions we use in the paper and in Section B of the online appendix. We index firms by i instead of j, otherwise we follow Herreno's notation throughout.

A.1 Cross-sectional elasticities

We start by deriving second-order approximations to β_{output} and β_{credit} (Propositions 3 and 4 in Herreño (2023)), as well to $\beta_{\text{fixed effect}}$, the model counterpart of the Khwaja and Mian (2008) within-firm estimator (equation (29) in Herreño (2023)).

Output A second-order approximation to R_i around $u = 0$ yields

$$R_i \approx R\left(1 + \bar{s}p_i u - \varphi\bar{s}(1-\bar{s})p_i^2\frac{u^2}{2} - \theta\bar{s}\sigma_i^2\frac{u^2}{2} + \bar{s}^2p_i^2\frac{u^2}{2}\right), \tag{8}$$

where $p_i = \sum_b \nu_{ib}p_b$ and $\sigma_i^2 = \sum_b \nu_{ib}(p_b - p_i)^2$ are the firm-level mean and variance of bank exposure, weighted by credit shares. Taking log differences with respect to the initial rate

R gives

$$\Delta \log R_i \approx \bar{s} p_i u - \varphi \bar{s}(1-\bar{s}) p_i^2 \frac{u^2}{2} - \theta \bar{s} \sigma_i^2 \frac{u^2}{2} + \bar{s}^2 p_i^2 \frac{u^2}{2}. \tag{9}$$

The coefficient on exposure in a regression of the change in log firm output on firm-level procurement exposure is given by

$$\beta_{ouptut} = \frac{\text{Cov}(\Delta \log Y_i, p_i)}{\text{Var}(p_i)}. \tag{10}$$

Using (9) and the expression for $\Delta \log Y_i$ given in Herreño (2023)'s proof of Proposition 3 leads to

$$\beta_{output} \approx -\frac{\eta\alpha}{\alpha+\eta} \left(\bar{s} u - \varphi \mathcal{M}_1 \bar{s}(1-\bar{s}) \frac{u^2}{2} - \theta \bar{s} \mathcal{M}_2 \frac{u^2}{2} + \bar{s}^2 \mathcal{M}_1 \frac{u^2}{2} \right), \tag{11}$$

where $\mathcal{M}_1 = \frac{\text{Cov}(p_i^2, p_i)}{\text{Var}(p_i)}$ and $\mathcal{M}_2 = \frac{\text{Cov}(\sigma_i^2, p_i)}{\text{Var}(p_i)}$.

Credit The coefficient on exposure in a regression of the change in log firm credit on firm-level procurement exposure is $\beta_{credit} = \beta_{share} + \frac{\alpha+1}{\alpha}\beta_{output}$, with $\beta_{share} = \frac{\text{Cov}(\Delta \log s_i, p_i)}{\text{Var}(p_i)}$. A second-order approximation to $\Delta \log s_i$ gives

$$\Delta \log s_i \approx -\varphi(1-\bar{s}) \left(p_i u - \theta \sigma_i^2 \frac{u^2}{2} + \varphi p_i^2 \bar{s} \frac{u^2}{2} - \varphi p_i^2 (1-\bar{s}) \frac{u^2}{2} \right). \tag{12}$$

This implies:

$$\beta_{credit} \approx \frac{1+\alpha}{\alpha} \beta_{output} - \varphi(1-\bar{s}) \left(u - \theta \mathcal{M}_2 \frac{u^2}{2} + \varphi \bar{s} \mathcal{M}_1 \frac{u^2}{2} - \varphi(1-\bar{s}) \mathcal{M}_1 \frac{u^2}{2} \right). \tag{13}$$

Within-firm Starting from the expression for bank-firm level credit in equation (28) in Herreño (2023), $\beta_{\text{fixed effect}}$ can be approximated as

$$\beta_{\text{fixed effect}} \approx -\theta\left(1-\frac{\text{Cov}(p_i,p_b)}{\text{Var}(p_b)}\right)u+\theta^2\frac{\text{Cov}\left((p_b-p_i)^2-\sigma_i^2,p_b\right)}{\text{Var}(p_b)}\frac{u^2}{2} \tag{14}$$

A.2 Aggregate effects

Output Next, we derive a second-order approximation to the change in aggregate output (Proposition 1 in Herreño (2023)). First, a second-order approximation to R_i^{-x} is given by

$$R_i^{-x} \approx R\left(1-x\bar{s}p_iu+x\varphi\bar{s}(1-\bar{s})p_i^2\frac{u^2}{2}+x\theta\bar{s}\sigma_i^2\frac{u^2}{2}+x^2\bar{s}^2p_i^2\frac{u^2}{2}\right). \tag{15}$$

Taking expectations across firms and log differences with respect to the initial point leads to

$$\Delta\log E(R_i^{-x}) \approx -x\bar{s}E(p_i)u+x\varphi\bar{s}(1-\bar{s})E(p_i^2)\frac{u^2}{2}+x\theta\bar{s}E(\sigma_i^2)\frac{u^2}{2}+x^2\bar{s}^2E(p_i^2)\frac{u^2}{2}. \tag{16}$$

Replacing $-x$ with the exponents of the R terms in equation (40) in Herreño (2023) gives

$$\Delta\log Y \approx -\frac{1}{\phi}\left(\bar{s}E(p_i)u-\varphi E(p_i^2)\bar{s}(1-\bar{s})\frac{u^2}{2}-\bar{s}\theta E(\sigma_i^2)\frac{u^2}{2}+\Omega\bar{s}^2E(p_i^2)\frac{u^2}{2}\right), \tag{17}$$

where $\Omega=\frac{\eta-\alpha+\eta\alpha(1-\phi)}{\alpha+\eta}$.

Credit We also derive a second-order approximation to the change in aggregate credit, which is not provided in Herreño (2023). Firm-level credit is equal to the firm's wage bill multiplied by the firm's share of funding obtained from banks:

$$
\begin{aligned}
Q_i &= s_i L_i w_i \\
&= L^{\phi-\frac{1}{\alpha}} s_i L_i^{\frac{1+\alpha}{\alpha}},
\end{aligned} \tag{18}
$$

where the second line uses the aggregate and firm-level labor supply functions in equations (12) and (13) in Herreño (2023).

Integrating (18) over firms and using the expressions for L_i and L in online appendix A.1 in Herreño (2023), aggregate credit can be expressed as

$$
Q = L^{1+\phi} \frac{E\left(s_i R_i^{-\eta\frac{\alpha+1}{\alpha+\eta}}\right)}{E\left(R_i^{-\eta\frac{\alpha+1}{\alpha+\eta}}\right)}. \tag{19}
$$

Second-order approximations to $E\left(s_i R_i^{-\eta\frac{\alpha+1}{\alpha+\eta}}\right)$ and $E\left(R_i^{-\eta\frac{\alpha+1}{\alpha+\eta}}\right)$ yield

$$
\begin{aligned}
E\left(s_i R_i^{-\eta\frac{\alpha+1}{\alpha+\eta}}\right) \approx s_i R^{-\eta\frac{\alpha+1}{\alpha+\eta}} \Bigg[& 1 - \varphi E(p_i)(1-\bar{s})u - \varphi(1-\bar{s})\left(-\theta E(\sigma_i^2) + \varphi E(p_i^2)(2\bar{s}-1)\right)\frac{u^2}{2} \\
& - \eta\frac{\alpha+1}{\alpha+\eta}\left(\bar{s}E(p_i)u - \varphi\bar{s}(1-\bar{s})E(p_i^2)\frac{u^2}{2} - \theta\bar{s}E(\sigma_i^2)\frac{u^2}{2} - \eta\frac{\alpha+1}{\alpha+\eta}\bar{s}^2E(p_i^2)\frac{u^2}{2}\right) \\
& + 2\eta\frac{\alpha+1}{\alpha+\eta}\varphi\bar{s}(1-\bar{s})E(p_i^2)\frac{u^2}{2}\Bigg],
\end{aligned} \tag{20}
$$

and

$$
\begin{aligned}
E\left(R_i^{-\eta\frac{\alpha+1}{\alpha+\eta}}\right) \approx R^{-\eta\frac{\alpha+1}{\alpha+\eta}} \Bigg[& 1 - \eta\frac{\alpha+1}{\alpha+\eta}\bigg(\bar{s}E(p_i)u - \varphi\bar{s}(1-\bar{s})E(p_i^2)\frac{u^2}{2} \\
& - \theta\bar{s}E(\sigma_i^2)\frac{u^2}{2} - \eta\frac{\alpha+1}{\alpha+\eta}\bar{s}^2E(p_i^2)\frac{u^2}{2}\bigg)\Bigg].
\end{aligned} \tag{21}
$$

Plugging these two expressions into (19) and taking log differences with respect to the initial point gives

$$
\begin{aligned}
\Delta \log Q = (1+\phi)\Delta \log Y - \varphi E(p_i)(1-\bar{s})u \\
- \varphi(1-\bar{s})\left(-\theta E(\sigma_i^2) + \varphi E(p_i^2)(2\bar{s}-1)\right)\frac{u^2}{2} \\
+ 2\eta\frac{\alpha+1}{\alpha+\eta}\varphi\bar{s}(1-\bar{s})E(p_i^2)\frac{u^2}{2},
\end{aligned} \tag{22}
$$

noting that constant returns to scale in production implies $\Delta \log L = \Delta \log Y$, and with $\Delta \log Y$ given by (17).

Elasticities Dividing $\Delta \log Y$ and $\Delta \log Q$ by $E(p_i)$ yields the elasticities of aggregate output and credit with respect to procurement exposure:

$$
\varepsilon_{output} = \frac{\Delta \log Y}{E(p_i)} \tag{23}
$$

$$
\varepsilon_{credit} = \frac{\Delta \log Q}{E(p_i)} \tag{24}
$$

In the main text, we employ first-order approximations to these elasticities. Collecting the first-order terms in (11), (13), (17), and (22), and after some algebra, a first-order approximation to ε_{credit} can be written as

$$
\varepsilon_{credit} \approx \beta_{credit}\left(\frac{1+\frac{1}{\phi}+\varphi\frac{1-\bar{s}}{\bar{s}}}{\eta\frac{1+\alpha}{\eta+\alpha}+\varphi\frac{1-\bar{s}}{\bar{s}}}\right). \tag{25}
$$

Similarly, equations (11) and (17) imply a first-order approximation to ε_{output} given by

$$
\varepsilon_{output} \approx \beta_{output}\frac{\eta+\alpha}{\phi\alpha\eta} \tag{26}
$$

These are the expressions in the paper.

B Aggregate results using second-order approximations

In this section we calculate second-order approximations to the aggregate effects of the shock, and compare them to the first-order results reported in the main text.

We calibrate the second-order expressions for ε_{output} and ε_{credit} in (23) and (24) as follows. First, we set α, ϕ, η to our baseline values in the paper. Second, the moments involving p_i, σ_i^2 and p_b are all directly observable in the data, with p_b given by equation (1) in the paper, and we set $\bar{s} = 0.88$, the average share of bank funding in total funding across firms. We measure these moments in 2010Q1. Third, we estimate θ, φ and u through a simple method of moments using (11), (13) and (14) and our point estimates of the corresponding elasticities. For (11) and (13), we use the values from columns 1 and 2 of Table 8 in the paper, respectively. For (14), we use the within-firm estimate from column 2 of Table 5 in the paper, noting the results are unchanged if we instead use our baseline estimate from column 1 of Table 3 in the paper. We obtain $\theta = 6.269$, $\varphi = 1.813$ and $u = 0.873$.

The first row of Table C.11 summarizes the results. The remaining rows present robustness checks for the alternative values of α, ϕ, η we consider in Table 9 in the paper. For each alternative set of parameters, we re-estimate θ, φ and u. As the table shows, our second-order estimates tend to be only slightly smaller in magnitude than the first-order ones. We conclude that our first-order approximations offer a reasonably accurate representation of the aggregate effect of the shock.

C Appendix Tables

Table C.1: Summary statistics for public procurement contracts

	Mean	P10	Median	P90	% of contracts	% of value
Total	132,217	523	12,132	95,950	100.00	100.00
By Procedure						
Open	821,491	8,695	128,565	1,299,385	6.40	39.74
Outright Award	37,051	471	10,910	67,146	92.75	25.99
Restricted	5,308,300	83,240	1,215,998	15061965	0.61	24.50
Negotiated	5,233,682	34,991	163,698	2,352,789	0.25	9.77
By Buyer						
Central	216,312	340	9,600	109,270	41.38	67.69
Local	72,883	2,100	14,985	99,966	58.62	32.31
By Product						
Construction work	452,950	2,900	25,000	391,849	16.18	55.42
Health and social work	1,248,029	222	7,400	52,800	0.97	9.20
Energy	615,271	3,491	26,659	717,725	1.18	5.48
Sewage, refuse and cleaning	133,581	2,800	18,350	146,376	3.31	3.35
Architecture and engineering	57,543	1,878	19,468	127,411	7.07	3.08
Business services	47,040	3,000	15,300	71,320	8.32	2.96
Medical equipment, pharmaceuticals	45,480	190	5,325	78,795	6.78	2.33
Repair and maintenance	51,366	177	6,030	49,500	5.25	2.04
IT services	59,091	5,665	22,605	114,453	3.53	1.58
Office and computing equipment	35,808	153	5,494	38,481	5.38	1.46
Transport equipment	49,038	204	11,997	75,580	3.33	1.23
Hotel, restaurant and retail trade	79,822	1,000	11,108	117,000	1.61	0.97
Construction materials	41,419	345	11,282	62,000	3.00	0.94
Other community services	32,586	402	11,500	52,549	3.77	0.93
Industrial machinery	103,514	608	10,451	51,332	1.12	0.88
Transport services	51,906	268	10,388	64,134	1.92	0.75
Furniture and domestic products	26,467	1,375	10,883	57,960	3.26	0.65
Software	45,130	3,875	16,330	76,781	1.60	0.55
Printed matter	47,886	218	8,194	41,450	1.47	0.53
Agriculture, forestry and fisheries	104,254	1,900	11,200	52,800	0.66	0.52
Other	33,540	395	9,172	54,000	20.29	5.15

This table reports summary statistics for public procurement contracts in 2010. Products are based on 2-digit Common Procurement Vocabulary (CPV) codes.

Table C.2: Large procurement cuts in the OECD (1995-2018)

			Composition of procurement cut (%)					
Episode	% cut	Cut as a % of GDP	Gross fixed capital formation	Inter-mediate cons.	Social transfers in kind	Banking crisis	IMF/EU bailout	Sovereign default or restructuring
Greece, 2009-2013	46.37	7.19	42.38	42.22	15.39	1.00	1.00	1.00
Portugal, 2010-2014	32.37	4.32	78.69	12.36	8.94	1.00	1.00	1.00
Spain, 2009-2014	28.99	4.02	77.80	12.97	9.23	1.00	1.00	0.00
Ireland, 2008-2013	28.50	3.61	91.04	21.75	-12.79	1.00	1.00	1.00
Slovak Republic, 1997-1999	24.49	3.98	59.74	45.67	-5.41	1.00	0.00	0.00
Lithuania, 2008-2009	18.92	2.40	67.54	31.24	1.22	0.00	0.00	0.00
Iceland, 2008-2010	17.60	2.90	62.19	36.27	1.54	1.00	1.00	0.00
Estonia, 2008-2010	17.19	2.43	80.94	19.14	-0.07	0.00	0.00	0.00
Czech Republic, 2009-2013	15.67	2.58	85.22	25.54	-10.76	0.00	0.00	0.00
Luxembourg, 2005-2006	14.92	1.87	85.50	8.85	5.65	0.00	0.00	0.00
Italy, 2009-2014	14.33	1.74	80.07	6.94	12.99	1.00	0.00	0.00
Norway, 1998-2000	11.52	1.50	68.32	26.01	5.67	0.00	0.00	0.00
Greece, 2004-2005	10.52	1.51	82.38	29.60	-11.98	0.00	0.00	0.00
United States, 2010-2014	10.51	1.22	44.19	55.81	-0.00	1.00	0.00	0.00
Slovenia, 2015-2016	10.32	1.41	107.72	-3.09	-4.63	0.00	0.00	0.00
Latvia, 2015-2016	10.30	1.31	89.00	24.04	-13.04	0.00	0.00	0.00
Average	19.53	2.75	75.17	24.71	0.12	0.50	0.31	0.19

This table characterizes the 16 episodes of cuts to real procurement spending of at least 10% we identify among OECD countries between 1995 and 2018. When cuts happen in consecutive years, we consider them to be part of the same episode. We drop cases where procurement increased by 10% or more in the year prior to the cuts, to exclude the effect of transitory spending fluctuations. Data on banking crises are from Laeven and Valencia (2020), data on IMF bailouts are from the Monitoring of Fund Arrangements database (we add the 2012 EU bailout of Spain, in which the IMF did not participate) and data on sovereign defaults and restructurings are from Beers and Mavalwalla (2017).

Table C.3: Summary statistics for government contractors

	Procurement/sales for contractors				Contractors/all firms			
	Mean	P10	Median	P90	Firms	Value added	Empl.	Credit
Total	0.18	0.00	0.06	0.57	0.05	0.33	0.26	0.19
By Sector								
Agriculture and farming	0.24	0.01	0.13	0.77	0.01	0.03	0.04	0.02
Mining and quarrying	0.11	0.01	0.03	0.25	0.10	0.17	0.30	0.28
Manufacturing	0.08	0.00	0.02	0.21	0.05	0.25	0.15	0.22
Electricity, gas, steam, water, air	0.07	0.00	0.01	0.10	0.02	0.30	0.44	0.10
Water and waste management	0.12	0.00	0.03	0.41	0.12	0.18	0.26	0.14
Construction	0.22	0.01	0.12	0.59	0.07	0.49	0.39	0.21
Wholesale and retail trade	0.07	0.00	0.02	0.19	0.05	0.34	0.28	0.25
Transportation and storage	0.18	0.00	0.07	0.50	0.02	0.18	0.13	0.24
Accommodation and food service	0.09	0.00	0.03	0.21	0.01	0.22	0.16	0.26
Information and communication	0.22	0.01	0.10	0.63	0.11	0.74	0.53	0.51
Real estate	0.31	0.00	0.18	1.00	0.00	0.02	0.02	0.01
Consulting	0.32	0.02	0.19	0.94	0.07	0.33	0.27	0.19
Administrative services	0.21	0.01	0.08	0.64	0.09	0.52	0.57	0.44
Education	0.31	0.01	0.17	1.00	0.04	0.24	0.19	0.26
Human health and social work	0.20	0.00	0.09	0.58	0.01	0.12	0.10	0.16
Arts, entertainment, sports	0.34	0.03	0.25	0.91	0.08	0.38	0.26	0.23
Other service	0.20	0.01	0.08	0.64	0.01	0.09	0.04	0.04

This table reports mean, 10th-percentile (P10), median and 90th-percentile (P90) for the share of public procurement contracts in firm sales for the sample of firms with procurement contracts in 2010. The table also reports the share of these firms in the universe of non-financial firms in Portugal in terms of number of firms, value added, employment and corporate credit in 2010.

Table C.4: Direct effect of procurement cuts on government contractors

	Value added	NPL ratio
	(1)	(2)
Contract Cut	-0.924	0.132
	(0.050)	(0.013)
Observations	17,278	17,278
Adjusted R^2	0.052	0.057

This table presents estimates of the direct effect of procurement cuts on government contractors. Column 1 presents estimates of a regression of log of cumulative value added growth between 2010 and 2015, defined analogously to cumulative credit growth in equation (3), on the firm's procurement cut as a fraction of sales. Procurement cuts are defined in equation (2) and sales are the 2009-2010 average. Column 2 presents estimates of a regression of the average change in the firm's NPL ratio between 2010 and each year between 2011 and 2015 on the firm's procurement cut as a fraction of sales. Robust standard errors are reported in parentheses.

Table C.5: Additional robustness tests: bank-firm level credit

Panel A. Alternative exposure measures

	NPL growth (1)	Procurement/ sales (2)	Include procurement increases (3)	Winsorize exposure (4)
Procurement Exposure	-12.885	-0.599	-2.444	-2.463
	(2.898)	(0.189)	(0.792)	(0.682)
BM degrees of freedom	2.9	3.4	3.7	3.3
Observations	76,289	76,289	76,289	76,289
Adjusted R^2	0.068	0.066	0.067	0.067

Panel B. Alternative samples

	Single relationship firms (1)	Drop high procurement sectors (2)	Contractor sample (3)	Weighted (4)
Procurement Exposure	-2.719	-2.541	-2.928	-2.444
	(0.530)	(0.598)	(0.891)	(0.678)
BM degrees of freedom	3.9	3.2	3.2	3.3
Observations	16,820	41,034	16,843	76,289
Adjusted R^2	0.058	0.059	0.086	0.068

This table presents robustness checks for the bank-firm results. The dependent variable is the log cumulative growth in credit between 2010Q4 and 2015Q4. Procurement exposure is the fraction of credit to government contractors in the bank's loan portfolio in 2010Q1, weighted by the share of contract cuts in firm sales. All regressions control for precrisis sovereign debt exposure, total assets, and leverage at the bank level, as well as for precrisis log total assets, return on assets, leverage, and the current ratio at the firm level. Panel A uses alternative definitions of procurement exposure. Column 1 of Panel A replaces procurement cuts with the national growth of NPLs by product (8-digit CPV). When a firm supplies more than one product, we take the average NPL growth weighted by firm-level contract amounts in 2010. Column 2 replaces procurement cuts with precrisis procurement levels. Column 3 accounts for procurement increases (negative cuts). Column 4 winsorizes exposure at the 2.5th and 97.5th percentiles. Panel B employs alternative samples. Column 1 restricts the sample to firms with a single credit relationship in 2010Q4. Column 2 drops firms in sectors with above median procurement cuts. Column 3 estimates the effect on the sample of government contractors. Column 4 weights observations by log credit. The sample consists of banks with at least 1% of the corporate credit market, firms without public procurement contracts (noncontractors) in 2009-2010, and lending relationships that existed in 2009 and 2010 and were above €25,000 in 2010Q4. Standard errors in parentheses are clustered at the bank level using the "LZ2" bias-reduction modification of Imbens and Kolesár (2016). The BM degrees of freedom row reports the degrees of freedom suggested by Bell and McCaffrey (2002) to compute t-distribution confidence intervals for the coefficient on procurement exposure.

Table C.6: Robustness: firm-level credit

Panel A. Controls for other shocks to credit supply

	Construction exposure (1)	Predicted growth in other NPLs (2)	Recapitalization (3)
Procurement Exposure	-1.405	-1.456	-1.343
	(0.303)	(0.294)	(0.266)
BM degrees of freedom	4.5	4.3	4.1
Observations	50,346	50,346	50,346
Adjusted R^2	0.087	0.087	0.087

Panel B. Controls for predicted growth in other credit

	Financing type (1)	Collateral type (2)	Sector (3)	Location (4)
Procurement Exposure	-1.428	-1.415	-1.363	-1.362
	(0.310)	(0.286)	(0.311)	(0.308)
BM degrees of freedom	4.2	4.3	4.4	4.8
Observations	50,346	50,346	50,346	50,346
Adjusted R^2	0.087	0.087	0.087	0.087

This table presents robustness checks for the firm-level credit results. The dependent variable is the log cumulative growth in credit between 2010Q4 and 2015Q4. Procurement exposure is the fraction of credit to government contractors in the bank's loan portfolio in 2010Q1, weighted by the share of contract cuts in firm sales. All regressions control for precrisis sovereign debt exposure, total assets, and leverage at the bank level, as well as for precrisis log total assets, return on assets, leverage, and the current ratio at the firm level. Panel A presents estimates including controls for other shocks to credit supply. Column 1 adds the share of credit to the construction sector in 2010Q1 to the set of bank controls. Column 2 adds a shift-share predictor of NPL growth for non-contractors during the crisis, in which the shares are bank exposures by sector in 2010Q1 and the shifters are the leave-one-out national changes in NPLs as a share of precrisis credit in each sector between 2010Q1 and 2015Q4. Column 3 adds an indicator for whether a bank was recapitalized. Panel B presents estimates including controls for predicted growth in other credit. Column 1 adds a shift-share predictor of credit growth for non-contractors during the crisis, where the shares are bank exposures by financing type in 2010Q1 and the shifters are the leave-one-out national credit growth rates for each financing type between 2010Q1 and 2015Q4. Columns 2, 3 and 4 add analogous predictors of credit growth based on precrisis exposures to credit collateral types, sectors and municipalities respectively. The sample consists of banks with at least 1% of the corporate credit market, and firms without public procurement contracts (non-contractors) in 2009-2010. Standard errors in parentheses are clustered at the level of the main bank by loan size, using the "LZ2" bias-reduction modification of Imbens and Kolesár (2016). The BM degrees of freedom row reports the degrees of freedom suggested by Bell and McCaffrey (2002) to compute t-distribution confidence intervals for the coefficient on procurement exposure.

Table C.7: Additional robustness tests: firm-level credit

Panel A. Alternative exposure measures

	NPL growth (1)	Procurement/ sales (2)	Include procurement increases (3)	Winsorize exposure (4)
Procurement Exposure	-4.825	-0.338	-1.477	-1.593
	(1.913)	(0.087)	(0.273)	(0.273)
BM degrees of freedom	4.5	4.5	4.7	4.7
Observations	50,346	50,346	50,346	50,346
Adjusted R^2	0.086	0.086	0.087	0.087

Panel B. Alternative samples

	Single relationship firms (1)	Drop high procurement sectors (2)	Contractor sample (3)	Weighted (4)
Procurement Exposure	-2.050	-1.279	-1.743	-1.341
	(0.460)	(0.236)	(0.440)	(0.279)
BM degrees of freedom	4.1	4.2	4.8	4.4
Observations	16,820	27,551	8,306	50,346
Adjusted R^2	0.070	0.082	0.046	0.087

This table presents additional robustness checks for the firm-level credit results. The dependent variable is the log cumulative growth in credit between 2010Q4 and 2015Q4. Procurement exposure is the fraction of credit to government contractors in the bank's loan portfolio in 2010Q1, weighted by the share of contract cuts in firm sales. All regressions control for precrisis sovereign debt exposure, total assets, and leverage at the bank level, as well as for precrisis log total assets, return on assets, leverage, and the current ratio at the firm level. Panel A uses alternative definitions of procurement exposure. Column 1 of Panel A replaces procurement cuts with the national growth of NPLs by product (8-digit CPV). When a firm supplies more than one product, we take the average NPL growth weighted by firm-level contract amounts in 2010. Column 2 replaces procurement cuts with precrisis procurement levels. Column 3 accounts for procurement increases (negative cuts). Column 4 winsorizes exposure at the 2.5th and 97.5th percentiles. Panel B employs alternative samples. Column 1 restricts the sample to firms with a single credit relationship in 2010Q4. Column 2 drops firms in sectors with above median procurement cuts. Column 3 estimates the effect on the sample of government contractors. Column 4 weights observations by log credit. The sample consists of banks with at least 1% of the corporate credit market, and firms without public procurement contracts (non-contractors) in 2009-2010. Standard errors in parentheses are clustered at the level of the main bank by loan size, using the "LZ2" bias-reduction modification of Imbens and Kolesár (2016). The BM degrees of freedom row reports the degrees of freedom suggested by Bell and McCaffrey (2002) to compute t-distribution confidence intervals for the coefficient on procurement exposure.

Table C.8: Robustness: value added

Panel A. Controls for other shocks to credit supply

	Construction exposure (1)	Predicted growth in other NPLs (2)	Recapitalization (3)
Procurement Exposure	-0.579	-0.558	-0.618
	(0.201)	(0.160)	(0.250)
BM degrees of freedom	4.5	4.3	4.1
Observations	50,346	50,346	50,346
Adjusted R^2	0.277	0.277	0.277

Panel B. Controls for predicted growth in other credit

	Financing type (1)	Collateral type (2)	Sector (3)	Location (4)
Procurement Exposure	-0.563	-0.563	-0.583	-0.570
	(0.152)	(0.148)	(0.139)	(0.197)
BM degrees of freedom	4.2	4.3	4.4	4.8
Observations	50,346	50,346	50,346	50,346
Adjusted R^2	0.277	0.277	0.277	0.277

This table presents robustness checks for the firm value added results. The dependent variable is the log cumulative growth in value added between 2010 and 2015. Procurement exposure is the fraction of credit to government contractors in the bank's loan portfolio in 2010Q1, weighted by the share of contract cuts in firm sales. All regressions control for precrisis sovereign debt exposure, total assets, and leverage at the bank level, as well as for precrisis log total assets, return on assets, leverage, and the current ratio at the firm level. Panel A presents estimates including controls for other shocks to credit supply. Column 1 adds the share of credit to the construction sector in 2010Q1 to the set of bank controls. Column 2 adds a shift-share predictor of NPL growth for non-contractors during the crisis, in which the shares are bank exposures by sector in 2010Q1 and the shifters are the leave-one-out national changes in NPLs as a share of precrisis credit in each sector between 2010Q1 and 2015Q4. Column 3 adds an indicator for whether a bank was recapitalized. Panel B presents estimates including controls for predicted growth in other credit. Column 1 adds a shift-share predictor of credit growth for non-contractors during the crisis, where the shares are bank exposures by financing type in 2010Q1 and the shifters are the leave-one-out national credit growth rates for each financing type between 2010Q1 and 2015Q4. Columns 2, 3 and 4 add analogous predictors of credit growth based on precrisis exposures to credit collateral types, sectors and municipalities respectively. The sample consists of banks with at least 1% of the corporate credit market, and firms without public procurement contracts (non-contractors) in 2009-2010. Standard errors in parentheses are clustered at the level of the main bank by loan size, using the "LZ2" bias-reduction modification of Imbens and Kolesár (2016). The BM degrees of freedom row reports the degrees of freedom suggested by Bell and McCaffrey (2002) to compute t-distribution confidence intervals for the coefficient on procurement exposure.

Table C.9: Additional robustness tests: value added

Panel A. Alternative exposure measures

	NPL growth (1)	Procurement/ sales (2)	Include procurement increases (3)	Winsorize exposure (4)
Procurement Exposure	-2.081	-0.141	-0.558	-0.651
	(0.852)	(0.036)	(0.156)	(0.163)
BM degrees of freedom	4.5	4.5	4.7	4.7
Observations	50,346	50,346	50,346	50,346
Adjusted R^2	0.277	0.277	0.277	0.277

Panel B. Alternative samples

	Single relationship firms (1)	Drop high procurement sectors (2)	Contractor sample (3)	Weighted (4)
Procurement Exposure	-0.348	-0.622	-0.606	-0.573
	(0.279)	(0.141)	(0.732)	(0.210)
BM degrees of freedom	4.1	4.2	4.8	4.3
Observations	16,820	27,551	8,306	50,345
Adjusted R^2	0.252	0.269	0.286	0.285

This table presents additional robustness checks for firm value added results. The dependent variable is the log cumulative growth in value added between 2010 and 2015. All regressions control for precrisis sovereign debt exposure, total assets, and leverage at the bank level, as well as for precrisis log total assets, return on assets, leverage, and the current ratio at the firm level. Panel A uses alternative definitions of procurement exposure. Column 1 of Panel A replaces procurement cuts with the national growth of NPLs by product (8-digit CPV). When a firm supplies more than one product, we take the average NPL growth weighted by firm-level contract amounts in 2010. Column 2 replaces procurement cuts with precrisis procurement levels. Column 3 accounts for procurement increases (negative cuts). Column 4 winsorizes exposure at the 2.5th and 97.5th percentiles. Panel B employs alternative samples. Column 1 restricts the sample to firms with a single credit relationship in 2010Q4. Column 2 drops firms in sectors with above median procurement cuts. Column 3 estimates the effect on the sample of government contractors. Column 4 weights observations by log value added. The sample consists of banks with at least 1% of the corporate credit market, and firms without public procurement contracts (non-contractors) in 2009-2010. Standard errors in parentheses are clustered at the level of the main bank by loan size, using the "LZ2" bias-reduction modification of Imbens and Kolesár (2016). The BM degrees of freedom row reports the degrees of freedom suggested by Bell and McCaffrey (2002) to compute t-distribution confidence intervals for the coefficient on procurement exposure.

Table C.10: Firm-level elasticity of labor supply

	(1)	(2)	(3)	(4)	(5)	(6)
Log Wage	-0.085	-0.097	-0.087	-0.097	-0.085	-0.085
	(0.000)	(0.000)	(0.001)	(0.001)	(0.001)	(0.001)
Implied α	0.865	0.980	0.886	0.981	0.860	0.862
	(-0.004)	(-0.004)	(-0.006)	(-0.007)	(-0.010)	(-0.011)
Observations	6,261,398	6,261,398	6,261,398	6,261,398	6,261,398	6,261,398
Adjusted R^2	0.008	0.013	0.066	0.072	0.070	0.071

This table presents estimates of the elasticity of worker-firm separations as a function of wages, and of α, the implied elasticity of labor supply to the firm. The latter is calculated as $-2\times$ the coefficient on log wages divided by the mean separations rate in the sample. The dependent variable is an indicator for whether a worker left a firm in a given year. All regressions include fixed effects for worker cells defined by a set of characteristics. We expand this set from left to right, adding one characteristic in each column. In column 1, the cells are defined by year alone. In columns 2 to 6, we respectively add the worker's region, professional category, educational attainment, age and gender. Standard errors are clustered at the individual level.

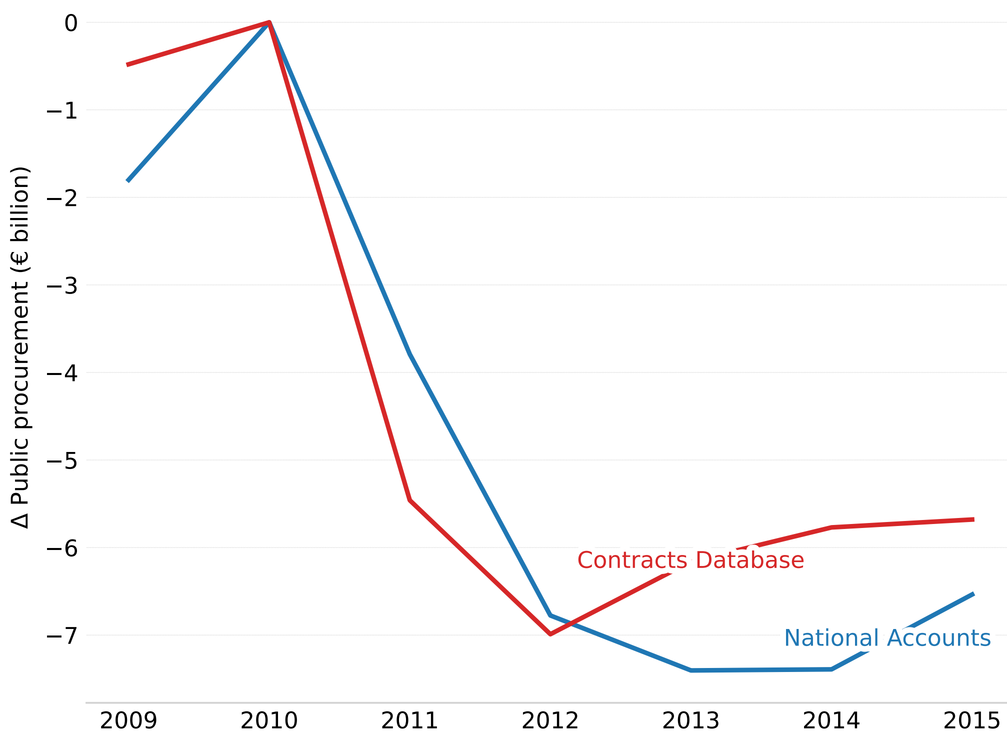
Table C.11: First versus second-order approximations to aggregate elasticities

Calibrated			Estimated			ε_{credit}		ε_{output}	
$1/\phi$	η	α	φ	θ	u	1st-order	2nd-order	1st-order	2nd-order
0.75	4.00	0.86	1.813	6.269	0.873	-1.604	-1.524	-0.595	-0.573
0.75	10.00	0.86	2.015	6.969	0.785	-1.457	-1.394	-0.532	-0.518
0.75	4.00	3.00	11.078	14.157	0.387	-1.255	-1.141	-0.246	-0.245
0.75	1.50	3.00	6.600	8.435	0.649	-1.665	-1.520	-0.422	-0.407
2.00	4.00	3.00	11.078	14.157	0.387	-1.665	-1.571	-0.656	-0.668
2.00	6.50	2.00	8.831	12.746	0.429	-1.691	-1.616	-0.736	-0.749

This table compares results from our baseline calibration (top row) and sensitivity tests using first and second-order approximations. The first three columns report the parameters used in each calibration. The next three columns report estimated parameters. The last four columns present the resulting estimates of the aggregate elasticities of credit and output.

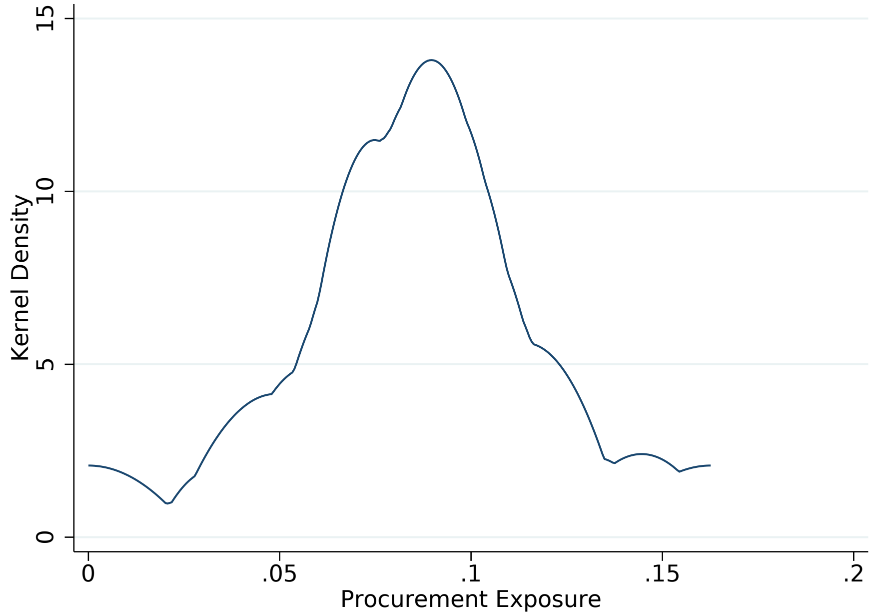
D Appendix Figures

Figure D.1: Procurement cuts: National Accounts vs contract data



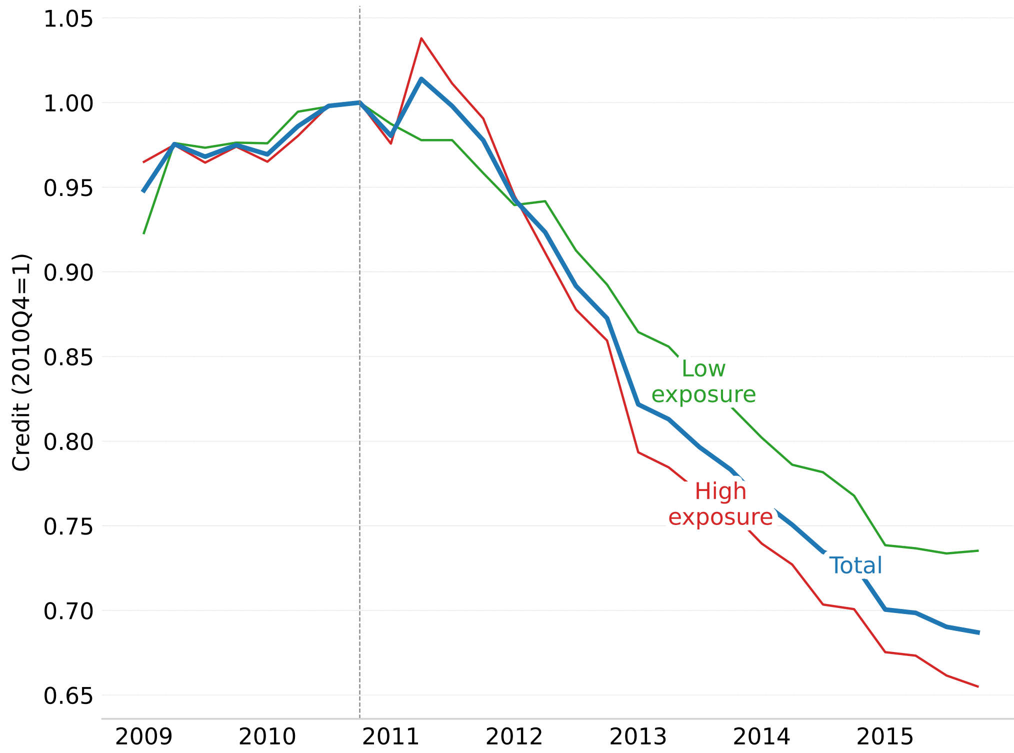
This figure compares the change in public procurement spending in Portugal in the postcrisis period calculated using System of National Accounts (SNA) data from the OECD and using our data on public procurement contracts. In SNA data, public procurement is defined as the sum of gross fixed capital formation, intermediate consumption and social transfers in kind via market producers for the general government sector.

Figure D.2: Distribution of procurement exposure across banks



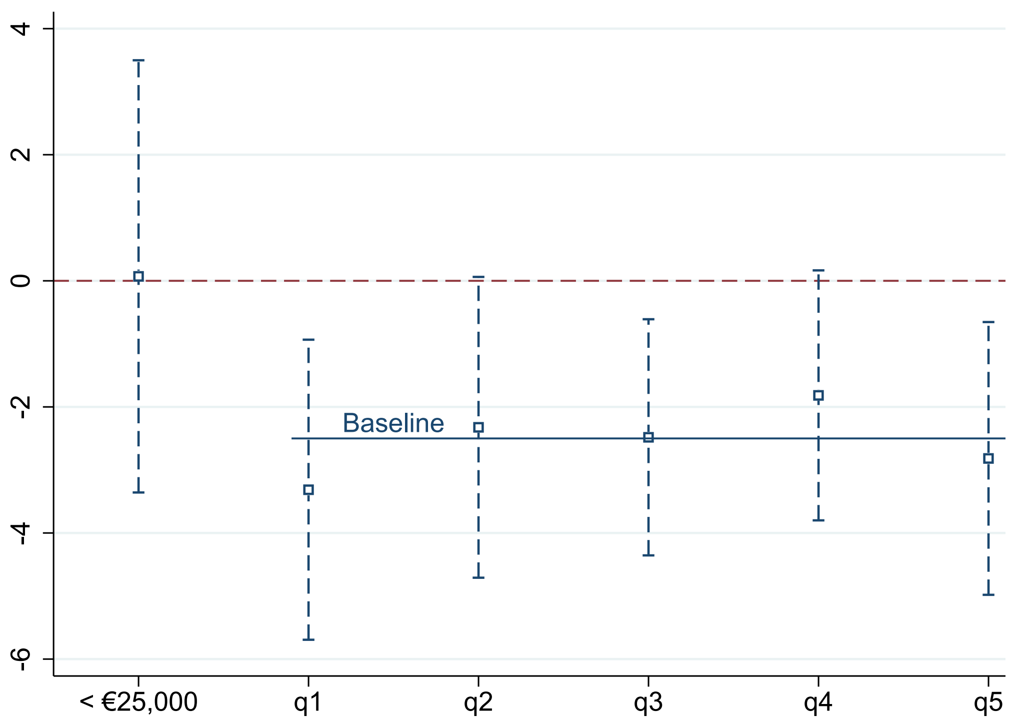
This figure shows kernel density estimates of the precrisis (2010Q1) distribution of bank exposure to firms with public procurement contracts in 2010.

Figure D.3: Credit from high and low exposure banks



This figure plots the evolution of credit for during the sample period for all banks in the sample (blue line), and for banks with above and below-median procurement exposure (red and green lines).

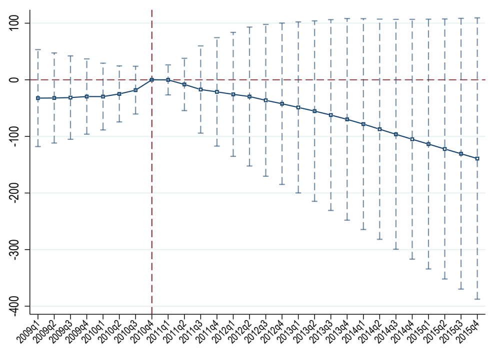
Figure D.4: Effect of procurement exposure on credit by relationship size



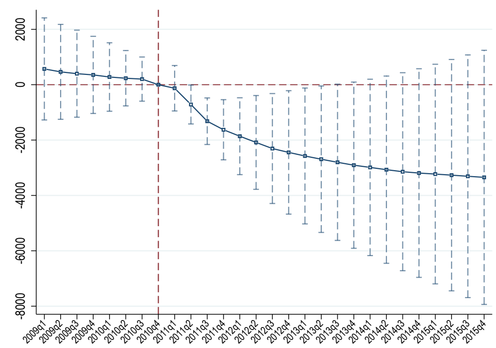
This figure shows point estimates and 95% confidence intervals for the effect of procurement exposure on credit supply at the bank-firm level as a function of loan size. The left-most point uses lending relationships under €25,000, which are excluded from our sample. The remaining points are obtained by splitting our regression sample by relationship size quintiles. The blue horizontal line corresponds to our baseline estimate, reported in column 1 of Table 3 in the paper. Standard errors are clustered at the bank level using the "LZ2" bias-reduction modification of Imbens and Kolesár (2016), and confidence intervals are calculated using a t-distribution with the degrees of freedom suggested by Bell and McCaffrey (2002).

Figure D.5: Weighted average contractor credit shares and bank-firm level credit

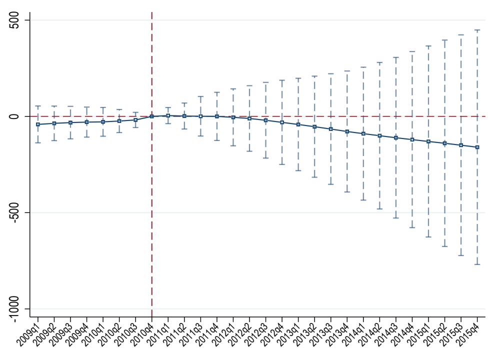
(a) Construction



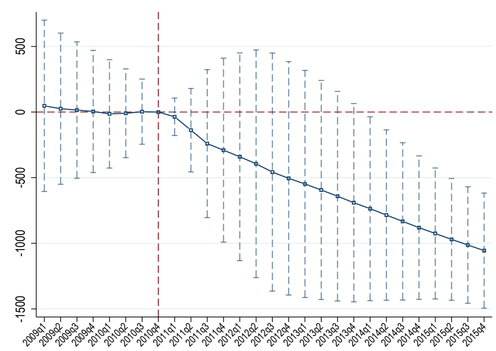
(b) Administrative services



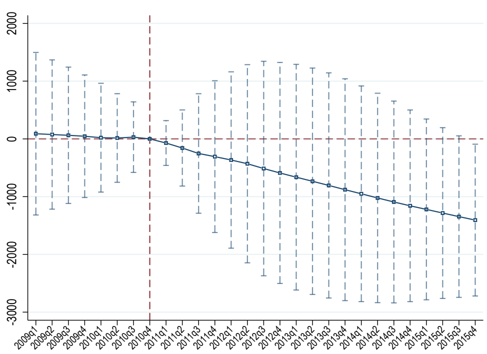
(c) Water and waste management



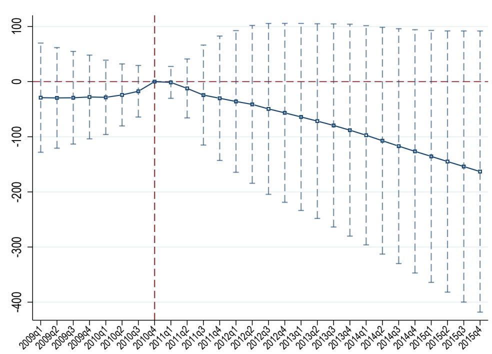
(d) Consulting



(e) Wholesale and retail trade



(f) All sectors



This figure plots point estimates and 95% confidence intervals from estimating equation (4) in the paper replacing procurement exposure with $\hat{\alpha}_i$-weighted average contractor credit shares by sector, where $\hat{\alpha}_i$ denote Rotemberg weights (Goldsmith-Pinkham, Sorkin and Swift, 2020). Standard errors are clustered at the bank level using the "LZ2" bias-reduction modification of Imbens and Kolesár (2016), and confidence intervals are calculated using a t-distribution with the degrees of freedom suggested by Bell and McCaffrey (2002).