Fiscal Policy and Credit Supply: The Procurement Channel*

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

July 31, 2024

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 persistent reduction in credit supply to other firms. We estimate a bank-level elasticity of credit supply with respect to procurement demand of 2.5. In a general equilibrium model, our findings point to large effects of fiscal policy on credit supply and output in a crisis.

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

*We thank Rui Albuquerque, Manuel Amador, Murillo Campello, Gabriel Chodorow-Reich, Hans Degryse, Tim Eisert, Miguel Faria-e-Castro, Nick Flamang, Felipe Iachan, Miguel H. Ferreira, Filipe Gomes, Helene Hall, Nicholas Kozeniauskas, Jean-Stéphane Mésonnier, Karsten Müller, Orkun Saka, Rui Silva, 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, 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, University of Venice, 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, Católica Lisbon and CEPR. 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 Cef.Up. 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, Drechsler and Schnabl, 2014; Gennaioli, Martin and Rossi, 2014). 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 19% of total corporate lending, 75% of total bank equity, and 90% of domestic sovereign debt 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 almost 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 cross-sectional bank-level estimate of the elasticity of credit supply with respect to aggregate demand shocks in general.

Our estimate for this elasticity is 2.5. Our identifying assumption 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 find 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, although our estimate of this interaction term is imprecise. We also estimate a smaller elasticity for banks recapitalized during the crisis.

Turning to the composition of bank credit portfolios, we find no evidence of heterogeneous effects as a function of firm credit risk, size, or age. Our results suggest that banks cut credit uniformly across the board.

Next, we evaluate whether firms were able to replace the credit lost from more exposed banks with credit from other banks. We estimate an elasticity of firm-level credit to the procurement exposure of the banks that the firm borrowed from of 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%). 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.

Using the same empirical design, we estimate an elasticity of firm value added to procurement exposure of 0.6. Dividing this coefficient by the firm-level credit 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.

We incorporate the mechanism we study into a general equilibrium model of the effect of demand shocks on credit supply, and we calibrate it to match our estimated bank-firm level elasticity. Our starting point is the framework developed by Herreño (2023), where firms borrow from multiple banks offering imperfectly substitutable credit. In our model, a decline in government spending causes firms to default on their loans with a probability proportional to the weight of government purchases in sales. The effect of loan losses on bank equity is amplified by leverage. And the effect of bank equity losses on credit supply depends on the ability of banks to offset those losses by increasing leverage, which is costly.

In general equilibrium, banks unexposed to loan defaults are affected via two channels with opposite effects. First, credit demand is reallocated towards unexposed banks, as their relative lending rate falls. Second, aggregate credit demand falls as lending rates from exposed banks rise. The reallocation effect dominates in our calibration, but the response of credit supply to the net change in credit demand depends on the elasticity of credit supply to lending rates, which is given by the inverse of the elasticity of bank funding costs to leverage.

If the elasticity of credit supply to lending rates is high, then lending is relatively insensitive to bank equity losses, and our micro estimates mostly reflect reallocation of credit across banks, with limited aggregate impact. If it is low, then credit supply is tightly linked to variations in bank net worth, and our regressions imply large declines in aggregate credit. We pin down this elasticity by targeting the elasticity of credit supply to bank net worth in the data, which we estimate using a 2SLS version of our baseline specification: we regress credit growth on bank equity growth at the bank-firm level, instrumenting equity growth with our procurement exposure measure. Our calibration yields a low elasticity of credit supply to lending rates of 0.5, which implies bank lending was severely constrained in our setting.

The calibrated model yields an aggregate elasticity of credit supply with respect to demand shocks of 2.1, and a credit-driven government spending multiplier of 0.9.[2] These estimates imply that the credit supply shock induced by the procurement cuts played a significant role in the prolonged recession that followed the crisis in Portugal, accounting for 83% of the drop in credit and 48% of the drop in output in the 2011-2015 period.

The aggregate effects of the shock increase linearly with the initial level of aggregate bank leverage, which multiplies the effect of loan losses on bank equity. Our calibration implies that this amplification is strong, with the aggregate credit supply elasticity increasing by 0.165 and the credit-driven multiplier by 0.071 per unit of leverage.

We conclude by gauging how the effects of the shock might vary outside a financial crisis like the one we study, in which credit spreads widened and banks faced intense pressure to deleverage. We focus on the role of the elasticity of credit supply to lending rates, which can be taken as a measure of credit market conditions in the model. We find this elasticity to be over ten times larger in a non-crisis setting. In a counterfactual using this higher elasticity, the aggregate effects we estimate fall to about 30% of their crisis values. In addition, we find that the initial level of bank leverage has a significantly weaker amplification effect outside a financial crisis.

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 feature two-way effects between credit supply and the real economy (Bernanke and Gertler, 1989; Gertler and Kiyotaki, 2010). 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 direction, from the real economy to credit supply, in the context of a financial crisis.[3] We also analyze the

[2]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.

[3]Another paper that studies a disruption in credit supply originating in the real sector is Federico, Hassan and Rappoport (2023), who examine the effect of bank exposure to firms in Italy that were affected by

aggregate implications of our findings in a general equilibrium model that incorporates the mechanism we study into the heterogeneous bank framework developed by Herreño (2023).

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 is driven by changes in private or public spending (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 demand shocks in general. This makes our cross-sectional elasticities useful in quantitatively disciplining macro models featuring interactions between aggregate demand and credit supply, including models of the credit channel of monetary policy (Bernanke, Gertler and Gilchrist, 1999; Gertler and Karadi, 2011).

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 Acharya, Drechsler and Schnabl (2014) and Gennaioli, Martin and Rossi (2014). 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 (Acharya et al., 2018; Gennaioli, Martin and Rossi, 2018), 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.[4]

Third, we add to the literature on fiscal multipliers. It is well known that multipliers increase in the presence of credit constraints, which makes current consumption more

increased competition from China, as a result of China's entry into the World Trade Organization.

[4] 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).

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 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 or, in other words, a bank credit channel of fiscal policy. 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.[5] 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; 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

[5] 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.

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 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.[6]

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)

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

database.[7] In addition, most EU countries also publish their own contract databases, 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).[8] 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.[9] 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

[7]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.

[8]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.

[9]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 8).

through the web portal Opentender.eu, taking care to avoid any duplication.

Our combined data set accounts for 44% of SNA-based public procurement expenditure in Portugal in 2010, well above the 22% average covered by TED in 2008 across EU countries (OECD, 2011). More importantly, Figure C.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 B.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 de Responsabilidades de Crédito* (Central Credit Register), 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).[10] 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 (i.e., 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.

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

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.[11,12] In Portugal, the government cut procurement 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.[13] In contrast, public procurement in Germany remained on a stable upward trend throughout the crisis.[14]

Table B.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

[11] Table B.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.

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

[13] 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.

[14] 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.

at the onset of the crisis, accounting for 19% of corporate lending (Table B.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.

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 23%, versus 10% for other firms, and this decline seems to have led to a substantial deterioration in their ability to repay their loans. Contractor NPLs grew nearly six-fold by 2015, while those for other firms only doubled.[15] 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 that suffered larger procurement cuts as a fraction of sales. We estimate an elasticity of contractor value added to procurement demand of 1.04 (the negative of the coefficient in column 1 of Table B.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 0.14 percentage points (column 2 of Table B.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

[15]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.

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 impacted 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, Drechsler and Schnabl, 2014; Gennaioli, Martin and Rossi, 2014; Brunnermeier et al., 2016).

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 is 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 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

$$g_{ib} = \beta Procurement\ Exposure_b + \gamma_{j(i)m(i)} + \lambda_1 X_b + \lambda_2 Z_i + \epsilon_{ib}. \tag{4}$$

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. $\gamma_{j(i)m(i)}$ denotes sector j by municipality m fixed effects, to control for credit demand. X_b and Z_i are sets of precrisis bank and firm controls measured in 2010Q1 and 2010, respectively. X_b includes bank exposure to domestic sovereign debt over equity, the log of total bank assets, and the ratio of bank equity to assets; 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 C.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 C.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.

Our estimates in Figure 5b mirror the steady increase in the aggregate stock of government contractor NPLs in Figure 4, in line with the mechanism we propose. NPLs took a long time to unwind after the sovereign debt crisis, and not just in Portugal (Aiyar et al., 2015). Beyond our setting, it has been documented that the level and persistence of unresolved NPLs is associated with the severity and duration of post-crisis recessions (Ari, Chen and Ratnovski, 2021). Consistent with a slow recovery of credit supply, data from Banco de Portugal's Bank Lending Survey shows that bank lending standards in the wake of the crisis continued to tighten until the end of 2012, and did not loosen significantly after that (see Figure C.4 in the online Appendix).

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 38% of the unconditional probability of relationship termination. 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.

[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 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 C.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 C.5 show that the same pattern holds for 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, as shown in Table 2.

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

[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 $\sum_j \frac{Credit_{b,j,-C,2010Q1}}{Credit_{b,2010Q1}} \times \frac{\overline{NPL}^{[2011Q1,2015Q4]}_{-b,j,-C} - \overline{NPL}^{[2009Q1,2010Q4]}_{-b,j,-C}}{\overline{Credit}^{[2009Q1,2010Q4]}_{-b,j,-C}}$.

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 B.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

[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 eight-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.

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.

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 (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 to our baseline regression an interaction between procurement exposure and the precrisis ratio of bank equity to assets. The sample consists of only 13 banks, so our estimates are necessarily imprecise. 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 a one percentage point increase in the equity-to-assets ratio.

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 equity-to-assets 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 bank leverage.

4.2.4 Heterogeneity

An important question is whether the procurement shock affected the composition of bank credit portfolios, namely whether procurement 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 firm 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.

We also observe no significant heterogeneity as a function of the size of the lending relationship (i.e., credit), with one exception (Figure C.6 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 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 their corresponding firm-level averages 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 (-2.222,-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. Column 2 of Table 8 replaces credit growth with an indicator for whether the firm survived until 2015. Our estimate for the firm-level effect of exposure along the extensive margin equals -0.400. Evaluated at the mean of exposure, this represents 23% of the unconditional probability of firm exit.

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 B.5 and B.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).

Figure 6a plots coefficients and 95% confidence intervals from estimating equation (4) for cumulative credit growth up to each quarter in [2009Q1, 2015Q4]. The rightmost point in the figure corresponds to our estimate for the overall effect, reported in column 1 of Table 8. 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 persistent decline in credit growth. The extensive margin effect accounts for part of this persistence. Still, the larger effect we estimate for single relationship firms also indicates that it may have been particularly difficult for firms to develop new banking relationships in this period. Indeed, most firms in our sample are informationally opaque small businesses, for whom relationship banking may be especially valuable in a crisis (Bolton et al., 2016).

Tables B.6 and B.7 in the online Appendix present firm-level tests analogous to those in Tables 3, 5 and B.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.

Column 3 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.

[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.

Tables B.8 and B.9 in the online Appendix replicate the robustness tests in Tables B.6 and B.7 for value added, and we again find that the results are robust. Columns 4 to 6 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 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

We investigate the aggregate implications of our findings by embedding the mechanism we study into a simple general equilibrium model of the effect of demand shocks on

[24]These elasticities should be interpreted with care since they may be driven by changes in loan 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).

[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).

credit supply. The setup is similar to the static model of credit supply shocks in Herreño (2023).[27] Firms borrow from multiple banks to finance their wage bill, and bank credit is imperfectly substitutable across banks. A cut to government spending causes some firms to default on their loans, depleting bank equity. Equity losses impact credit supply because increasing bank leverage is costly, as in Ulate (2021) and Abadi, Brunnermeier and Koby (2023).

The model yields simple closed-form expressions linking our cross-sectional credit regressions at the micro level to the effect of demand shocks on credit supply at the aggregate level, and to a credit-driven government spending multiplier.

6.1 Model setup

The economy is composed of a representative household, a government, a unit mass of firms, and a unit mass of banks.

Households The representative household chooses consumption C and labor L to maximize:

$$U(C, L) = C - \frac{L^{1+\phi}}{1+\phi}, \tag{5}$$

subject to the budget constraint $C = wL + \Pi - T$, where Π denotes firm and bank profits, to be described below, and T is a lump sum tax paid to the government. We rule out wealth effects on labor supply to isolate the effect of changes in government spending on credit supply operating through changes in demand, which is the channel we are interested in.

The final consumption good, which we take as the numeraire, is an aggregate of a unit

[27] Relative to Herreño (2023), we abstract from non-bank sources of funding. Firms in Portugal are heavily bank-dependent, with 88% of total firm funding being provided by banks on average in our sample, which suggests this is not a significant omission in our setting. We also assume a continuum of banks, which implies loan markups are homogeneous.

mass of differentiated varieties indexed by i, each produced by one firm:

$$C = \left(\int_0^1 c_i^{\frac{\sigma-1}{\sigma}} di \right)^{\frac{\sigma}{\sigma-1}}. \tag{6}$$

Labor L is an aggregate of labor supplied to individual firms, and w is the composite wage from an optimal allocation of labor:

$$L = \left(\int_0^1 l_i^{\frac{\alpha+1}{\alpha}} di \right)^{\frac{\alpha}{\alpha+1}}, \tag{7}$$

$$w = \left(\int_0^1 w_i^{\alpha+1} di \right)^{\frac{1}{\alpha+1}}, \tag{8}$$

where l_i denotes labor supplied to firm i and w_i the wage paid by firm i. A finite α introduces frictions in the reallocation of labor across firms.

Government The government sets T to fund an exogenous level of public consumption G, which consists of the same bundle of differentiated varieties as private consumption:

$$G = \left(\int g_i^{\frac{\sigma-1}{\sigma}} di \right)^{\frac{\sigma}{\sigma-1}}, \tag{9}$$

Firms Firms are monopolistic competitors facing demand $y_i = Y p_i^{-\sigma}$, where $Y = C + G$ and p_i is the price of good i. A firm with productivity z_i produces output $y_i = z_i l_i$. Firms must finance their wage bill by borrowing $w_i l_i$ from a unit mass of banks, and loans from different banks are imperfect substitutes. We assume firm credit demand q_i is a bundle of differentiated financial varieties indexed by b, each offered by a single bank:

$$q_i = \left(\int_0^1 \omega_{ib}^{\frac{1}{\theta}} q_{ib}^{\frac{\theta-1}{\theta}} db \right)^{\frac{\theta}{\theta-1}}, \tag{10}$$

where ω_{ib} represents idiosyncratic demand factors such as the strength of long-term lending relationships, and is independent of z_i. We normalize $\int_0^1 \omega_{ib} db = 1$ for all i and assume banks face the same overall idiosyncratic demand: $\int_0^1 \omega_{ib} di = 1$ for all b. Aggregate credit demand is given by $Q = \int_0^1 q_i di$.

Banks Banks also operate under monopolistic competition. Firm i's credit demand from bank b is given by

$$q_{ib} = q_i \omega_{ib} \left(\frac{R_b}{R_i} \right)^{-\theta}, \tag{11}$$

where R_b in the gross lending rate charged by bank b, and R_i is the composite rate paid by firm i when minimizing interest costs weighted by idiosyncratic credit demand:

$$R_i = \left(\int_0^1 \omega_{ib} R_b^{1-\theta} db \right)^{\frac{1}{1-\theta}}, \tag{12}$$

Banks finance their lending by combining exogenous equity e_b with funding obtained outside the economy at an exogenous rate that we set to 1. We denote a bank's leverage ratio by v_b, such that the bank's credit supply equals $v_b e_b$. To introduce a role for equity as a constraint on credit supply, we assume that banks incur a cost ${v_b}^{\frac{1}{\eta}}$ per unit of lending, where $\frac{1}{\eta} > 0$, and that e_b is small enough that $v_b > 1$ for all b in equilibrium.[28] Banks maximize profits by lending at a markup over this cost:

$$R_b = \frac{\theta}{\theta - 1} {v_b}^{\frac{1}{\eta}}, \tag{13}$$

and use leverage to equate credit supply and demand:

$$v_b e_b = q_b, \tag{14}$$

[28]We assume these leverage costs are rebated back to households as part of Π, so the aggregate resource constraint is unaffected.

where $q_b \equiv \int_0^1 q_{ib} di$.

The model thus features an external finance premium for banks given by $v_b^{\frac{1}{\eta}}$, which is reflected in firm borrowing costs. Several macro-finance models feature leverage costs as a proxy for the type of bank leverage constraints studied by Gertler and Kiyotaki (2010) and Gertler and Karadi (2011). Recent examples include Ulate (2021), Abadi, Brunnermeier and Koby (2023) and Eggertsson et al. (2024). The log-linear formulation we adopt yields simple closed-form expressions for the effects of the shock we study.

Since credit supply expands via leverage and banks pass on leverage costs to borrowers, the elasticity of credit supply to lending rates is given by η, the inverse of the elasticity of the external finance premium to leverage. This elasticity can be seen as a measure of credit market conditions. When η is high, bank leverage is cheap and lending is mostly demand-driven. When it is low, banks deleverage and lending becomes more sensitive to bank net worth. Financial crises, which are typically characterized by sharp increases in credit spreads and declines in bank leverage (Bernanke, 1983; Krishnamurthy and Muir, 2017), can be interpreted in the model as periods when η is significantly lower than in normal times.

6.2 Effect of a government spending cut

To link the model to our empirical work in a simple manner, we assume that when a firm suffers a negative demand shock equal to a fraction s of its sales, it defaults on its loans with probability ρs. Defaulted loans are fully written off by banks. In an extended model, these loan defaults might be driven by asset fire sales or costs associated with finding alternative customers, for example. Defaulting firms exit and are immediately replaced by new firms, but credit losses deplete bank equity.

We study the case where the government cuts spending G by a fraction u of aggregate demand. Along with the spending cut, the government makes a set of lump sum transfers $\tau_i u$ to firms, where $\tau_i \in [-1, 1]$, such that firm i's probability of default after the shock equals $\rho u(1 - \tau_i)$. We impose $\int_0^1 \tau_i di = 0$ and allow τ_i to be correlated with ω_{ib}. These transfers introduce variation in firm and hence bank exposure to the shock, which we require to interpret our regressions through the model, without having the government and households consume different bundles of goods, which preserves tractability. They might, for example, reflect lobbying efforts by some firms to get compensated for the cut through subsidies, at the expense of other firms.

In the absence of defaults, a cut to G would have no effect: it would be offset by an increase in C, leaving aggregate credit and output unchanged. With defaults, the cut causes bank b to experience equity losses of $\int_0^1 q_{ib}\rho u(1 - \tau_i)di$. This implies that:

$$\frac{de_b}{e_b} = -\rho v_b u_b, \tag{15}$$

where $u_b \equiv u \int_0^1 \frac{q_{ib}}{q_b}(1 - \tau_i)di$ is the model counterpart of our empirical measure of bank exposure to procurement cuts defined in equation 1.

We assume that all banks start from a common equity value $\bar{e}$, and hence a common leverage ratio $\bar{v}$. The following propositions, proved in section A of the online Appendix, summarize our key results.

Proposition 1 *The model analog of the coefficient on procurement exposure in our bank-firm level credit regression is given by:*

$$\beta = -\rho\bar{v}\frac{\theta}{\theta + \eta}. \tag{16}$$

Proposition 1 characterizes the effect of the shock on the credit supply of exposed banks relative to unexposed banks. The effect is homogeneous across firms, in line with the evidence in Table 7), and is given by the product of three terms. The probability of default ρ determines the effect of demand shocks on loans losses. Initial leverage $\bar{v}$ amplifies the effect of loan losses on bank equity, which matches our findings in Table 6. And $\frac{\theta}{\theta+\eta}$ governs how credit supply responds to equity losses, which depends on the relative magnitude of the elasticities of credit supply and demand to lending rates. The smaller η is relative to θ, the more costly it is for exposed banks to offset equity losses through higher leverage, and the stronger the reallocation of credit towards unexposed banks, strengthening the cross-sectional effect.

In general equilibrium, there are two effects on the credit supply of unexposed banks. First, unexposed banks benefit from the reallocation of credit just mentioned as their relative lending rate falls. Second, aggregate credit demand falls as loan rates from exposed banks rise. The net effect of these two opposing channels leads unexposed banks to adjust their credit supply accordingly. The link between β and the effect of the shock on aggregate credit in general equilibrium is summarized in Proposition 2.

Proposition 2 *The elasticity of credit supply with respect to demand shocks, $\varepsilon^Q \equiv -\frac{d \log Q}{du}$, is given by:*

$$\varepsilon^Q = -\frac{\psi}{\theta}\frac{\theta+\eta}{\psi+\eta}\beta, \tag{17}$$

where $\psi \equiv \frac{1}{\phi}+1$ is the negative of the elasticity of aggregate credit demand to lending rates.

The strength of the reallocation and aggregate credit demand channels is governed by the elasticities of credit demand to lending rates at the bank and aggregate level, respectively. If $\theta > \psi$ the reallocation channel dominates, otherwise the aggregate credit demand channel does. Our estimates of θ, discussed below, and the micro evidence on the Frisch elasticity $\frac{1}{\phi}$ (Chetty et al., 2011) suggest that reallocation dominates. This implies that the aggregate effect of the shock on credit is dampened in general equilibrium. But the extent of dampening depends on η. If η is high, β mainly reflects reallocation of credit, and the aggregate impact of the shock is limited. As η falls, reallocation is muted by the increasing sensitivity of bank funding costs to leverage, and both β and ε^Q converge to the equity loss suffered by banks.

The effect of the cut on aggregate output can be interpreted as a credit-driven government spending multiplier, since $\varepsilon^Y \equiv -\frac{d\log Y}{du} = \frac{d\log Y}{\frac{dG}{Y}} = \frac{dY}{dG}$. This leads to our third and final result:

Proposition 3 *The credit-driven fiscal multiplier associated with a government spending cut,* $\varepsilon^Y \equiv -\frac{d\log Y}{du}$*, is:*

$$\varepsilon^Y = \frac{1}{1+\phi}\varepsilon^Q. \tag{18}$$

One implication of these results worth highlighting is that, given an estimate of β, the aggregate effects we derive are fully determined by the credit supply and demand elasticities η, θ and ψ. Namely, the effects are independent of the elasticities of substitution in the product and labor markets, σ and α. These elasticities govern the extent of reallocation of credit and output across firms as a result of the shock, with no first-order impact on aggregate outcomes.

6.3 Calibration

Baseline The cross-sectional and aggregate effects of the shock in the model are a function of five parameters: ρ, $\bar{v}$, η, θ and ϕ. Table 9 summarizes our calibration of these parameters.

We set the Frisch elasticity $\frac{1}{\phi}$ equal to 0.75, following Herreño (2023) and the micro evidence discussed in Chetty et al. (2011). For the initial level of bank leverage $\bar{v}$, we use the aggregate leverage ratio of the banks in our sample in 2010Q4, which equals 12.860.

We estimate θ using a data set on new credit operations managed by Banco de Portugal, which includes loan dates, volumes, interest rates, maturity and collateral for all new loans granted by banks from 2013 onwards. Using equation (11), we run the following regression:

$$\log Credit_l = \chi_{itc} + \mu_{bt} + \xi_{ib} - \theta \log R_l, \tag{19}$$

where l indexes loans, R_l is the loan's gross interest rate, χ_{itc} are firm-by-year-by-loan-characteristics fixed effects, μ_{bt} are bank-by-year fixed effects and ξ_{ib} are bank-firm fixed effects. We restrict the sample to the 2013-2015 period. Table B.10 in the online Appendix presents the results. We set $\theta = 4.550$ in our calibration, the value reported in column 4. This is our most restrictive specification, where we define loan characteristics by interacting ten loan maturity bins with dummies for whether the loan has a fixed rate and for whether it is collateralized.

To set η, we start by noting that the elasticity of credit supply with respect to bank equity at the bank-firm level equals $\frac{\theta}{\theta+\eta}$, as equation (43) in the online Appendix shows. We estimate this elasticity using a 2SLS version of equation (4): we regress log cumulative growth in credit on log cumulative growth in bank equity instrumented by procurement exposure, using the same sample and set of controls.[29] We obtain an elasticity of 0.902, reported in Table B.11 of the online Appendix, which shows banks were largely unable to

[29]We exclude foreign branches from this regression; these are not independent legal entities and their equity values have little economic meaning. For one bank, equity in 2010Q4 was depressed by a set of large provisions made in 2010 and reversed in 2011, as part of a restructuring process. We use the pre-restructuring value of equity in the denominator of log cumulative equity growth for this bank. We obtain nearly identical results when we exclude this bank.

offset the effect of equity losses on credit supply. Using our estimate for θ, this implies $\eta = 0.492$.

Finally, we choose ρ so that β in equation (16) matches our baseline estimate from column 1 of Table 3, given our calibration of the remaining parameters. We obtain $\rho = 0.212$. The coefficient of 0.143 in column 2 of Table B.4 can be interpreted as a direct estimate of ρ, with the caveats that banks recover part of their NPLs, and that NPL ratios exclude written-off loans. Reassuringly, the two values are not far apart. We see this as a useful sanity check for our calibration.

Alternative using firm-level credit regression As a robustness check, we also use the coefficient in our firm-level credit regression, reported in column 1 of Table 8, to back out implied values of θ and η. As we show in section A.5 of the online Appendix, $\theta = \frac{\beta}{\beta_{firm}} \sigma \frac{\alpha+1}{\alpha+\sigma}$, where β_{firm} is the model counterpart of the coefficient in our firm-level credit regression. Following Herreño (2023), we set $\sigma = 4$ and we consider two values of α: a rigid labor market with $\alpha = 1$, and a flexible labor market with $\alpha = 1000$. The former, which is likely to be the relevant case for Portugal (Blanchard and Portugal, 2001; OECD, 2013), yields $\theta = 2.751$, while the latter gives $\theta = 6.856$. Combined with $\frac{\theta}{\theta+\eta} = 0.902$, these values of θ imply $\eta = 0.297$ and $\eta = 0.741$, respectively. We examine the sensitivity of the results to these alternative parameters below.

6.4 Results

6.4.1 Effect of the cut in our setting

Our results on ε^Q and ε^Y are summarized in Table 10. Our baseline calibration yields $\varepsilon^Q = 2.128$, with a 95% confidence interval of (0.351,3.905),[30] and $\varepsilon^Y = 0.912$ with a 95% confidence interval of (0.150,1.673). We note that ε^Y should not be interpreted as the total multiplier associated with the procurement cuts, only the effect operating through bank credit supply, since our model abstracts from other mechanisms.

Multiplying our estimates of ε^Q and ε^Y by the size of the shock u, we get a first-order approximation to the effect of the cut on credit and output. We find that the shock led to a contraction of 18.0% in credit, which amounts to 84% of the average drop in real corporate credit in Portugal in the 2011-2015 period relative to 2010Q4, and to a drop of 7.7% in output, which represents 49% of the average drop in real corporate value added in the same period relative to 2010.

We should emphasize that the model and these results abstract from the role of nominal rigidities and monetary policy. Away from the zero lower bound, monetary policy might dampen the effect of the shock. At the zero lower bound, household consumption may also fall with a cut to government spending (Christiano, Eichenbaum and Rebelo, 2011), leading to additional loan defaults and amplifying the effect of the shock. Even if the zero lower bound is not binding, interest rate cuts in the low interest rate environment that characterized this period become less effective at stimulating lending, and may even become contractionary (Abadi, Brunnermeier and Koby, 2023). On the other hand, Gertler and Karadi (2011) show that unconventional monetary policy, in the form of an expansion of central bank intermediation such as that implemented by the ECB,

[30] We calculate this confidence interval using the confidence interval for β, taking the remaining parameters as given.

can attenuate the effects of a shock to bank net worth like the one we study, and that this type of policy can be particularly effective at the zero lower bound.

We also do not account for supply-side spillovers, such as those studied by Huber (2018, 2023) and by Andersen et al. (2022), which might also amplify the effect of the shock. Our results should thus be seen as illustrative of the general equilibrium effects featured in the model, rather than a full evaluation of the shock's impact.

Our estimates do not change much when we use the alternative values of θ and η we infer from the firm-level credit regression, as shown in columns 2 and 3 of Table 10. With $\theta = 2.751$ and $\eta = 0.297$, the values implied by setting $\alpha = 1$, we obtain $\varepsilon^Q = 2.330$ and $\varepsilon^Y = 0.999$. And with $\theta = 6.856$ and $\eta = 0.741$, which we get by setting $\alpha = 1000$, $\varepsilon^Q = 1.915$ and $\varepsilon^Y = 0.821$. Combining equations (16) and (17) shows that ε^Q and hence ε^Y do not depend on θ. Higher reallocation increases the effect of the shock on exposed banks relative to unexposed banks, but does not affect aggregate credit. θ only affects ε^Q and ε^Y by pinning down η, in conjunction with the elasticity of credit supply to bank equity. And for η in the range implied by these alternative values of θ, the results remain relatively stable.

6.4.2 Amplification through initial leverage

As equations (16), (17) and (18) show, β, ε^Q and ε^Y increase linearly with the initial level of bank leverage $\bar{v}$, with slopes given by $\frac{d\beta}{d\bar{v}} = \frac{\theta}{\theta+\eta}\rho$, $\frac{d\varepsilon^Q}{d\bar{v}} = \frac{\psi}{\psi+\eta}\rho$ and $\frac{d\varepsilon^Y}{d\bar{v}} = \frac{1}{1+\phi}\frac{d\varepsilon^Q}{d\bar{v}}$.

In our baseline calibration, we get $\frac{d\beta}{d\bar{v}} = 0.191$, $\frac{d\varepsilon^Q}{d\bar{v}} = 0.165$ and $\frac{d\varepsilon^Y}{d\bar{v}} = 0.071$, which implies that the initial level of bank leverage plays a significant amplification role. We illustrate this in columns 4 and 5 of Table 10, which present counterfactual estimates of ε^Q and ε^Y when initial leverage is lower ($\bar{v} = 8$) and higher ($\bar{v} = 20$) than in our setting, where $\bar{v} = 12.860$. In the low leverage case, we obtain $\varepsilon^Q = 1.324$ and $\varepsilon^Y = 0.567$, about two thirds of our baseline estimates. With high leverage, we get $\varepsilon^Q = 3.309$ and $\varepsilon^Y = 1.418$, over 50% larger than our baseline.

6.4.3 Effect outside a financial crisis

Our results characterize the effects of government spending cuts in the context of a financial crisis, when banks were under strong pressure to deleverage. Bank equity losses in this environment are likely to have a stronger impact on lending than in normal times. In our model, as discussed above, a crisis environment where credit is tight can be interpreted as a low value of η. This implies that the value of η we infer in our setting may be substantially lower than in normal times.

To gauge what the value of η might be outside a crisis, we rely on the elasticity of bank credit with respect to bank equity estimated by Ulate (2021) using a sample of over 5,000 banks from 19 countries between 1990 and 2017. His estimate, reported in online Appendix Section B.7 of his paper, equals 0.4549. Recalling that in our model this elasticity equals $\frac{\theta}{\theta+\eta}$, and using our baseline estimate of $\theta = 4.550$, we obtain $\eta = 5.452$, which is over ten times larger than the elasticity we estimate in the crisis.

Setting $\eta = 5.452$ yields $\varepsilon^Q = 0.662$ and $\varepsilon^Y = 0.284$ (column 6 of Table 10), about 30% of our crisis estimates. This suggests that outside a financial crisis the elasticity of credit supply to demand shocks and the credit-driven fiscal multiplier are strongly diminished.

In addition, the amplification effect of initial leverage is also significantly weaker outside a crisis. With $\eta = 5.452$, we get $\frac{d\beta}{d\bar{v}} = 0.096$, $\frac{d\varepsilon^Q}{d\bar{v}} = 0.052$ and $\frac{d\varepsilon^Y}{d\bar{v}} = 0.022$. Columns 7 and 8 of Table 10 present counterfactuals for ε^Q and ε^Y using $\bar{v} = 8$ and $\bar{v} = 20$ when $\eta = 5.452$. With $\bar{v} = 8$, we get $\varepsilon^Q = 0.412$ and $\varepsilon^Y = 0.177$, while $\bar{v} = 20$ yields $\varepsilon^Q = 1.030$ and $\varepsilon^Y = 0.442$. Even in the high leverage counterfactual, the aggregate elasticities are less than half than in a crisis.

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, our findings provide a cross-sectional bank-level estimate of the elasticity of credit supply with respect to aggregate demand shocks in general.

We find that the procurement cuts implemented in Portugal in the wake of the European sovereign debt crisis had a long-lasting effect on credit supply at the bank level. In a simple general equilibrium model, our cross-sectional estimates imply a large decline in aggregate credit supply, which can explain a sizable fraction of the protracted decline in aggregate credit and output in this period.

Our findings raise several questions. First, we focus on the effect of procurement cuts on banks, but there may be an effect in the opposite direction. A deterioration in bank health may increase the likelihood of bank bailouts and lower tax revenue by depressing real activity, putting pressure on financially constrained governments to cut spending. The fact that the Irish government cut procurement spending in 2009, after bailing out the banking sector but before the sovereign debt crisis, is consistent with that possibility.

Second, given the large and persistent effects of the shock, it is unclear what prevents the bank debt overhang from being resolved faster. Mian, Sufi and Trebbi (2014) suggest that political economy plays a role. We think exploring this issue further is an interesting direction for future research.

Finally, our model abstracts from a number of important factors, namely the role of conventional and unconventional monetary policy, how the mechanism we study might interact with other channels of fiscal policy, and how the shock might feed back into consumption through a reduction in credit supply to households. Incorporating these channels into a richer model would deliver a more accurate quantitative evaluation of the effects of the mechanism we study.

References

Abadi, Joseph, Markus Brunnermeier, and Yann Koby. 2023. "The Reversal Interest Rate." *American Economic Review,* 113(8): 2084–2120.

Acharya, Viral, Itamar Drechsler, 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.

Aiyar, Shekhar, Wolfgang Bergthaler, Jose M. Garrido, Anna Ilyina, Andreas Jobst, Kenneth Kang, Dmitriy Kovtun, Yan Liu, Dermot Monaghan, and Marina Moretti. 2015. *A Strategy for Resolving Europe's Problem Loans.* International Monetary Fund.

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

Andersen, Asger, Kilian Huber, Niels Johannesen, Ludwig Straub, and Emil Toft Vestergaard. 2022. "Disaggregated Economic Accounts." National Bureau of Economic Research Working Paper 30360.

Ari, Anil, Sophia Chen, and Lev Ratnovski. 2021. "The Dynamics of Non-Performing Loans during Banking Crises: A New Database with Post-COVID-19 Implications." *Journal of Banking & Finance,* 133: 106140.

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." November, 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.

Bernanke, Ben. 1983. "Nonmonetary Effects of the Financial Crisis in the Propagation of the Great Depression." *American Economic Review,* 73(3): 257–276.

Bernanke, Ben, and Mark Gertler. 1989. "Agency Costs, Net Worth, and Business Fluctuations." *American Economic Review,* 79(1): 14–31.

Bernanke, Ben, Mark Gertler, and Simon Gilchrist. 1999. "Chapter 21 The Financial Accelerator in a Quantitative Business Cycle Framework." In *Handbook of Macroeconomics.* Vol. 1, 1341–1393. Elsevier.

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.

Bolton, Patrick, Xavier Freixas, Leonardo Gambacorta, and Paolo Emilio Mistrulli. 2016. "Relationship and Transaction Lending in a Crisis." *Review of Financial Studies,* 29(10): 2643–2676.

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.

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.

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 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.

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.

Eggertsson, Gauti B, Ragnar E Juelsrud, Lawrence H Summers, and Ella Getz Wold. 2024. “Negative Nominal Interest Rates and the Bank Lending Channel.” *Review of Economic Studies*, 91(4): 2201–2275.

Federico, Stefano, Fadi Hassan, and Veronica Rappoport. 2023. “Trade Shocks and Credit Reallocation.”

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.

Krishnamurthy, Arvind, and Tyler Muir. 2017. "How Credit Cycles across a Financial Crisis." National Bureau of Economic Research Working Paper 23850.

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.

Mian, Atif, Amir Sufi, and Francesco Trebbi. 2014. "Resolving Debt Overhang: Political Constraints in the Aftermath of Financial Crises." *American Economic Journal: Macroeconomics,* 6(2): 1–28.

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

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.

Ulate, Mauricio. 2021. "Going Negative at the Zero Lower Bound: The Effects of Negative Nominal Interest Rates." *American Economic Review*, 111(1): 1–40.

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
Equity-to-Assets	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
Total 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
Equity-to-Assets	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)
Equity-to-Assets	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)
Construction Exposure	0.156	0.227	-0.071	(0.033)
Predicted Growth in Other NPLs	0.084	0.079	0.005	(0.008)
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 reports 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 the equity-to-assets ratio 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 above €25,000 in 2010Q4 that existed in 2009 and 2010. 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

	$\hat{\alpha}_j$ (1)	Top 5% share (2)	$\hat{\beta}_j$ (3)
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). Column 2 displays the fraction of weights within each sector accounted for by the top 5% of contractors by weight. Column 3 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 the equity-to-assets ratio 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 above €25,000 in 2010Q4 that existed in 2009 and 2010. 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

	Equity-to-assets (1)	Recapitalizations (2)	Equity-to-assets with public injections (3)	Equity-to-assets with all injections (4)
Procurement Exposure	-4.276	-4.515	-4.668	-4.716
	(0.990)	(0.852)	(0.819)	(0.660)
Equity-to-Assets	-0.366		-0.403	-0.439
	(0.274)		(0.300)	(0.259)
Procurement Exposure $\times$ Equity-to-Assets	28.180		33.843	34.640
	(18.643)		(18.537)	(15.157)
Recapitalized		-0.028		
		(0.006)		
Procurement Exposure $\times$ 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 the equity-to-assets ratio 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, recapitalized is an indicator for whether a bank was recapitalized in the 2010-2013 period. In column 3, the equity-to-assets ratio includes the capital injections banks received from the government in 2010-2013. In column 4, the equity-to-assets ratio 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 above €25,000 in 2010Q4 that existed in 2009 and 2010. 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 the equity-to-assets ratio 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 above €25,000 in 2010Q4 that existed in 2009 and 2010. 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)	Survival (2)	Value added (3)	Sales (4)	Assets (5)	Employment (6)
Procurement Exposure	-1.431	-0.400	-0.563	-0.623	-0.317	-0.410
	(0.293)	(0.094)	(0.148)	(0.218)	(0.075)	(0.134)
BM degrees of freedom	4.3	4.3	4.3	4.3	4.3	4.3
Observations	50,346	50,346	50,346	50,346	50,346	50,346
Adjusted R^2	0.087	0.108	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), except in column 2, where it is an indicator for whether the firm survived until 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 the equity-to-assets ratio 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: Calibration

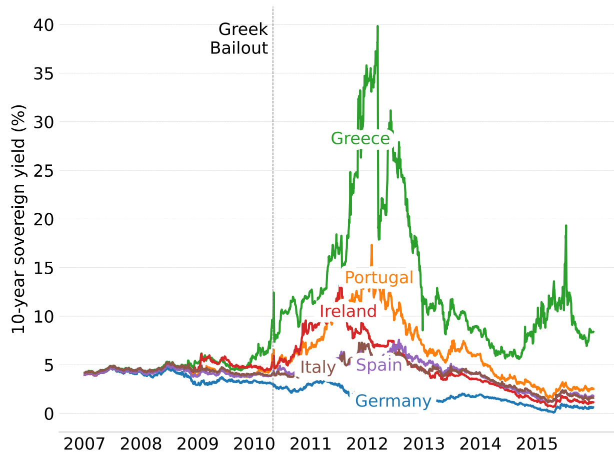
Parameter	Description	Value	Source/Target
$\frac{1}{\phi}$	Frisch elasticity	0.750	Herreño (2023), based on Chetty et al. (2011)
$\bar{v}$	Initial bank leverage	12.860	Aggregate bank leverage ratio in 2010Q4
θ	Elasticity of substitution across banks	4.550	Elasticity of credit demand to loan rates (Column 4 of Table B.10 in online Appendix)
η	Elasticity of credit supply to loan rates	0.492	Elasticity of credit supply to bank net worth (Column 2 of Table B.11 in online Appendix)
ρ	Probability of default	0.212	Elasticity of credit supply to procurement demand (Column 1 of Table 3)

Table 10: Aggregate effects of the shock in the model

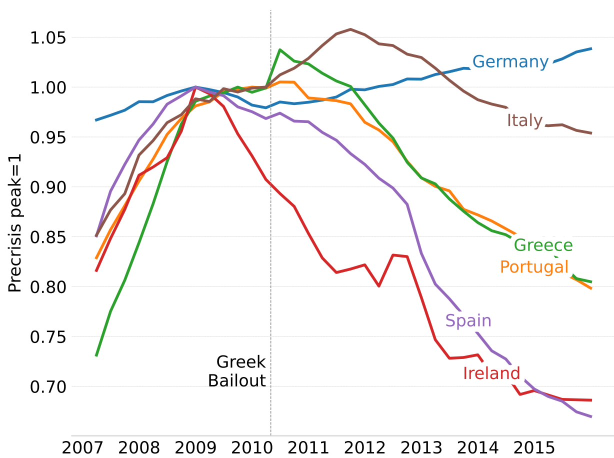
	Calibrations			Counterfactuals				
		Using β_{firm}				No crisis ($\eta = 5.452$)		
	Baseline	$\alpha = 1$	$\alpha = 1000$	$\bar{v} = 8$	$\bar{v} = 20$	Baseline $\bar{v}$	$\bar{v} = 8$	$\bar{v} = 20$
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
ε^Q	2.128	2.330	1.915	1.324	3.309	0.662	0.412	1.030
ε^Y	0.912	0.999	0.821	0.567	1.418	0.284	0.177	0.442

Column 1 presents results from our baseline calibration. Columns 2 and 3 correspond to the alternative calibrations using the firm-level credit regression. The remaining columns report results for counterfactuals. In columns 4 and 5, we vary the level of initial bank leverage $\bar{v}$. In columns 6, 7 and 8, we use the elasticity of credit supply to lending rates η estimated in a non-crisis context, along with variation in $\bar{v}$.

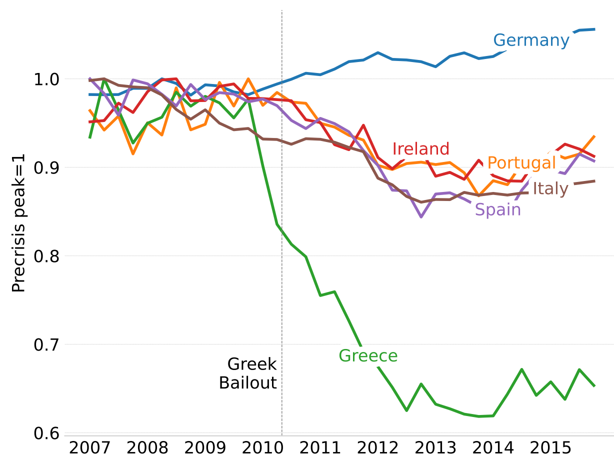
Figure 1: The European sovereign debt crisis



(a) Sovereign yields



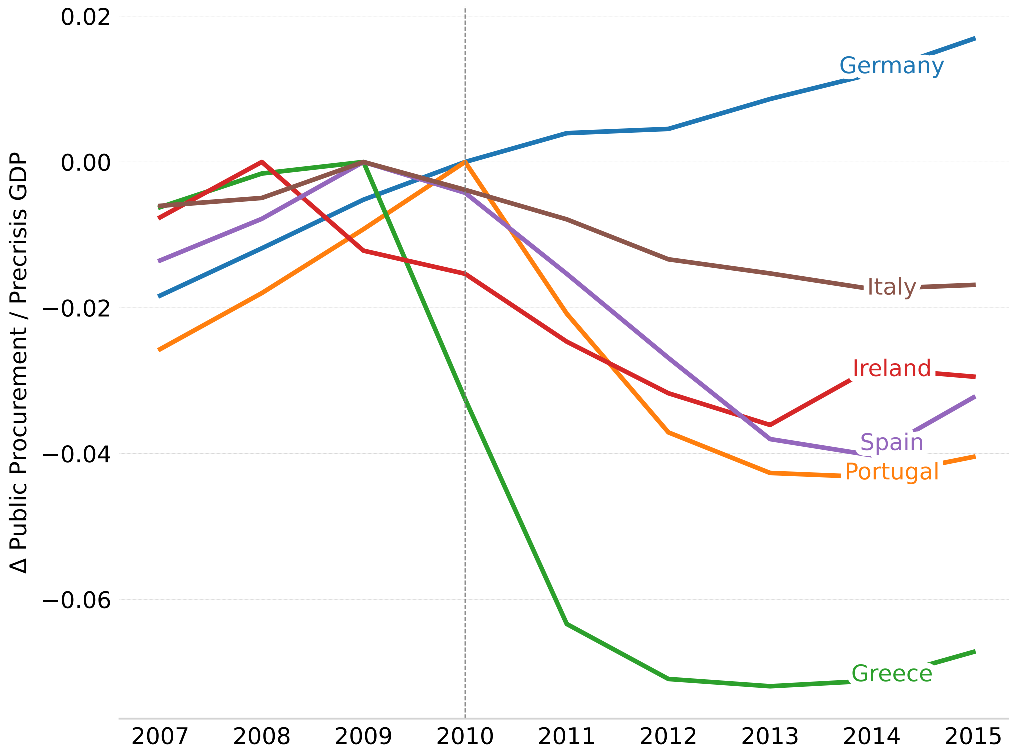
(b) Credit to private non-financial sector



(c) Household disposable income

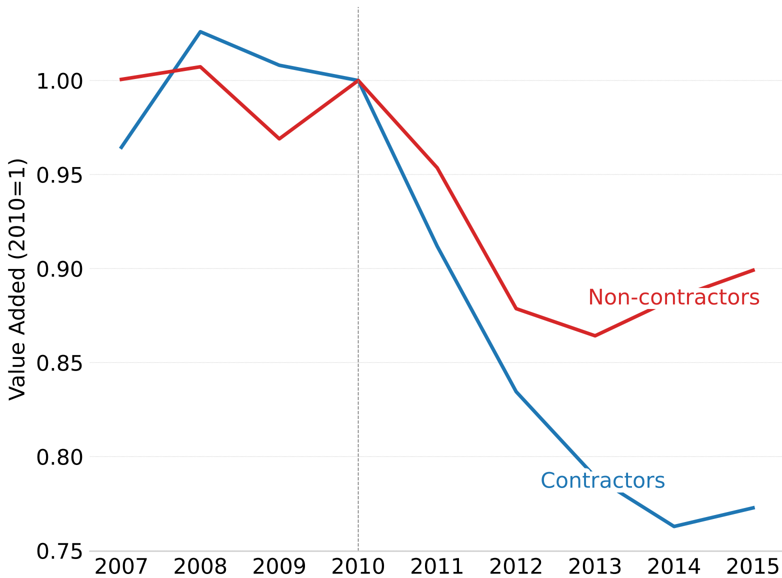
This figure plots the evolution of 10-year sovereign yields, credit to the domestic private non-financial sector, and real household disposable income per capita for the set of countries at the center of the European sovereign debt crisis and for Germany. Sovereign yield data are from Refinitiv. Credit 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

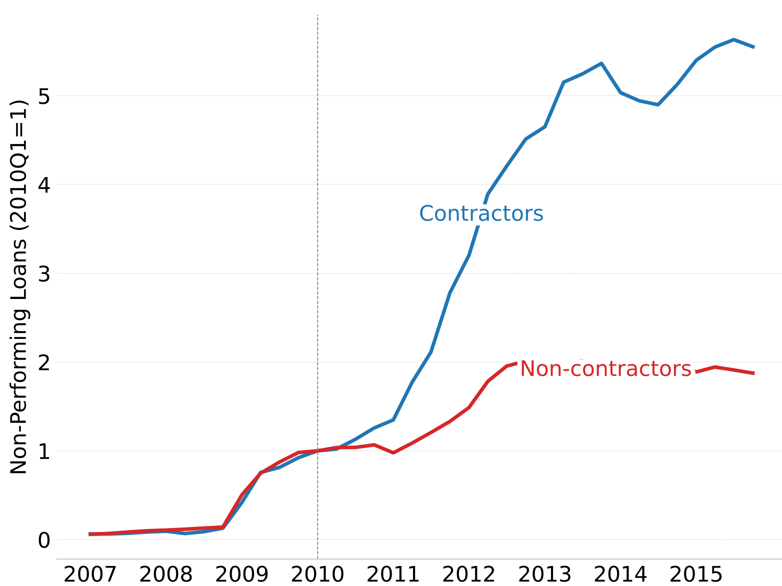


This figure plots the change in real public procurement spending relative to its precrisis peak, as a fraction of precrisis GDP, for crisis-hit countries and Germany. Data are from the OECD.

Figure 3: Impact of procurement cuts on firms



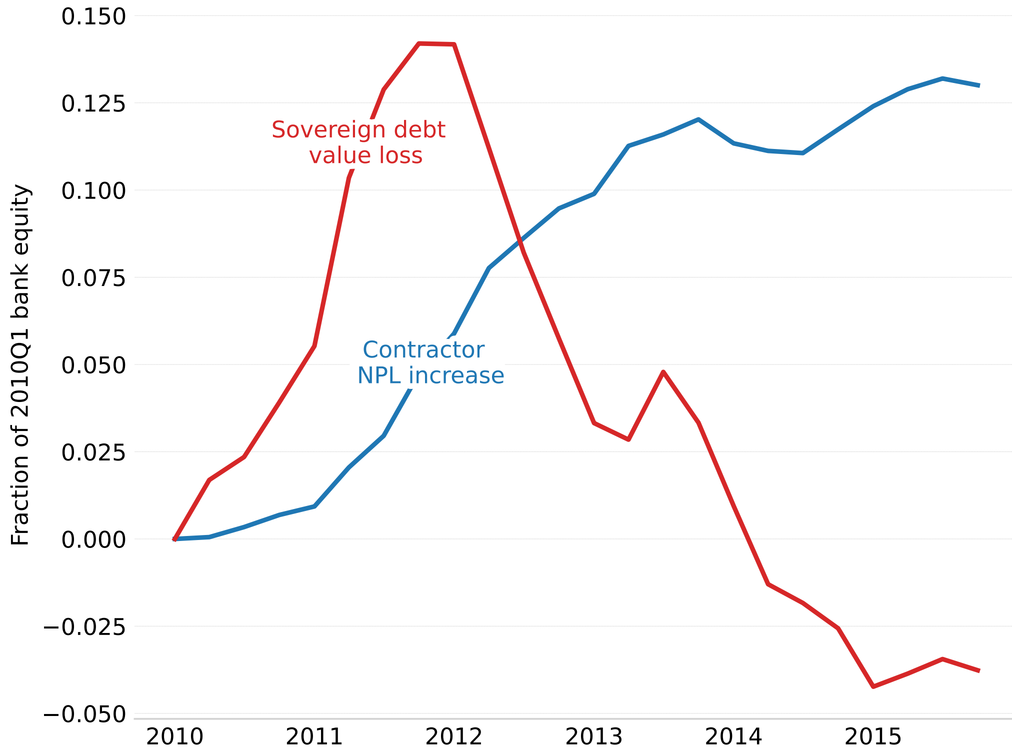
(a) Value added



(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 2009-2010 (contractors) versus firms without such contracts in 2009-2010 (non-contractors). When a firm supplies more than one product or service, we use the average cut weighted by firm-level contract amounts in 2010.

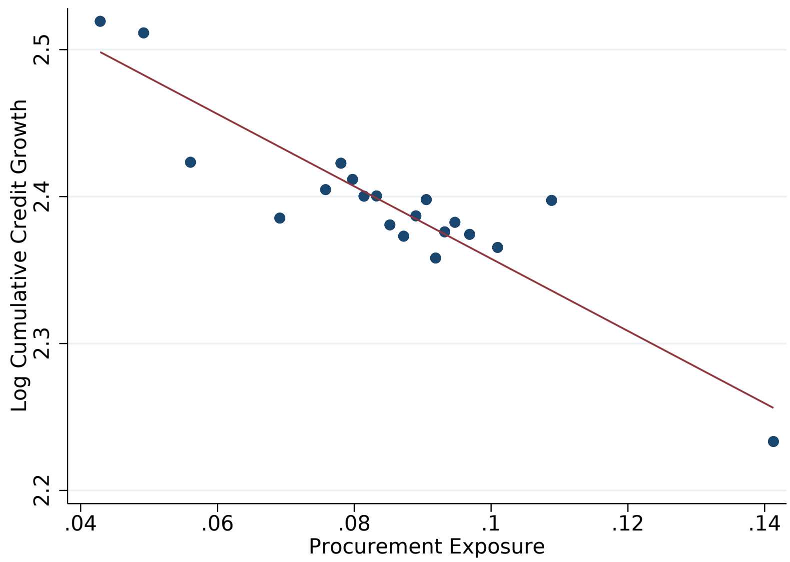
Figure 4: Impact of the procurement and sovereign debt shocks on NPLs



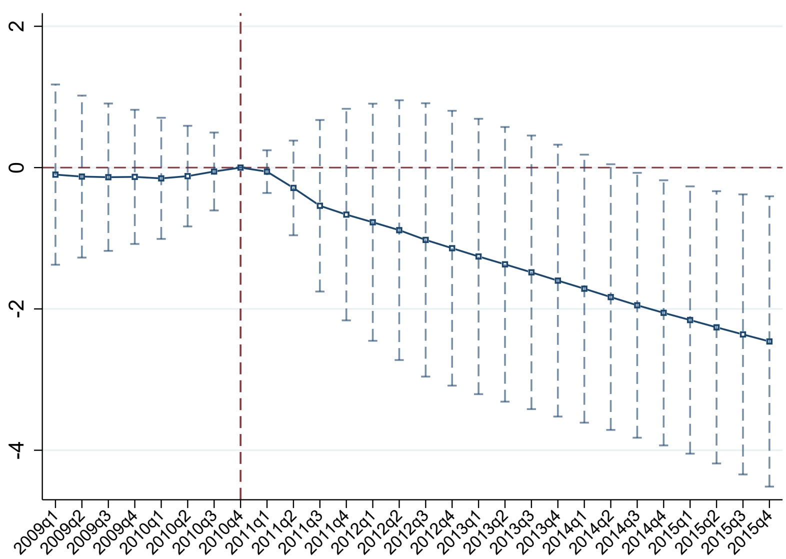
This figure plots the increase in NPLs from firms with public procurement contracts in 2009-2010 (contractors), along with the loss in the market value of bank domestic sovereign debt holdings, in the period between 2010Q1 and 2015Q4. Both series are plotted as a fraction of total bank equity in 2010Q1. 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



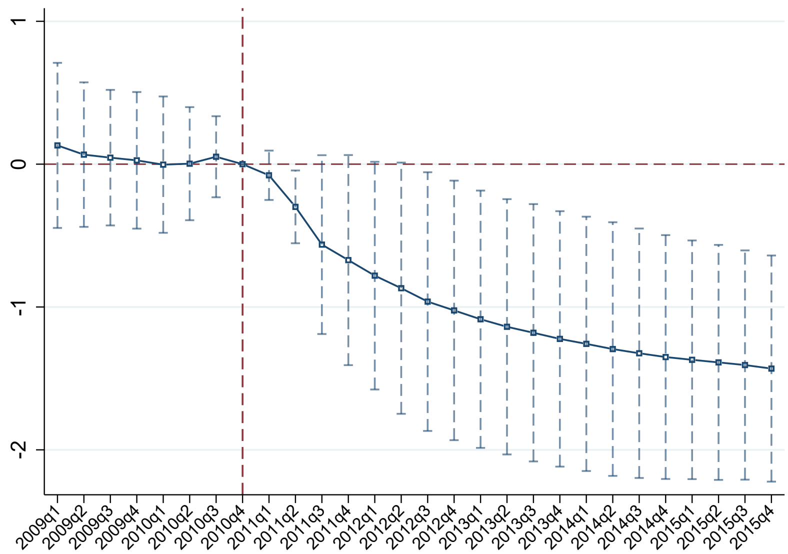
(b) Effect over time



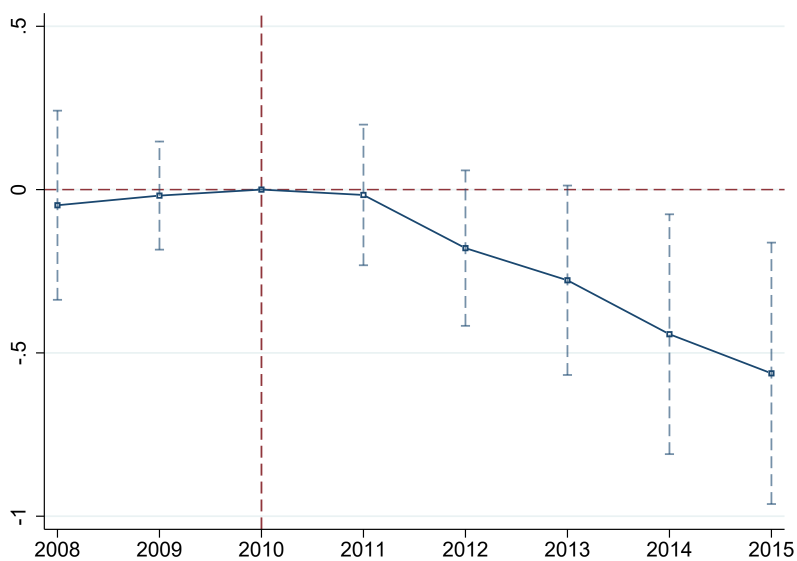
Panel (a) presents a binned scatter plot of the log of cumulative credit growth between 2010Q4 and 2015Q4 vs. procurement exposure at the bank-firm level. Panel (b) shows point estimates and 95% confidence intervals for the bank-firm level effect of procurement exposure on log cumulative credit growth 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
Equity-to-assets ratio	Ratio of bank equity to total assets
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 loans 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 (i.e., 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)
Total assets	Book value of total assets
Cash	Ratio of cash and short-term investments to 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 Proofs

A.1 Derivation of aggregate credit and output

Household optimization implies that labor supply satisfies:

$$L = w^{\frac{1}{\phi}} \tag{20}$$

$$l_i = L\left(\frac{w_i}{w}\right)^{\alpha}. \tag{21}$$

Using the expression for l_i, product demand $y_i = Yp_i^{-\sigma}$ and the production function $y_i = z_i l_i$, firm profits can be expressed as:

$$\Pi_i = \max_{l_i}(z_i l_i)^{\frac{\sigma-1}{\sigma}} Y^{\frac{1}{\sigma}} - R_i w L^{-\frac{1}{\alpha}} l_i^{\frac{\alpha+1}{\alpha}}. \tag{22}$$

The FOC for l_i gives:

$$l_i = \left(\frac{\sigma}{\sigma-1}\right)^{-\sigma\frac{\alpha}{\alpha+\sigma}} \left(Yw^{-\sigma}\right)^{\frac{\alpha}{\alpha+\sigma}} z_i^{(\sigma-1)\frac{\alpha}{\alpha+\sigma}} L^{\frac{\sigma}{\alpha+\sigma}} R_i^{-\sigma\frac{\alpha}{\alpha+\sigma}}. \tag{23}$$

Elevating to $\frac{\alpha+1}{\alpha}$, integrating over firms, elevating to $\frac{\alpha}{\alpha+1}$ and re-arranging gives:

$$L = \left(\frac{\sigma}{\sigma-1}\right)^{-\sigma} Yw^{-\sigma} \left(\int_0^1 z_i^{\frac{(\alpha+1)(\sigma-1)}{\alpha+\sigma}} di\right)^{\frac{\alpha+\sigma}{\alpha+1}} \left(\int_0^1 R_i^{-\sigma\frac{\alpha+1}{\alpha+\sigma}} di\right)^{\frac{\alpha+\sigma}{\alpha+1}}. \tag{24}$$

Using equation (24) to plug in for $Yw^{-\sigma}$ in equation (23) yields:

$$l_i = Lz_i^{(\sigma-1)\frac{\alpha}{\alpha+\sigma}} R_i^{-\sigma\frac{\alpha+1}{\alpha+\sigma}} \left(\int_0^1 z_i^{\frac{(\alpha+1)(\sigma-1)}{\alpha+\sigma}} di \right)^{-\frac{\alpha}{\alpha+1}} \left(\int_0^1 R_i^{-\sigma\frac{\alpha+1}{\alpha+\sigma}} di \right)^{-\frac{\alpha}{\alpha+1}}. \tag{25}$$

Multiplying equation (25) by z_i gives an expression for y_i. Elevating that expression to $\frac{\sigma-1}{\sigma}$, integrating over firms, elevating to $\frac{\sigma}{\sigma-1}$ and re-arranging gives:

$$Y = LZ \left(\int_0^1 R_i^{-\alpha\frac{\sigma-1}{\alpha+\sigma}} di \right)^{\frac{\sigma}{\sigma-1}} \left(\int_0^1 R_i^{-\sigma\frac{\alpha+1}{\alpha+\sigma}} di \right)^{-\frac{\alpha}{\alpha+1}}, \tag{26}$$

where $Z \equiv \left(\int_0^1 z_i^{\frac{(\alpha+1)(\sigma-1)}{\alpha+\sigma}} di \right)^{\frac{\alpha+\sigma}{(\alpha+1)(\sigma-1)}}$.

Aggregate credit must equal the aggregate wage bill:

$$Q = Lw = L^{1+\phi}, \tag{27}$$

where the second equality follows from equation (20). Using equations (20) and (26) to plug in for w and Y into equation (24) and combining it with equation (27) leads to:

$$Q = \left(\frac{\sigma-1}{\sigma} \frac{Z}{R} \right)^{\frac{1}{\phi}+1}, \tag{28}$$

where $R \equiv \left(\int_0^1 R_i^{-\alpha\frac{\sigma-1}{\alpha+\sigma}} di \right)^{-\frac{1}{\sigma-1}} \left(\int_0^1 R_i^{-\sigma\frac{\alpha+1}{\alpha+\sigma}} di \right)^{-\frac{1}{\alpha+1}}$ is the composite aggregate lending rate.

The remaining step is to solve for R. The definition of R_i in equation (12) and the loan pricing rule in equation (13) imply that $R_i = \frac{\theta}{\theta-1} v_i^{\frac{1}{\eta}}$, where $v_i \equiv \left(\int_0^1 \omega_{ib} v_b^{\frac{1-\theta}{\eta}} db \right)^{\frac{\eta}{1-\theta}}$. Multiplying the expression for l_i in equation (25) by w_i gives an expression for firm-level credit demand q_i. Using equations (20), (21) and (27) to plug in for w, w_i and L in the resulting expression, q_i can be expressed as:

$$q_i = Q\left(\frac{z_i}{Z}\right)^{\frac{(\alpha+1)(\sigma-1)}{\alpha+\sigma}} \frac{v_i^{-\frac{\sigma}{\eta}\frac{\alpha+1}{\alpha+\sigma}}}{\int_0^1 v_i^{-\frac{\sigma}{\eta}\frac{\alpha+1}{\alpha+\sigma}} di}. \tag{29}$$

Plugging for q_i into equation (11), integrating over firms and using equations (13) and (14) gives:

$$v_b e_b = Q\frac{\int_0^1 v_i^{-\frac{\sigma\frac{\alpha+1}{\alpha+\sigma}-\theta}{\eta}} di}{\int_0^1 v_i^{-\frac{\sigma}{\eta}\frac{\alpha+1}{\alpha+\sigma}} di} v_b^{-\frac{\theta}{\eta}}. \tag{30}$$

Solving equation (30) for v_b and plugging into v_i yields:

$$v_i = \left[\int_0^1 \omega_{ib}\left(Q\frac{\int_0^1 v_i^{-\frac{\sigma\frac{\alpha+1}{\alpha+\sigma}-\theta}{\eta}} di}{\int_0^1 v_i^{-\frac{\sigma}{\eta}\frac{\alpha+1}{\alpha+\sigma}} di} e_b\right)^{\frac{1-\theta}{\eta+\theta}} db\right]^{\frac{\eta}{1-\theta}}. \tag{31}$$

Raising both sides to $-\frac{\sigma(\alpha+1)}{\alpha+\sigma}$, integrating over firms, and solving for $\int_0^1 v_i^{-\frac{\sigma}{\eta}\frac{\alpha+1}{\alpha+\sigma}} di$:

$$\int_0^1 v_i^{-\frac{\sigma}{\eta}\frac{\alpha+1}{\alpha+\sigma}} di = \left[\left(Q\int_0^1 v_i^{-\frac{\sigma\frac{\alpha+1}{\alpha+\sigma}-\theta}{\eta}} di\right)^{-\frac{\sigma(\alpha+1)}{(1+\frac{\theta}{\eta})(\alpha+\sigma)}} \int_0^1 e_i^{\frac{\sigma(\alpha+1)}{(1+\frac{\theta}{\eta})(\alpha+\sigma)}} di\right]^{\frac{(1+\frac{\theta}{\eta})(\alpha+\sigma)}{(1+\frac{\theta}{\eta})(\alpha+\sigma)-\sigma(\alpha+1)}} \tag{32}$$

where:

$$e_i \equiv \left(\int_0^1 \omega_{ib} e_b^{-\frac{1-\theta}{\eta+\theta}} db\right)^{-\frac{\eta+\theta}{1-\theta}}. \tag{33}$$

Plugging into equation (31), raising both sides to $-\frac{\sigma\frac{\alpha+1}{\alpha+\sigma}-\theta}{\eta}$, integrating over firms, and solving for $\int_0^1 v_i^{-\frac{\sigma\frac{\alpha+1}{\alpha+\sigma}-\theta}{\eta}} di$:

$$\int_0^1 v_i^{-\frac{\sigma\frac{\alpha+1}{\alpha+\sigma}-\theta}{\eta}} di = \left(\frac{Q}{\int_0^1 e_i^{\frac{\sigma(\alpha+1)}{(1+\frac{\theta}{\eta})(\alpha+\sigma)}} di}\right)^{\frac{\frac{\theta}{\eta}(\alpha+\sigma)-\sigma(\alpha+1)}{\alpha+\sigma}} \left(\int_0^1 e_i^{-\frac{\frac{\theta}{\eta}(\alpha+\sigma)-\sigma(\alpha+1)}{(1+\frac{\theta}{\eta})(\alpha+\sigma)}} di\right)^{\frac{(1+\frac{\theta}{\eta})(\alpha+\sigma)-\sigma(\alpha+1)}{\alpha+\sigma}}. \tag{34}$$

Using equations (31), (32) and (34), v_i can be expressed as:

$$v_i = \frac{Q}{\int_0^1 e_i^{\frac{\sigma(\alpha+1)}{(\eta+\theta)(\alpha+\sigma)}} di} \frac{\int_0^1 e_i^{-\frac{\theta(\alpha+\sigma)-\sigma(\alpha+1)}{(\eta+\theta)(\alpha+\sigma)}} di}{e_i^{\frac{\eta}{\eta+\theta}}}, \tag{35}$$

which implies that:

$$R_i = \frac{\theta}{\theta-1}\left(\frac{Q}{\int_0^1 e_i^{\frac{\sigma(\alpha+1)}{(\eta+\theta)(\alpha+\sigma)}} di} \frac{\int_0^1 e_i^{-\frac{\theta(\alpha+\sigma)-\sigma(\alpha+1)}{(\eta+\theta)(\alpha+\sigma)}} di}{e_i^{\frac{\eta}{\eta+\theta}}}\right)^{\frac{1}{\eta}}. \tag{36}$$

Using equation (36) to plug in for R_i leads to an expression for R as a function of aggregate leverage:

$$R = \frac{\theta}{\theta-1}\left(\frac{Q}{E}\right)^{\frac{1}{\eta}}, \tag{37}$$

where

$$E \equiv \frac{\left(\int_0^1 e_i^{\frac{\alpha(\sigma-1)}{(\eta+\theta)(\alpha+\sigma)}} di\right)^{\frac{\eta}{\sigma-1}} \left(\int_0^1 e_i^{\frac{\sigma(\alpha+1)}{(\eta+\theta)(\alpha+\sigma)}} di\right)^{\frac{\eta+\alpha+1}{\alpha+1}}}{\int_0^1 e_i^{-\frac{\theta(\alpha+\sigma)-\sigma(\alpha+1)}{(\eta+\theta)(\alpha+\sigma)}} di}. \tag{38}$$

Using equation (37) to plug in for R in equation (28) gives the final expression for aggregate credit:

$$Q = \left[\frac{(\sigma-1)(\theta-1)}{\sigma\theta} Z E^{\frac{1}{\eta}}\right]^{\frac{\eta\left(\frac{1}{\phi}+1\right)}{\eta+\frac{1}{\phi}+1}}. \tag{39}$$

Using equations (27) and (36) to plug in for L and R_i in equation (26) gives the final expression for aggregate output:

$$Y = Q^{\frac{1}{\phi+1}} Z \left(\int_0^1 e_i^{\frac{\alpha(\sigma-1)}{(\eta+\theta)(\alpha+\sigma)}} di \right)^{\frac{\sigma}{\sigma-1}} \left(\int_0^1 e_i^{\frac{\sigma(\alpha+1)}{(\eta+\theta)(\alpha+\sigma)}} di \right)^{-\frac{\alpha}{\alpha+1}}. \tag{40}$$

A.2 Proof of Proposition 1

Using equation (35) to plug in for v_i in equation (30) and solving for v_b gives:

$$v_b = \frac{Q}{\int_0^1 e_i^{\frac{\sigma(\alpha+1)}{(\eta+\theta)(\alpha+\sigma)}} di} \frac{\int_0^1 e_i^{-\frac{\theta(\alpha+\sigma)-\sigma(\alpha+1)}{(\eta+\theta)(\alpha+\sigma)}} di}{e_b^{\frac{\eta}{\eta+\theta}}}. \tag{41}$$

Using equation (29) to plug in for q_i in equation (11), and the expressions for v_i and v_b from equations (35) and (41), bank-firm level credit demand can be expressed as:

$$q_{ib} = Q \left(\frac{z_i}{Z} \right)^{\frac{(\alpha+1)(\sigma-1)}{\alpha+\sigma}} \frac{e_i^{\frac{\sigma(\alpha+1)}{(\eta+\theta)(\alpha+\sigma)}}}{\int_0^1 e_i^{\frac{\sigma(\alpha+1)}{(\eta+\theta)(\alpha+\sigma)}} di} \omega_{ib} \left(\frac{e_b}{e_i} \right)^{\frac{\theta}{\theta+\eta}}, \tag{42}$$

Let $\hat{x} = \frac{dx}{x}$. The model analog of our bank-firm credit regression is then:

$$\hat{q}_{ib} = \hat{Q} - \hat{\Theta} + \frac{\sigma(\alpha+1) - \theta(\alpha+\sigma)}{(\eta+\theta)(\alpha+\sigma)} \hat{e}_i + \frac{\theta}{\theta+\eta} \hat{e}_b, \tag{43}$$

where $\Theta \equiv \int_0^1 e_i^{\frac{\sigma(\alpha+1)}{(\eta+\theta)(\alpha+\sigma)}} di$ and $\hat{z}_i = \hat{T}_{ib} = 0$ by assumption. Using equation (15) and evaluating at the initial point where $e_b = \bar{e}$, we get:

$$\hat{q}_{ib} = \hat{Q} - \hat{\Theta} + \frac{\sigma(\alpha+1) - \theta(\alpha+\sigma)}{(\eta+\theta)(\alpha+\sigma)} \hat{e}_i + \beta u_b, \tag{44}$$

where

$$\beta = -\rho \bar{v} \frac{\theta}{\theta+\eta}. \tag{45}$$

A.3 Proof of Proposition 2

Taking the log of Q in equation (39) and differentiating with respect to u gives:

$$\frac{d\log Q}{du} = \frac{\frac{1}{\phi}+1}{\eta+\frac{1}{\phi}+1}\frac{d\log E}{du}, \tag{46}$$

Using equation (38) to evaluate $\frac{d\log E}{du}$, we get:

$$\frac{d\log E}{du} = \frac{\eta}{\sigma-1}x_1\frac{\int_0^1 e_i^{x_1-1}\frac{de_i}{du}di}{\int_0^1 e_i^{x_1}di} + \frac{\eta+\alpha+1}{\alpha+1}x_2\frac{\int_0^1 e_i^{x_2-1}\frac{de_i}{du}di}{\int_0^1 e_i^{x_2}di} - x_3\frac{\int_0^1 e_i^{x_3-1}\frac{de_i}{du}di}{\int_0^1 e_i^{x_3}di}, \tag{47}$$

where $x_1 \equiv \frac{\alpha(\sigma-1)}{(\eta+\theta)(\alpha+\sigma)}$, $x_2 \equiv \frac{\sigma(\alpha+1)}{(\eta+\theta)(\alpha+\sigma)}$ and $x_3 \equiv -\frac{\theta(\alpha+\sigma)-\sigma(\alpha+1)}{(\eta+\theta)(\alpha+\sigma)}$.

Differentiating equation (33) yields:

$$\frac{de_i}{du} = e_i\frac{\int_0^1 \omega_{ib}e_b^{-\frac{1-\theta}{\eta+\theta}-1}\frac{de_b}{du}db}{\int_0^1 \omega_{ib}e_b^{-\frac{1-\theta}{\eta+\theta}}db}. \tag{48}$$

Equation (15) implies:

$$\frac{de_b}{du} = -e_b\rho v_b\int_0^1 \frac{q_{ib}}{q_b}(1-\tau_i)di. \tag{49}$$

Combining the last three equations and evaluating at the initial point where $e_b = \bar{e}$, all equity terms cancel, and we get:

$$\frac{d\log E}{du} = -\rho\bar{v}\int_0^1\int_0^1 \omega_{ib}\int_0^1 \frac{q_{ib}}{\bar{q}_b}(1-\tau_i)didbdi \tag{50}$$

$$= -\rho\bar{v}\int_0^1\int_0^1 \omega_{ib}\int_0^1 \omega_{ib}(1-\tau_i)didbdi \tag{51}$$

$$= -\rho\bar{v}, \tag{52}$$

where the second line follows from evaluating equation (42) at the initial point, and using the fact that z_i and ω_{ib} are independent.

Let $\psi \equiv -\frac{d\log Q}{d\log R} = \frac{1}{\phi} + 1$, which follows from equation (28). Using this and equation (52) to plug into equation (46) gives:

$$\frac{d\log Q}{du} = -\frac{\psi}{\psi+\eta}\rho\bar{v} \tag{53}$$
$$= -\frac{\psi}{\theta}\frac{\theta+\eta}{\psi+\eta}\beta, \tag{54}$$

where the second line uses equation (45).

A.4 Proof of Proposition 3

Differentiating the log of equation (40), using equations (48) and (49) and evaluating at the initial point gives:

$$\frac{d\log Y}{du} = \frac{1}{1+\phi}\frac{d\log Q}{du}. \tag{55}$$

A.5 Coefficient on exposure at the firm level

Combining equations (29) and (35), firm-level credit can be expressed as:

$$q_i = Q\left(\frac{z_i}{Z}\right)^{\frac{(\alpha+1)(\sigma-1)}{\alpha+\sigma}} \frac{e_i^{\frac{\sigma}{\eta+\theta}\frac{\alpha+1}{\alpha+\sigma}}}{\int_0^1 e_i^{\frac{\sigma}{\eta+\theta}\frac{\alpha+1}{\alpha+\sigma}} di}. \tag{56}$$

Letting $\hat{x} = \frac{dx}{\bar{x}}$, the model counterpart of our firm-level credit regression is:

$$\hat{Q}_i = \hat{Q} - \hat{\Theta} + \frac{\sigma}{\eta+\theta}\frac{\alpha+1}{\alpha+\sigma}\hat{e}_i, \tag{57}$$

where $\Theta \equiv \int_0^1 e_i^{\frac{\sigma(\alpha+1)}{(\eta+\theta)(\alpha+\sigma)}} di$ and $\hat{z}_i = 0$ by assumption. Using equations (48) and (49) and evaluating at the initial point where $e_b = \bar{e}$, we get:

$$\hat{Q}_i = \hat{Q} - \hat{\Theta} + \beta_{firm} u_i, \tag{58}$$

where $u_i \equiv \int_0^1 \omega_{ib} \int_0^1 \frac{q_{ib}}{\bar{q}_b}(1 - \tau_i) di db$ is the model counterpart of our empirical measure of firm-level procurement exposure, and β_{firm} is given by:

$$\beta_{firm} = -\frac{\sigma}{\eta + \theta} \frac{\alpha + 1}{\alpha + \sigma} \rho \bar{v}. \tag{59}$$

Using equation (45), θ can then be expressed as:

$$\theta = \frac{\beta}{\beta_{firm}} \sigma \frac{\alpha + 1}{\alpha + \sigma}. \tag{60}$$

B Appendix Tables

Table B.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 two-digit Common Procurement Vocabulary (CPV) codes.

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

Episode	% cut	Cut as a % of GDP	Composition of procurement cut (%): 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 B.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 sales for the sample of firms with procurement contracts in 2009-2010. 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 B.4: Direct effect of procurement cuts on government contractors

	Value added (1)	NPL ratio (2)
Contract Cut	-1.041	0.143
	(0.059)	(0.016)
Observations	13,402	13,402
Adjusted R^2	0.069	0.088

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 2010Q4 and each quarter between 2011Q1 and 2015Q4 on the firm's procurement cut as a fraction of sales. The sample is restricted to government contractors with credit outstanding in 2010Q4. Both regressions control for log total assets, return on assets, leverage, and the current ratio. Robust standard errors are reported in parentheses.

Table B.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	-3.169	-1.822	-2.444	-2.463
	(0.536)	(0.575)	(0.792)	(0.682)
BM degrees of freedom	3.4	3.4	3.7	3.3
Observations	76,289	76,289	76,289	76,289
Adjusted R^2	0.070	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 the equity-to-assets ratio 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 replaces procurement cuts with the national growth of NPLs by product (eight-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 procurement 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 (non-contractors) in 2009-2010, and lending relationships above €25,000 in 2010Q4 that existed in 2009 and 2010. 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 B.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 the equity-to-assets ratio 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 B.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	-1.487	-1.029	-1.477	-1.593
	(0.355)	(0.265)	(0.273)	(0.273)
BM degrees of freedom	4.7	4.5	4.7	4.7
Observations	50,346	50,346	50,346	50,346
Adjusted R^2	0.087	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 the equity-to-assets ratio 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 (eight-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 procurement 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 B.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-level 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 the equity-to-assets ratio 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 B.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	-0.434	-0.430	-0.558	-0.651
	(0.129)	(0.110)	(0.156)	(0.163)
BM degrees of freedom	4.7	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-level 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 the equity-to-assets ratio 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 (eight-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 procurement 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 B.10: Elasticity of substitution across banks

	(1)	(2)	(3)	(4)
Interest Rate	-6.598	-4.490	-4.537	-4.550
	(0.071)	(0.080)	(0.078)	(0.079)
Observations	1,205,360	1,203,176	1,203,046	1,202,865
Adjusted R^2	0.720	0.777	0.786	0.793

This table presents estimates from regressions of log credit on log gross interest rates for new loans in the 2013-2015 period. All columns include bank-year fixed effects and bank-firm fixed effects. Column 2 includes firm-year fixed effects. Column 1 includes firm-year fixed effects. Column 2 includes firm-year-maturity fixed effects, using ten loan maturity bins. Column 3 includes firm-year-maturity-fixed rate fixed effects, where fixed rate is a dummy for whether the loan has a fixed interest rate. Column 4 includes firm-year-maturity-fixed rate-collateral fixed effects, where collateral is a dummy for whether the loan is collateralized. The sample consists of loans issued by banks with at least 1% of the corporate credit market in 2010Q1.

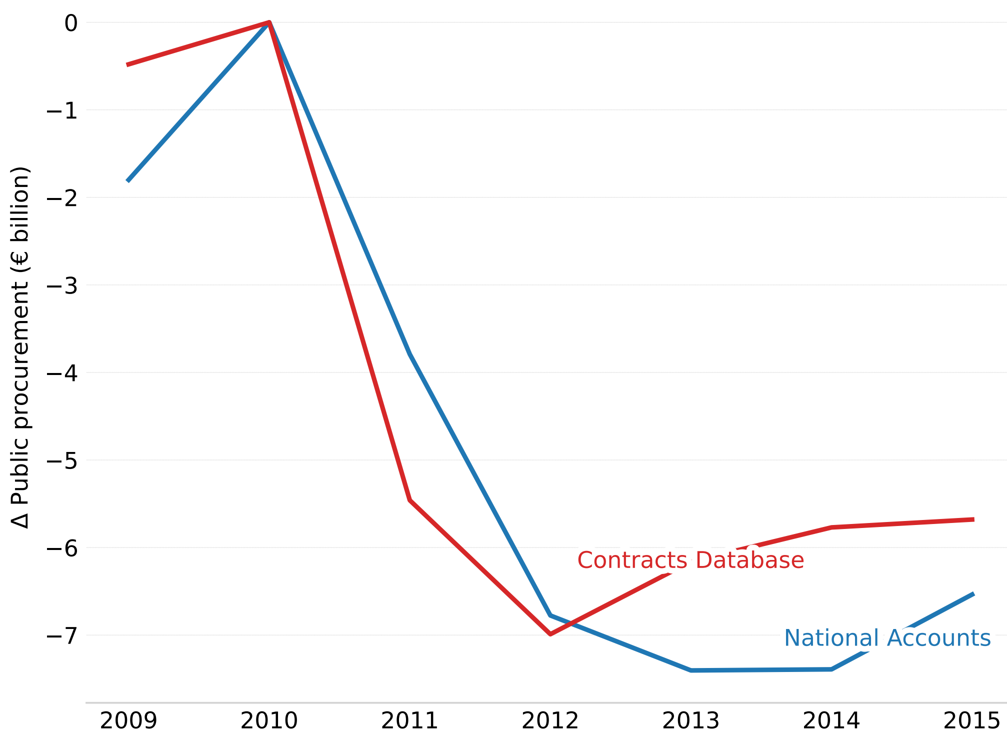
Table B.11: Elasticity of credit supply with respect to bank equity

	First Stage (1)	Second Stage (2)
Procurement Exposure	-2.561	
	(1.353)	
Equity Growth		0.902
		(0.312)
BM degrees of freedom	3.0	2.9
Observations	72,648	72,648
Adjusted R^2	0.363	0.068

This table presents estimates of a 2SLS regression of log cumulative growth in credit at the bank-firm level between 2010Q4 and 2015Q4 on log cumulative growth in bank equity over the same period. Equity growth is instrumented with procurement exposure, defined as 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. Column 1 shows the first-stage estimates, and column 2 the second-stage estimates. Both regressions control for sovereign debt exposure, total assets, and the equity-to-assets ratio at the bank level, as well as for log total assets, return on assets, leverage, and the current ratio at the firm level. 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 above €25,000 in 2010Q4 that existed in 2009 and 2010. Foreign branches are excluded from the sample. 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. Robust standard errors are presented in parentheses.

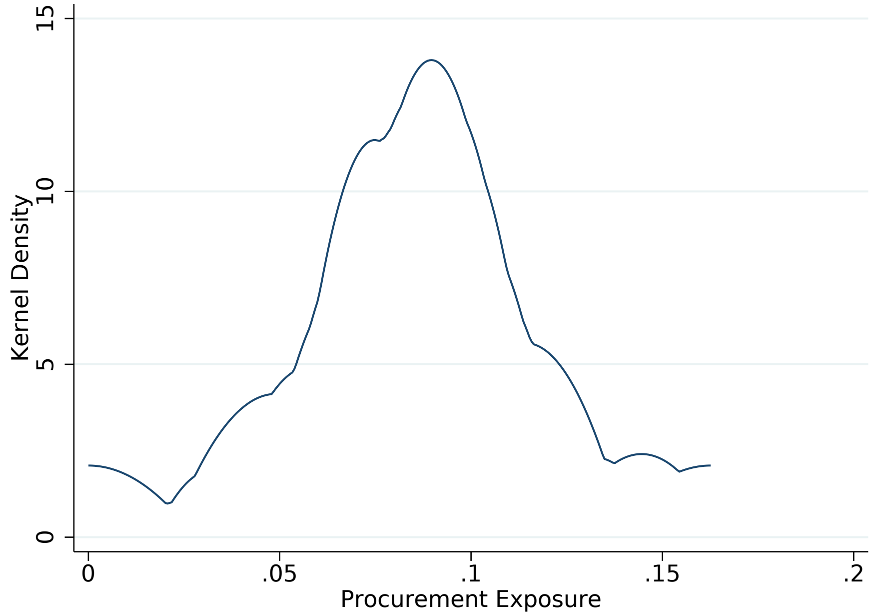
C Appendix Figures

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



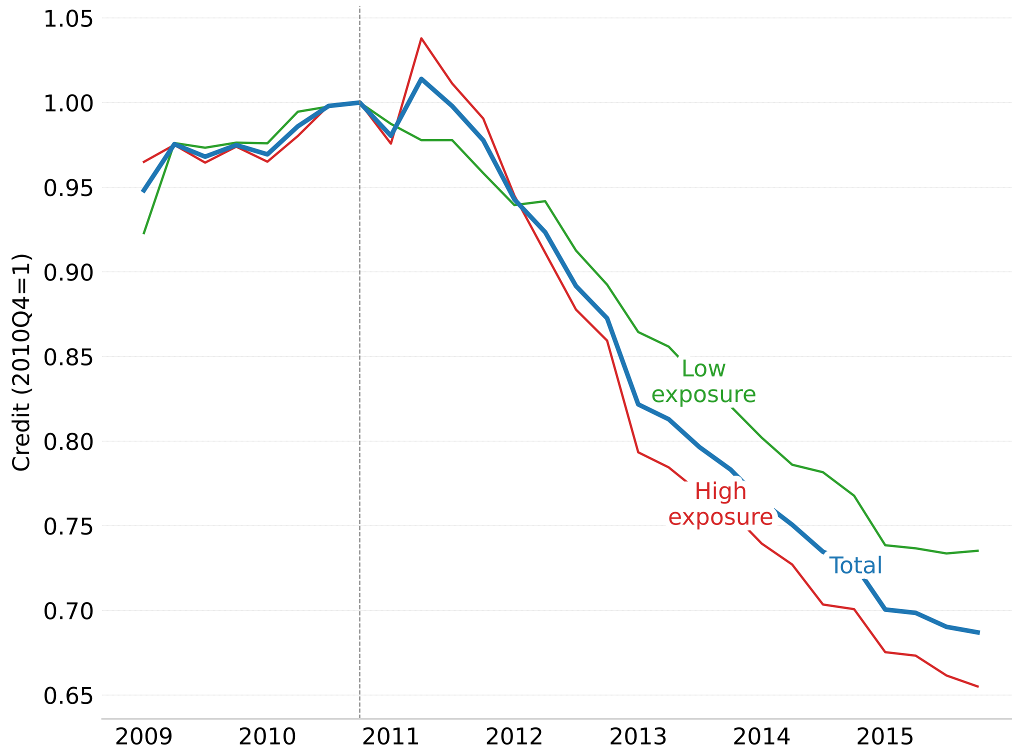
This figure compares the change in public procurement spending in Portugal in the postcrisis period calculated using System of National Accounts (SNA) data from the OECD and using our data on public procurement contracts. In SNA data, public procurement is defined as the sum of gross fixed capital formation,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

Figure C.2: Distribution of procurement exposure across banks



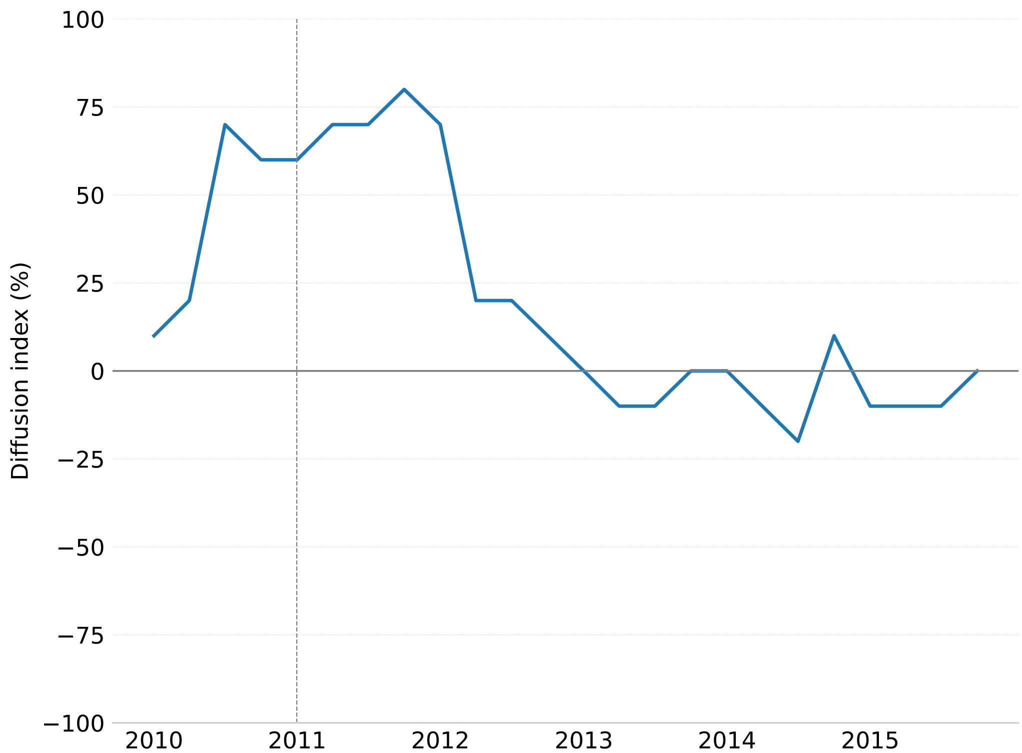
This figure shows kernel density estimates of the precrisis (2010Q1) distribution of bank exposure to firms with public procurement contracts in 2010.

Figure C.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).

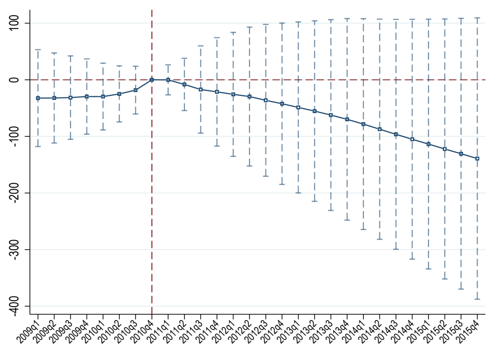
Figure C.4: Change in bank lending standards versus previous quarter



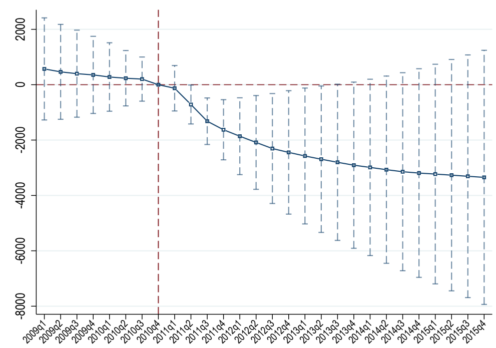
This figure plots data from Banco de Portugal's Bank Lending Survey. Banks are asked the following question: "Over the past three months, how have your bank's credit standards as applied to the approval of loans or credit lines to enterprises changed?" The diffusion index aggregates answers from all banks and varies between -100 and 100. Values above zero correspond to a tightening of credit standards, and values below zero to a loosening of those standards.

Figure C.5: Credit at the bank-firm level weighted average contractor credit shares

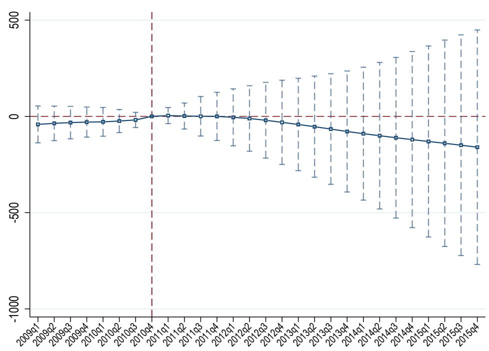
(a) Construction



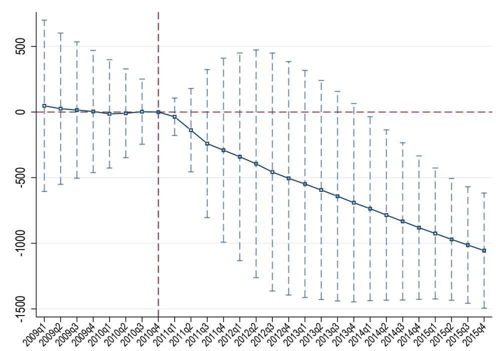
(b) Administrative services



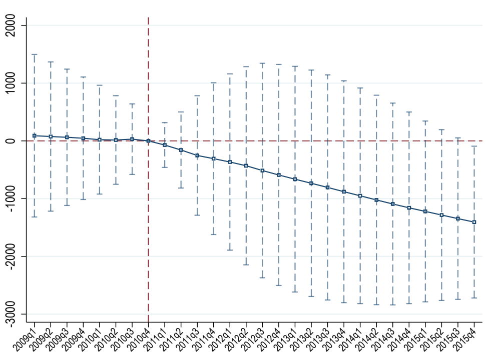
(c) Water and waste management



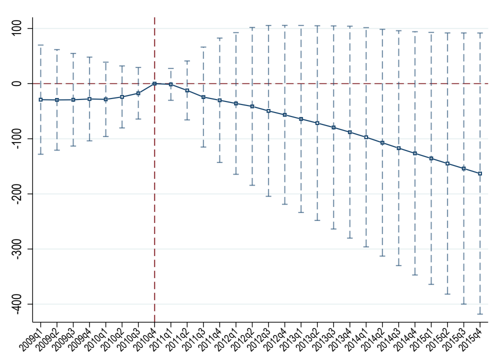
(d) Consulting



(e) Wholesale and retail trade

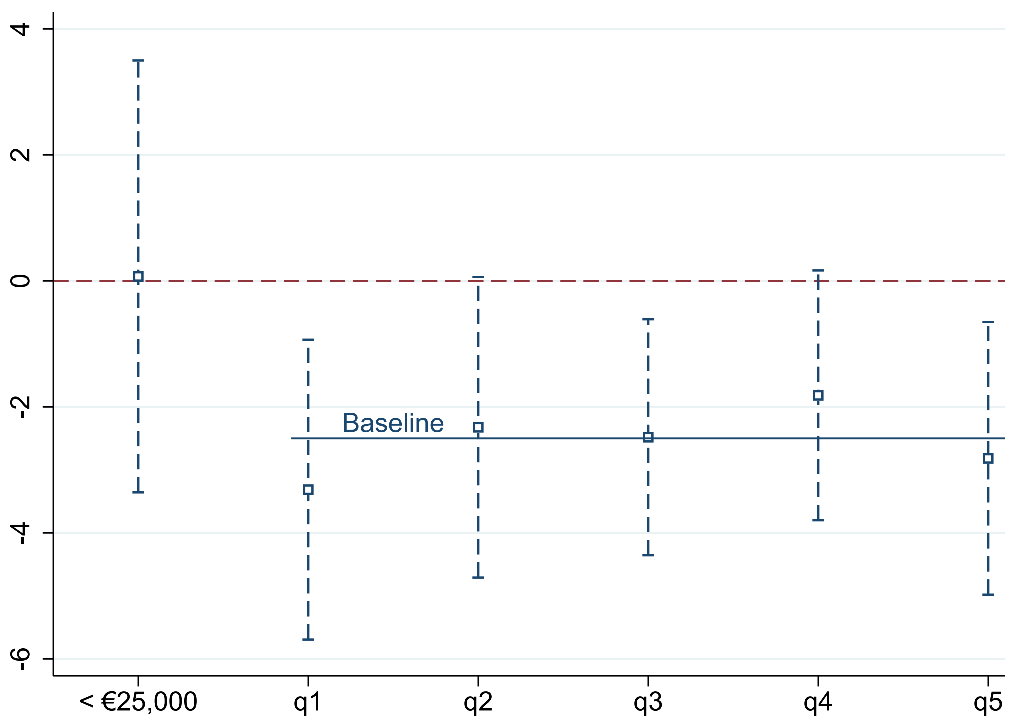


(f) All sectors



This figure plots point estimates and 95% confidence intervals from estimating regression equation (4) replacing procurement exposure with $\hat{\alpha}_i$-weighted average contractor credit shares by sector, where $\hat{\alpha}_i$ are the 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).

Figure C.6: 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).