Sovereign-Bank Links: the Procurement Channel*

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Abstract

Research and policy interest on the links between sovereign and bank distress has mainly focused on the role of bank holdings of sovereign debt. We show that this misses an important channel: bank lending to firms with government contracts. We study this procurement channel in the context of the eurozone crisis, using administrative data on government contracts, firms and banks in Portugal. On the eve of the crisis, credit to firms with contracts corresponded to 64% of bank equity and 87% of domestic sovereign debt bank holdings. As the crisis hit and the government lost access to capital markets, public procurement was sharply cut by 32%, or 4.3% of GDP. We find that this cut saddled banks with non-performing loans from firms that held contracts, and that this led to a significant reduction in credit supply to other firms. The credit supply shock in turn caused firm output to decline. Our findings can account for nearly 40% of the output loss during the crisis, and can also help explain the large multipliers associated with fiscal consolidation in this period.

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1 Introduction

A central amplification mechanism in the 2010-2011 eurozone crisis was the feedback between sovereign and bank distress operating through bank holdings of domestic sovereign debt, whose value plunged as sovereign yields rose (Gennaioli, Martin, and Rossi, 2014; Acharya, Dreschsler, and Schnabl, 2014; Bocola, 2016). Yet, despite the fact that by the end of 2012 sovereign yields had largely normalized, with the exception of Greece, crisis-hit economies remained depressed for several years afterwards (figure 1). At the same time, the multipliers associated with fiscal consolidation efforts in these countries during the crisis appear to have been substantially larger than expected (Blanchard and Leigh, 2013).

In this paper we identify and study a mechanism that can help explain the weak recovery and the large multipliers. In addition to sovereign debt, sovereigns and banks are linked indirectly through bank lending to firms with government procurement contracts. When governments cut procurement spending, this increases default risk for firms with government contracts, which affects the balance sheets of banks that lend to these firms. Weaker banks in turn tighten credit supply, depressing real activity. Conversely, a deterioration in bank health increases the likelihood of bank bailouts, and a drop in real activity lowers tax revenue, both of which put pressure on financially constrained governments to cut spending. These bidirectional effects can potentially generate the same type of negative feedback loop as exposure to sovereign debt, along with an increase in fiscal multipliers.

We start by establishing a set of facts we see as suggestive of the relevance of this mechanism, using data on OECD countries between 1995 and 2018. First, when governments cut spending they disproportionately rely on cuts to public procurement. Procurement accounts for 30% of government expenditure, but over half of spending cuts on average. Second, large cuts to public procurement are not rare occurrences. We doc-

ument 16 episodes in 15 different countries where procurement was cut by at least 10% during this period. The average cut amounted to 20%, or 2.8% of GDP. Third, large procurement cuts are often associated with periods of stress for banks and sovereigns. Half of these 16 episodes overlapped with systemic banking crises, 5 of them with IMF/EU bailouts and 3 with sovereign debt defaults or restructurings.

Some of the largest procurement cuts we document occurred precisely in the context of the eurozone crisis. In Greece, Ireland, Italy, Portugal and Spain, the countries at the epicenter of the crisis, public procurement was cut by between 14% and 46%, as governments strove to restore investor confidence. We study the case of Portugal in particular, where we are able to merge administrative data on the universe of government contracts, bank-firm lending relationships, and firm financial statements.

We first show that these cuts and the distress they induced were large enough to affect the Portuguese banking system. At the onset of the crisis, in 2010, government contracts in our matched data accounted for 26% of sales for the firms that held them, henceforth labeled contract firms, and these firms in turn accounted for 35% of value added in the corporate sector. Bank lending to contract firms corresponded to 16% of total corporate lending, 64% of total bank equity, and 87% of domestic sovereign debt bank holdings at the time. 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 implemented an aggressive fiscal consolidation package, which included cutting procurement by 32%, or 4.3% of 2010 GDP. Contract firms subsequently experienced steep declines in output, and the resulting distress spilled over visibly into bank balance sheets. Non-performing loans (NPLs) from contract firms increased seven-fold in the years after the crisis, an amount equivalent to 13.4% of precrisis bank equity.[1]

[1]This is likely to be a lower bound for the impact of the procurement cut on banks, since it ignores knock-on effects on other firms through supply chain connections with contract firms.

For comparison, the loss in market value of precrisis domestic sovereign debt bank holdings attained a maximum of 14.9% of equity in early 2012. But the sharp drop in sovereign yields from 2012 onwards quickly reversed this loss and eventually led to large gains in the market value of sovereign debt (Acharya et al., 2019). The stock of contract firm NPLs, on the other hand, only peaked in 2015, plausibly curtailing lending for a much longer period than sovereign debt holdings.

To estimate the effect of these procurement cuts on credit supply, we exploit variation across banks in precrisis loan exposure to contract firms. Our baseline specification examines changes in credit within industry-municipality-quarter and within bank-firm relationship. We show that the results are slightly stronger when we control for time-varying firm-level credit demand in a within firm-quarter specification estimated on the sample of firms with at least two banking relationships (Khwaja and Mian, 2008), which suggests our baseline estimates are conservative. Our approach has the advantages of including firms with only one banking relationship and enabling us to use the same specification in our bank-firm and firm-level analyses.

We find that lending from banks with different levels of exposure followed similar trends before the crisis, supporting the validity of our research design. In the post-crisis period, more exposed banks significantly reduced lending relative to less exposed banks, and the difference grew stronger over time, in parallel with the above-mentioned growth of contract firm NPLs. By 2015, a one percentage point increase in exposure led to a cumulative drop of 1.4% in credit in our baseline specification.

We then evaluate whether firms were able to replace the credit lost from more exposed banks. We find that firm-level credit also decreased significantly with bank exposure in the post-crisis period. The magnitude of our estimates is just over half of their relationship-level counterparts, which suggests that firms were able to substitute almost half of the credit lost from more exposed banks. We also show that this effect was mostly driven by the extensive margin of firm exit, not by intensive margin effects among sur-

viving firms.

Having documented the effect of the shock on credit, we turn to real effects. Using the same design, we show that firms borrowing from more exposed banks experienced substantial declines in output, assets, cash and employment after the crisis, relative to firms borrowing from less exposed banks. In line with the effect on credit, these effects were entirely driven by firm exit.

Finally, we quantify the role of the mechanism we document in accounting for the path of corporate lending and output in the wake of the crisis. We follow the partial equilibrium approach developed by Chodorow-Reich (2014),[2] and find that the shock can explain 46% of the cumulative drop in corporate lending between 2010 and 2015, and 39% of the drop in real GDP. The impact on output implies a fiscal multiplier associated with our mechanism of 0.7 at the one year horizon, in 2011, and of about 0.5 at the two to five year horizons, between 2012 and 2015. Blanchard and Leigh (2013) report that multipliers during the eurozone crisis were larger than expected by about 1.1 in 2010-2011, and by around 0.4 later in the crisis. If the effect of our channel was unanticipated, it can help explain a good part of this increase.

Our paper contributes to the literature on the links between sovereign and bank distress. This literature has focused on bank holdings of sovereign debt as the central mechanism linking sovereigns and banks, as formalized in the models of Gennaioli et al. (2014), Acharya et al. (2014), Bocola (2016) and Farhi and Tirole (2018). Firm-level evidence on the credit supply and real effects of this exposure is provided by Gennaioli et al. (2018), Acharya et al. (2018), De Marco (2019), Bottero et al. (2020) and Arellano et al. (2020)

We highlight a different source of bank exposure to the sovereign, operating through firms with government contracts. Our results have important implications for the design

[2]This approach ignores the general equilibrium effects on unaffected firms through changes in aggregate demand and wages, but both Chodorow-Reich (2014) and Arellano et al. (2020) develop and calibrate general equilibrium models to quantify these effects and find that they are likely to be small.

of sovereign bailouts and of policies aiming to break the sovereign-bank loop. Bailouts that are conditional on sharp fiscal consolidation efforts may precipitate or amplify stress in the banking system. Policies focused on the role of sovereign debt, such as the creation of a European safe asset (Brunnermeier et al., 2016) or limits to bank holdings of sovereign debt, do not address the channel we document and may be insufficient to break the loop.

Our paper also adds to the literature on fiscal multipliers, which has experienced a revival since the 2008 financial crisis. One topic that has received much attention is how multipliers depend on the state of the economy (Christiano et al., 2011; Auerbach and Gorodnichenko, 2012; Ramey and Zubairy, 2018). Our work is particularly related to evidence that multipliers are larger during financial crises (Corsetti et al., 2012; Born et al., 2020), in line with models where multipliers are amplified by financial frictions (Eggertsson and Krugman, 2012; Faria-e Castro, 2022). We identify a novel channel through which fiscal contractions themselves increase credit constraints, and provide evidence on its impact on lending and output in the context of a financial crisis. Our findings may help explain why fiscal multipliers in the eurozone crisis appear to have been unusually large (Blanchard and Leigh, 2013).

In addition, the measurement of fiscal multipliers is plagued by identification issues, limited variation and lack of evidence on mechanisms. A growing strand in the literature has moved beyond direct estimates using aggregate time series data to employing less direct but better identified designs and richer evidence, namely by estimating cross-sectional multipliers at the sub-national level (Shoag, 2010; Cohen et al., 2011; Chodorow-Reich et al., 2012; Acconcia et al., 2014). Nakamura and Steinsson (2014) and Chodorow-Reich (2019) show how these relative multipliers provide bounds on aggregate multipliers when monetary policy does not respond, e.g. in a monetary union or at the zero lower bound. Our work contributes towards extending this approach to the bank and firm level, offering causal evidence on a specific mechanism operating via credit

constraints. We ignore general equilibrium effects and draw only tentative implications for the size of aggregate multipliers, but note that our findings can be taken as identified moments to be targeted in richer models (Nakamura and Steinsson, 2018).

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 et al., 1997; Bosio et al., 2022). Winning government contracts spurs firm growth (Ferraz et al., 2015; Lee, 2021; Hvide and Meling, 2022), and improving payment efficiency on procurement can alleviate financial constraints and lead to job creation (Barrot and Nanda, 2020). On a less positive note, procurement is associated with corruption (Porter and Zona, 1993; Olken, 2007), favoritism (Burgess et al., 2015) and waste (Bandiera et al., 2009). We contribute to this literature by showing that procurement also creates a link between governments and the financial system that may lead to vulnerability in times of crisis.

The rest of the paper is organized as follows. Section 2 describes the data, and section 3 presents motivating facts. Section 4 describes the impact of the procurement cuts implemented in Portugal during the crisis on the banking system, and presents our main results. Section 5 concludes.

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 defined as the sum of government gross fixed capital formation, intermediate consumption and social transfers in kind via market producers. The 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 it includes some non-procurement expenditures, potentially overstating the amount of procurement (OECD, 2011). We use SNA data from 1995 to 2018 to document a set of facts on public procurement for OECD countries, and to characterize the evolution of procurement during the eurozone crisis in particular.[3]

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*,[4] and data on these contracts are made available online through the Tenders Electronic Daily (TED) database. In addition, most EU countries also publish their own contract databases, often employing lower thresholds than TED.[5]

The contract micro data tend to yield aggregate procurement amounts that are 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).[6] Despite the more limited coverage, a key advantage of the contract-

[3]We obtain the data from: https://stats.oecd.org/Index.aspx?DataSetCode=SNA$_TABLE12$

[4]In 2010, the threshold was 4.8 million euros for works, and either 125,000 or 193,000 euros 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.

[5]See here for links to individual country portals: https://ec.europa.eu/info/policies/public-procurement/tools-public-buyers/public-procurement-eu-countries$_en$

[6]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%, so this difference is significant. Third, contracts in some sectors are often exempted from publication altogether, namely real estate, media services, legal services, financial services, public transport, R&D, defense and security contracts. These are the main categories exempted

based data is that it can be linked to the firms providing goods and services to the government, which is essential for our purposes.

We obtain micro-level data on government contracts in Portugal from BASE, a web portal managed by the *Instituto dos Mercados Públicos, do Imobiliário e da Construção* (IMPIC). Except for the exempted sectors mentioned above, all public procurement contracts must be communicated to this portal by law, without a minimum threshold, and this communication is a precondition for contracts to become legally binding. 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 for the firms involved. One limitation of BASE is that it only includes comprehensive coverage of open tenders, the procedure typically adopted for the largest contracts, from 2011 onward. To overcome this, we complement BASE with data from TED, which we obtain through the web portal Opentender.eu, taking care to avoid any duplication.

Our combined dataset can account for 44% of SNA-based public procurement expenditure in Portugal in 2010, well above the 22% average reported for TED in 2008 in Europe (OECD, 2011). More importantly, as we show in appendix figure A.2, 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 1 presents summary statistics for our contract data for 2010, the year before the procurement cuts we study. The median contract was worth 12,132 euros, and the 10th percentile just 523 euros. This illustrates how well the data cover small contracts, given the absence of a reporting threshold in Portugal. At the same time, large contracts in the right tail generated considerable skewness in the distribution: the mean contract

from publication in TED under EU directive 2014/24. Public procurement in Portugal is governed by the *Código dos Contratos Públicos* enacted in 2008, which also exempts the same categories. Finally, SNA is 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.

was worth 132,217 euros, higher than the 90th percentile of 95,950 euros. The vast majority of contracts (93%) took the form of outright awards, but these only accounted for 26% of contracting volume. The 7% of contracts awarded through open, negotiated and restricted tenders tended to be much larger, and accounted for the remaining 74% of volume. In terms of buyers, central and local government accounted for about two thirds and one third of contract volume respectively.

When it comes to the type of goods or services purchased, construction accounted for the largest share of contract volume (55%), which reflects the large role played by infrastructure projects. The remainder was distributed across a wide range of goods and services, including health and social work services (9%), energy (5%), sewage services (3%), and other smaller categories.

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.[7] Loan data come from *Central de Responsabilidades de Crédito* (CRC), a database covering all credit exposures above 50 euros in Portugal. From CRC we obtain bank-firm level credit data from 2009 to 2015, at a quarterly frequency. We collect bank characteristics from supervisory data reported to Banco de Portugal, also measured quarterly. Finally, we draw firm characteristics from the *Informação Empresarial Simplificada* (IES) database, a joint project of Banco de Portugal, Statistics Portugal, the Ministry of Finance and the Ministry of Justice which includes detailed financial statements for all non-financial firms operating in Portugal. From IES, we obtain annual data from 2007 to 2015 on firm outcomes such as sales, value added, employment, cash and total assets. In our regressions, we winsorize all variables at the 2.5% and 97.5% level. Appendix table A.1 provides definitions for the

[7] TED data is sometimes missing firm tax identifiers. For these cases we rely on a combination of fuzzy matching and manual work to merge the data using firm names.

variables we use in our analysis.

3 Motivating facts

In this section we present a series of suggestive facts on the potential relevance of the link between sovereigns and banks that we study, using data on public procurement in OECD countries. We focus our analysis on the period between 1995 and 2018, since very few countries report data before 1995.

First, public procurement among OECD countries is not just a large component of government spending and overall economic activity, it plays a disproportionate role when governments engage in fiscal consolidation on the spending side. Figure 2 illustrates this point. We identify 83 instances when governments cut year-on-year primary expenditure excluding capital transfers[8] by at least 1% in real terms. On average, procurement accounts for 31% of spending in the year prior to these cuts, or 12% of GDP, and for 53% of spending cuts. Wages and social benefits, the other two major components of expenditure and each roughly the same size as procurement, account only for 22% and 5% of spending cuts respectively. Within procurement, gross fixed capital formation explains the bulk of this out-sized contribution, representing 10% of spending and 35% of spending cuts.

Second, the mechanism we posit is premised on cuts to government contracts being sufficiently large to materially affect firms and bank balance sheets. We provide direct evidence on this for the episode we study in Portugal below, but here we establish that this was not an isolated event. We identify 16 episodes in 15 different countries when governments cut procurement spending by at least 10%.[9] Appendix table A.2 lists these

[8]We exclude capital transfers to ignore transitory fluctuations in spending driven by one-time items, such as bank recapitalizations.

[9]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

episodes along with information on their magnitude and composition. On average, procurement was cut by 20%, or 2.8% of GDP, and gross fixed capital formation again plays a central role, accounting for three quarters of the cuts.

Third and last, episodes of large procurement cuts are often associated with periods of stress for banks and sovereigns. Some of the largest cuts happened during the eurozone crisis, including in Greece, Ireland, Italy, Portugal and Spain, the so-called GIIPS countries. Other prominent episodes include the United States and Iceland, whose governments cut procurement in the wake of the 2008 financial crisis. Overall, as reported in appendix table A.2, half of the 16 episodes we identify overlapped with a systemic banking crisis, 5 of them overlapped with a bailout from the IMF or the EU, and 3 with sovereign defaults or restructurings.[10]

4 Portugal in the eurozone crisis

4.1 Procurement cuts

Prior to the crisis, differences in sovereign yields across euro area countries were negligible, but the Greek bailout by the IMF/EU in May 2010 set off a rise in yields in Ireland, Italy, Portugal and Spain relative to those in Germany (figure 1). These rising yields brought all four countries under severe financial pressure, and eventually all but Italy received bailouts of their own.

In order to restore investor confidence and meet the bailout conditions, the GIIPS countries turned to aggressive fiscal consolidation efforts, and these efforts included

transitory spending fluctuations.

[10]Data on systemic 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).

sharp cuts to government procurement, as shown in figure 3.[11] In Portugal, the government cut procurement by 32% between 2010 and 2014, or 4.3% of 2010 GDP. This compares with an even stronger cut of 46% in Greece, similar cuts of 29% in Ireland and Spain, and a milder cut of 14% in Italy, relative to their respective precrisis peaks. In contrast, public procurement in Germany remained on a stable upward trend throughout the crisis.[12]

Public procurement represented an important source of demand for the private sector in Portugal before the crisis, as illustrated by table 2. In 2010, the government contracts in our data amounted to 26% of the sales of the firms that held them on average, and to 60% of sales at the 90th percentile. Although these firms represented only 4% of all firms, they accounted for 35% of value added and 27% of employment in the corporate sector.

In addition, firms with government contracts in 2010 also held 77% of the contract volume awarded in 2009, which suggests that holding government contracts is a persistent firm characteristic and that firms with contracts tend to rely on government demand on an on-going basis. We focus on this set of firms that held contracts in 2010 as those most likely to be affected by the procurement cuts, and henceforth refer to them as contract firms.

These contract firms held a substantial amount of credit from the banking system at the onset of the crisis, accounting for 16% of corporate lending. To put this figure in perspective, credit to contract firms corresponded to 64% of bank equity and 87% 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 that triggered the rise in sovereign yields.

[11] 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 highlights how the mechanism we study can be triggered by either sovereign or bank distress.

[12] Precrisis 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.

Figure 4 shows that contract firms were severely affected by the cuts to procurement. Between 2010 and 2015, the value added of contract firms dropped by 26% percent, versus 13% for other firms (panel a), and this decline seems to have led to a substantial deterioration in their ability to repay their loans. Contract firm NPLs grew seven-fold by 2015, while those for other firms only doubled (panel b). Prior to the crisis, both value-added and NPLs for the two sets of firms exhibited similar trends. Moreover, the post-crisis decline in value added and increase in NPLs were stronger for the firms supplying the government with products that suffered above-median procurement cuts, as one would expect if the shock to contract firms was caused by the cuts to public procurement.[13]

The growth in troubled loans from contract firms in turn had a significant effect on banks, as shown in figure 5. Between 2010 and 2015, NPLs from these firms increased by an amount equivalent to 13.4% of precrisis total bank equity. We see this as a lower bound for the impact of the procurement cuts on bank balance sheets, since it only includes firms directly exposed to government contracts. In all likelihood, the cuts also impacted other firms through supply chain relationships with contract firms.

For comparison, we estimate that the decrease in the market value of precrisis bank holdings of domestic sovereign debt attained a maximum of 14.9% of precrisis equity in early 2012.[14] 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 Agency (EBA) in late 2011 to face potential sovereign debt losses. This amounted to 14.5% of precrisis equity, very close to our estimate.[15] 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 central focus of the

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

[14]Our estimate is based on data on domestic sovereign 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).

[15]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.

literature on the sovereign-bank nexus (Acharya et al., 2014; Gennaioli et al., 2014; Brunnermeier et al., 2016; Farhi and Tirole, 2018).

Moreover, figure 5 shows the impact of the sovereign debt shock was relatively short-lived. Sovereign yields dropped sharply after ECB President Mario Draghi's famous "whatever it takes" speech in July 2012, effectively defusing the sovereign-debt driven loop,[16] as can be seen in figure 1. This drop not just erased any losses but eventually generated large gains in the market value of domestic sovereign debt holdings (Acharya et al., 2019). In constrast, the contract firm NPL shock persisted well beyond the acute phase of the crisis, as did the procurement cuts documented in figure 3.

4.2 Effect on credit supply

4.2.1 Methodology

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 contract firms and how exposed firms were to government contracts. We measure the former through the fraction of credit to contract firms over bank equity, and the latter through the share of contracts in firm sales:

$$Contract\ Exposure_b = \frac{\sum_i^n Credit_{ib} \times \frac{Contracts_i/Sales_i}{\sum_i^n \frac{1}{n} Contracts_i/Sales_i}}{Equity_b} \tag{1}$$

where b and i denote banks and firms, and n is the number of firms in the sample. Credit and bank equity are measured in 2010Q1, and contract amounts and firm sales in 2010.[17] We divide the share of contracts in firm sales by its sample mean, so that an increase in

[16] https://www.ecb.europa.eu/press/key/date/2012/html/sp120726.en.html

[17] Most contracts in the data are short term, but some span several quarters or years. We do not observe the timing of payments or when firms recognize sales on their books, but in order to approximate this we allocate the awarded amount equally across quarters whenever the contract term exceeds one quarter.

our exposure measure can be interpreted as an increase in the share of credit to contract firms in total assets for the mean level of firm exposure to government contracts. We consider alternative definitions of exposure in our robustness tests presented in the appendix.

Empirical strategy We estimate the effect of exposure to procurement on credit supply at the bank-firm relationship level by exploiting within industry-municipality-quarter and within relationship variation in credit across banks with different levels of precrisis contract exposure. We contrast our results with those from a within firm-quarter specification, which allows us to control for time-varying firm-level credit demand (Khwaja and Mian, 2008), in the sample of firms with at least two banking relationships. Our specification has two advantages over the within firm-quarter estimator. First, we are able to include single-relationship firms, which represent 64% of firms and 26% of credit in our sample, thus strengthening the external validity of our results (Degryse et al., 2019). Second, it enables us to use the same specification throughout the analysis, including in our firm-level regressions, where we necessarily rely on cross-firm variation. Our estimating equation is a dynamic difference-in-differences:

$$\begin{aligned}\sinh^{-1} Y_{ibt} = &\sum_{\tau=2009Q1,\tau\neq 2010Q1}^{2015Q4} \alpha_{1\tau} Contract\ Exposure_b + \\ &\sum_{\tau=2009Q1,\tau\neq 2010Q1}^{2015Q4} \alpha_{2\tau} X_b + \rho_{ib} + \gamma_{jmt} + \epsilon_{ibt}\end{aligned} \tag{2}$$

Our dependent variable Y_{ibt} is the credit granted by bank b to firm i in quarter t. We use the inverse hyperbolic sine of credit, instead of the log, so we can account for extensive margin effects by assigning zeros to ended relationships. X_b is a set of precrisis bank controls, all measured in 2010Q1, which includes bank exposure to domestic sovereign debt over equity, total assets, leverage and an indicator for foreign-owned banks, whose

behavior may have also depended on the parent group's financial position.

We allow the coefficients on contract exposure and on bank controls to vary by quarter, to flexibly capture the dynamic effects of the procurement shock and potential confounders, and we make 2010Q4 the omitted quarter so that the coefficients can be interpreted as changes relative to the quarter immediately preceding the procurement cuts, which were implemented starting in 2011. The quarter-by-quarter coefficients also enable us to test for parallel trends before the crisis. Finally, ρ_{ib} are bank-firm relationship fixed effects and γ_{jmt} are industry-municipality-quarter fixed effects. We cluster standard errors at the bank-quarter level.

4.2.2 Analysis sample

We restrict our sample to 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.[18] There are 13 banking groups in Portugal that meet this condition, and together they accounted for 95% of corporate credit in 2010Q1.

We also restrict our bank-firm sample to relationships that existed in 2009 and 2010, and our firm-level sample to firms that existed in 2009 and 2010. Since we assign zeros to ended relationships and to firm exits in the 2011-2015 period, to account for extensive margin effects, this ensures both panels are balanced. We further restrict the sample to non-contract firms, that is those without government contracts in 2010, so that our estimates reflect only effects operating through the banking system, and not the direct effects of contract exposure on contract firms.

[18] In appendix table x we show that the results are very similar when we include all banks and instead drop very small credit relationships. Our results also hold when these relationships are included, but become noisier.

Summary statistics for our sample at the bank-firm and firm levels are given in table 3, and table 4 compares banks with above and below median contract exposure in 2010Q1. Banks with higher contract exposure were more exposed to domestic sovereign debt, smaller, employed somewhat higher leverage and were more likely to be foreign-owned than those with lower exposure, although the differences are mostly insignificant given the small number of banks in our sample. We include all of these variables in X_b in our regressions, in addition to including bank-firm fixed effects. Other than that, the two groups of banks were balanced in terms of liquidity, credit over total assets and NPL ratio.

4.2.3 Results

Figure 6 presents point estimates and 95% confidence intervals for the $\alpha_{1\tau}$ coefficients estimated from equation 2. Our identifying assumption is that, in the absence of government procurement cuts and conditional on our controls, credit would have followed similar paths across banks with different levels of contract exposure. Consistent with this assumption, contract exposure was largely unrelated with changes in credit prior to the cuts. After the cuts, contract exposure leads to a sizeable and persistent decline in credit. In line with the evidence on NPLs in figure 5, the effect strengthens over time.

We report this result in column 1 of table 5. To avoid cluttering our tables, we report yearly instead of quarterly coefficients for contract exposure.[19] By 2015, a one percentage point increase in contract exposure caused bank credit supply to decrease by 1.4% in our baseline specification. In column 2 we focus on the intensive margin only, by restricting the sample to relationships that survive until 2015. We find that over half of the effect is driven by reductions in credit among surviving relationships.

[19] We re-estimate (2) replacing quarter with year interactions for contract exposure and for the bank controls. The 2010 interactions correspond to Q1, Q2 and Q3 only, and 2010Q4 remains the omitted category.

As mentioned above, one concern with our specification is that the results could be driven by changes in credit demand, not supply. To give an example, some non-contract firms may be connected to contract firms through supply chains, and be negatively affected 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 where we replace industry-municipality-quarter fixed effects with firm-quarter fixed effects, which absorb time-varying firm-level credit demand (Khwaja and Mian, 2008). This requires restricting the sample to firms with at least two banking relationships in 2010, and columns 3 and 4 present coefficients from our baseline and within firm-quarter specifications estimated in this sample. The coefficients in the two specifications are similar, and in fact slightly larger in the within firm-quarter specification. This supports the validity of our design and suggests our baseline estimates are conservative.

One additional concern is that construction accounts for 42% of contract volume in our data (table **??**), and the results might be driven by exposure to the construction sector, rather than to public procurement. In column 5 we control for the share of credit to the construction sector in bank equity, interacted with time, and the coefficients on contract exposure are slightly smaller but broadly similar to column 1. Lastly, exposure to the EBA capital exercise in 2011 has been shown to affect credit supply in this period (Blattner et al., 2021; Degryse et al., 2021). In column 6 we show that controlling for exposure to the EBA shock, also interacted with time, again yields similar results. We present several other robustness checks in appendix table x.

4.3 Effect on firms

4.3.1 Credit

To evaluate the impact of the credit supply shock documented in the previous section at the firm level, we need to ask to what extent firms were able to substitute credit from less exposed banks for credit from more exposed banks. To do that we must first aggregate the bank level variables in equation (2) 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. To capture extensive margin effects we assign zeros to firm exits, as we did with relationships.

We estimate this firm-level regression using our baseline specification from equation (2), replacing bank-level variables with their firm-level counterparts, and bank-firm with firm fixed effects. We plot the estimated coefficients on contract exposure in figure 7. As in our bank-firm results, credit for firms borrowing from banks with different levels of contract exposure followed similar trends before the procurement cuts. After the cuts, firms with more exposed lenders suffered a sharp decline in credit, which again grew over time.

We report annual coefficients corresponding to this plot in column 1 of table 7. By 2015, the cut amounted to 0.8% of credit for each percentage point of contract exposure, slightly over half of our relationship-level estimates. This implies that firms were able to substitute almost half of the credit they lost from more exposed banks. In column 2 we report results for the intensive margin, by conditioning the sample on firm survival up to 2015. Only about one sixth of the effect is driven by the intensive margin, with firm exit accounting for the remainder.

4.3.2 Real outcomes

Our last set of firm-level findings focus on the real effects of the procurement-driven shock to credit supply. We take the firm-level analog of equation (2) that we used for firm credit and estimate it for several other firm outcomes, with t now indexing years rather than quarters since our outcomes are observed annually. Since firm outcomes are available for earlier years, and in order to still be able to test for differences in precrisis trends, we extend our sample period back in time to start in 2007. Our main focus is on the effect on firm value added, which we map into an effect on aggregate output below.

We plot the coefficients for value added in figure 8. In line with the effect on credit, there are no significant differences in trends between 2007 and 2010, prior to the procurement cuts. After the cuts, firms borrowing from more exposed banks experience a sizable and persistent decline in value added. We report these coefficients in column 1 of table 7. By 2015, a one percentage point increase in contract exposure led to a 0.3% decline in value added.

Column 2 shows that all of this decline was driven by the extensive margin of firm exit. The intensive margin coefficients for surviving firms are small and actually positive. The remaining columns present results for firm sales, assets, cash and employment, all of which are similar to value added. The intensive margin effects for these outcomes, which we do not report, are also positive but insignificant. We conclude that firm output was significantly affected along the extensive margin by the credit supply shock.

4.4 Aggregate implications

To what extent can the credit supply shock induced by the cuts to public procurement account for the aggregate decline in lending and output in Portugal in the aftermath of the crisis? We exploit our firm-level results to answer this question in a partial equilib-

rium framework, following the approach developed by Chodorow-Reich (2014).

We assume that the set of banks that lent to the firm at the 5th percentile of the distribution of contract exposure (0.074) in our firm-level sample were unaffected by the shock, and we calculate a counterfactual where all firms borrowed from these unconstrained banks using our difference-in-differences estimates. This is a conservative assumption, since if these banks were still affected by the procurement shock then we will understate the true effect. Alternatively, we could calculate a counterfactual where firms borrow from a bank with zero contract exposure, but this would involve a degree of extrapolation from our results that may not be warranted. Formally, we define the counterfactual outcome at time t if a firm had borrowed from the bank in the qth percentile of contract exposure as:

$$\log Y_{it}(q) = \log Y_{it} + \hat{\beta}_{1t} \left(Contract\ Exposure_i - Contract\ Exposure_q \right) \tag{3}$$

where $\hat{\beta}_{1t}$ is estimated from our firm-level estimates of equation (2). We then sum the losses caused by the shock across firms and divide by the aggregate drop in Y to get the fraction of the drop explained by the credit supply shock in year t:

$$\frac{\sum_{Y_{it}(q)>Y_{it}} [Y_{it}(q) - Y_{it}]}{\sum_i (Y_{i2010} - Y_{it})} \tag{4}$$

Finally, summing the numerator and denominator in (4) across years yields the fraction of the cumulative drop in Y explained by the shock:

$$\frac{\sum_{t=2011}^{2015} \sum_{Y_{it}(q)>Y_{it}} [Y_{it}(q) - Y_{it}]}{\sum_{t=2011}^{2015} \sum_i (Y_{i2010} - Y_{it})} \tag{5}$$

As noted above, this is a partial equilibrium exercise, which assumes that the credit supply shock had no effect on firms that borrowed from unaffected banks. In general

equilibrium, there are two opposing effects on these unconstrained firms (Chodorow-Reich, 2014). First, some output is reallocated from firms that borrowed from affected banks, and became financially constrained as a result, to unconstrained firms, attenuating the aggregate effect of the shock. This is driven by a decrease in the relative price of output for unconstrained firms and by a decline in the equilibrium wage, as constrained firms raise prices and reduce their labor demand. Second, the output reduction for constrained firms reduces demand for the output of unconstrained firms, through complementarities across goods. This causes unconstrained firms to contract, amplifying the shock. The net effect of these two channels can be positive or negative, so that the effect of the shock in general equilibrium can be larger or smaller than our partial equilibrium estimates.

Chodorow-Reich (2014) develops and calibrates a model to quantify these general equilibrium effects of a credit supply shock, and contrasts them with the partial equilibrium effect obtained from a difference-in-differences specification like ours. He finds that general equilibrium effects can either magnify or dampen the effect of the shock depending on parameter values, but that across a range of plausible calibrations they remain quantitatively small. This conclusion is echoed by Arellano et al. (2020) using an alternative model and calibration in the context of the eurozone crisis in Italy. We therefore believe our partial equilibrium estimates offer a reasonable approximation to the aggregate effect of the shock.

We start with lending. We calculate credit lost for each year in our sample using equation (3) and our baseline coefficient estimates from figure 7. Table 8 reports the aggregate drop in real corporate credit from 2011 to 2015 relative to its value in 2010, along with the fraction of that drop that is explained by the credit supply shock. This fraction ranges from 97% in 2011 to 37% by 2015. Summing across years using (5), we find that our estimates can account for 46% of the cumulative loss in real credit in this period.

We then repeat the exercise for output, letting Y equal value added and using the

coefficients from figure 8. We obtain similar results using the drop in corporate real value added or in real GDP in the denominator of (4), and we report the latter in table 8. As with credit, we find that the shock can explain nearly all of the loss in real GDP in the early stages of the crisis (90% in 2011) and a lower but still sizable fraction in later stages, from 30% in 2013 to 47% in 2015. Overall, the shock can account for 39% of the cumulative output loss in this period.

Having computed the output losses caused by the credit supply shock, it is straightforward to calculate an implied fiscal multiplier at different horizons, by dividing the lost output by the change in real procurement spending over the same period. We report the change in procurement relative to 2010 and the implied multiplier at each horizon in table 8. The multiplier ranges from 0.7 at the one year horizon, in 2011, to around 0.5 at the two to five year horizons, from 2012 to 2015.

We stress that these numbers correspond to the multiplier associated with our mechanism, which operates through bank credit supply, not to the total multiplier associated with the procurement cuts in Portugal. What our results tentatively suggest, ignoring general equilibrium effects, is that the total multiplier would be 0.7 higher at the one-year horizon, for example, than the multiplier working through the standard aggregate demand channel.

5 Conclusion

This paper shows that the link between governments and banks through firms with public procurement contracts can significantly amplify banking stress during a crisis, with large effects on credit supply and output. We focus our analysis on the case of Portugal in the eurozone crisis, but also show that the large procurement cut we study does not stand out compared to other crisis-hit countries, namely Greece, Ireland, Italy and

Spain, and that large procurement cuts are not rare occurrences among OECD countries. Additional research on this mechanism in other countries and time periods would shed valuable light on how broadly it operates and in what contexts.

Our results raise clear policy issues on two fronts. First, in the design of policies aiming to break the sovereign-bank loop that attracted so much attention during the eurozone crisis. Focusing on the role of sovereign debt in bank balance sheets is insufficient, as it does not address the indirect link we document. Second, in the design of lender-of-last-resort interventions for sovereigns. Sharp fiscal consolidations centered around cuts to public procurement in periods of sovereign and bank distress may amplify fiscal multipliers considerably. We do not discuss possible policy solutions in either case, and see those as important topics for future study.

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Table 1: Contract summary statistics

	Mean	P10	P50	P90	% Contracts	% 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
Architectural 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 services	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 fishing	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 government contracts in our dataset for 2010. Product classifications are based on Common Procurement Vocabulary (CPV) codes reported in each contract, which we aggregate to the two-digit level here.

Table 2: Contract firm summary statistics

	Contracts/Sales for Contract Firms				Contract Firms/All Firms			
Industry	Mean	P10	P50	P90	Firms	Value Added	Emp.	Credit
Total	0.26	0.00	0.07	0.60	0.04	0.35	0.27	0.16
By Sector								
Agriculture and farming	0.35	0.02	0.12	0.82	0.01	0.03	0.03	0.01
Mining and quarrying	0.09	0.01	0.03	0.24	0.08	0.13	0.22	0.22
Manufacturing	0.09	0.00	0.02	0.21	0.04	0.23	0.13	0.17
Electricity, gas, steam, water, air	0.01	0.00	0.00	0.02	0.04	0.34	0.63	0.11
Water and waste management	0.14	0.00	0.04	0.37	0.13	0.38	0.42	0.22
Construction	0.30	0.01	0.13	0.68	0.06	0.47	0.36	0.21
Wholesale and retail trade	0.08	0.00	0.02	0.20	0.04	0.34	0.25	0.20
Transportation and storage	0.27	0.00	0.08	0.58	0.02	0.36	0.29	0.31
Accommodation and food service	0.11	0.00	0.03	0.24	0.01	0.19	0.15	0.18
Information and communication	0.25	0.01	0.10	0.57	0.13	0.77	0.58	0.49
Real estate	0.35	0.01	0.16	0.90	0.00	0.01	0.01	0.00
Consultancy, scientific and technical	0.54	0.02	0.19	1.00	0.09	0.34	0.28	0.10
Administrative and support service	0.25	0.01	0.08	0.66	0.10	0.61	0.68	0.33
Education	0.38	0.01	0.18	0.94	0.04	0.20	0.16	0.21
Human health and social work	0.25	0.01	0.10	0.65	0.01	0.06	0.06	0.07
Arts, entertainment, sports	0.43	0.02	0.25	0.90	0.10	0.42	0.29	0.25
Other service	0.25	0.00	0.09	0.71	0.01	0.15	0.08	0.06

The first four columns of this table report summary statistics for the share of government contracts in firm sales among firms with government contracts in 2010. The last four columns report the share of these firms in the universe of firms in Portugal in terms of counts, value added, employment and credit, all measured in 2010.

Table 3: Sample summary statistics

Panel A: Bank-Firm Matched Sample					
	Mean	Std. Dev.	P10	P50	P90
Bank-Level Variables					
Contract Exposure	0.32	0.31	0.16	0.24	0.64
Sovereign Exposure	0.63	0.63	0.12	0.31	1.85
Total Assets (billion)	60.08	36.66	16.47	56.03	106.18
Leverage Ratio	0.08	0.03	0.05	0.08	0.10
Foreign Bank	0.19	0.40	0.00	0.00	1.00
Liquidity	0.04	0.02	0.02	0.04	0.06
Credit/Assets	0.69	0.06	0.60	0.66	0.77
NPL/Total Credit	0.10	0.07	0.02	0.12	0.20
Relationship-Level Variables					
Total Credit (thousand)	373.88	3,662.85	1.34	33.33	460.59
Non-Performing Credit (thousand)	58.95	1,441.20	0.00	0.00	0.22
Observations	5,907,890				

Panel B: Firm-Level Sample					
	Mean	Std. Dev.	P10	P50	P90
Bank-Level Variables					
Contract Exposure	0.28	0.18	0.16	0.24	0.56
Sovereign Exposure	0.63	0.56	0.12	0.43	1.78
Total Assets (billion)	10.82	0.69	9.72	10.93	11.57
Leverage Ratio	0.08	0.02	0.05	0.08	0.10
Foreign Bank	0.17	0.32	0.00	0.00	0.88
Liquidity	0.04	0.01	0.02	0.04	0.06
Credit/Assets	0.69	0.05	0.64	0.66	0.76
NPL/Total Credit	0.10	0.05	0.03	0.11	0.18
Firm-Level Variables					
Sales (thousand)	971.94	2,027.60	33.13	244.50	2,387.26
Value Added (thousand)	249.47	476.43	9.69	78.84	615.44
Assets (thousand)	1,179.39	2,416.84	54.23	317.34	2,878.79
Cash (thousand)	70.04	141.47	0.74	16.49	184.52
Employment	10.13	15.99	1.00	4.00	25.00
Observations	941,303				

This table reports summary statistics for the bank-firm matched sample in Panel A and firm-level sample in Panel B. The bank-firm matched sample at the quarterly frequency over the 2009-2015 period is drawn from the Portuguese credit register and contains banks with at least 1 percent of the corporate credit market. We restrict the sample to firms without government contracts in 2010 and with at least one bank relationship in 2009 and 2010. Bank-level variables are measured in 2010Q1 and firm-level variables in 2010. Bank variables are aggregated to the firm level using loan shares as weights. Variable definitions are provided in appendix table A.1.

Table 4: Balance checks for banks

	Contract Exposure			
	Below median	Above median	Diff.	S.E.
Contract Exposure	0.163	0.760	-0.597**	(0.206)
Sovereign Exposure	0.395	0.875	-0.480	(0.359)
Total Assets (billion)	10.402	9.599	0.803	(0.586)
Leverage Ratio	0.075	0.057	0.019	(0.018)
Foreign Bank	0.286	0.500	-0.214	(0.290)
Liquidity	0.040	0.037	0.002	(0.011)
Credit/Assets	0.735	0.753	-0.019	(0.054)
NPL/Total Credit	0.099	0.102	-0.003	(0.047)
Observations	7	6		

This table compares the precrisis (2010q1) characteristics of banks with below and above median government contract exposure. We report means for each group of banks, the difference in means and the standard error of the difference in means. The sample includes only banks with a market share of more than 1 percent of the corporate credit market in Portugal. Variable definitions are provided in appendix table A.1. ***, ** and * indicate statistical significance at the 1%, 5% and 10% level, respectively.

Table 5: Effect of contract exposure on bank-firm level credit

			2+ Relationships			
	Baseline	Intensive margin	Baseline	Within firm	Additional controls	
	(1)	(2)	(3)	(4)	(5)	(6)
Contract Exposure ×						
2009	-0.077	-0.016	-0.106	-0.111	-0.119*	-0.063
	(0.076)	(0.101)	(0.082)	(0.091)	(0.068)	(0.065)
2010Q1-Q3	-0.016	0.006	-0.026	-0.028	-0.040	-0.019
	(0.074)	(0.096)	(0.080)	(0.089)	(0.070)	(0.050)
2011	-0.135**	-0.185*	-0.025	-0.043	-0.051	-0.043
	(0.068)	(0.099)	(0.072)	(0.077)	(0.072)	(0.048)
2012	-0.308***	-0.344***	-0.166	-0.262**	-0.110	-0.101
	(0.094)	(0.098)	(0.104)	(0.113)	(0.097)	(0.093)
2013	-0.588***	-0.416***	-0.514***	-0.705***	-0.377***	-0.325***
	(0.089)	(0.094)	(0.102)	(0.113)	(0.094)	(0.077)
2014	-1.016***	-0.630***	-1.019***	-1.252***	-0.877***	-0.783***
	(0.104)	(0.096)	(0.123)	(0.124)	(0.123)	(0.089)
2015	-1.442***	-0.876***	-1.562***	-1.825***	-1.351***	-1.208***
	(0.092)	(0.105)	(0.102)	(0.100)	(0.102)	(0.081)
Bank Chars × Year	Yes	Yes	Yes	Yes	Yes	Yes
Ind. × Mun. × Quarter FE	Yes	Yes	Yes	No	Yes	Yes
Firm × Quarter FE	No	No	No	Yes	No	No
Bank-Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Const. Exposure × Year	No	No	No	No	Yes	No
EBA × Year	No	No	No	No	No	Yes
Observations	6,228,725	3,138,403	3,478,247	3,478,247	6,228,725	6,228,725
Adjusted R^2	0.672	0.823	0.676	0.811	0.672	0.672

This table presents estimates from credit regressions using the quarterly bank-firm matched sample. The dependent variable is the inverse hyperbolic sine of total credit. Contract exposure is the fraction of credit to contract firms in the bank's loan portfolio, weighted by the share of contracts in firm sales, measured in 2010Q1. Bank controls include sovereign debt exposure, total assets, leverage, liquidity and a dummy for foreign ownership, all measured in 2010Q1. We use yearly instead of quarterly interactions for contract exposure to preserve readability, and we do the same for bank controls for consistency. The yearly coefficients for 2010 include only Q1, Q2 and Q3, and the omitted time period is 2010Q4. The sample consists of banking relationships that existed in 2009 and 2010 held by firms without government contracts in 2010. Column 2 restricts the sample to relationships that survived until 2015. Columns 3 and 4 restrict the sample to firms with at least two banking relationships. Column 5 adds exposure to the construction sector in 2010Q1 to the set of bank controls. Column 6 adds a dummy for whether the bank was included in the EBA capital exercise in 2011. Standard errors clustered at the bank-quarter level are reported in parentheses. ***, ** and * indicate statistical significance at the 1%, 5% and 10% level, respectively.

Table 6: Effect of contract exposure on firm-level credit

	Baseline (1)	Intensive margin (2)
Contract Exposure ×		
2009	-0.077***	-0.020
	(-3.48)	(-0.84)
2010Q1-Q3	-0.053***	-0.028*
	(-3.89)	(-1.87)
2011	-0.349***	-0.094***
	(-8.43)	(-5.86)
2012	-0.542***	-0.110***
	(-9.09)	(-4.52)
2013	-0.703***	-0.104***
	(-9.74)	(-3.47)
2014	-0.818***	-0.144***
	(-10.06)	(-4.17)
2015	-0.816***	-0.139***
	(-9.20)	(-3.51)
Bank Chars × Year	Yes	Yes
Ind. × Mun. × Quarter FE	Yes	Yes
Firm FE	Yes	Yes
Observations	3,816,495	2,484,158
Adjusted R^2	0.668	0.820

This table presents estimates from credit regressions using the quarterly firm-level sample. The dependent variable is the inverse hyperbolic sine of total credit. Contract exposure is the fraction of credit to contract firms in the bank's loan portfolio, weighted by the share of contracts in firm sales, measured in 2010Q1. Bank controls include sovereign debt exposure, total assets, leverage, liquidity and a dummy for foreign ownership, all measured in 2010Q1. Contract exposure and bank controls are aggregated to the firm level using bank shares of total firm credit. We use yearly instead of quarterly interactions for contract exposure to preserve readability, and we do the same for bank controls for consistency. The yearly coefficients for 2010 include only Q1, Q2 and Q3, and the omitted time period is 2010Q4. The sample consists of firms without government contracts in 2010 and with at least one banking relationship in 2009 and 2010. Column 2 restricts the sample to firms that survived until 2015. Standard errors clustered at firm level are reported in parentheses. ***, ** and * indicate statistical significance at the 1%, 5% and 10% level, respectively.

Table 7: Effect of contract exposure on firm-level real outcomes

	Value Added					
	Baseline	Intensive margin	Sales	Assets	Cash	Employees
	(1)	(2)	(3)	(4)	(5)	(6)
Contract Exposure ×						
2007	-0.082*	-0.068*	-0.023	-0.089***	-0.145**	-0.045***
	(-1.66)	(-1.69)	(-0.68)	(-4.19)	(-2.21)	(-3.03)
2008	-0.069	-0.063*	-0.017	-0.070***	-0.074	-0.023*
	(-1.60)	(-1.84)	(-0.55)	(-3.78)	(-1.20)	(-1.85)
2009	-0.016	-0.039	-0.002	-0.029*	0.050	-0.008
	(-0.44)	(-1.41)	(-0.06)	(-1.83)	(0.92)	(-0.80)
2011	-0.235***	0.034	-0.103***	-0.162***	-0.162**	-0.047***
	(-3.92)	(1.10)	(-3.03)	(-5.16)	(-2.35)	(-3.48)
2012	-0.322***	0.088**	-0.176***	-0.232***	-0.321***	-0.086***
	(-3.83)	(2.17)	(-3.77)	(-5.23)	(-3.75)	(-4.55)
2013	-0.332***	0.096*	-0.196***	-0.285***	-0.346***	-0.109***
	(-3.31)	(1.75)	(-3.57)	(-5.39)	(-3.57)	(-4.89)
2014	-0.324***	0.143**	-0.212***	-0.303***	-0.309***	-0.114***
	(-2.91)	(2.18)	(-3.51)	(-5.16)	(-2.95)	(-4.66)
2015	-0.317***	0.127*	-0.201***	-0.267***	-0.255**	-0.112***
	(-2.70)	(1.70)	(-3.16)	(-4.28)	(-2.32)	(-4.31)
Bank Chars × Year	Yes	Yes	Yes	Yes	Yes	Yes
Ind. × Mun. × Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	896,964	710,816	896,964	896,964	896,964	896,964
Adjusted R^2	0.610	0.678	0.686	0.672	0.540	0.789

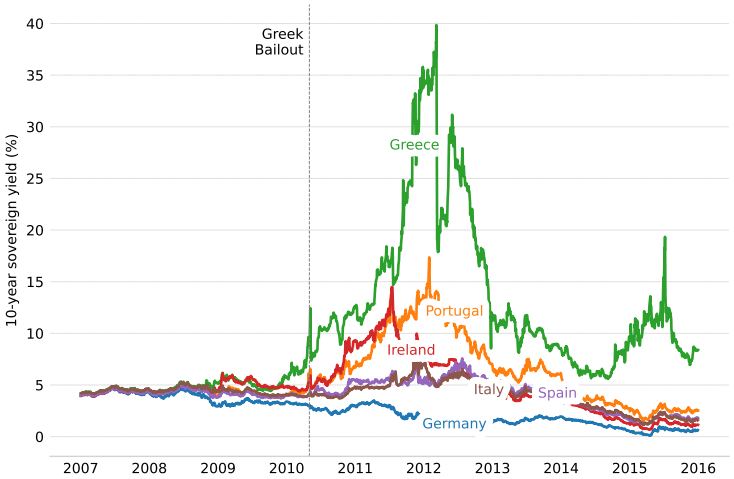
This table presents estimates from regressions for several firm outcomes using the annual firm-level sample. The dependent variables are listed in each column. Contract exposure is the fraction of credit to contract firms in the bank's loan portfolio, weighted by the share of contracts in firm sales, measured in 2010Q1. Bank controls include sovereign debt exposure, total assets, leverage, liquidity and a dummy for foreign ownership, all measured in 2010Q1. Contract exposure and bank controls are aggregated to the firm level using bank shares of total firm credit. The sample consists of firms without government contracts in 2010 and with at least one banking relationship in 2009 and 2010. Column 2 restricts the sample to firms that survived until 2015. Standard errors clustered at firm level are reported in parentheses. ***, ** and * indicate statistical significance at the 1%, 5% and 10% level, respectively.

Table 8: Aggregate effects

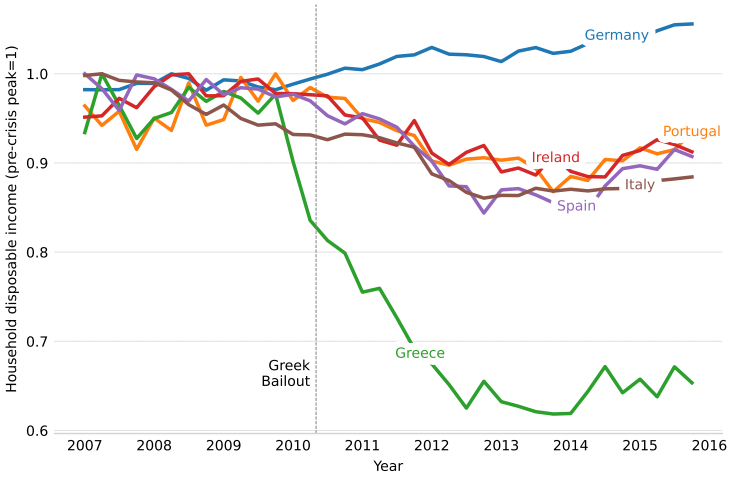
Year	2011	2012	2013	2014	2015	Cumulative
Credit						
Δ from 2010 (%)	-7.65	-20.05	-29.25	-34.81	-39.16	
Fraction explained	0.97	0.50	0.47	0.43	0.37	0.46
Output						
Δ from 2010 (%)	-1.70	-5.68	-6.55	-5.81	-4.13	
Fraction explained	0.90	0.34	0.30	0.33	0.47	0.39
Procurement						
Δ from 2010 (%)	-15.59	-27.78	-31.95	-32.37	-30.30	
Implied multiplier	0.74	0.52	0.46	0.44	0.48	0.50

This table presents partial equilibrium estimates for the aggregate effect of the procurement-driven credit supply shock. The fraction of the aggregate credit loss explained by the shock is computed as follows. We first estimate firm-level counterfactual credit for each year, using our regression estimates and assuming that the banks that lent to the firm at the 5th percentile of contract exposure were unaffected by the shock. Second, we sum the difference between actual and counterfactual credit across firms, and divide by the aggregate change in credit since 2010. The fraction of the output loss explained by the shock is computed analogously, using our value added regression and dividing the difference between actual and counterfactual value added by the aggregate change in real GDP. The implied multiplier is calculated by dividing the effect on output by the change in real procurement spending at each horizon relative to 2010.

Figure 1: The Eurozone crisis



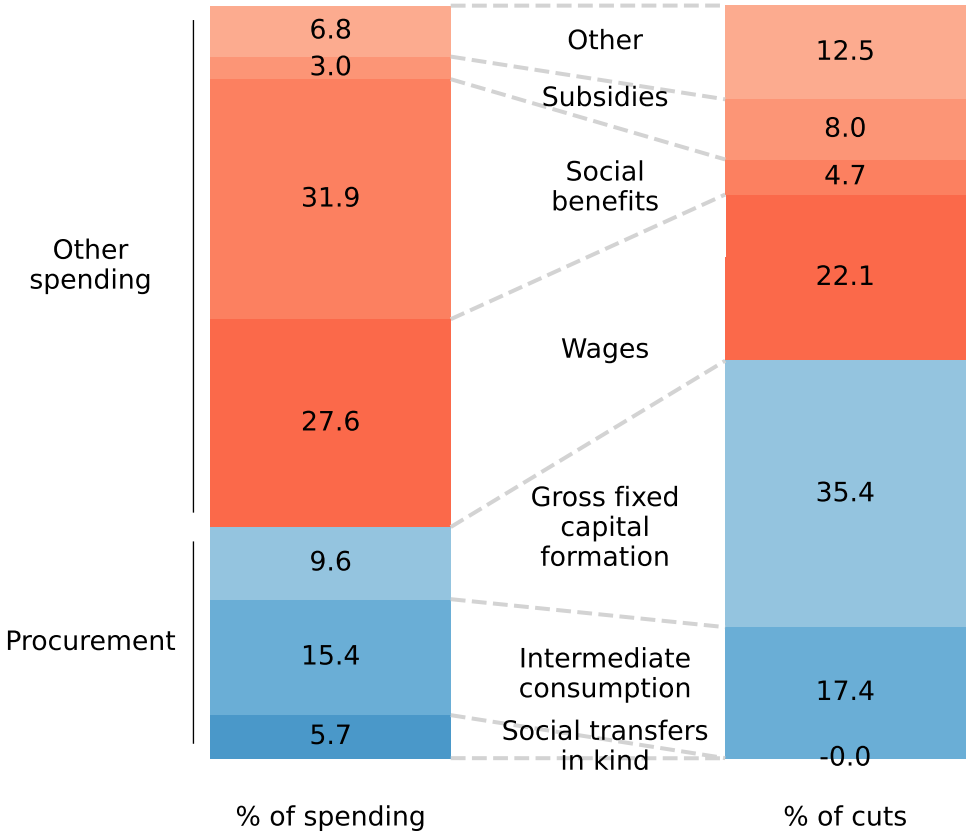
(a) Sovereign yields



(b) Household disposable income

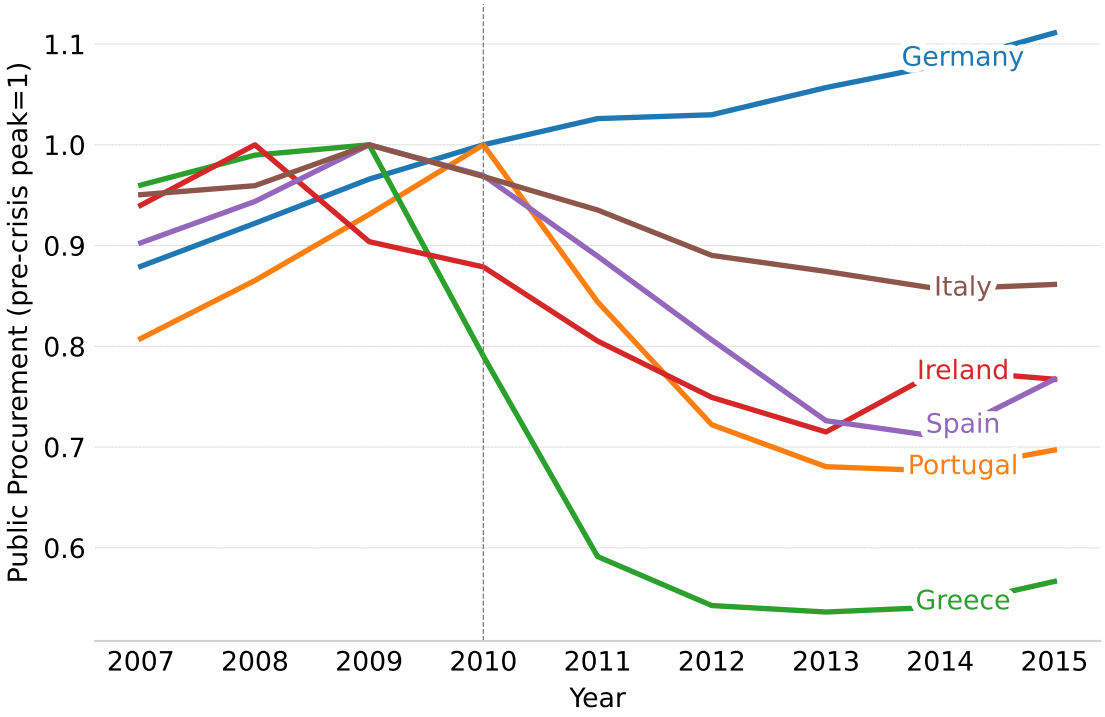
This figure plots the evolution of 10-year sovereign yields and real household disposable income per capita for the set of countries at center of the eurozone crisis and for Germany, for comparison. Sovereign yield data is from Refinitiv, and household income is from the OECD. We present household income 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: Composition of government spending and spending cuts in OECD countries



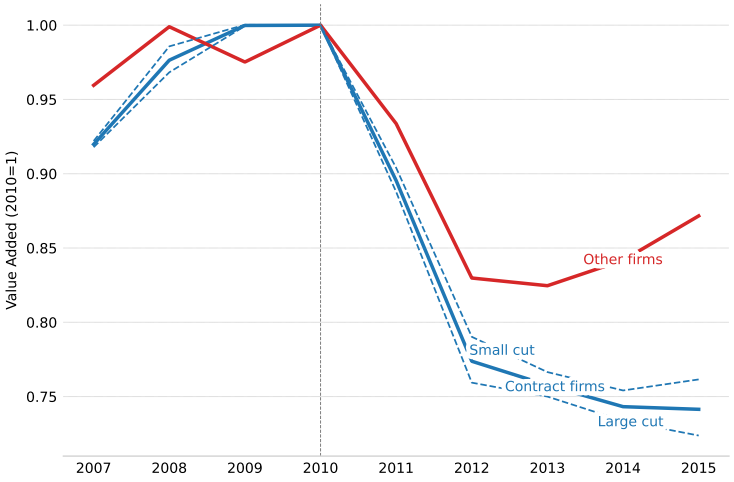
This figure characterizes the 83 instances of year-on-year cuts to real primary government spending of at least 1% that we identify for OECD countries between 1995 and 2018. We exclude capital transfers to ignore transitory fluctuations in spending driven by one-time items, such as bank recapitalizations. The left column shows the average composition of government spending in the year prior to the cuts, distinguishing between public procurement and other types of spending, each broken into their respective key components. The right column shows the average contribution of each component towards these spending cuts. Data on government spending is from the OECD.

Figure 3: Public procurement in the eurozone crisis

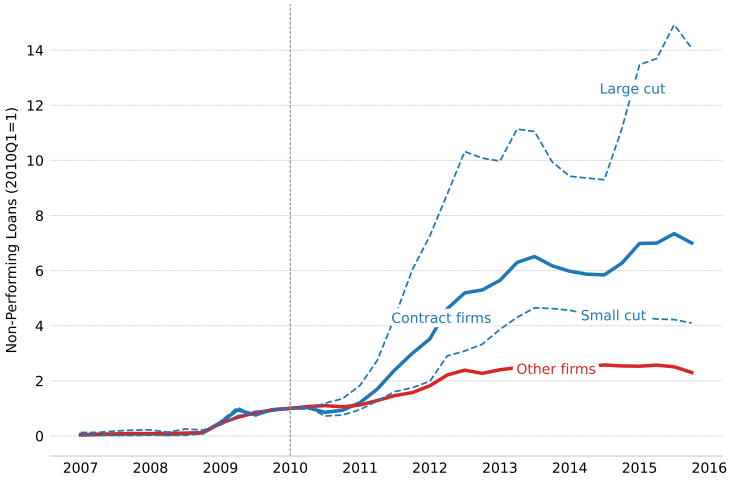


This figure presents the evolution of real public procurement spending for crisis-hit countries and for Germany, relative to their respective precrisis spending peaks. Precrisis peaks are defined as the highest level of procurement spending between 2007 and 2010. Data on procurement spending is from the OECD.

Figure 4: Impact of procurement cuts on contract firms



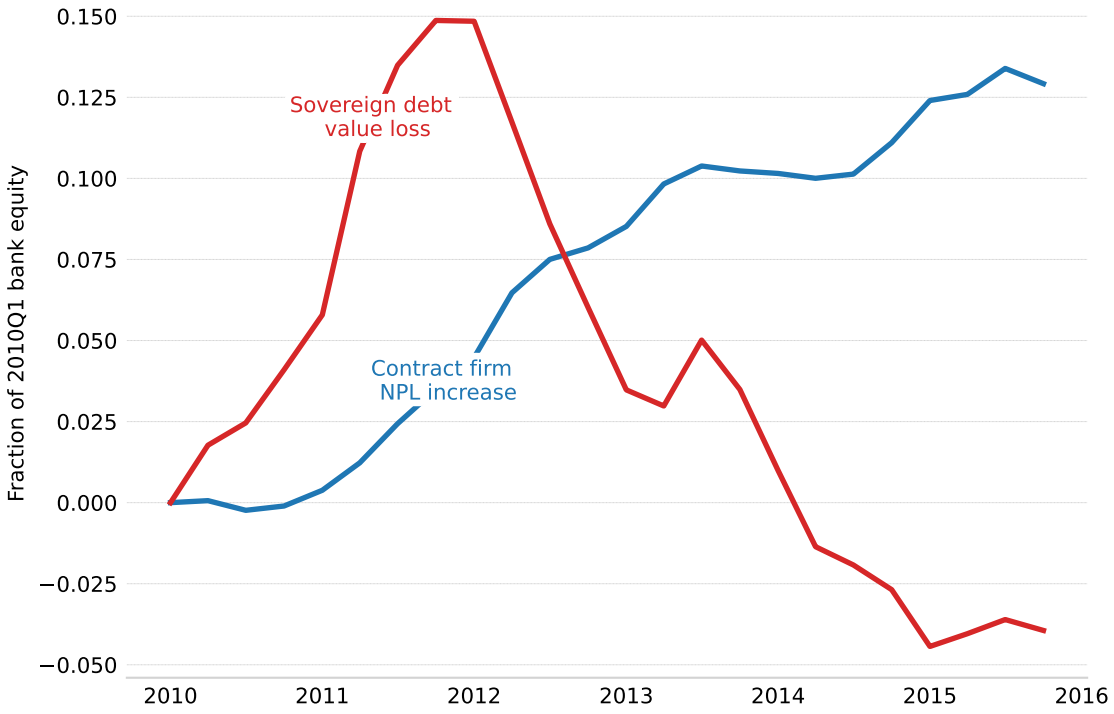
(a) Value added



(b) Non-performing loans

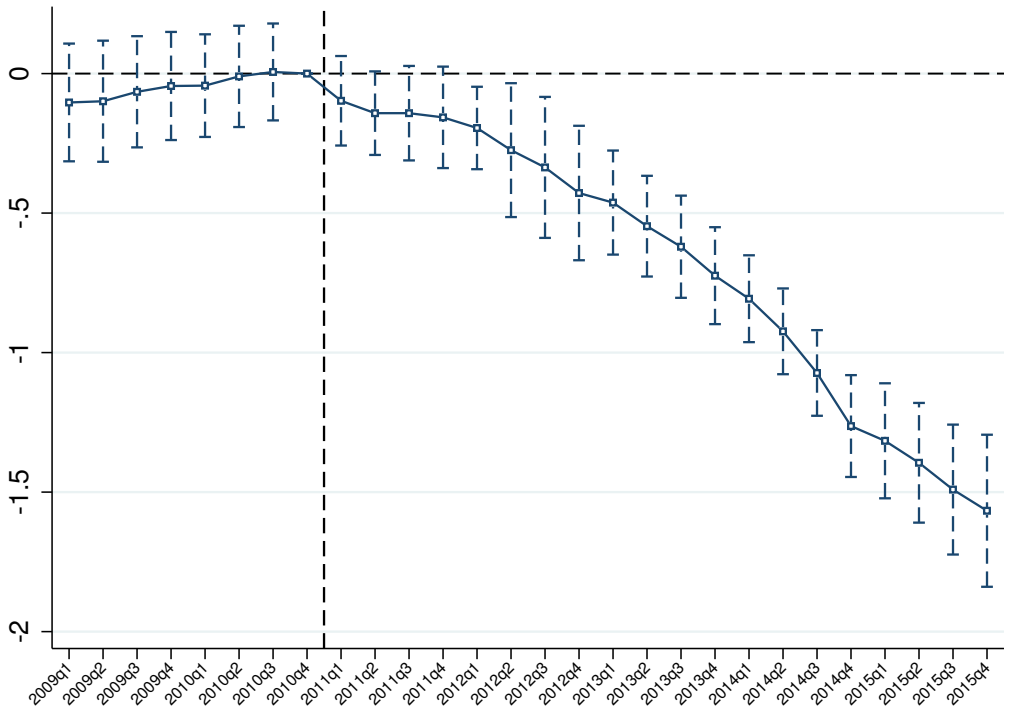
This figure presents the evolution of value added and NPLs for contract firms, i.e. those that held government contracts in 2010, versus other firms that existed in 2010. The dashed lines further separate contract firms into those supplying products that suffered above and below median procurement cuts. We use the 8-digit Common Procurement Vocabulary (CPV) codes reported in contracts to calculate product-level contract cuts. When a firm supplies more than one product, we take the average cut weighted by firm-level contract amounts in 2010.

Figure 5: Impact of the procurement and sovereign debt shocks on banks



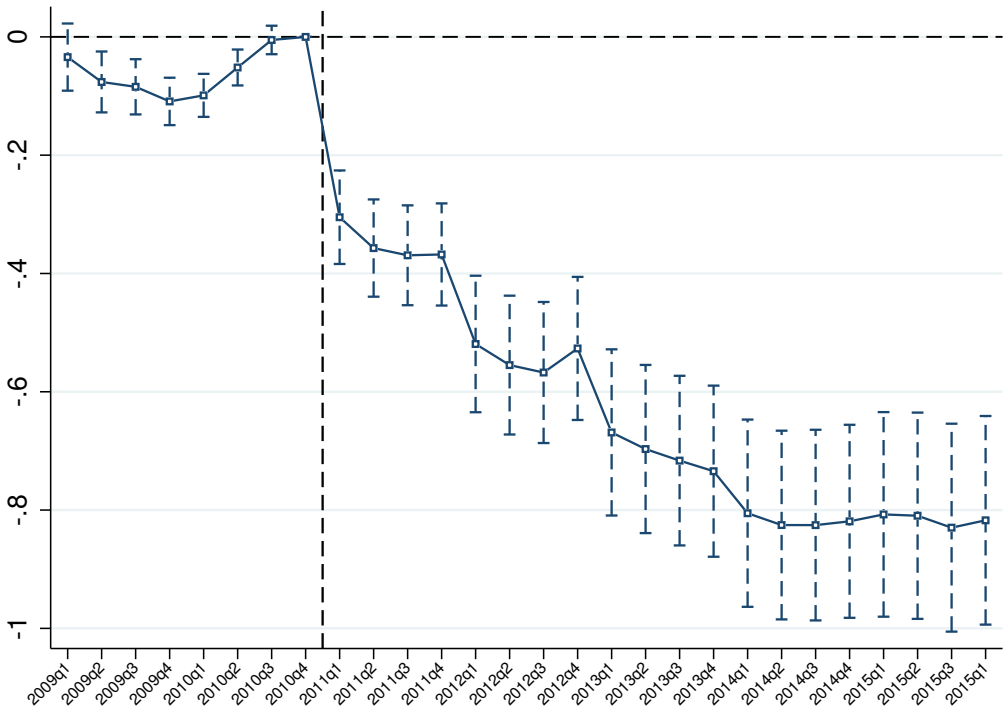
This figure plots the increase in NPLs from contract firms, i.e. those that held government contracts in 2010, along with the loss in the market value of bank domestic sovereign debt holdings, in the period between 2010Q1 and 2015Q4. Both series are plotted as a fraction of total bank equity in 2010Q1. Our estimate for the change in the 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 6: Effect of contract exposure on credit at the bank-firm level



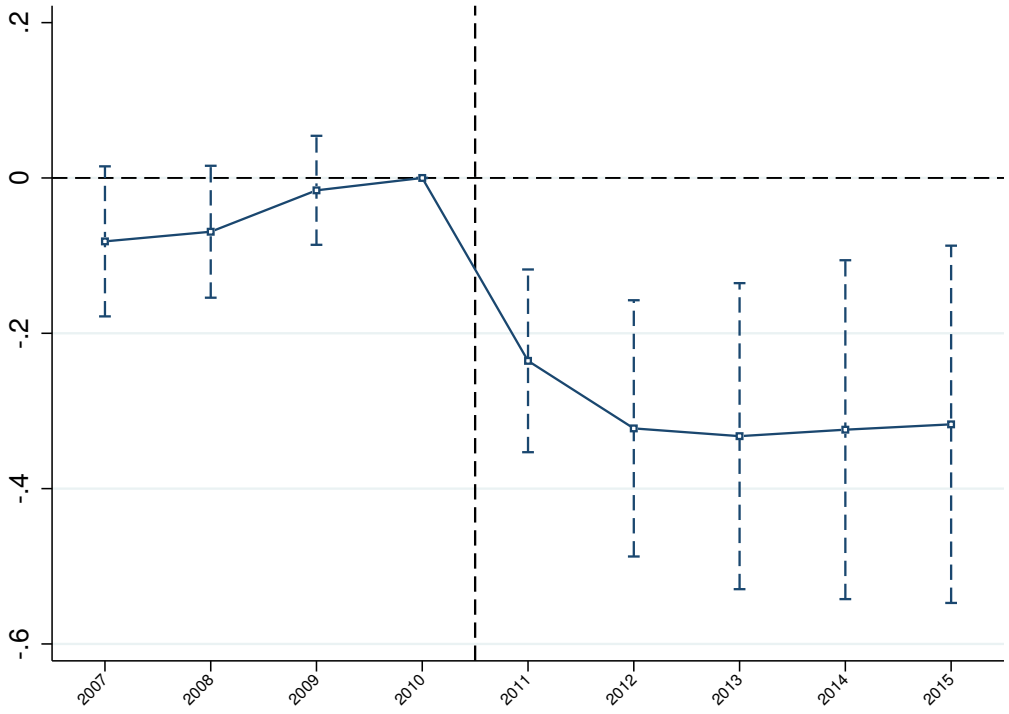
This figure shows point estimates and confidence intervals for the effect of contract exposure on credit supply at the bank-firm relationship level, estimated from equation (2). The sample is a balanced panel of all credit relationships that existed in 2009 and 2010 and were held by firms without government contracts. The dependent variable is the inverse hyperbolic sine of total credit, and we assign zeros to ended relationships. Standard errors are clustered at the bank-quarter level.

Figure 7: Effect of contract exposure on credit at the firm level



This figure shows point estimates and confidence intervals for the effect of contract exposure on credit supply at the firm level, estimated from equation (2). The sample is a balanced panel of all firms without government contracts that had at least one credit relationship in 2009 and 2010. The dependent variable is the inverse hyperbolic sine of total credit, and we assign zeros to firm exits. Standard errors are clustered at the firm level.

Figure 8: Effect of contract exposure on firm value added



This figure shows point estimates and confidence intervals for the effect of contract exposure on firm output, estimated from equation (2). The sample is a balanced panel of all firms without government contracts that had at least one credit relationship in 2009 and 2010. The dependent variable is the inverse hyperbolic sine of value added, and we assign zeros to firm exits. Standard errors are clustered at the firm level.

A Appendix

Table A.1: Variable definitions

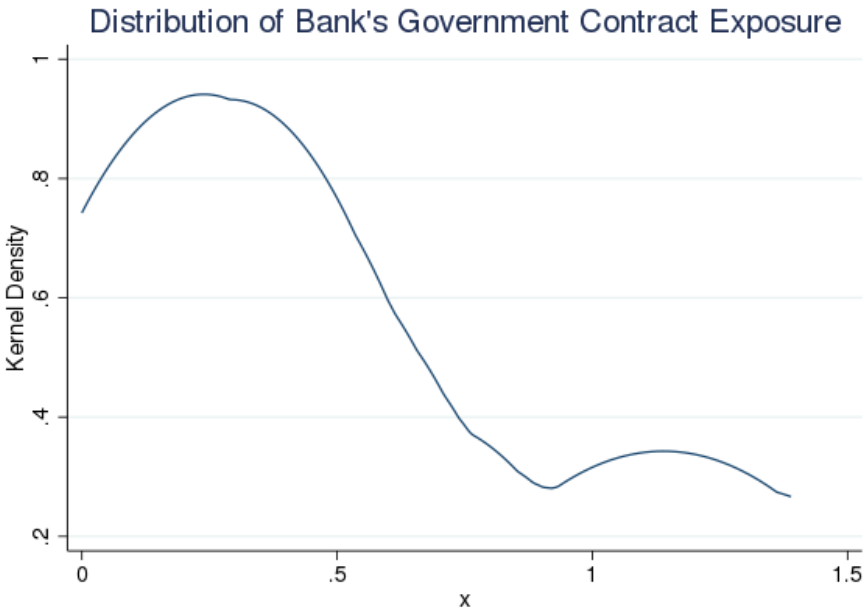
Bank Exposures	
Contract exposure	Bank exposure to firms with government contracts in 2010Q1 (see text for details)
Sovereign Exposure	Bank exposure to domestic sovereign debt, i.e., the ratio of sovereign debt holdings to total equity.
Bank Characteristics	
Total assets	Logarithm of bank's total assets.
Non-Performing Loans	Ratio of non-performing to total corporate loans.
Bank Leverage	Ratio of bank equity to total assets.
Bank Liquidity	Ratio of current to total assets.
Credit Characteristics	
Total Credit	Inverse hyperbolic sine of firm's total credit outstanding
Firm Characteristics	
Assets	Inverse hyperbolic sine of firm's total assets.
Sales	Inverse hyperbolic sine of firm's total sales.
Value Added	Inverse hyperbolic sine of firm's value added, where value added is the difference between sales (turnover plus remaining income) and intermediate input costs (i.e., costs of goods sold and material consumed plus cost related to supplies and external services and indirect taxes).
Employment	Inverse hyperbolic sine of the number of employees.
Cash	Inverse hyperbolic sine of the firm's cash reserves.

Table A.2: Large procurement cuts in the OECD, 1995-2018

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

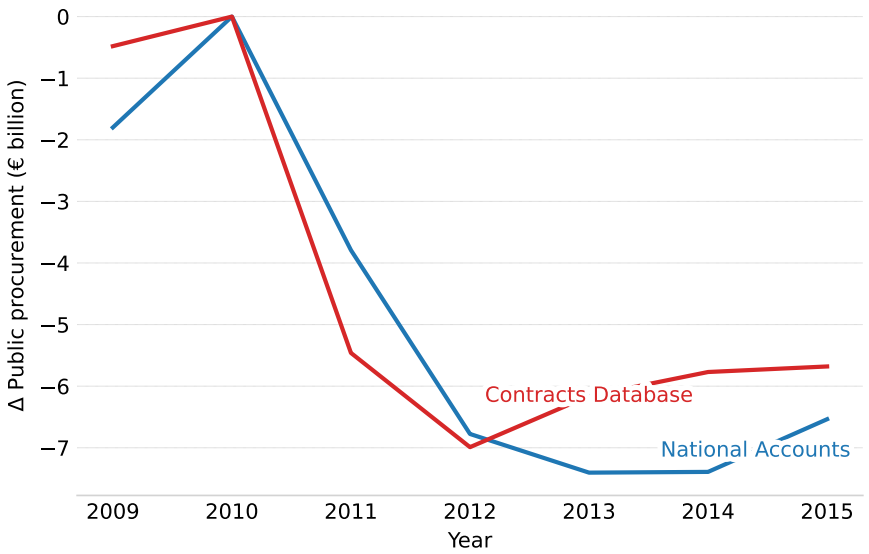
This table characterizes the 16 episodes of cuts to real procurement spending of at least 10% we identify among OECD countries between 1995 and 2018. When cuts happen in consecutive years we consider them to be part of the same episode. We drop cases where procurement increased by 10% or more in the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

Figure A.1: Distribution of contract exposure across banks



This figure shows the distribution of bank exposure to government contracts as of 2010Q1, for banks with a market share of at least 1% of the corporate credit market in Portugal.

Figure A.2: Procurement cuts in Portugal: National Accounts vs contract data



This figures compares the change in public procurement spending in Portugal in the crisis period calculated using System of National Accounts (SNA) data from the OECD and using our data on government 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. Data on government contracts is obtained from the BASE webportal and from the EU's Tenders Electronic Daily (TED) database.