

## Fiscal Policy and Credit Supply in a Crisis<sup>†</sup>

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*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 percent of GDP. We find that this cut saddled banks with nonperforming 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 E23, E44, E62, G01, G21, H57)*

Credit market turmoil can have significant macroeconomic effects, and macroeconomic shocks in turn can disrupt credit markets (Bernanke 1983; Bernanke and Gertler 1989). Credible estimates of these bidirectional effects are crucial for policy analysis and for disciplining models of macroeconomic fluctuations. While the impact of credit supply shocks on the real economy has been well documented (Chodorow-Reich 2014; Huber 2018), micro-level evidence on the reverse effect—how macroeconomic shocks influence credit markets—remains limited.

In this paper, we examine how a large fiscal shock affects credit supply in a financial crisis. The shock we study involves public procurement in the 2010–2011 European sovereign debt crisis. In Greece, Ireland, Italy, Portugal, and Spain, the countries at the epicenter of the crisis, procurement spending was cut by 1.7 percent

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to 7.2 percent of GDP as governments strove to restore access to capital markets. These cuts were a major component of fiscal consolidation efforts in these countries, accounting for 57 percent to 98 percent of the reductions in primary budget deficits achieved in the same period. Our analysis focuses on 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 percent of sales for the firms that held them, henceforth, called government contractors, and these firms accounted for 33 percent of value added in the corporate sector. In addition, banks were significantly exposed to public procurement. Bank lending to government contractors amounted to 19 percent of total corporate lending and 75 percent of total bank equity.

As the crisis hit, the government cut procurement by 4.3 percent of GDP. Government contractors subsequently experienced steep declines in output, and the resulting distress spilled over into bank balance sheets. Nonperforming loans (NPLs) from contractors increased almost sixfold in the following years, an amount equivalent to 13 percent of precrisis bank equity. For comparison, losses in the market value of precrisis domestic sovereign debt bank holdings, a channel thought to have played a central role in the crisis (Acharya, Drechsler, and Schnabl 2014; Gennaioli, Martin, and Rossi 2014), attained a similar peak of 14 percent of bank equity in early 2012. But while the drop in sovereign yields from 2012 onward 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. This persistence can help explain why credit in crisis-hit countries remained depressed for several years after sovereign yields had normalized.

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 noncontractors 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 noncontractors 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.<sup>1</sup> 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.

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 percent of its bank-firm level counterpart. This suggests that firms were able to substitute 42 percent 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 percent). The high prevalence of firms with multiple relationships in Portugal (Kosekova et al. forthcoming) 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.

<sup>1</sup>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.

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 toward 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.<sup>2</sup> 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 percent of the drop in credit and 48 percent 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 10 times larger in a noncrisis setting. In a counterfactual using this higher elasticity, the aggregate effects we estimate fall

<sup>2</sup>Blanchard and Leigh (2013) find that fiscal multipliers in the European crisis were larger than anticipated by forecasters by about 1 in 2010–2011 and 0.4 in 2011–2013. To the extent that our mechanism was unanticipated, it can help explain these larger multipliers.

to about 30 percent 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.<sup>3</sup> We also analyze the 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, we add to the literature on fiscal policy transmission. It is well-known that fiscal multipliers increase in the presence of credit constraints, which makes current consumption more dependent on current income (Mankiw 2000). In models featuring this mechanism, such as Eggertsson and Krugman (2012); Kaplan and Violante (2014); and Brinca et al. (2016), 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 US cities. They conjecture that part of the effect may operate through improvements in the balance sheets of local contractors and their lenders.<sup>4</sup> We provide direct evidence of this mechanism in the case of a spending cut and quantify its impact on credit and output in a crisis.

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

<sup>3</sup>Another paper that studies a disruption in credit supply originating in the real sector is Federico, Hassan, and Rappoport (2025), who examine the effect of bank exposure to firms in Italy that were affected by increased competition from China, as a result of China's entry into the World Trade Organization.

<sup>4</sup>In contemporaneous work, and consistent with that view, Goldman, Iyer, and Nanda (2022) find that increases in defense spending also lower nonperforming loans and increase lending at the county level.

evidence on a specific mechanism at the firm level, which can be used to discipline models of the overall multiplier.

Third, 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.<sup>5</sup>

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. 2024; Hvide and Meling 2023) and facilitates access to credit through the use of contract revenues as collateral (Gabriel 2024). However, procurement is also associated with corruption (Porter and Zona 1993), favoritism (Burgess et al. 2015), and waste (Bandiera, Prat, and Valletti 2009). We contribute to this literature by showing that public procurement creates a link between governments and the financial system that may lead to fragility in times of crisis.

## I. Data

### A. 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 nongovernment public entities, such as state-owned enterprises, and includes some nonprocurement expenditures, potentially overstating the amount of procurement (OECD 2011). We use SNA data from OECD (2022b) to characterize the evolution of public procurement during the European sovereign debt crisis.

<sup>5</sup>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).

At the micro level, many countries make data on individual procurement contracts publicly available. In the European Union (EU), all contracts above a legally prescribed threshold must be published in the *Official Journal of the European Union*, and data on these contracts are made available online through the Tenders Electronic Daily (TED) database.<sup>6</sup> 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 percent of SNA-based public procurement in 2008 across EU countries (OECD 2011).<sup>7</sup> 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 2018). All public procurement contracts in nonexempt 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.<sup>8</sup> Data are available starting in 2009 and include information about the amount, date, and duration of the contracts, as well as the identification of contractors and awarding entities, including tax identifiers. One limitation of BASE is that it only includes comprehensive coverage of open tenders, the procedure typically adopted for the largest contracts, from 2011 onward. To overcome this, we complement BASE with data from TED, which we obtain through the Opentender web portal (DIGIWHIST 2020), taking care to avoid any duplication.

Our combined dataset accounts for 44 percent of SNA-based public procurement expenditure in Portugal in 2010, well above the 22 percent average covered by TED in 2008 across EU countries (OECD 2011). More importantly, Figure C.1 of the Supplemental 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 of the Supplemental 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 tenth 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

<sup>6</sup>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.

<sup>7</sup>These differences could be driven by several factors. First, as mentioned above, the SNA-based measure includes some nonprocurement 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 percent. 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.

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

distribution: The mean contract is worth €132,217, above the ninetieth percentile of €95,950. The vast majority of contracts (93 percent) take the form of outright awards, but these only account for 26 percent of contracting volume. The 7 percent of contracts awarded through open, negotiated, and restricted tenders tend to be much larger and account for the remaining 74 percent 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 percent), 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 percent), energy (5 percent), and sewage services (3 percent).

### *B. Loan, Bank, and Firm Data*

Using firm tax identifiers, we merge our contract data with loan, bank, and firm data from three administrative datasets managed by Banco de Portugal. Quarterly loan-level data from 2009 to 2015 come from BPLIM (2019), a database covering all credit exposures above €50 in Portugal. We collect quarterly bank characteristics from BPLIM (2021, 2023).<sup>9</sup> And we draw annual firm characteristics from BPLIM (2020), which includes detailed financial statements for all nonfinancial 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 percent and 97.5 percent 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 A1 in the Appendix provides definitions for the variables we use.

## **II. 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 1, panel A. This rise in yields brought all four countries under severe financial pressure, and eventually all but Italy received bailouts of their own. Ireland followed Greece in November 2010, Portugal was next in May 2011, and Spain in June 2012.

In order to restore access to capital markets and meet the bailout terms, these countries turned to aggressive fiscal consolidation efforts, which included the large

<sup>9</sup>We assign parent group equity-to-assets ratios, obtained from Moody's (2023), to foreign branches, as branches are not independent legal entities and their equity-to-assets ratios have limited economic meaning.

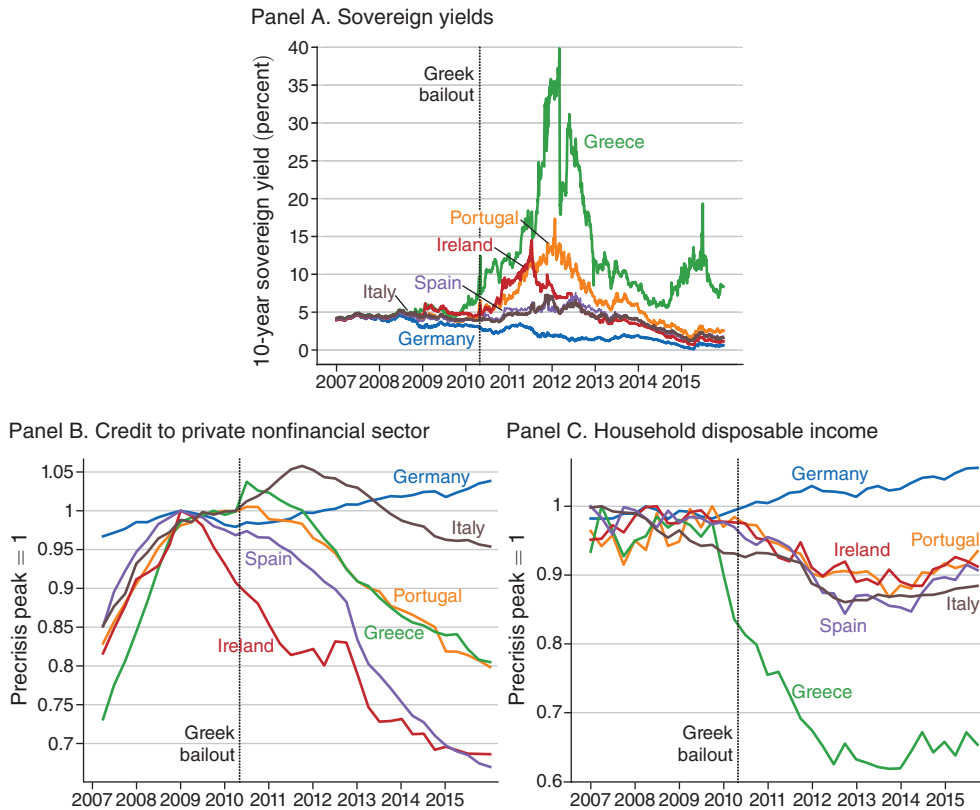


FIGURE 1. THE EUROPEAN SOVEREIGN DEBT CRISIS

*Notes:* Panel A plots ten-year sovereign yield data from LSEG (2022). Panel B plots credit data from ECB (2022), with the exception of credit from monetary and financial institutions in Greece, for which we use data from Bank of Greece (2022) that correct for a series break in June 2010. Panel C plots household income data from OECD (2022c). 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).

cuts to public procurement shown in Figure 2.<sup>10, 11</sup> In Portugal, the government cut procurement by 4.3 percent of precrisis GDP between 2010 and 2014, with almost 90 percent of the cut taking place between 2010 and 2012. This compares with an even stronger cut of 7.2 percent in Greece, similar cuts of 4.0 percent and 3.6 percent in Spain and Ireland, and a milder cut of 1.7 percent 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 percent in Portugal, 65 percent in Greece, 57 percent in Spain, 98 percent in Ireland,

<sup>10</sup>Table B.2 of the Supplemental Appendix shows that these large procurement cuts were not isolated events. We identify 16 episodes in 15 OECD countries where procurement was cut by at least 10 percent between 1995 and 2018. The average cut amounted to 20 percent, or 2.8 percent 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.

<sup>11</sup>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 raises the possibility of a negative feedback loop between procurement cuts and the deterioration of bank balance sheets.

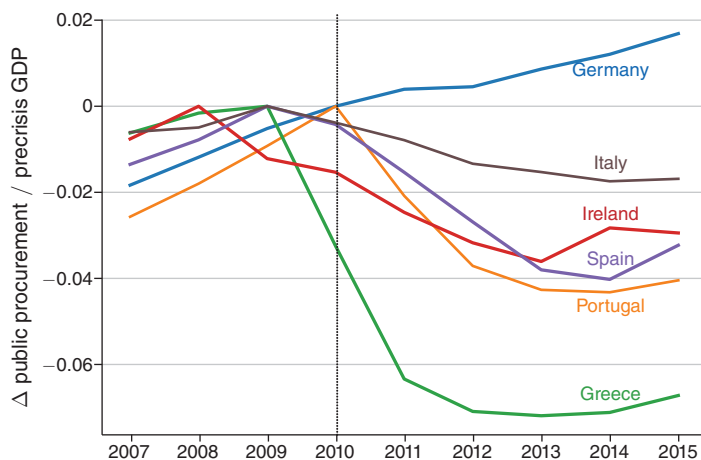


FIGURE 2. PUBLIC PROCUREMENT IN THE CRISIS

Note: This figure plots the change in real public procurement spending relative to its precrisis peak, as a fraction of precrisis GDP. Data are from OECD (2022a, b).

and 76 percent in Italy.<sup>12</sup> In contrast, public procurement in Germany remained on a stable upward trend throughout the crisis.<sup>13</sup>

Table B.3 of the Supplemental 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 percent of the sales of the firms that held them, on average, and to 57 percent of sales at the ninetieth percentile. Although these firms represented only 5 percent of all firms, they accounted for 33 percent of value added and 26 percent of employment in the corporate sector. We focus on the set of firms that held contracts in 2009 or 2010, which we refer to as government contractors, as those most likely to be affected by the cuts.

Government contractors held a substantial amount of credit from the banking system at the onset of the crisis, accounting for 19 percent of corporate lending (Table B.3 of the Supplemental Appendix). To put this figure in perspective, credit to contractors corresponded to 75 percent of bank equity and 108 percent of domestic sovereign debt bank holdings, including both bonds and loans. Here and throughout the paper, we measure precrisis exposures and bank characteristics in 2010:I, before the Greek bailout in May 2010 that triggered the rise in sovereign yields.

Figure 3, panels A and B show that contractors were severely affected by the cuts to procurement. Between 2010 and 2015, the value added of these firms dropped by 23 percent, versus 10 percent for other firms, and this decline seems to have led to a substantial deterioration in their ability to repay their loans. Contractor NPLs grew

<sup>12</sup> 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.

<sup>13</sup> Precrisis spending peaks were fairly similar across countries: 12.2 percent of GDP in Italy, 12.7 percent in Ireland, 13.4 percent in Portugal, 13.9 percent in Spain, 15.3 percent in Germany, and 15.5 percent in Greece.

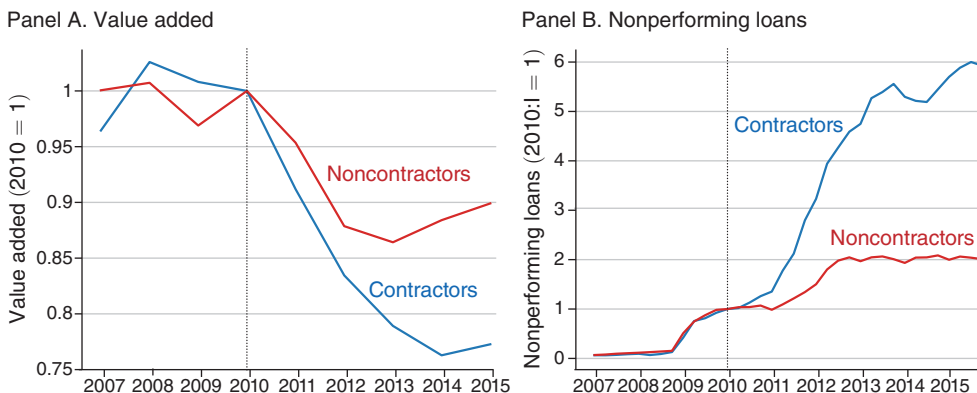


FIGURE 3. IMPACT OF PROCUREMENT CUTS ON FIRMS

Note: Panels A and B plot the evolution of value added and NPLs, respectively, for firms with public procurement contracts in 2009–2010 (contractors) versus firms without such contracts in 2009–2010 (noncontractors).

nearly sixfold by 2015, while those for other firms only doubled.<sup>14</sup> 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 1.046 (the negative of the coefficient in column 1 of Table B.4 of the Supplemental Appendix, 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 1 percent of sales increased contractor NPL ratios by 0.14 percentage points (column 2 of Table B.4 of the Supplemental Appendix).

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 percent of precrisis total bank equity. We see this as a lower bound for the impact of the procurement cuts on bank balance sheets since it only includes firms directly exposed to procurement contracts. The cuts could also have 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 percent 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 (EBA 2011a). This also amounted to 14 percent of precrisis bank equity. These numbers suggest that the shock to banks through the procurement channel we documented was

<sup>14</sup>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 nonperforming 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 nonperforming. In Banco de Portugal (2016), an exposure is also defined as nonperforming 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 percent of aggregate NPLs reported at the bank level (Banco de Portugal 2017–2019); we scale our NPL measure by this factor.

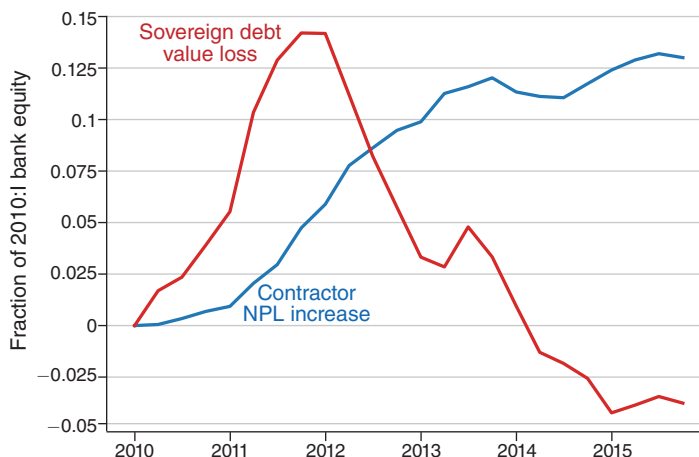


FIGURE 4. IMPACT OF THE PROCUREMENT AND SOVEREIGN DEBT SHOCKS

*Notes:* This figure plots the increase in NPLs from firms with public procurement contracts in 2009–2010 (contractors) versus the loss in the market value of bank domestic sovereign debt holdings. Both series are plotted as a fraction of total bank equity in 2010:I. Our estimate for the change in the aggregate market value of domestic sovereign debt is based on data on debt holdings from BPLIM (2021), the average residual maturity from the EBA’s 2011 stress test data (EBA 2011b), sovereign yield data from LSEG (2022), and the average interest rate on outstanding debt in 2010 reported by IGCP (2018).

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, panel A). 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. This suggests that the mechanism we study could help explain the persistent drop in credit and output in crisis-hit countries shown in Figure 1, panels B and C.

### III. Effect on Credit Supply

#### A. 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 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:

$$(1) \quad \text{Procurement Exposure}_b = \kappa \sum_{i=1}^n \frac{\text{Credit}_{ib}}{\text{Credit}_b} \times \frac{\text{Procurement Cut}_i}{\text{Sales}_i}.$$

Banks and firms are indexed by  $b$  and  $i$ . Credit is measured in 2010:I, 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:<sup>15</sup>

$$(2) \quad ProcurementCut_i = \frac{1}{2} \sum_{t=2009}^{2010} Procurement_{it} - \frac{1}{5} \sum_{t=2011}^{2015} Procurement_{it}.$$

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, 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 (i) only include credit to final goods producers in the shares or (ii) 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  $\kappa$  such that the sample mean of exposure equals the aggregate ratio of procurement cuts to value added.

*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 2010:IV and 2015:IV:

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

and our regression equation is

$$(4) \quad g_{ib} = \beta ProcurementExposure_b + \gamma_{j(i)m(i)} + \lambda_1' \mathbf{X}_b + \lambda_2' \mathbf{Z}_i + \varepsilon_{ib}.$$

The coefficient of interest is  $\beta$ . Since exposure represents a *drop* in demand,  $\beta$  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.  $\mathbf{X}_b$  and  $\mathbf{Z}_i$  are sets of precrisis bank and firm controls measured in 2010:I and 2010, respectively.  $\mathbf{X}_b$  includes bank exposure to domestic sovereign debt over equity, the log of total bank assets, and the ratio of bank equity to assets;  $\mathbf{Z}_i$  comprises the log

<sup>15</sup>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 percent 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](https://www.utap.gov.pt/PPP_saude.htm)).

of total firm assets, return on assets, leverage, and the current ratio. To test for pre-existing 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 2011:I and 2015:IV. 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)—henceforth, BM—to compute confidence intervals.<sup>16</sup>

*Sample.*—We restrict our sample to banking groups (which we refer to as banks) with at least 1 percent of the corporate credit market in 2010:I, 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 percent of corporate credit in 2010:I. Figure C.2 of the Supplemental 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 noncontractors, that is, 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 2010:IV, the reporting threshold set by the ECB for AnaCredit.<sup>17</sup> These small relationships, which represented 1.2 percent of corporate credit in 2010:IV, behaved differently than the rest of the sample, as we show in Section IIIB. 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 of the Supplemental 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.

<sup>16</sup>We use the Stata program *clustermore*, written by Gabriel Chodorow-Reich for Chodorow-Reich et al. (2020), to implement the “LZ2” standard errors.

<sup>17</sup>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).

TABLE 1—SAMPLE SUMMARY STATISTICS

	Mean	SD	P10	Median	P90
<i>Panel A. Bank-firm matched sample</i>					
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				
<i>Panel B. Firm-level sample</i>					
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)	81.19	146.12	1.64	25.61	213.56
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				

Notes: This table reports summary statistics for the bank-firm matched sample (panel A) and firm-level sample (panel B). Bank-level variables are measured in 2010:I and firm-level variables in 2010. In panel B, bank variables are aggregated by firm using the credit shares of each bank as weights.

TABLE 2—BALANCING OF BANK COVARIATES

	Procurement exposure		Difference	SE
	Below median	Above median		
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	0.034	0.007	(0.009)
Foreign bank	0.500	0.286	0.214	(0.290)
Credit/assets	0.738	0.745	-0.007	(0.061)
NPL/total credit	0.051	0.139	-0.089	(0.036)
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)

Notes: This table compares precrisis (2010:I) 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. Variable definitions are provided in Table A1 in the Appendix.

TABLE 3—EFFECT OF PROCUREMENT EXPOSURE ON BANK-FIRM-LEVEL CREDIT

	Baseline (1)	Survival (2)	Controls for other credit supply shocks		
			Construction exposure (3)	Predicted growth in other NPLs (4)	Recapitalizations (5)
Procurement exposure	−2.460 (0.682)	−1.522 (0.766)	−2.578 (0.672)	−2.597 (0.821)	−2.557 (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

*Notes:* This table presents estimates from credit regressions using bank-firm matched data. The dependent variable is the log cumulative growth in credit between 2010:IV and 2015:IV, except in column 2, where it is an indicator for whether a relationship survived until 2015:IV. 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. Column 3 adds the share of credit to the construction sector in 2010:I to the set of bank controls. Column 4 adds a shift-share predictor of NPL growth for noncontractors during the crisis, in which the shares are bank exposures by sector in 2010:I and the shifters are the leave-one-out national changes in NPLs as a share of precrisis credit in each sector between 2010:I and 2015:IV. Column 5 adds an indicator for whether a bank was recapitalized. 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.

## B. Results

Our baseline estimate for  $\beta$ , reported in column 1 of Table 3, is  $-2.460$ , with a 95 percent 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 5, panel A shows the corresponding binned scatterplot, which suggests the effect is approximately linear in exposure.

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 5, panel B presents point estimates and 95 percent confidence intervals for  $\beta$  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 sizable and persistent decline in credit growth.

Our estimates in Figure 5, panel B 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 postcrisis recessions (Ari, Chen, and Ratnovski 2021). Consistent with a slow recovery of credit supply, data from the Euro Area Bank Lending Survey (ECB 2024) shows that bank lending standards in Portugal 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 of the Supplemental Appendix).

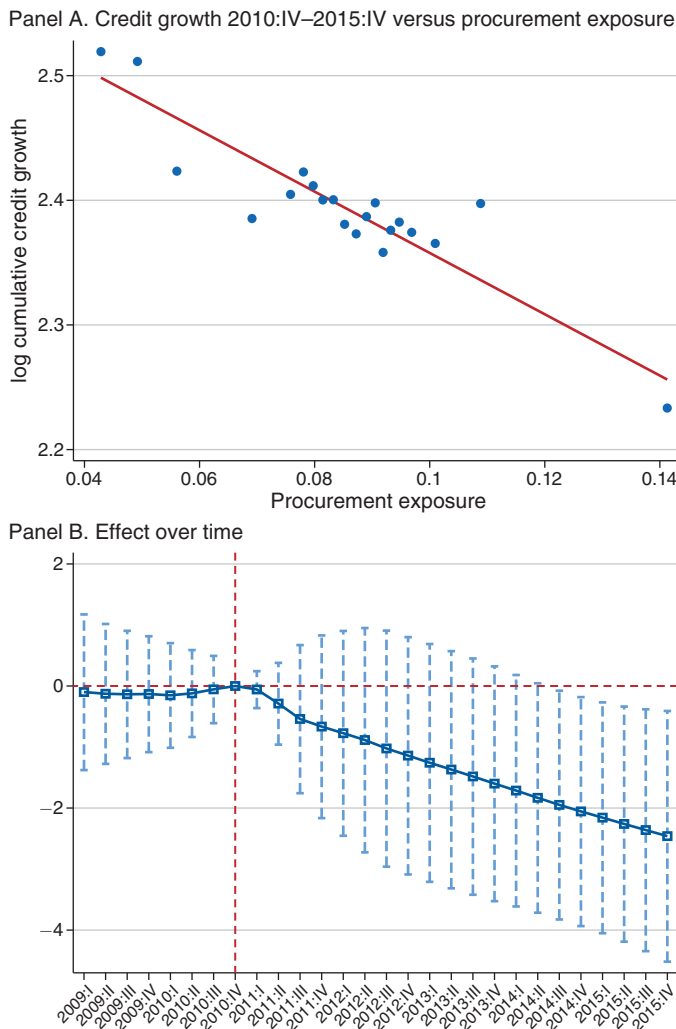


FIGURE 5. EFFECT OF PROCUREMENT EXPOSURE ON CREDIT AT THE BANK-FIRM LEVEL

Notes: Panel A presents a binned scatterplot of the log of cumulative credit growth between 2010:IV and 2015:IV versus procurement exposure at the bank-firm level. Panel B shows point estimates and 95 percent confidence intervals for the bank-firm-level effect of procurement exposure on log cumulative credit growth between 2010:IV and each quarter between 2009:I and 2015:IV.

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 2015:IV. 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 percent of the unconditional probability of relationship termination. Columns 3–5 report robustness checks that we discuss below.

*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

TABLE 4—DECOMPOSING THE EFFECT OF EXPOSURE

	$\hat{\alpha}_j$	Top 5% share	$\hat{\beta}_j$
	(1)	(2)	(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	0.466	-3.61

Notes: This table lists the top five sectors by the sum of Rotemberg weights ( $\hat{\alpha}_j$ ), calculated 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 percent of contractors by weight. Column 3 reports the weighted average coefficient on exposure obtained from the decomposition for each sector.

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.<sup>18</sup>

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 percent 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 percent of the weight. Moreover, within construction, the top 5 percent of contractors by  $\hat{\alpha}_i$  account for 81 percent of the sector's weight, and weights in the remaining top sectors are also highly concentrated. The table also reports  $\hat{\beta}_j = \frac{\sum_i \hat{\alpha}_i \hat{\beta}_i}{\sum_i \hat{\alpha}_i}$ , the weighted average  $\hat{\beta}_i$  in each sector. We find that the  $\hat{\beta}_j$  for each of the top 3 sectors, which together represent 92 percent of the weight, are close to our overall  $\hat{\beta}$ , while those for the remaining 2 sectors are higher.

The decomposition shows that the validity of our design hinges to a large extent on the exogeneity of the credit shares of a subset of construction contractors. To evaluate the plausibility of this assumption, we test for pre-trends in credit growth as a function of the  $\hat{\alpha}_i$ -weighted average credit share to the construction sector, as suggested by Goldsmith-Pinkham, Sorkin, and Swift (2020). We do this by replicating Figure 5, panel B, with the weighted credit share replacing procurement exposure in equation (4). Figure C.5, panel A in the Supplemental Appendix plots the resulting estimates. Like procurement exposure, the  $\hat{\alpha}_i$ -weighted average credit share was

<sup>18</sup>Let  $c_i$  denote firm  $i$ 's procurement cut,  $\mathbf{Z}_i$  the vector of firm  $i$ 's credit shares across banks, and  $\mathbf{P}^\perp$  the vector of procurement exposure across banks, residualized on the controls in equation (4). Then  $\hat{\alpha}_i = \frac{c_i \mathbf{Z}_i \mathbf{P}^\perp}{\sum_i c_i \mathbf{Z}_i \mathbf{P}^\perp}$ .

TABLE 5—ALTERNATIVE CONTROLS FOR CREDIT DEMAND

	Firms with multiple relationships		Controls for predicted growth in other credit			
	Baseline	Within firm	Financing type	Collateral type	Sector	Location
	(1)	(2)	(3)	(4)	(5)	(6)
Procurement exposure	−2.306 (0.898)	−2.593 (0.810)	−2.444 (0.727)	−2.420 (0.521)	−2.196 (0.689)	−2.412 (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

*Notes:* This table presents estimates from credit regressions using bank-firm matched data. The dependent variable is the log cumulative growth in credit between 2010:IV and 2015:IV. 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 noncontractors during the crisis, where the shares are bank exposures by financing type in 2010:I and the shifters are the leave-one-out national credit growth rates for each financing type between 2010:I and 2015:IV. Columns 4, 5, and 6 add analogous predictors of credit growth based on precrisis exposures to credit collateral types, sectors and municipalities, respectively. 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.

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 in the Supplemental Appendix 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.

*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 reestimating our baseline regression including exposure to the construction sector in the set of bank controls. The coefficient on procurement exposure, reported in column 3 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 noncontractors. 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 2010:I and 2015:IV.<sup>19</sup> This captures the expected change in bank NPL ratios driven

<sup>19</sup>Let  $j$  index sectors,  $-b$  denote the set of all banks except  $b$ , and  $-C$  the set of noncontractors. Let  $\bar{x}^{[T_1, T_2]}$  denote the mean of  $x$  over the period between  $T_1$  and  $T_2$ . We define bank  $b$ 's predicted NPL growth for noncontractors as

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

by noncontractors. Column 4 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 2014a), and affected 6 out of the 13 banks in the sample (Augusto and Félix 2014b). Column 5 of Table 3 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, noncontractors may be connected to contractors through supply chains and be negatively affected by the cuts through such connections. These firms may also be more likely to borrow from more exposed banks, biasing our coefficients. We address this concern by estimating an alternative specification with firm fixed effects, which absorb firm-level credit demand (Khwaja and Mian 2008). This requires restricting the sample to firms with at least two lending relationships in 2010. Columns 1 and 2 of Table 5 present coefficients from our baseline and within-firm specifications estimated in this sample. The coefficients in the two specifications are similar and, if anything, larger in the within-firm specification. This supports the validity of our design and suggests that our baseline estimates are conservative.

The within-firm strategy assumes that credit demand is not bank specific. Ivashina, Laeven, and Moral-Benito (2022) and Paravisini, Rappoport, and Schnabl (2023) show that bank specialization can invalidate that assumption. We test if bank-specific credit demand shocks can explain our findings by constructing predictors of credit growth for noncontractors 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 2010:I and 2015:IV.<sup>20</sup> 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.

<sup>20</sup>Let  $k$  index financing or collateral types,  $-b$  denote the set of all banks except  $b$ , and  $-C$  the set of noncontractors. Let  $\bar{x}^{[T_1, T_2]}$  denote the mean of  $x$  over the period between  $T_1$  and  $T_2$ . We define bank  $b$ ’s predicted credit growth for noncontractors as  $\sum_k \frac{\text{Credit}_{b,k,-C,2010Q1}}{\text{Credit}_{b,2010Q1}} \times \frac{\text{Credit}_{-b,k,-C}^{[2011Q1,2015Q4]} - \text{Credit}_{-b,k,-C}^{[2009Q1,2010Q4]}}{\text{Credit}_{-b,k,-C}^{[2009Q1,2010Q4]}}$ .

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 (0.990)	−4.515 (0.852)	−4.668 (0.819)	−4.716 (0.660)
Equity-to-assets	−0.366 (0.274)		−0.403 (0.300)	−0.439 (0.259)
Procurement exposure × equity-to-assets	28.180 (18.643)		33.843 (18.537)	34.640 (15.157)
Recapitalized		−0.028 (0.006)		
Procurement exposure × recapitalized		3.121 (0.535)		
BM degrees of freedom 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

*Notes:* This table presents estimates from credit regressions using bank-firm matched data. The dependent variable is the log cumulative growth in credit between 2010:IV and 2015:IV. 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. 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.5 in the Supplemental 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.<sup>21</sup> The results also hold when we replace the cuts with precrisis procurement levels in column 2, when we do not assign zeros to procurement increases (column 3), or when we winsorize exposure at the 2.5th and 97.5th percentiles, in line with the other variables in the regression (column 4).

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

<sup>21</sup> 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.

TABLE 7—HETEROGENEOUS EFFECTS

	Credit risk		Firm size		Firm age	
	Low (1)	High (2)	Small (3)	Large (4)	Young (5)	Old (6)
Procurement exposure	-2.435 (0.805)	-2.587 (0.644)	-2.277 (0.511)	-2.735 (0.866)	-2.500 (0.573)	-2.450 (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

*Notes:* This table presents estimates from credit regressions using bank-firm matched data. The dependent variable is the log cumulative growth in credit between 2010:IV and 2015:IV. 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. 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.

*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 1 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 percent to 7.7 percent of precrisis bank equity (Augusto and Félix 2014). 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.

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 (0.293)	−0.400 (0.094)	−0.563 (0.148)	−0.623 (0.218)	−0.317 (0.075)	−0.410 (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

*Notes:* 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 (2010:IV and 2015:IV for credit), except in column 2, where it is an indicator for whether the firm survived until 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. Procurement exposure and bank controls are aggregated to the firm level using the credit shares of each bank as weights. 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.

*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 toward 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 (Banco de Portugal 2021). 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 Supplemental Appendix). When we estimate equation (4) for the set of relationships below €25,000, which we excluded from the sample in Section IIIA, 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 percent of corporate credit in 2010:IV).

#### IV. Effect on Firms

##### A. 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  $\beta$  for firm-level cumulative credit growth, reported in column 1 of Table 8, equals  $-1.431$ , with a 95 percent confidence interval of  $(-2.222, -0.640)$ . This corresponds to 58 percent of our bank-firm-level estimate and implies that firms were able to substitute 42 percent 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 percent 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 percent 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 percent) when they restrict the sample to firms with multiple lending relationships. This also holds in our data: We estimate substitution of only 25 percent for single-relationship firms (column 1 of panel B of Tables B.5 and B.7 in the Supplemental 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. forthcoming).

Figure 6, panel A plots coefficients and 95 percent confidence intervals from estimating equation (4) for cumulative credit growth up to each quarter in [2009:I, 2015:IV]. 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 Supplemental Appendix present firm-level tests analogous to those in Tables 3 and 5 and Table B.5 in the Supplemental Appendix. We find that our firm-level credit results are equally robust.

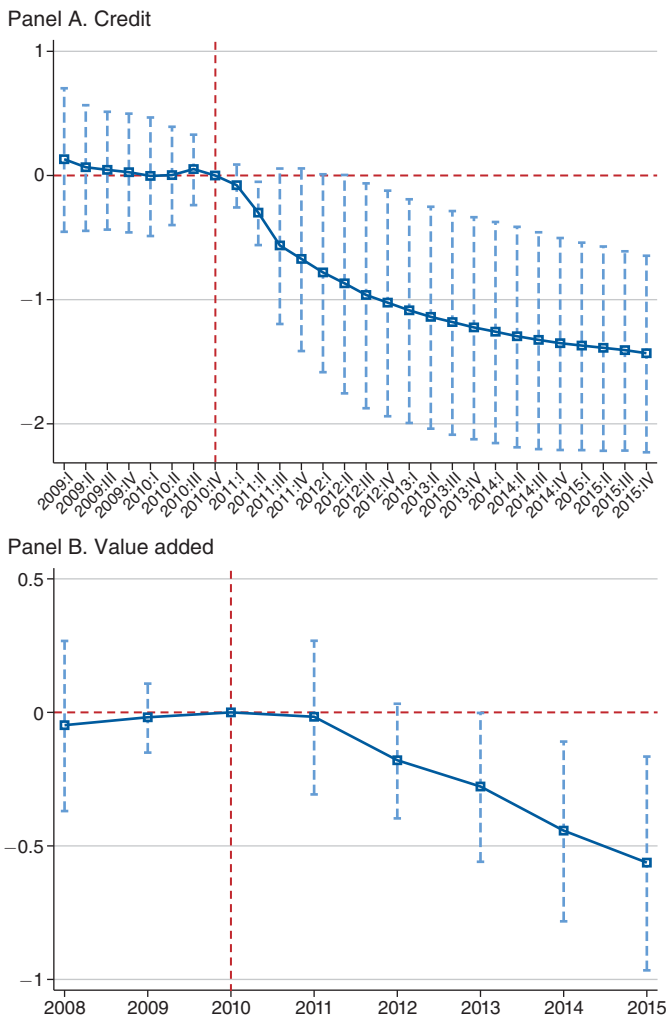


FIGURE 6. EFFECT OF PROCUREMENT EXPOSURE AT THE FIRM LEVEL

Notes: Panel A shows point estimates and 95 percent confidence intervals for the firm-level effect of procurement exposure on log cumulative credit growth between 2010:IV and each quarter between 2009:I and 2015:IV. Panel B presents estimates for log cumulative value added growth between 2010 and each year between 2008 and 2015.

### B. 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 percent 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 6, panel B plots the coefficients and 95 percent confidence intervals from estimating regressions for cumulative value added growth between 2010 and  $T \in [2008, 2015]$ .<sup>22</sup> In line with the effect on credit growth, there are no significant preexisting differential trends. After the cuts, firms borrowing from more exposed banks experienced a persistent decline in value added growth relative to firms borrowing from less exposed banks.

Tables B.8 and B.9 in the Supplemental Appendix replicate the robustness tests in Tables B.6 and B.7 in the Supplemental Appendix and 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.<sup>23</sup> 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.<sup>24</sup> 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.<sup>25</sup> The implied elasticity for employment in our setting is 0.29, close to the mean of the four studies.

## V. 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 credit supply. The setup is similar to the static model of credit supply shocks in Herreño (2023).<sup>26</sup> 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).

<sup>22</sup>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.

<sup>23</sup>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.

<sup>24</sup>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).

<sup>25</sup>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).

<sup>26</sup>Relative to Herreño (2023), we abstract from nonbank sources of funding. Firms in Portugal are heavily bank dependent, with 88 percent 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.

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.

### A. 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

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

subject to the budget constraint  $C = wL + \Pi - T$ , where  $\Pi$  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 mass of differentiated varieties indexed by  $i$ , each produced by one firm:

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

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

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

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

where  $l_i$  denotes labor supplied to firm  $i$ , and  $w_i$  the wage paid by firm  $i$ . A finite  $\alpha$  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:

$$(9) \quad G = \left( \int_0^1 g_i^{\frac{\sigma-1}{\sigma}} di \right)^{\frac{\sigma}{\sigma-1}}.$$

*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:

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

where  $\omega_{ib}$  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

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

where  $R_b$  is 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:

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

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  $v_b^{1/\eta}$  per unit of lending, where  $1/\eta > 0$ , and that  $e_b$  is small enough that  $v_b > 1$  for all  $b$  in equilibrium.<sup>27</sup> Banks maximize profits by lending at a markup over this cost,

$$(13) \quad R_b = \frac{\theta}{\theta - 1} v_b^{\frac{1}{\eta}},$$

and use leverage to equate credit supply and demand:

$$(14) \quad v_b e_b = q_b,$$

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

The model thus features an external finance premium for banks given by  $v_b^{1/\eta}$ , which is reflected in firm borrowing costs. Several macrofinance 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.

<sup>27</sup>We assume these leverage costs are rebated back to households as part of  $\Pi$ , so the aggregate resource constraint is unaffected.

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  $\eta$ , 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  $\eta$  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  $\eta$  is significantly lower than in normal times.

### B. 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  $\rho s$ . 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  $\tau_i$  to be correlated with  $\omega_{ib}$ . 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

$$(15) \quad \frac{de_b}{e_b} = -\rho v_b u_b,$$

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 Supplemental 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*

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

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  $\rho$  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  $\theta/(\theta + \eta)$  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  $\eta$  is relative to  $\theta$ , the more costly it is for exposed banks to offset equity losses through higher leverage, and the stronger the reallocation of credit toward 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  $\beta$  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,  $\varepsilon^Q \equiv -d\log Q/du$ , is given by*

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

where  $\psi \equiv 1/\phi + 1$  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  $\theta$ , discussed below, and the micro evidence on the Frisch elasticity  $1/\phi$  (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  $\eta$ . If  $\eta$  is high,  $\beta$  mainly reflects reallocation of credit, and the aggregate impact of the shock is limited. As  $\eta$  falls, reallocation is muted by the increasing sensitivity of bank funding costs to leverage, and both  $\beta$  and  $\varepsilon^Q$  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*

$$(18) \quad \varepsilon^Y = \frac{1}{1 + \phi} \varepsilon^Q.$$

TABLE 9—CALIBRATION

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

One implication of these results worth highlighting is that, given an estimate of  $\beta$ , the aggregate effects we derive are fully determined by the credit supply and demand elasticities  $\eta$ ,  $\theta$ , and  $\psi$ . Namely, the effects are independent of the elasticities of substitution in the product and labor markets,  $\sigma$  and  $\alpha$ . 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.

### C. Calibration

*Baseline.*—The cross-sectional and aggregate effects of the shock in the model are a function of five parameters:  $\rho$ ,  $\bar{v}$ ,  $\eta$ ,  $\theta$ , and  $\phi$ . Table 9 summarizes our calibration of these parameters.

We set the Frisch elasticity  $1/\phi$  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 2010:IV, which equals 12.860.

We estimate  $\theta$  using a dataset on new credit operations managed by Banco de Portugal (Banco de Portugal 2023), which includes loan dates, volumes, interest rates, maturity, and collateral for all new loans granted by banks from 2013 onward. Using equation (11), we run the following regression:

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

where  $l$  indexes loans,  $R_l$  is the loan's gross interest rate,  $\chi_{itc}$  are firm-by-year-by-loan-characteristics fixed effects,  $\mu_{bt}$  are bank-by-year fixed effects, and  $\xi_{ib}$  are bank-firm fixed effects. We restrict the sample to the 2013–2015 period. Table B.10 in the Supplemental 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 10 loan maturity bins with dummies for whether the loan has a fixed rate and for whether it is collateralized.

To set  $\eta$ , we start by noting that the elasticity of credit supply with respect to bank equity at the bank-firm level equals  $\theta/(\theta + \eta)$ , as equation (24) in the Supplemental 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

TABLE 10—AGGREGATE EFFECTS OF THE SHOCK IN THE MODEL

	Calibrations			Counterfactuals				
	Baseline (1)	Using $\beta_{firm}$		$\bar{v} = 8$ (4)	$\bar{v} = 20$ (5)	No crisis $\eta = 5.452$		
		$\alpha = 1$ (2)	$\alpha = 1,000$ (3)			Baseline $\bar{v}$ (6)	$\bar{v} = 8$ (7)	$\bar{v} = 20$ (8)
$\varepsilon^Q$	2.128	2.330	1.915	1.324	3.309	0.662	0.412	1.030
$\varepsilon^Y$	0.912	0.999	0.821	0.567	1.418	0.284	0.177	0.442

Notes: 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  $\eta$  estimated in a noncrisis context, along with variation in  $\bar{v}$ .

set of controls.<sup>28</sup> We obtain an elasticity of 0.902, reported in Table B.11 of the Supplemental Appendix, which shows banks were largely unable to offset the effect of equity losses on credit supply. Using our estimate for  $\theta$ , this implies  $\eta = 0.492$ .

Finally, we choose  $\rho$  so that  $\beta$  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 0.142 in column 2 of Table B.4 of the Supplemental Appendix can be interpreted as a direct estimate of  $\rho$ , 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  $\theta$  and  $\eta$ . As we show in Section A.5 of the Supplemental Appendix,  $\theta = \frac{\beta}{\beta_{firm}} \sigma \frac{\alpha + 1}{\alpha + \sigma}$ , where  $\beta_{firm}$  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  $\alpha$ : 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  $\theta/(\theta + \eta) = 0.902$ , these values of  $\theta$  imply  $\eta = 0.297$  and  $\eta = 0.741$ , respectively. We examine the sensitivity of the results to these alternative parameters below.

#### D. Results

*Effect of the Cut in Our Setting.*—Our results on  $\varepsilon^Q$  and  $\varepsilon^Y$  are summarized in Table 10. Our baseline calibration yields  $\varepsilon^Q = 2.128$ , with a 95 percent confidence

<sup>28</sup>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 2010:IV 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.

interval of (0.351, 3.905),<sup>29</sup> and  $\varepsilon^Y = 0.912$  with a 95 percent confidence interval of (0.150, 1.673). We note that  $\varepsilon^Y$  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  $\varepsilon^Q$  and  $\varepsilon^Y$  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 percent in credit, which amounts to 84 percent of the average drop in real corporate credit in Portugal in the 2011–2015 period relative to 2010:IV, and to a drop of 7.7 percent in output, which represents 49 percent 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, 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  $\theta$  and  $\eta$  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  $\varepsilon^Y = 0.999$ .<sup>30</sup> 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  $\varepsilon^Q$  and hence  $\varepsilon^Y$  do not depend on  $\theta$ . Higher reallocation increases the effect of the shock on exposed banks relative to unexposed banks but does not affect aggregate credit.  $\theta$  only affects  $\varepsilon^Q$  and  $\varepsilon^Y$  by pinning down  $\eta$ , in conjunction with the elasticity of credit supply to bank equity. And for  $\eta$  in the range implied by these alternative values of  $\theta$ , the results remain relatively stable.

*Amplification through Initial Leverage.*—As equations (16), (17), and (18) show,  $\beta$ ,  $\varepsilon^Q$ , and  $\varepsilon^Y$  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  $\frac{d\varepsilon^Y}{d\bar{v}} = \frac{1}{1 + \phi} \frac{d\varepsilon^Q}{d\bar{v}}$ .

<sup>29</sup>We calculate this confidence interval using the confidence interval for  $\beta$ , taking the remaining parameters as given.

<sup>30</sup>This alternative calibration yields an effect of exposure on firm-level output of 0.716, not far from our empirical estimate for firm value added reported in column 3 of Table 8.  $\varepsilon^Y = 0.999$  thus implies a modest amount of general equilibrium amplification. Huber (2018) also finds that the effects of credit supply shocks are amplified in general equilibrium, while Herreño (2023) finds they are mildly dampened.

In our baseline calibration, we get  $d\beta/d\bar{v} = 0.191$ ,  $d\varepsilon^Q/d\bar{v} = 0.165$ , and  $d\varepsilon^Y/d\bar{v} = 0.071$ , 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  $\varepsilon^Q$  and  $\varepsilon^Y$  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 percent larger than our baseline.

*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  $\eta$ . This implies that the value of  $\eta$  we infer in our setting may be substantially lower than in normal times.

To gauge what the value of  $\eta$  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  $\theta/(\theta + \eta)$ , and using our baseline estimate of  $\theta = 4.550$ , we obtain  $\eta = 5.452$ , which is over 10 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 percent 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  $d\beta/d\bar{v} = 0.096$ ,  $d\varepsilon^Q/d\bar{v} = 0.052$ , and  $d\varepsilon^Y/d\bar{v} = 0.022$ . Columns 7 and 8 of Table 10 present counterfactuals for  $\varepsilon^Q$  and  $\varepsilon^Y$  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.

## VI. 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 at the bank level, 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. The extent of persistence we find at the firm level also raises the question of whether firms converge back to trend after shocks. We think exploring these issues 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.

## APPENDIX

TABLE A1—VARIABLE DEFINITIONS

<i>Bank-firm variables</i>	
Total credit	Firm's total credit outstanding in each bank
<i>Bank variables</i>	
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 nonperforming 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 noncontractors, that is, 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 2010:I and 2015:IV
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 occurred between 2010 and 2013)
Predicted growth in other credit by financing type	Shift-share predictor of credit growth for noncontractors, that is, 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 2010:I and 2015:IV
Predicted growth in other credit by collateral type	Shift-share predictor of credit growth for noncontractors, that is, 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 2010:I and 2015:IV
Predicted growth in other credit by sector	Shift-share predictor of credit growth for noncontractors, that is, 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 2010:I and 2015:IV
Predicted growth in other credit by location	Shift-share predictor of credit growth for noncontractors, that is, 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 2010:I and 2015:IV
<i>Firm variables</i>	
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

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