Does Government Debt Crowd Out Corporate Investment? A Bank Lending Channel*

Noémie Pinardon-Touati†

Preliminary version

October 7, 2020

Abstract

Bank loans are an important source of financing for governments, accounting for 19% of total public sector debt in EU countries. Using the French credit register over 2006-2018 and leveraging variation in public sector debt dynamics at the bank-location level, I show that increases in the demand for bank debt by public sector entities causally crowd out bank lending to private corporations. When public sector entities borrow an additional 1€ from the median bank, this bank lends 0.78€ less to the private sector, with sizeable effects on firm-level investment. My results suggest that crowding out effects may substantially reduce (local) fiscal multipliers.

Keywords: Government debt, Crowding out, Banks, Credit supply.
JEL Code: E5, E6, G2, H6.

*The author is grateful to Johan Hombert, Denis Gromb and Adrien Matray for their continued guidance and support. The author is also grateful to Bank of France for granting access to their data. The views expressed herein are those of the author and do not necessarily reflect those of Bank of France.

†HEC Paris. Contact: noemie.pinardon-touati@hec.edu

1 Introduction

The consequences of high government debt levels have obtained significant attention during the recent financial crisis. Among the potential adverse effects pointed out, crowding-out effects on the private sector are high on the list. This question has been hard to settle empirically due to pervasive endogeneity issues. This paper investigates this question by showing that increases in the bank debt of public sector entities (PSEs)[1] causally leads to a supply-driven reduction in corporate credit and thereby in corporate investment.

While there are various channels through which fiscal expansions can boost economic activity, government debt can also have crowding-out effects on the domestic private sector. In particular, the resources used by the private sector to acquire government debt can detract from investment. The basic mechanism is the following: if the private sector is restricted in its ability to increase the supply of financial capital/savings, then increasing government debt puts upward pressure on the cost of capital, which in turn leads to lower private investment.[2] This argument was first put forth to analyze the effect of a national-level increase in government debt, but it applies at all scales – e.g. subnational regions, banks – as long as the units considered are restricted in their ability to increase the supply of capital, i.e. as long as capital markets are segmented.

In this paper, I use French administrative credit register and corporate tax filings data over 2006-2018 to provide evidence for a crowding out effect of public sector debt operating locally through the banking system. I show that because capital markets are segmented across geographies and across banks, hikes in the bank debt of PSEs affect local firms' ability to obtain bank loans and thereby invest.

The market for the bank debt of PSEs offers an ideal setting to pin down the causal relationship from PSE debt to private firms' financing conditions. First, bank loans are

[1]PSEs comprise the central government, local governments and all the entities they control (e.g. public schools, hospitals, government agencies, etc.). The terms public sector entities and government entities are used indifferently.

[2]This argument relies on the idea that government debt and corporate debt are perfect substitutes from the point of view of investors. A more refined version of this argument put forth by Friedman (1978) is that an increase in the supply of government debt will affect the relative prices of other securities depending on their degree of substituability with government debt.

an important source of financing for PSEs: over the last 10 years, bank debt accounted for 14% of total PSE debt in France, and the Euro area average is even higher (19%). Second, 97% of the bank debt of French PSEs stems from local entities (local governments, hospitals, etc.) inducing a large geographical heterogeneity in the dynamics of PSE debt across local lending markets. I leverage this variation in PSE loans growth along with variation in banks' geographical footprint to obtain a proxy for the demand pressure on the PSE bank debt market at the bank×region level. Because of segmentation, this heterogeneous exposure to PSE debt shocks translates into differentiated shifts in credit supply curves, allowing me to detect crowding out effects at the bank×region level.

My research design consists in comparing the change in credit supply to private firms across banks and regions differentially exposed to PSE debt shocks. Furthermore, I take advantage of the widespread presence of firms that established simultaneous lending relationships with different banks to control for changes in firms' unobservable determinants of credit demand and creditworthiness (Khwaja and Mian (2008)). This allows me to abstract from the endogeneity of local PSE debt with respect to private firms' prospects, which is a pervasive issue in this literature.

I find that PSE debt shocks causally lead to a reduction of credit supply to the corporate sector. When PSEs borrow an additional 1€ from the median bank, this bank lends 0.78€ less to the private sector. Importantly, this effect is only capturing variation in credit supply due to the crowding out channel, holding constant firms' credit demand and thus partialling out any local macroeconomic effect of PSE debt.

I then assess the effect of PSE debt shocks at the firm level. Firm-level effects may be smaller than the within-firm elasticity if firms are able to easily switch across banks. I add to the literature on this issue (Jiménez et al. (2019)) by formally incorporating the possibility of substitution effects in the standard Khwaja-Mian framework and showing how the effect of the credit supply shock and the substitution effect can be separately identified. In my setting, I find that the contraction in credit supply caused by government debt shocks has a sizable effect on firms' ability to access bank financing and on firms' investment.

The large crowding out effects I document reveal the extent of segmentation in credit markets, both geographically and across banks. I further show that my results are driven by banks' limited ability to expand their balance sheets and by imperfect capital mobility within banks.

My results suggest that the effects of fiscal expansions on private firms' outcomes not only depend on how spending is financed (taxes vs. deficit), but also on who is financing the debt (bonds vs. bank debt, local vs. global banks). The severity of the crowding out effect is determined by the elasticity of the supply of capital of the agents who acquire the debt of the government, i.e. by the degree of capital markets segmentation. A government who wishes to share the burden of crowding out with other areas should aim at issuing debt in a market that is as little segmented as possible. This is a rationale for issuing debt in the form of bonds rather than bank debt since financial markets are less segmented than banks, as well as for promoting international capital mobility. As a result, who finances the debt should matter when estimating and interpreting fiscal multipliers.

This work contributes to several strands of the literature. First, I contribute to the literature on the crowding out effect of government debt on corporate debt and investment. There exist a recent empirical literature relying either on cross-country data or on time-series variation to examine the effect of government debt on leverage (Graham et al. (2015), Demirci et al. (2018), Akkoyun (2018)). These papers show that this relationship is strongest for the debt of firms that are considered close substitutes to government bonds, that is large firms, in accordance with the portfolio crowding out mechanism emphasized by Friedman (1978). Within the crowding out literature, the paper that is closest to mine is Huang et al. (2017). This paper identifies a local crowding-out channel in China whereby the debt issued by Chinese local governments is shown to reduce the investment of private firms, all the more so for private firms that depend more heavily on external funding.

I contribute to this literature in several dimensions. First, I provide direct evidence of a new mechanism, namely, a crowding out effect operating through a bank lending channel, and operating mainly through quantities. Contrary to portfolio crowding out, which implies only a decrease in leverage, the bank lending channel has direct implications for investment. Besides, while portfolio crowding out affects mostly large firms, the bank lending channel will affect primarily small firms that are bank-dependent. Second, this work represents a significant improvement in terms of internal validity compared to existing work. By combining credit register data with a bank-location-specific measure of government debt shocks exposure, I can isolate the effects of government debt through an increased scarcity of debt for the private sector, excluding that the observed decline in credit was solely the result of a reduction in credit demand. Besides, using credit-register data allows me to precisely characterize what drives banks' inability to absorb government debt shocks.

Second, this paper adds to the evidence on the transmission of shocks through banks, or the so-called bank lending channel, which occurs precisely because banks are segmented intermediaries. The literature on the effects of the European sovereign crisis already showed that banks' exposure to government debt could lead to a contraction in lending (Popov and Van Horen (2015), Acharya et al. (2018), Bottero et al. (2018), Becker and Ivashina (2017)). In these papers, the mechanism is the impairment of the value of existing sovereign holdings. The mechanism I describe is very different and occurs not only in crisis times. In terms of economic mechanism, my work is closest to Chakraborty et al. (2018) who show that in strong housing markets, commercial loans are crowded out by banks responding to opportunities in mortgage lending. I contribute to the bank lending channel literature by showing that government debt shocks act as a specific form of bank-level credit supply shocks. These shocks are interesting for two reasons: government debt shocks are large from the point of view of banks and government debt is a policy tool. The effect documented in this paper should thus serve to inform policy makers. I also contribute to this literature by augmenting the Khwaja-Mian framework often used to study the BLC channel to incorporate the possibility that firms substitute credit across banks and showing how the effect of credit supply shocks can be separately identified from substitution.

Third, this work feeds into the more general literature on the effects of government debt on growth, and notably on the size of debt-financed fiscal multipliers. Adelino et al. (2017) and Dagostino (2018) use cross-sectional variation in local governments debt issuance to provide such estimates. Interestingly, Dagostino (2018) who studies bank-financed fiscal multipliers finds a point estimate twice lower than Adelino et al. (2017) who study bonds-financed fiscal multipliers, suggesting larger crowding out effects for bank debt.

Section 2 discusses the relationship between capital markets segmentation and crowding out. Section 3 provides institutional details on the bank debt of government entities and briefly presents the data. Section 4 details the identification strategy. Section 5 presents my main results on the crowding out effect of PSE debt shocks on corporate credit. Section 6 provides more insights on the economic mechanism. Section 7 presents evidence on the real effects of the crowding out mechanism on corporate investment. Section 8 discusses implications for the mode of financing of government debt and fiscal multipliers. Section 9 concludes.

2 Capital markets segmentation and crowding out

Crowding out occurs because of the limited ability of the private sector to increase the supply of capital when government debt rises. There are two cases under which the supply of capital rises endogenously so that government debt has no effect on interest rates and corporate borrowing. The first case is perfect capital mobility. With fully integrated capital markets, domestic interest rates only rise in proportion to the share of debt issued globally. For a small open economy like France, the crowding out effect tends to zero. The second case is Ricardian agents. Ricardian agents anticipate higher future taxes and the increase in private savings constitutes additional capital that fully offsets the effect of higher government debt.

This reasoning readily translates to crowding out occurring as a response to an increase in the bank debt of PSE. To detect crowding out effects at the bank-location level, it must be that bank-location units are constrained in their ability to increase the supply of capital. If credit markets were perfectly integrated across locations, an increase in government debt in one location would trigger an increase in local interest rates, drawing in capital from the rest of the country. Any crowding out of private investment would occur at the national level and not at the local level. Likewise, if credit markets were perfectly integrated across banks, an increase in government debt directed to one bank would induce this bank to draw in capital using the interbank market so that interest rates would rise at all banks and no bank-specific crowding out would take place. This section documents the segmentation of the credit market, both geographically and across banks, which drives bank-location units limited ability to increase the supply of capital in face of government debt shocks.

First, it is well known that credit markets are segmented across banks. On one side, banks have a limited ability to smooth shocks through the interbank market or by raising additional deposits or equity. On the other side, frictions prevent firms from costlessly switching lenders so that when some banks face a shock, firms are unable to compensate the reduction in credit by expanding their borrowing from less exposed lenders.

Second, the French credit market is geographically segmented. A large fraction of banks are local: local banks[3] account for 55% of banks and represent 37% of total lending, their market share being even higher in rural areas. The size of the geographic footprint of local banks is closest to that of regions: federations of cooperative banks often follow regional boundaries, and other local banks often operate at scales that resemble regions.[4] Besides, even the national banks present throughout France often conduct business on a local basis. Investigating the management structure of these banks shows the main intermediary level is also a regional management. Figure A.2 shows the number of banks and the total credit volume when we sort banks according to the number of regions in which they operate. It shows that the vast majority of banks operates in one region only, although national banks tend to be much bigger in volume terms.

[3] I define local banks as either: (i) cooperative banks which have designated geographical areas of business ; (ii) banks that operate in no more than 3 regions.

[4] I take regions before the 2015 redistricting, which correspond best to the boundaries of banks. There are 22 regions in mainland France. The average (median) population of a region is 2,8 (1,9) million in 2016. Figure A.1 shows these subdivisions on a map.

Banks being local intermediaries applies to both sides of the balance sheet. On the liability side, banks rely heavily on local deposits for funding. On the asset side, bank lending is very local (Petersen and Rajan, 2002). Lending markets are likely to be even smaller than the geographic footprints of banks: typically they are closer to the level of the bank branch than that of the bank itself. In France, lending markets appear to correspond well to municipality groupings[5] (henceforth "counties") as private firms located in a county have a 0.97 probability of borrowing from a bank branch within the same county. Likewise, PSEs located in a county have a 0.96 probability of borrowing from a bank branch within the same county.

There are several ways in which capital can be moved geographically: borrowers borrowing from banks further away ; banks using financing that is less local than deposits (e.g., public equity, bonds or interbank funding) ; and transfers between branches of a bank present in multiple areas (internal capital markets). As my results will show, these channels however do not completely undo credit market segmentation.

The appropriate geographical scale to study crowding out is the level at which the agent who acquires the debt – typically a bank in the county where the borrowing PSE is located – faces an inelastic supply of capital. From the discussion above, this is likely to be the bank×region level, either because of segmentation across banks in the case of local banks, or because of segmentation within banks if the internal capital market of national banks is imperfect. In such segmented markets, increased lending to PSEs by one bank×region would thus either have to be financed by raising additional deposits locally at that bank[6] or by reducing lending to other borrowers. This paper shows that we observe the latter.

[5] I build time-invariant municipality groupings based on 2010 inter-municipal cooperations (*EPCI*). I obtain with 2081 units with an average (median) 2016 population of 31 000 (11 500). Figure A.1 shows these subdivisions on a map.

[6] This highlights the fact that with segmentation across banks, crowding out may occur even if agents are Ricardian as agents may increase their savings by increasing their deposits at a bank that is not the bank exposed to the government debt shock.

3 Institutional background and data

3.1 Bank loans to government entities in France

The bank debt of French PSEs is large and growing. In 2017, the total bank debt of French PSEs amounted to €245 Bn (12.3% of GDP) and it grew at an average rate of 3.5% per annum over the last 12 years. These numbers are similar in other EU countries, as shown by Figure 1. Figure 2 depicts the evolution of the the bank debt of PSEs by type of entity. 64% of the bank debt of PSEs comes from local governments. The other main borrowers are public hospitals and *EPIC* (state-owned enterprises that provide a public service but are partly financed by their customers).

[Figure 1 about here.]

[Figure 2 about here.]

The split between public entity categories implies that the bank debt of PSEs mostly stems from entities that are scattered throughout the French territory, inducing a large geographical heterogeneity in the dynamics of PSE debt growth. The maps presented in Figure 3 highlight the heterogeneity in the growth rate of PSE bank debt across French counties. They show the large variation in growth rates across counties, both within and across regions, as well as across time for a given county.

[Figure 3 about here.]

The bank debt of French PSEs is held by both state-owned and private banks. Averaging over the sample period, private banks hold 59% of the bank debt of PSEs. As a result, the debt of PSEs is very large from the point of view of private banks' balance sheets: as shown by Panel (a) of Figure 4, loans to PSEs account for on average 28% of private banks' total loan portfolio. In terms of distribution of these loans across banks, only 29% of banks participate in the PSE lending market, but these banks represent 82% of lending to private firms, as shown in Panel (b).

[Figure 4 about here.]

3.2 Data

My analysis draws on three main sources: (i) the French Credit Register, which contains information on credit relationships for both private firms and PSEs; (ii) the French corporate tax files, which contain firms' balance sheet and income statement information; (iii) the bank's regulatory filings. This section briefly discusses these datasets.

My main data source is the French credit registry administered by the Bank of France and collecting data on corporate borrowers with total exposure (debt and guarantees) above 25,000 euros toward financial intermediaries operating in France. For each firm-bank pair, I recover the end-of-month total outstanding credit, for each month from January 2006 to December 2018, with a monthly average of 2.5 million bank-firm pairs. I focus on drawn credit.[7] I also restrict my sample to firms located in mainland France. In all my analysis, I exclude inter-bank lending, loans to real-estate investment trusts and sole proprietorships. I also exclude all firms belonging to industries that are most dependent on public procurement contracts: I drop firms in industries where more than 5% of industry revenues are accounted for by public contracts.[8]

I am left with 1,654,720 unique firms, 633 unique banks, and 3,259,266 unique bank-firm relationships. This sample is close to the full population of French corporations. Since my identification strategy relies on within firm variation, the main results are estimated on the sample of firms with at least two contemporaneous banking relationships, which represent 29% of firms (as shown on Figure 5) and 73% of total credit. In this reduced sample I obtain 598,498 unique firms and 2,091,113 unique bank-firm relationships.

In the second part of my analysis, I complement this dataset with the balance sheets and income statements of borrowing firms obtained from the corporate tax files collected

[7] As Ivashina and Scharfstein (2010) show in their analysis of the US subprime crisis, firms' drawing on pre-committed credit lines can result in an apparent increase in the availability of bank credit, but this increase may be only apparent as banks simultaneously cut the provision of new loans. To avoid this measurement problem, I exclude credit lines, factoring and other liabilities with very short maturities.

[8] These industries are: construction (construction of buildings, civil engineering and specialized construction activities), manufacture of pharmaceutical products, and manufacture of medical equipment, instruments and supplies.

by Banque de France, which are the tax files for all firms with revenues above 750,000 euros. The frequency of this dataset is annual. Once I merge the tax files with the credit register data restricted to multibank firms, I obtain a reduced sample with 97,318 unique firms, which account for 34% of aggregate lending. Table 1 display the summary statistics of the bank-firm and firm-level variables of interest, both for single-bank firms and for multibank firms.

[Table 1 about here.]

I obtain the data on bank lending to PSEs from the credit registry. I have 63,545 unique PSE and 208,174 unique PSE-bank relationships. In my analysis, I aggregate the bank debt of PSEs at the county level. Table 1 displays summary statistics of government debt variables.

4 Empirical strategy

4.1 Identification challenges

How does government borrowing affect private firms' outcomes through the crowding out channel? One would like to take government debt shocks and look at private firms' outcomes in reaction to these shocks. However, such an analysis is plagued with endogeneity issues. A first source of endogeneity is reverse causality, where private firms' credit outcomes may affect national and local governments' debt decisions. This could be the case if governments run deficits and hence increase their debt in bad times. A second source of endogeneity is common shocks to government borrowing and private activity such as demand effects: if government borrowing increases local activity (e.g. when building public infrastructure), a rise in government debt would trigger an increase in credit demand of private firms.

Therefore, we need an empirical strategy that allows to investigate the effect of PSE debt on corporate borrowing through the crowding out channel, partialling out any other channel related to the local macroeconomic environment.

4.2 Identification strategy: relationship-level outcomes

To circumvent these issues, I rely on the approach pioneered by Khwaja and Mian (2008) to identify the effect of bank-level shocks on credit supply while accounting for observed and unobserved determinants of credit demand.

I investigate the crowding out effect related to PSE debt shocks by studying changes in credit supply across banks with different exposure to these shocks. Following the discussion on banks' capital markets segmentation in Section 2, I define exposure to PSE debt shocks at the bank×region level which is the relevant scale to study the crowding out phenomenon.[9] I estimate a model of the form:

$$\Delta D_{f,b,t} = d_{f,t} + \beta BankExposure_{b,r,t} + \Phi \cdot \mathbf{X}_{b,r,t} + \varepsilon_{f,b,t} \tag{1}$$

$\Delta D_{f,b,t}$ is the change in loan amount from $t-1$ to t extended to firm f located in region r by bank b, normalized by the firm's total bank liabilities in period $t-1$. $BankExposure_{b,r,t}$ captures the demand for PSE debt received by bank b in region r. Its construction is detailed below. $\mathbf{X}_{b,r,t}$ includes bank-time and bank-region-time level controls.

The key issue is that the *BankExposure* variable is likely to be correlated with (potentially unobservable) firm-specific shocks to credit demand. The first concern is endogeneity through the local macroeconomic channel: for instance, if PSE debt systematically rises in bad times, *BankExposure* will tend to be negatively correlated with shocks to ΔD. A second concern is assortative matching if firms with declining prospects systematically match with banks with high *BankExposure*. Following Khwaja and Mian (2008), I address the identification problem by focusing on firms with multiple lending relationships and adding firm×time fixed effects. The fixed effect $d_{f,t}$ captures firm-specific determinants of credit flows, which is usually interpreted as a measure of credit demand.[10] Importantly, in this setting I do not need PSE debt shocks to be exogenous to

[9] I can alternatively define the independent variable of interest at the bank level for local banks and at the bank×region level for national banks.

[10] Credit demand must here be interpreted in a broad sense: provided credit demand is symmetric across lenders, $d_{f,t}$ captures a firm's propensity to receive a loan independently of its lenders and is thus affected by both its investment opportunities and its demand for bank loans and its creditworthiness

firms' credit demand: any endogenous relationship between PSE debt and private firms' non bank-specific outcomes is controlled for by the firm×time fixed effect.

The independent variable of interest is the growth rate of PSE debt at the bank×region level: we want to know by how much banks reduce corporate lending for a given increase in PSE debt. However, estimating equation (1) using the actual growth rate of $GovDebt_{b,r,t}$ still presents an endogeneity issue. This quantity could be endogenous to bank b's current lending opportunities to the private sector: a bank could accept to grant more credit to PSEs precisely because it knows that its lending prospects with the corporate sector are poor. I circumvent this issue by leveraging PSE debt dynamics across local lending markets (counties) and banks' pre-determined geographic footprints to construct the $BankExposure$ variable in the following way:

$$BankExposure_{b,r,t} = \sum_{c \in r} \omega_{b,c,t-1} \times \Delta GovDebt_{c,t} \tag{2}$$

where $\Delta GovDebt_{c,t}$ are county-level growth rates in PSE debt and $\omega_{b,c,t-1} = \frac{GovDebt_{b,c,t-1}}{GovDebt_{b,r,t-1}}$ captures bank b's exposure to PSE debt dynamics in county c. $BankExposure_{b,r,t}$ proxies the PSE debt demand pressure directed to bank b in region r.[11]

In my favorite specification, I cluster standard errors at the bank×region level, which is the level of the treatment. A negative and statistically significant value of the coefficient β indicates the presence of a crowding out effect triggered by PSE debt shocks and operating through banks.

This approach is equivalent to a within-firm difference-in-differences model, where banks with lower exposure to PSEs debt shocks are used as the control group for banks with higher exposure to PSEs debt shocks. The key identifying assumption of my design is thus the parallel-trend assumption: it must be true that, in the absence of the PSE debt shocks, banks with high exposure would have displayed a credit supply trend comparable to banks with low exposure. While my specification fully controls for demand-side effects,

(observable symmetrically by all lenders).

[11] My results would remain essentially unchanged if I defined $BankExposure$ as $BankExposure_{b,r,t} = \frac{GovDebt_{b,r,t-1}}{GovDebt_{r,t-1}} \times \Delta GovDebt_{r,t}$.

this assumption may be violated if banks receive unobserved credit-supply shocks that are systematically correlated to the *BankExposure* variable.

Panel A of Table 2 tests whether banks with higher PSE debt shocks exposure are systematically different. I regress various lagged bank characteristics against banks' exposure and report the coefficient. The left panel reports unconditional correlations and displays large differences between exposed and non-exposed banks. As the right panel shows, these differences are mainly driven by differences between banks that do take part in the PSE debt market – and thus that are more likely to be exposed to PSE debt shocks – and others: once we condition on whether banks are active in this market, these differences tend to be greatly reduced.[12]

[Table 2 about here.]

I address this issue in several ways. First, equation (1) includes a large set of bank- and bank×region-level controls precisely to account for these differences in banks' characteristics. However, it may still be the case that *BankExposure* is correlated to some unobservable characteristics that are themselves correlated with other credit supply shocks. While the parallel trend assumption is fundamentally untestable, Section 5.2 presents extensive indirect evidence that supports it.

4.3 Substitution across banks and firm-level credit outcomes

Bank-level credit supply shocks may not aggregate to lower credit supply for the firm if it is able to substitute across lenders.[13] The standard procedure to study firm-level

[12] The differences that remain are that more exposed banks tend to be larger, to be less capitalized, to have a larger supply of deposit compared to their loan portfolio, to be net creditors on the interbank market, and to have a lower share of non-performing assets. While this does not portray more exposed banks as being systematically "worse banks", it still raises the concern that high exposure banks tend to receive credit-supply shocks correlated to those characteristics but independent of the crowding out mechanism.

[13] The identifying assumption of the Khwaja-Mian estimator relies on the possibility of substitution across banks: for firms' credit demand shocks to be absorbed by the fixed effect it must be that they are evenly spread across lenders. This implies that from the firm's point of view, credit from different banks are substitutes. It does not imply that firms indeed manage to substitute across banks when some banks face credit supply shocks, but it highlights the importance of accounting for this possibility.

outcomes is to aggregate the bank-firm model (1) at the firm-level and to estimate the following equation:

$$\Delta D_{f,t} = \bar{\beta} FirmExposure_{f,t} + \Phi \cdot \mathbf{X}_{f,t} + \varepsilon_{f,t} \tag{3}$$

where $FirmExposure_{f,t}$ captures the exposure of a firm to credit supply shocks through its banking relationships:

$$FirmExposure_{f,t} = \sum_{b=1}^{n_{f,t}} \omega_{b,f,t-1} BankExposure_{b,r,t} \tag{4}$$

with $\omega_{b,f,t-1}$ bank shares in firm f's total borrowing. One then compares the within-firm coefficient to the between-firm coefficient to gauge the magnitude of substitution across banks. However, if $BankExposure$ is correlated to firm-specific shocks to credit demand this is incorrect. This section details the issue and provides a new methodology to disentangle the direct effect from the shock from substitution across banks. I provide the key intuitions in the main text and leave all formal derivations to Appendix C.

If firms can substitute across banks, model (1) is misspecified and the true data-generating process is of the form:

$$\Delta D_{f,b,t} = d_{f,t} + \beta BankExposure_{b,r,t} + \gamma BankExposure_{-b,r,t} + \Phi \cdot \mathbf{X}_{b,r,t} + \varepsilon_{f,b,t} \tag{5}$$

where $BankExposure_{-b,r,t}$ captures the shocks of the other banks f borrows from. One usually thinks of β and γ as having opposite signs.[14]

First, in this case the standard within-firm estimator omitting the substitution effect β_{FE} overestimates the true β.[15] Intuitively, the within-firm estimator is akin to a within-firm diff-in-diff and substitution implies that the control group is affected by the shock

[14]If the shock is contractionary ($\beta < 0$), a large $BankExposure_{-b,r,t}$ (controlling for $BankExposure_{b,r,t}$) means that other banks will contract their credit supply more than bank b, which should induce firm f to try to offset some this reduction by borrowing more from bank b ($\gamma > 0$).

[15]Equation (18) in the Appendix C shows that under simplifying assumptions $\beta_{FE} = \beta - \frac{1}{n-1}\gamma$ where n is the (constant) number of banks per firm.

in a direction opposite to that of the treated group, so that taking the difference between the two overestimates the true effect.

Second, if $BankExposure$ is correlated to firm-level unobserved demand shocks $d_{f,t}$, then $FirmExposure$ is also mechanically correlated to $d_{f,t}$ so that the OLS coefficient in equation (3) $\bar{\beta}_{OLS}$ is biased.[16] Therefore, part of the difference between β_{FE} and $\bar{\beta}_{OLS}$ comes from the correlation of $FirmExposure$ with $d_{f,t}$ so that comparing the two is not informative of the extent of substitution.

To circumvent this issue, Cingano et al. (2016) and Jiménez et al. (2019) have proposed to use the estimated fixed effects $\hat{d}_{f,t}$ from (1). If the $\hat{d}_{f,t}$ are correctly estimated, they are unbiased estimates of the true firm-specific determinants of credit growth $d_{f,t}$. Including them in (3) then precisely allows to control for the correlation between $FirmExposure$ and $d_{f,t}$. This methodology properly deals with the correlation between $FirmExposure$ and $d_{f,t}$ in the absence of substitution across banks. However, with substitution across banks, β_{FE} estimated at the within-firm stage is biased, implying that the estimated $\hat{d}_{f,t}$ are biased as well, so that including these estimates in the between-firm regression (3) will in turn lead to a bias in the $\bar{\beta}$ coefficient (which is proportional to that of β_{FE}).

Building upon this, I propose two alternative methods to solve this issue and to disentangle the effect of bank-specific shocks from substitution effects. Each relies on additional identifying restrictions.

The first method relies on within-firm variation in bank shares $\omega_{b,f,t}$, along with a specific functional form for the substitution term. If (i) $BankExposure_{-b,r,t}$ is defined as a weighted mean of firm f's other banks' shocks where the weights depend on banks' shares $\omega_{b,f,t}$, (ii) there is within-firm variation in $\omega_{b,f,t}$, and (iii) $n_{f,t} > 2$, then equation (5) is identified.[17] In this case, the identification of γ relies on the assumption that the substitution effect towards bank b emanating from the shock of bank b' will be larger

[16]Let ρ_{bd} the correlation between $d_{f,t}$ and $BankExposure$. Then under simplifying assumptions, $cov(FirmExposure, d) = cov(BankExposure, d) = \rho_{bd}$ and $\bar{\beta}_{OLS} = (\beta + \gamma) + \frac{\rho_{bd}}{Var(FirmExposure)}$ (equation (21) in Appendix C).

[17]Intuitive functional forms for $BankExposure_{-b,r,t}$ are the bank-shares weighted mean of the shocks of other banks or the shock of the bank with the highest share apart from bank b.

the larger the share of bank b' in firm f borrowing. It requires significant within-firm variation in bank shares $\omega_{b,f,t}$ – otherwise the weighted average would just come down to a simple average in which case (5) is not identified – as well as a substantial set of firms with $n_{f,t} > 2$.

The second source of identification relies on variation in the number of banks per firm $n_{f,t}$. This method is useful if there is no variation in $\omega_{b,f,t}$ or if we believe the substitution term should depend on an equal-weighted mean of other banks' shocks. The intuition is that the greater the number of banks per firm, the smaller the substitution effects directed towards each individual bank. Substitution means that a firm can partially offset a negative shock from bank b by increasing its demand to its $n_{f,t} - 1$ other banks. If $n_{f,t}$ is large, then each one of the other banks will receive only a small share of this increased demand. If $n_{f,t}$ varies across firms and $n_{f,t} \perp BankExposure_{b,r,t}$ and $n_{f,t} \perp d_{f,t}$, we can use this intuition to disentangle the direct effect of the shock from substitution. Practically speaking, by collecting the coefficients from equation (5) estimated (i) with the firm fixed effects but without the substitution term, (ii) without the firm fixed effects but with the substitution term and (iii) without the firm fixed effects and without the substitution term, one obtains an invertible system of 3 equations in the 3 unknowns β, γ and ρ_{bd} (the correlation between $d_{f,t}$ and $BankExposure$).

These two methods both allow me (i) to get unbiased estimates of the effect of credit supply shocks β and of substitution effects γ ; and (ii) to quantify the firm-level effect accounting for the possibility of substitution across banks, which is equal to $\beta+\gamma$. Besides, recovering the true β allows to recover unbiased estimates of $d_{f,t}$.[18]

4.4 Firm-level real effects

Once we have investigated the effect of PSE debt shocks on firm-level credit outcome, a natural question is whether they also have effects on firm-level real variables like investment and employment. Let us denote these outcomes $Y_{f,t}$. The key issue here is to

[18]This renders possible the procedure of Cingano et al. (2016) and Jiménez et al. (2019) and estimating (3) including $\hat{d}_{f,t}$ would then precisely lead to $\bar{\beta} = \beta + \gamma$.

disentangle the effect of the credit from unobservable shocks to growth opportunities. As was the case for credit outcomes, exposure to PSE debt shocks is likely to be correlated with (unobservable) firm-level determinants of $Y_{f,t}$. Unlike for credit outcomes, we cannot resort to a within-firm estimator to control for unobservable firm-level determinants of $Y_{f,t}$. However, as shown in Appendix C, once we have recovered unbiased estimates of the firm-level demand shocks $d_{f,t}$ we can use the logic of Cingano et al. (2016) and Jiménez et al. (2019) to recover the coefficient of interest. I estimate the following equation:

$$\Delta Y_{f,t} = \mu FirmExposure_{f,t} + \Phi \cdot \mathbf{X}_{f,t} + \tilde{d}_{f,t} + \varepsilon_{f,t} \tag{6}$$

where $\tilde{d}_{f,t}$ are estimates of $d_{f,t}$ recovered after estimating the values of β and γ and $\mathbf{X}_{f,t}$ are firm-level controls. Consistency with the estimation of equation (1) requires that $\mathbf{X}_{f,t}$ contains the firm-level weighted averages of the bank-region controls $\mathbf{X}_{b,r,t}$. I also include additional firm-level controls.

The coefficient μ measures the effect on firms of the crowding out channel of PSE debt shocks that operates through banks, keeping constant any other channel through which PSE debt shocks may affect firms. It is therefore different from the total effect of PSE debt on firms (e.g. through demand effects).

Here, the main threat to identification is the correlation between FirmExposure and firm-level unobservable shocks to Y_f. As shown by Cingano et al. (2016) and Jiménez et al. (2019), including the estimated $\tilde{d}_{f,t}$ controls for the correlation between $FirmExposure$ and firm-level shocks to D_f. When looking at the effect of $FirmExposure$ on other outcomes than debt, the identifying assumption is that the firm-level unobservable determinants of Y_f are the same as the unobservable determinants of D_f so that $\tilde{d}_{f,t}$ properly controls for the correlation between $FirmExposure$ and firm-level shocks to Y_f (see Appendix C for more details).

I further strengthen the validity of my identification by looking at the effect of $FirmExposure$ *within* county×time cells, that is within firms experiencing a similar local-level increase in PSE debt, but *across* firms differentially exposed to this increase through their banking relationships. I thus partial out the local-level macroeconomic relationship between government debt an private firms' prospects. Further interacting these fixed effects with industry dummies allow for the local macroeconomic effect of government debt to differ at the sectoral level. I also include main bank×time fixed effects in my specification, so that I compare firms that are matched to the same bank.

Panel B of Table 2 shows whether firms with higher PSE debt shocks exposure are systematically different. As for Panel A, I report both unconditional correlations and correlations conditional on the set of fixed effects included in my baseline specification as well as controlling for whether the firm borrows from banks that are active in the PSE debt market. Panel B shows that conditional on these controls, firms with high $FirmExposure$ are similar to firms with low $FirmExposure$ on size, operating margin, investment rate, productivity, leverage, tangibility ratio and cash ratio, and have a slightly higher ROA and a slightly lower ratio of interest expenses over value added. Hence, if anything, highly exposed firms seem to be slightly healthier. Section 7.2 provides further test that support my identification strategy.

5 The crowding out effect of government entity debt shocks on corporate credit

5.1 Baseline results

I start by presenting the within-firm results corresponding to model (1), which are reported in Table 3. In my baseline results, the sample contains all banks, all firms except those in sectors highly reliant on government contracts, and I focus on regions with positive aggregate PSE debt growth, since the crowding out mechanism is most likely to be at play when the aggregate demand for government debt increases. Table B.1 presents the results for alternative samples.

[Table 3 about here.]

In the first column of Table 3, I investigate the relationship between exposure to government debt shocks and credit supply in a simple OLS model. In other words, I estimate model (1) without firm×time fixed effects or controls. I find that banks' exposure to government debt shocks significantly predicts lower credit to firms. In Column (2) I add a large set of bank- and bank×region-level controls and find a very similar point estimate.

As previously explained, these results could potentially be driven by a contemporaneous, unobservable decline in firms' credit demand. To address this concern, I augment my model with firm×time fixed effects (Column 3). This specification only exploits within-firm variation, comparing changes in credit provided to the same firm by different intermediaries. Also in this case, I find a negative relationship between government debt exposure and credit. The coefficient is slightly larger but of similar magnitude as that of Column (1). My baseline specification is Column (4) which includes firm×time fixed effects as well as bank- and bank×region-level controls to ensure that the coefficient of interest is not driven by variation in other banks' characteristics. The point estimate is again very similar.

Columns (5) and (6) show that my results continue to hold under even stricter identification assumptions. Column (5) augments model (1) with bank×time fixed effects. These fixed effects control for any time-varying bank-level shocks that may be correlated to bank's exposure to government debt shocks. This specification is very conservative: it identifies whether *within* bank, regions that see a larger increase in the demand for PSE debt experience a larger reduction in bank lending to private firms. Thus, if capital flows perfectly across regions within each bank, one should not see any effect. The coefficient is smaller in magnitude but remains very significant. This alleviate the concern that my results are driven by unobservable bank-specific shocks correlated to BankExposure. Column (6) adds bank×region fixed effects so that the effect is identified using only time-series variation *within* bank×region cells, accounting for any pattern of assortative matching between regions and banks. While I lose all the variation related to cross-sectional heterogeneity across regions, I still find a significantly negative coefficient, with a magnitude also slightly lower.

The comparison between columns (2) and (4) shows that adding or removing firm×time fixed effects to control for credit demand only slightly affect the size of the lending channel coefficient. This result suggests that correlation between changes in demand and supply at the firm level is not a sizable force in my setting.

Finally, Columns (7) and (8) provide the result of the estimation when the shift-share $BankExposure$ is used as an instrument for the actual shock received by the bank $\Delta GovDebt_{b,r,t}$. In Column (8) I further restrict the sample to $\Delta GovDebt_{b,r,t}$ positive. To gauge the economic significance of the point estimate, I perform a simple quantification exercise. When government debt increased by 1€, how much less does the private sector receive? Suppose that for exogenous reasons one additional euro of government debt must be absorbed by the median bank × region. Then, $\Delta GovDebt_{b,r,t}$ would increase by 1 divided by the median $GovDebt_{b,r,t}$ (€35M). This leads to a decline in borrowing from exposed banks relative to the other banks equal to $0.0244 \times \frac{1}{35\times10^6}$ in $\Delta D_{f,b,t}$, which for the median firm represents $0.0244 \times \frac{307\times10^3}{35\times10^6} = 0.0002$€. Considering that the median bank×region lends to 3500 firms, this leads to a decline in lending by this bank×region of 0.78€.

Substitution across banks and firm-level effects. As explained in Section 4.3, the standard within-firm estimator is biased if affected firms attenuate the effect of the shock by shifting some of their demand for loans to banks not exposed to the shock. To account for this possibility, I implement the method outlined in Section 4.3 and detailed in Appendix C to disentangle the direct effect of PSE debt shocks from substitution across banks.

I first examine the assumptions upholding these two methods. Recovering β and γ using variation in bank shares and a functional form for $BankExposure_{-b,r,t}$ requires that we have sufficient variation in $\omega_{b,f,t}$ within firms and that we have a sufficiently high number of observations with $n_{f,t} > 2$ to perform the estimation. Figure 5 provides evidence for these requirements. Panel (a) shows that 9% of the full dataset (58% of the multibank firms dataset) satisfy $n_{f,t} > 2$ while panel (b) shows that there exist significant within firm variation in shares, the mean (median) of the distribution of $|\omega_{b,f,t} - \frac{1}{n_{f,t}}|$ being around 0.15 (0.18). As for the second method for recovering β and γ, it requires that there is enough variation in $n_{f,t}$ across firms, along with the orthogonality conditions $n_{f,t} \perp BankExposure_{b,r,t}$ and $n_{f,t} \perp d_{f,t}$. While the last orthogonality condition cannot be tested, I find that the correlation between $n_{f,t}$ and $BankExposure_{b,r,t}$ is equal to -0.02 with a p-value smaller than 10^{-5}, so that I can strongly reject the claim that $n_{f,t} \perp BankExposure_{b,r,t}$. This methodology is therefore not suitable to be used in this case, and I instead rely on the first.

[Figure 5 about here.]

Table 4 presents the results of the analysis. In the left panel, I use within firm variation in bank shares along with a functional form for the substitution term to disentangle the direct effect from PSE debt shocks from the substitution channel. I find that the main effect β remains significantly negative, while the coefficient on the substitution term γ is also negative, although not statistically significant. This suggests that if firm f's other banks face a large shock (controlling for bank b's shock), firm f will end up borrowing even less from bank b, compared to a situation in which firm f's other banks are not shocked. This is the opposite of substituting across banks to alleviate the effect of one bank's shock. While this result appears counterintuitive, the mechanism could be the following: my within-firm diff-in-diff specification only captures relative effects across banks differentially exposed to PSE debt shocks. However, PSE debt shocks affecting all banks in a region may lead to an overall rarefaction of capital at the local level, which further exacerbates the crowding out effect. While our empirical setting is not well-suited to study this kind of local general-equilibrium effects, the evidence provided in Table 4 points into this direction.

What this implies is that omitting the substitution term in the baseline KM estimator is innocuous (if we consider γ to be 0 because it is not statistically significant) or conservative (if we consider γ to be negative). I the remainder of the text, I thus abstract from the substitution term so that my firm-level analysis probably underestimates the true effect. As a sanity check, Table B.2 reports the between-firm coefficient when estimating (3). Note that the estimated coefficients have very small standard errors. This is precisely due to the fact that running this regression does not add any new information compared to the within-firm stage: if this equation was taken to be the strict weighted sum of equation (1) (without including additional FE, controls or adjusting the winsorization), we would have an R^2 equal to 1.

[Table 4 about here.]

5.2 Investigating the identifying assumption

As discussed in the previous section, a causal interpretation of my analysis relies on the validity of the parallel-trend assumption. That is, lending by exposed banks would have behaved similarly to lending by non-exposed banks in the absence of the government debt shock. While my specification fully controls for demand-side effects, this assumption may be violated if banks receive unobserved credit-supply shocks that are systematically correlated to the Bank Exposure variable. The fact that my results remain significant when including bank×time and bank×region fixed effects is reassuring in this respect. While the parallel-trend assumption is fundamentally untestable, this section provides several tests that support it.

Test #1: Pre-shocks common trend. One worry could be that lending by high exposure banks is systematically on a declining trend, independently of PSE debt shocks. Thus, a way to test the parallel trend assumption is to show that Bank Exposure measured at the time of the shock is not correlated with the patterns of credit before the shock. I perform this exercise by including leads and lags of the dependent variable in my baseline specification (1). Figure 6 shows the absence of a significant pre-trend.

[Figure 6 about here.]

Test #2: Differential sensitivity to the business cycle. Another version of this argument is that high exposure banks may have a differential sensitivity to the macroeconomic cycle which is itself correlated to government debt growth. For instance, if government debt is countercyclical and if high exposure banks have a higher sensitivity to the business cycle, it might explain why lending by more exposed banks falls by more in bad times i.e. when government debt rises. Table B.3 investigates these two hypothesis. Panel A of Table B.3 looks at the correlation between GDP growth and government debt growth, at the national and at the regional level. I find that at the national level, the growth of total PSE debt is negatively correlated with GDP growth, but that growth in the bank debt of PSEs is uncorrelated to GDP growth. This is because the bank debt of PSEs mostly represent the debt of local governments, which is much less correlated to aggregate GDP. At the regional level, I find that growth in the bank debt of PSEs is again negatively correlated to regional GDP growth, although this correlation is much smaller than the one between aggregate GDP and aggregate government debt growth. Panel B of Table B.3 provides estimates of the beta of corporate credit growth with respect to GDP growth for banks when I sort banks according to their exposure to PSE debt shocks. I find that the beta of corporate credit growth with respect to GDP growth (national or regional) is either equal or lower for high exposure banks compared to low exposure banks. Hence, if anything, high exposure banks are less sensitive to the business cycle and this should bias the results against finding a disproportionate decrease in corporate lending by more exposed banks in bad times.

Test #3: Placebo exercise with randomly assigned government debt shocks. Another issue may be that banks which conduct a large share of their business in some areas, which happen to be the areas with high PSE debt growth, are systematically subject to credit supply shocks that are related not to PSE debt shocks but to the geography of their operations. One way to rule out this hypothesis is to conduct a placebo exercise in the spirit of Adao et al. (2019) in which I estimate the effect of a shift-share regressor constructed with randomly generated PSE debt shocks for each county but the actual county weights for each bank-region. The 100 placebo samples thus differ exclusively in the random shifters, drawn from a normal distribution with mean and variance equal to that of the distribution of county-level PSE debt shocks. For each sample, I compute β as in model (1) and test the null hypothesis that $\beta = 0$. Since the shifters were randomly generated, their true effect is indeed zero. However, if my results were only driven by the matching pattern between banks and counties, I would find a negative and significant coefficient. Figure A.3 shows the cumulative distribution function of the estimated t-stats and shows that we find that the rejection rate for 5% level tests is 6%.[19]

All in all, this evidence suggests that banks with lower exposure to government debt shocks represent a valid control group for more exposed intermediaries, providing strong support for the identifying assumptions behind my empirical strategy.

5.3 Heterogeneous effects on firms

In this section, I explore whether the crowding out effect is stronger for some types of firms. This sheds light on which firms tend to be rationed first when government debt crowds out corporate credit. To so so, I estimate equation (1) where the $BankExposure_{b,r,t}$ variable is interacted with firms characteristics at time $t-1$. I find evidence of strategic behavior by banks when they have to cut corporate lending: the effect is stronger for small firms, newly created firm-bank relationships, while the effect is weaker for firms that represent a large share of the bank's portfolio or firms that have experienced a downgrade in their credit ratings. This suggest that banks do not necessarily cut credit to the least performing firms first, but instead favor firms that are strategic to them. This result is in line with previous evidence (Blattner et al. (2019)). It underlines an additional detrimental effect of PSE debt shocks which potentially trigger an increase in credit misallocation.

[19] Adao et al. (2019) show that in the classic sectoral shifters-labor shares setting, a caveat of shift-share designs is that regression residuals may tend to be correlated across locations with similar sectoral shares, leading to an overrejection of the null hypothesis problem. This is less of an issue in our case since the shares are defined using a variable that is not the outcome variable.

[Table 5 about here.]

5.4 Exploiting the near failure of Dexia as a natural experiment

The $BankExposure$ measure defined in Section 4 has the value of being general, in that it can be attributed to all firms and measured at any date for which there is bank-firm data on credit granted. This feature allows to investigate the crowding out phenomenon in a systematic manner and not only in crisis time. Furthermore, the panel variation in $BankExposure$ allows to test whether the crowding out channel operates within bank-region units, as in Column 6 of Table 3. However, since its construction relies on bank-counties connections as well as realized PSE debt shocks, estimates of equation (1) might suffer from the identification problems highlighted in Section 5.2. Although I have already discussed several robustness exercises to mitigate such concerns, here I propose an alternative strategy to strengthen the robustness of my results: I use the 2008 near-failure of Dexia as a specific "natural experiment" in which credit supply shifts were arguably exogenous with respect to bank observed and unobserved characteristics. In addition, such variation came unexpectedly both to lenders and to borrowers.

Namely, I exploit the failure of Dexia – a Franco-Belgian bank specialized in lending to local PSEs – as an exogenous shock forcing municipalities which relied heavily on Dexia to borrow more from their other relationship lenders, thereby creating a large demand shock for other banks. I defined the variable $DexiaExposure$ at the bank×region level in a manner very similar to my baseline $BankExposure$ variable:

$$DexiaExposure_{b,r} = \sum_{c \in r} \omega_{b,c,2008} \times DexiaDependent_{c,2008} \tag{7}$$

where $DexiaDependent_{c,t}$ is a dummy equal to 1 if the market share of Dexia in 2008 is above the sample median and $\omega_{b,c,2008}$ are as before the share of each county within region r in bank b's PSE loan portfolio. I then estimate the following equation:

$$\Delta D_{f,b,t} = d_{f,t} + \beta_1 DexiaExposure_{b,r} \times Post_t + \beta_0 DexiaExposure_{b,r} + \Phi \cdot \mathbf{X}_{b,t} + \Psi \cdot \mathbf{Z}_{r,b,t} + \varepsilon_{f,b,t} \tag{8}$$

where $Post_t$ is a dummy equal to 1 after the near-failure of Dexia in October 2008.

Figure A.4 compares lending patterns to PSEs when banks are sorted along the $DexiaExposure$ variable. It shows that banks who operated in counties largely dependent on Dexia experienced a large increase in their lending to PSEs after the near-failure of Dexia. Table B.5 shows how this increase in the demand for government debt at the bank-region level crowded out lending to private firms.

6 Exploring the mechanism

6.1 What prevents banks from increasing their supply of credit?

The driving mechanism for my results is the presence of constraints that prevent more exposed banks from meeting corporate loan demand in the face of strong government debt demand. As explained in section 2, the severity of the crowding out effect is inversely related to the elasticity of the supply of capital at the bank- or bank×region-level.

Ideally, bank should be able to match the additional demand of credit by an additional supply of capital, by borrowing more (from depositors or from the interbank market) or by raising additional equity. However, banks only have a limited ability to attract more deposits or to raise equity. Banks relying heavily on the interbank market are thought to have a more elastic supply of capital, but again there are frictions. Besides, banking regulation constrains the total amount of lending banks can do.

This section shows how the severity of the crowding out effect is related to the elasticity of the supply of capital at the bank level and at the bank×region level, and investigates its drivers.

At the bank-level, I show that the mechanism is related to banks' inability to increase the size of their balance sheets, that is a low elasticity of the supply of capital. I use several proxies for banks' balance sheet constraints: banks with high reliance on deposits, bank already highly indebted on the interbank market, cash-poor banks and highly leveraged banks should be more constrained. I then interact these proxies with the $BankExposure$ variable in (1). Columns (1) to (7) of Table 6 show that these proxies explain well the variation in the severity of the crowding out effects across banks.

[Table 6 about here.]

My results also uncover that *within banks*, more exposed regions have trouble meeting corporate loan demand in the face of strong government debt demand. There can be two types of frictions explaining this phenomenon: frictions in the functioning of banks' internal capital markets (inelastic supply of capital at the bank×region level) or frictions related to the time constraints of loan officers (inelastic supply of labor at the bank×region level). Columns (8) of Table 6 show that the within bank crowding out effects are stronger for smaller banks, for which the internal capital market arguably offers less ability to smooth shocks. In Columns (10) I compare the severity of the crowding out effects between national banks and networks of cooperative banks which have cash-management agreements but keep separate balance sheets, and I find stronger effect for networks of cooperative banks, which is also in line with the internal capital markets hypothesis. To further disentangle the two hypothesis, I rely on the insight that if the constraint is related to loan officers not having enough time to process more loans, the constraint should operate at the bank branch level and not the region level so that we should observe crowding out within bank branches. I repeat the analysis by constructing the $BankExposure$ variable at the bank branch level. Table B.4 presents the results. I find that the $BankExposure$ variable constructed at the branch-level has less explanatory power, in particular when we estimate the effect without bank×time FE. When including both measures, the $BankExposure$ measure constructed at the bank-region level seems to have more bite in terms of significance than the one constructed at the bank-branch level, although the coefficients have a similar size. Overall, it seems that at least part of the effect is related to frictions in internal capital markets.

7 Effect of government entity debt shocks on real corporate outcomes

7.1 Baseline results

The previous results show that: (i) lenders that are more exposed to government debt shocks reduce their supply of credit to private firms ; (ii) this effect is not undone by firms substituting across lenders so that these shocks impact firm-level borrowing capacity.

I now investigate whether the contraction in credit has an effect on firm-level real variables. To do so, I estimate model (6). The outcomes I consider are investment, measured as the growth rate of fixed assets ($gr(Assets)$) and employment, measured as the growth in the number of employees ($gr(Emp)$). As the frequency of the corporate tax files data is annual, I repeat the construction of the BankExposure and FirmExposure variables at the annual frequency (from Q4 in $t-1$ to Q4 in t). To merge this data with the tax files, I redefine year t as year $t-1$ for firms for which fiscal year t ends before September of calendar year t (33% of the sample) so that the outcome variables measured from the tax files are never anterior to the time period when the shock is defined. In robustness checks, I restrict the sample to firms reporting in Q4.

As explained in Section 4 and further detailed in Appendix C, I address the potential correlation between FirmExposure and unobservable firm-specific shocks to $Y_{f,t}$ by including the vector of estimated coefficients $\hat{d}_{f,t}$ in the regression. The assumption is (i) that these are unbiased estimates of the true $d_{f,t}$ and ; (ii) that the unobserved firm-specific determinants of credit are the same as the unobserved firm-specific determinants of $Y_{f,t}$.

Table 7 presents the results from estimating (6).

[Table 7 about here.]

Columns (1) to (4) report the effects on employment, which is very close to zero and never statistically significant. Columns (5) to (8) report the effects on investment. Column (5) reports the coefficient when including the estimated $\hat{d}_{f,t}$, the weighted-average of bank controls, firm-level controls as well as sector×county×time FE and lead bank×time FE. I find the coefficient is negative and statistically significant. This coefficient is estimated on the cross-section of firms (conditional on the FE), hence a potential caveat is that firms with high exposure are systematically different in their investment behavior, in a manner that is not captured by the credit demand parameter $\hat{d}_{f,t}$.

Column (6) to (8) address this concern by showing the same coefficient under more stringent identification assumptions. Column (6) includes lagged credit growth as a control. Column (7) includes the lagged dependent variable, i.e. lagged investment, as a control. Finally, column (8) adds firm FE. My preferred specifications are (7) and (8). Specification (7) restricts the comparison to firms with similar investment dynamics in $t-1$. The rationale is that investment is positively autocorrelated at the firm-level, so that controlling for the lagged value ensures that the effect measured in t comes from the difference in exposure at t and not from differences in lagged values. Specification (8) includes firm FE. In this case, the identification purely comes from within-firm changes, ruling out that our results are driven by firms who invest little having a systematically high FirmExposure.

In terms of economic magnitude, I find that a one-standard deviation increase in firm exposure to government debt shocks leads to a xx% decline in asset growth at the firm level, which amounts to xx% of the mean of this variable. Section 8 provides an estimation of the aggregate effect on capital and output loss.

7.2 Robustness checks

TBC

8 Aggregate implications

TBC

9 Conclusion

In this study, I document the existence of a crowding-out effect of the bank debt of PSEs, operating locally through the banking system. I show that crowding out effects, which have for long been hypothesized in macroeconomics, do exist and have sizable real effects. These effects arise as long as capital markets are segmented, which is the case not only across countries but also across banks or geographical areas within countries. I provide an estimate of the size of the crowding out effect using variation in PSE debt dynamics across French regions. This estimate can be compared to the local-level fiscal multipliers for instance in Adelino et al. (2017).

References

Acharya, V. V., T. Eisert, C. Eufinger, and C. Hirsch (2018). Real effects of the sovereign debt crisis in europe: Evidence from syndicated loans. *The Review of Financial Studies 31*(8), 2855–2896.

Adao, R., M. Kolesár, and E. Morales (2019). Shift-share designs: Theory and inference. *The Quarterly Journal of Economics 134*(4), 1949–2010.

Adelino, M., I. Cunha, and M. A. Ferreira (2017). The economic effects of public financing: Evidence from municipal bond ratings recalibration. *The Review of Financial Studies 30*(9), 3223–3268.

Akkoyun, H. C. (2018). How does government debt impact corporate financing? evidence from war finance.

Becker, B. and V. Ivashina (2017). Financial repression in the european sovereign debt crisis. *Review of Finance 22*(1), 83–115.

Blattner, L., L. Farinha, and F. Rebelo (2019). When losses turn into loans: The cost of undercapitalized banks.

Bottero, M., S. Lenzu, and F. Mezzanotti (2018). Sovereign debt exposure and the bank lending channel : impact on credit supply and the real economy.

Chakraborty, I., I. Goldstein, and A. MacKinlay (2018). Housing price booms and crowding-out effects in bank lending. *The Review of Financial Studies 31*(7), 2806–2853.

Cingano, F., F. Manaresi, and E. Sette (2016). Does credit crunch investment down? new evidence on the real effects of the bank-lending channel. *The Review of Financial Studies 29*(10), 2737–2773.

Dagostino, R. (2018). The impact of bank financing on municipalitiesâ bond issuance and the real economy. *Work. Pap., Univ. Rochester, Rochester, NY Google Scholar Article Location*.

Demirci, I., J. Huang, and C. Sialm (2018). Government debt and corporate leverage: International evidence.

Friedman, B. M. (1978). Crowding out or crowding in? economic consequences of financing government deficits. *Brookings Papers on Economic Activity 1978*(3), 593–641.

Graham, J. R., M. T. Leary, and M. R. Roberts (2015). A century of capital structure: The leveraging of corporate america. *Journal of Financial Economics 118*(3), 658–683.

Huang, Y., M. Pagano, and U. Panizza (2017). Local crowding out in china.

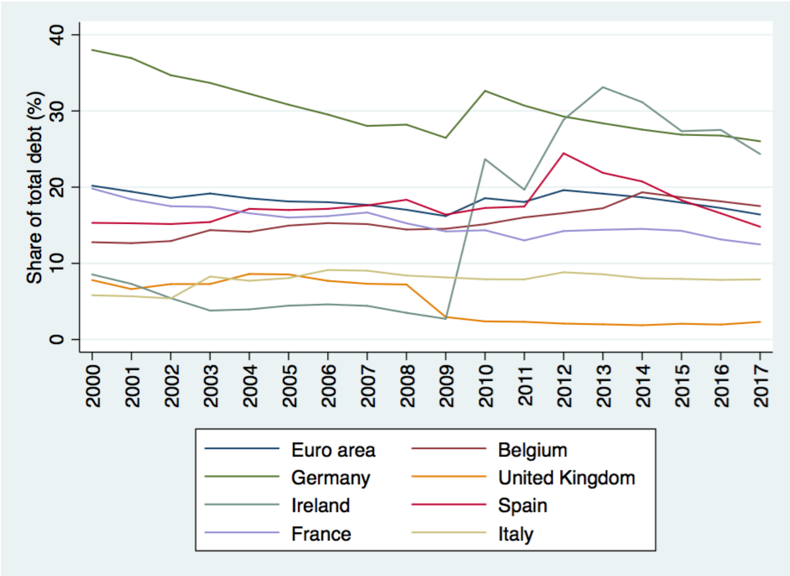
Jiménez, G., A. Mian, J.-L. Peydró, and J. Saurina (2019). The real effects of the bank lending channel. *Journal of Monetary Economics*.

Khwaja, A. I. and A. Mian (2008). Tracing the impact of bank liquidity shocks: Evidence from an emerging market. *American Economic Review 98*(4), 1413–42.

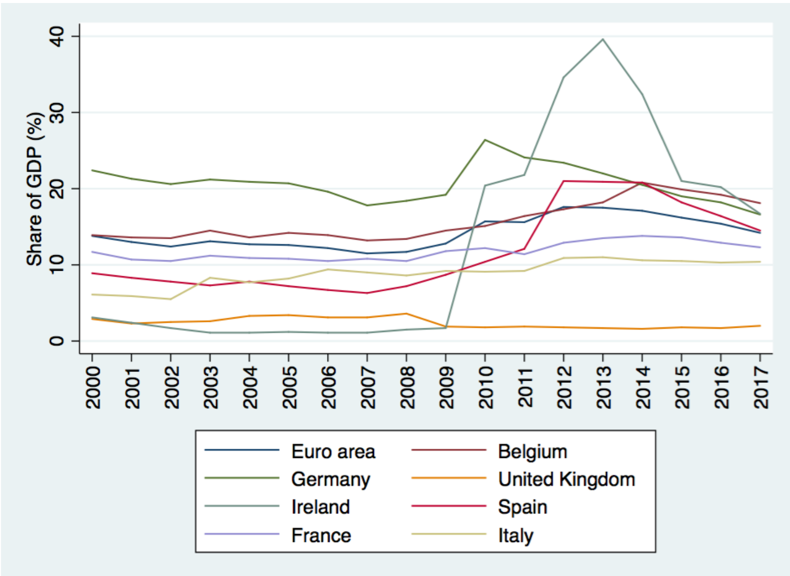
Popov, A. and N. Van Horen (2015). Exporting sovereign stress: Evidence from syndicated bank lending during the euro area sovereign debt crisis*. *Review of Finance 19*(5), 1825–1866.

Figures

Figure 1: Bank debt of government entities in Europe



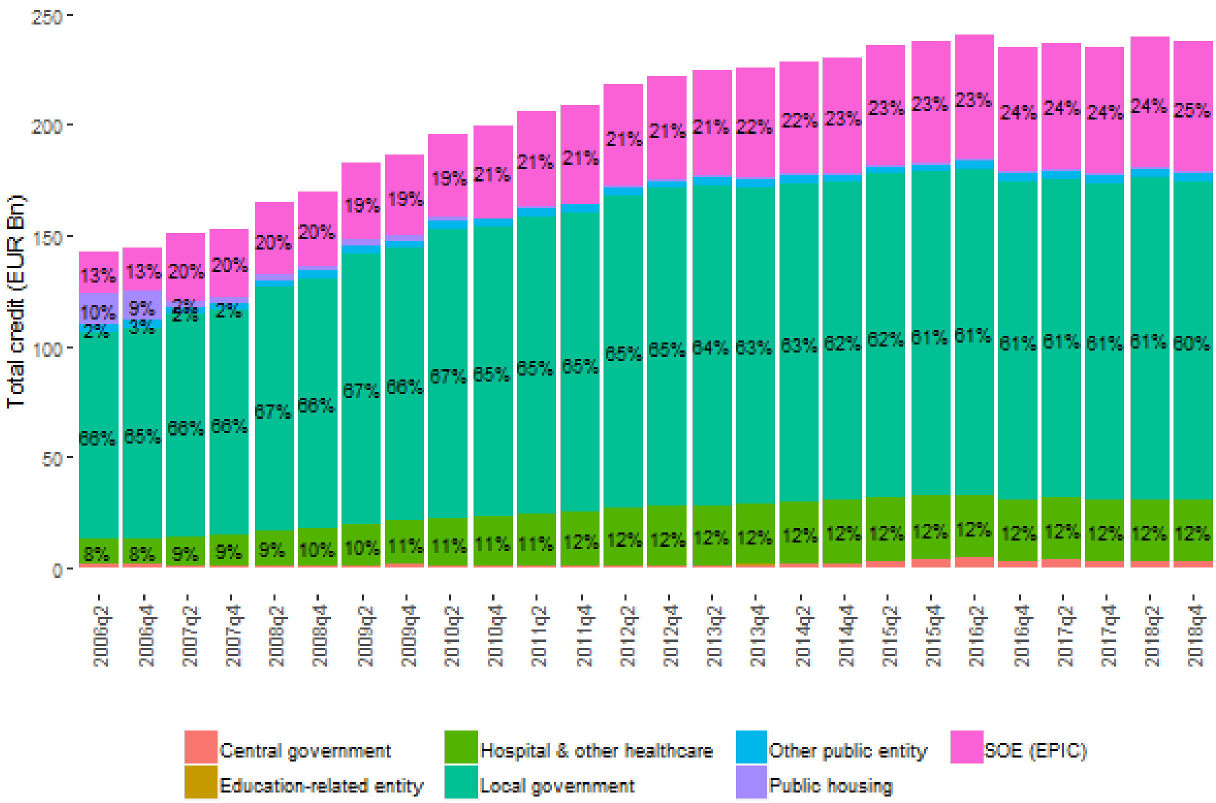
(a) Bank debt of government entities as a share of total debt of government entities



(b) Bank debt of government entities as a share of GDP

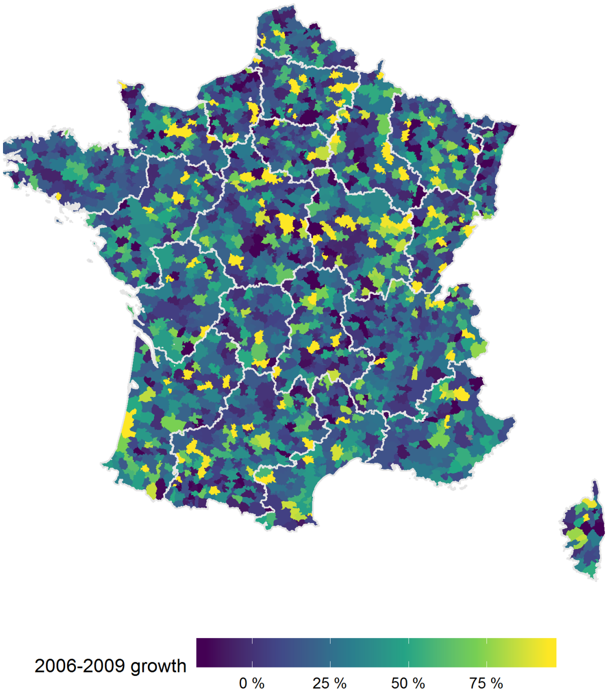
This figure plots the bank debt of public sector entities as a share of their total debt (bank debt and bonds) in subfigure (a) and as a share of GDP in subfigure (b) for several European countries. Data comes from Eurostat.

Figure 2: Loans to public entities by public entity category

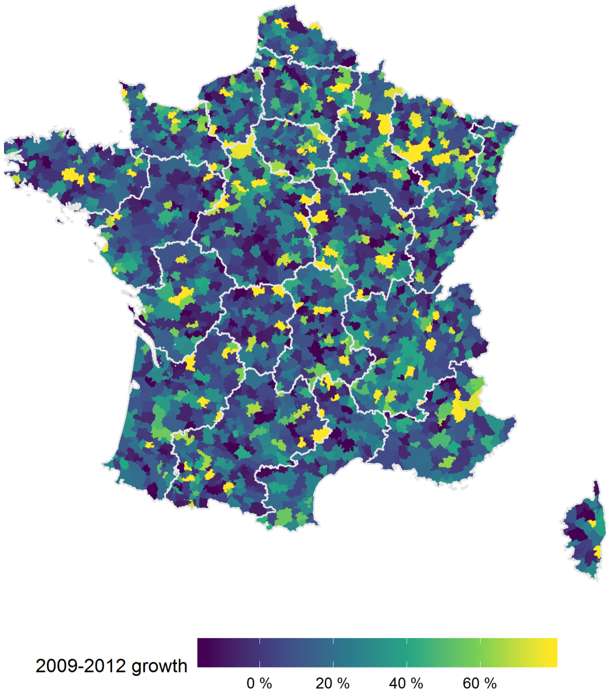


This figure shows the evolution of bank lending to public sector entities by type of public sector entity.

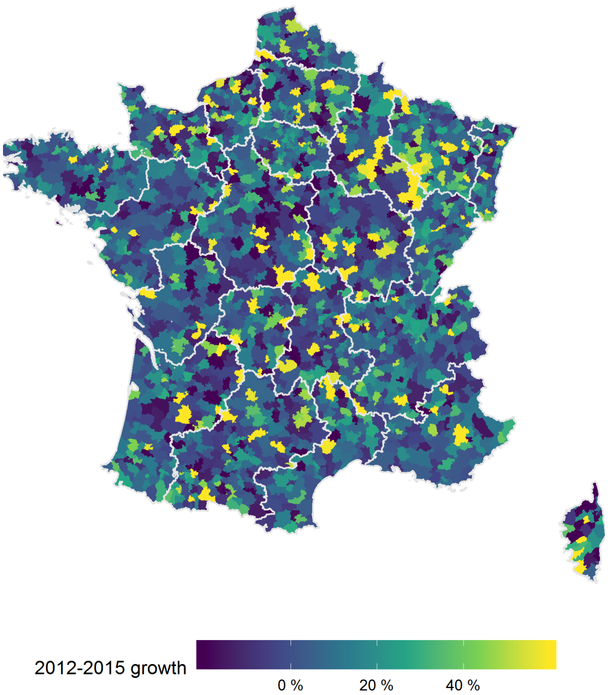
Figure 3: Growth rate of public sector entity debt by county



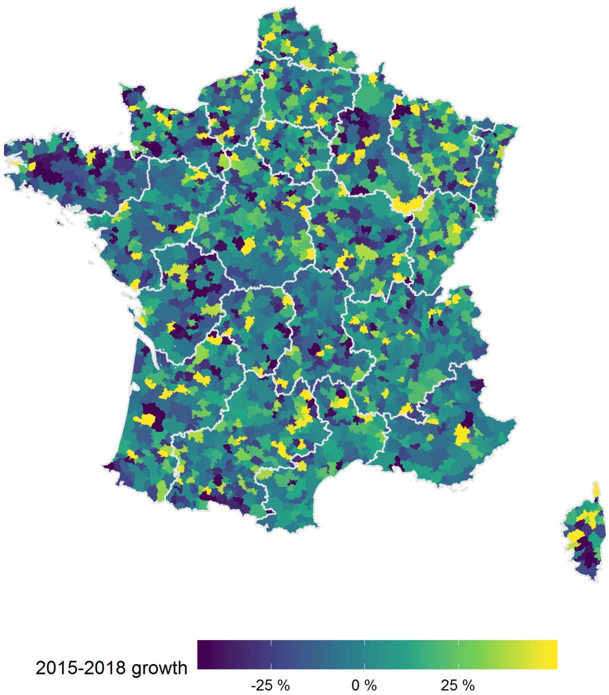
(a) 2006-2009



(b) 2009-2012



(c) 2012-2015

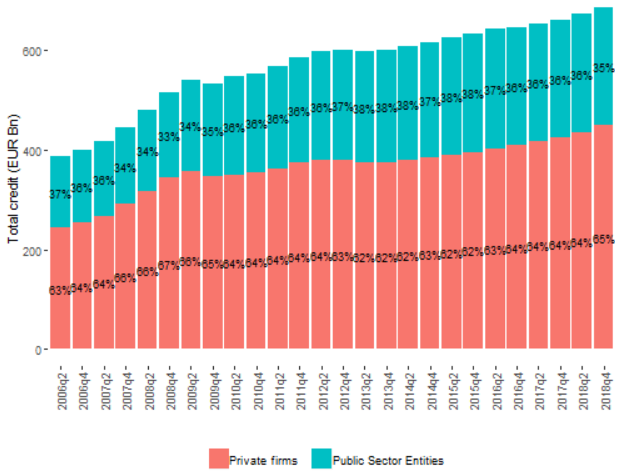


(d) 2015-2018

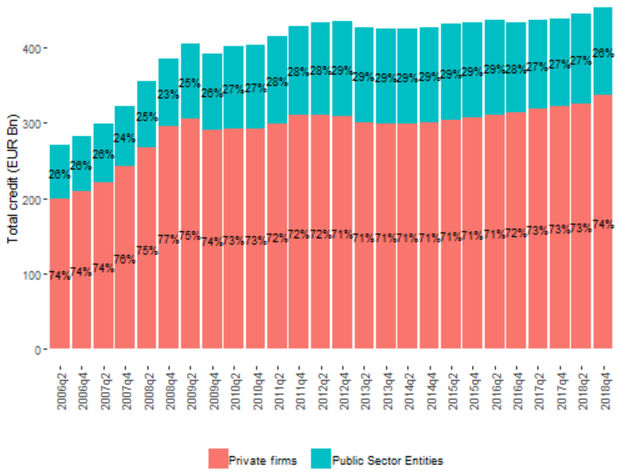
These maps depict the growth rate of bank lending to public sector entities across counties for four equal subperiods. The more towards bright yellow (dark blue), the higher (lower) the growth rate of public sector entity debt. Regional boundaries appear in light gray.

Figure 4: Public sector entity loans in banks' balance sheets

Panel A: Share of public entity loans in banks' loan portfolio in the time series

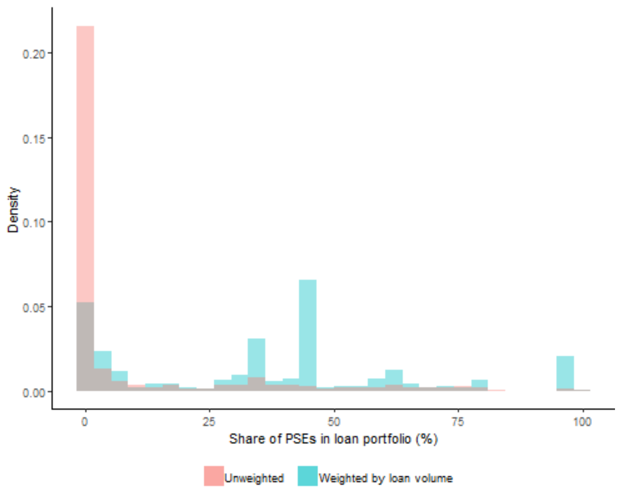


(a) All banks

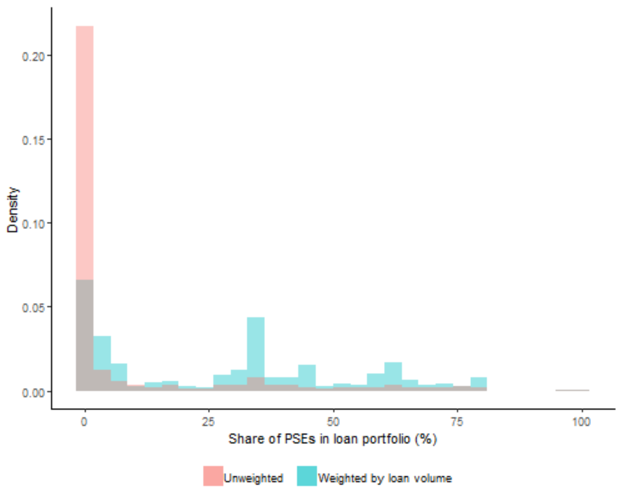


(b) Excluding state-owned banks

Panel B: Share of public entity loans in banks' loan portfolio in the cross-section of banks



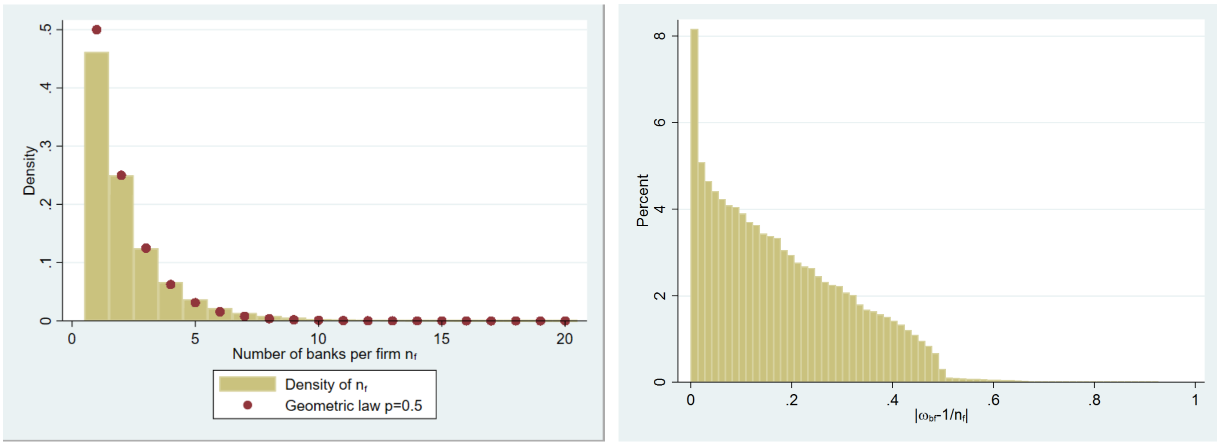
(c) All banks



(d) Excluding state-owned banks

This figure shows the share public sector entities represent in banks' loan portfolio in the time series (Panel (a) and (b)) and in the cross-section of banks (Panel (c) and (d)), for all banks (Panel (a) and (c)) and excluding state-owned banks (Panel (b) and (d)).

Figure 5: Number of banks per firm and banks' shares within firms

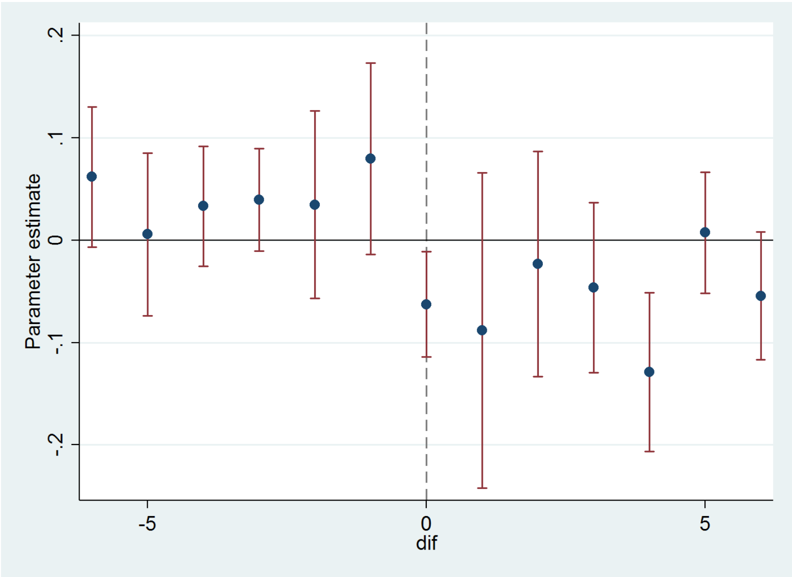


(a) Number of banks per firm

(b) Distribution of banks' shares deviation from equal weights

xxx.

Figure 6: Pre-trends test for the baseline specification



This figure plots the estimated coefficients obtained when regressing credit growth $\Delta D_{f,b,t}$ on leads and lags of $BankExposure_{b,r,\tau}$, with $\tau \in [t-6, t+6]$. The data is at quarterly frequency.

Tables

Table 1: Summary statistics

Panel A: Firm-bank level (quarterly)

	Single bank						Multibank				
	count	mean	sd	p10	p50	p90	count	mean	sd	p10	p50
Outstanding credit $D_{f,b,t}$ (K EUR)	23,070,465	175	365	20	77	368	16,396,476	344	757	16	96
Credit growth $\Delta D_{f,b,t}$ (norm. diff.)	20,080,195	-0.042	0.089	-0.119	-0.040	0.000	15,048,697	-0.029	0.099	-0.106	-0.021
Credit growth $\Delta D_{f,b,t}$ (MPGR)	23,070,465	-0.012	0.664	-0.167	-0.039	0.177	16,396,476	-0.016	0.700	-0.245	-0.050
Bank-region PSE debt $GovDebt_{b,r,t}$ (K EUR)	23,070,465	499427	668437	343	241896	1310623	16,396,451	365537	630236	0	42516
Bank-region PSE debt growth $\Delta GovDebt_{b,r,t}$	22,127,653	-0.000	0.059	-0.043	-0.003	0.053	15,536,723	-0.003	0.059	-0.046	-0.001
$BankExposure_{b,r,t}$	22,909,503	0.008	0.021	-0.010	0.004	0.032	16,283,878	0.007	0.021	-0.010	0.001

Panel B: Firm level (yearly)

Table 2: Correlation between exposure to PSE debt shocks and pre-determined characteristics

Panel A: Correlation between *BankExposure* and banks' characteristics

	Unconditional		Conditional	
	Coef.	S.E.	Coef	S.E.
Equity/Assets	-.0103**	(.0047)	-.008***	(.0031)
Deposits/Assets	-.0021**	(.0011)	.0006	(.0006)
Non-Performing Loans/Assets	-.1826***	(.0132)	-.0006	(.011)
log(Assets)	.0004***	(.0001)	.0003***	(.0001)
(Deposits-Loans)/Assets	-.0039***	(.001)	.0007	(.0005)
(Interbank lending - Interbank borrowing)/Assets	-.0007	(.0009)	.0014***	(.0005)
Non-French bank	-.0037***	(.0006)	-.0009**	(.0004)
Cooperative bank	.0021***	(.0005)	-.0004	(.0003)
State-owned bank	-.0054***	(.0006)	-.0017*	(.0009)
ROA	-.3526***	(.0705)	-.0761	(.0543)
Cash/Assets	.3674***	(.1389)	.0623	(.1128)
Non-Performing Assets/Assets	-.0217*	(.0132)	-.022**	(.0111)
Interest paid on deposits/Deposits	-.5171***	(.1542)	-.35***	(.1001)
Net Interest Margin	-.0654***	(.0194)	.0045	(.0188)

Panel B: Correlation between *FirmExposure* and firms' characteristics

	Unconditional		Conditional	
	Coef.	S.E.	Coef	S.E.
log(Sales)	-.002***	(.0002)	-.0001	(.0001)
log(Assets)	-.002***	(.0002)	-.0001	(.0001)
EBIT/Sales	.0108***	(.0011)	.0009	(.0006)
ROA	.0028*	(.0015)	.0013*	(.0007)
CAPEX/Assets	.0006***	(.0002)	.00003	(.0001)
TFP	-.0045***	(.0005)	.0002	(.0002)
VA/Wages	.00002	(.00005)	.00001	(.00002)
Debt/Assets	.0008***	(.0002)	-.00001	(.0001)
Tangibles/Assets	.0045***	(.0011)	.0002	(.0002)
Interest Expenses/VA	.0291***	(.0028)	-.0023*	(.0012)
Cash/Assets	-.0222***	(.0018)	.0001	(.0007)

Panel A shows the correlation between the *BankExposure* variable defined in (2) and various bank characteristics measured at $t-1$. The right panel reports unconditional correlations while the left panel reports correlations conditional on firm×time fixed effects and the bank being active in the PSE lending market. Panel B shows the correlation between the *FirmExposure* variable defined in XX and various firm characteristics measured at $t-1$. The right panel reports unconditional correlations while the left panel reports correlations conditional on county×sector×time fixed effects, lead bank×time fixed effects and the share of lending from banks being active in the PSE lending market. Standard errors are clustered at the region×bank (or lead bank) level. ***, ** and * indicate significance at the 1%, 5% and 10% levels, respectively.

Table 3: The bank lending channel of the crowding out effect

	Growth rate of credit							
	(reduced form)						(IV 2nd stage)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Actual gvt debt gr.							-0.0137	-0.0244**
							(0.0140)	(0.00970)
Bank Exposure	-0.0360***	-0.0382***	-0.0411***	-0.0442***	-0.0350***	-0.0281***		
	(0.00819)	(0.00784)	(0.0145)	(0.0159)	(0.00710)	(0.00611)		
Bank controls	–	✓	–	✓	✓	✓	✓	✓
Firm×Time FE	–	–	✓	✓	✓	✓	✓	✓
Bank×Time FE	–	–	–	–	✓	✓	–	–
Bank×Region FE	–	–	–	–	–	✓	–	–
Observations	8143151	7578731	7884500	7040727	7880216	7879868	6572011	2306424
R^2	0.000	0.001	0.472	0.487	0.505	0.507	0.000	0.000

This table examines the crowding out effect of government debt via the bank lending channel. The outcome variable is the normalized growth rate of total MLT bank loans granted to firm f by bank b. The main independent variable is exposure to public entity debt shocks measured at the bank×region×time level, defined as a shift-share with county level public entity debt shocks as shifters weighted by the share of each county within banks' loan portfolio in the preceding period. All regressions are estimated on the sample of firms with multiple credit relationships in regions with positive public sector entity debt growth. Standard errors are clustered at the region×bank level. ***, ** and * indicate significance at the 1%, 5% and 10% levels, respectively.

Table 4: Recovering the true β in the presence of substitution across banks

	Panel A: Using variation in ω_{bf}		Panel B: Using variation in n_f	
	(1)	(2)	(1)	(2)
	$\phi = 1$	$\phi = +\infty$	$\phi = 0$	$\phi = 0$
β	-0.072**	-0.132**		
	(0.036)	(0.061)		
γ	-0.028	-0.215*		
	(0.032)	(0.112)		

xx

Table 5: The crowding out effect by firms' characteristics

	Growth rate of credit							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Bank Exposure	-0.0299**	-0.0260***	-0.0227**	-0.0516***	-0.0469***	-0.0532***	-0.113***	-0.00832*
	(0.0124)	(0.00998)	(0.00886)	(0.0180)	(0.0167)	(0.0178)	(0.0301)	(0.00505)
High Leverage × Bank Exposure	0.00962							
	(0.0102)							
High Tangibles × Bank Exposure		0.00486						
		(0.00972)						
High Trade debt × Bank Exposure			-0.00137					
			(0.00886)					
Large × Bank Exposure				0.0457***				
				(0.0156)				
Risky × Bank Exposure					0.0197			
					(0.0123)			
Downgraded × Bank Exposure						0.0370***		
						(0.0110)		
Strategic firm × Bank Exposure							0.261***	
							(0.0368)	
Young relationship × Bank Exposure								-0.0767***
								(0.0297)
Bank controls	✓	✓	✓	✓	✓	✓	✓	✓
Firm×Time FE	✓	✓	✓	✓	✓	✓	✓	✓
Observations	2413306	2413312	2414782	7022889	7040727	7040727	5452686	5869872
R^2	0.457	0.457	0.457	0.487	0.487	0.487	0.508	0.518

This table examines the crowding out effect of government debt via the bank lending channel. The outcome variable is the normalized growth rate of total MLT bank loans granted to firm f by bank b. The main independent variable is exposure to public entity debt shocks measured at the bank×region×time level, defined as a shift-share with county level public entity debt shocks as shifters weighted by the share of each county within banks' loan portfolio in the preceding period. *Large* is a dummy equal to 1 for firms with more than 250 employees or revenues above 50 M€and total assets above 43 M€(corresponding to the legal definition of mid-size and large firms in France). *Risky* is defined using the Banque de France credit ratings and is a dummy equal to 1 if Banque de France has received any unfavourable information about the firm (trade bill payment incidents, court rulings or judicial information) or if the firm's ability to meet its financial commitments is deemed to be poor by Banque de France. *PastDowngrade* is a dummy equal to 1 if the firm was downgraded in the past, using the Banque de France credit ratings. *Highleverage* is a dummy equal to 1 if the firm has an above median leverage, defined as Debt/Fixed Assets. *Hightangibles* is a dummy equal to 1 if the firm has an above median tangibles ratio, defined as Tangibles Assets/Fixed Assets. *HighTradeDebt* is a dummy equal to 1 if the firm has above an median Trade Debt/Total Debt ratio. *Newrelationship* is a dummy equal to 1 if bank b has lent to firm f for less than 6 quarters (p25). *Strategicfirm* is a dummy equal to 1 if firm f represents more than xx% of the loan portfolio of bank b (p75). All regressions are estimated on the sample of firms with multiple credit relationships in regions with positive public sector entity debt growth. Standard errors are clustered at the region×bank level. ***, ** and * indicate significance at the 1%, 5% and 10% levels, respectively.

Table 6: The crowding out effect by banks' characteristics

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Bank Exposure	-0.115***	-0.0825***	-0.0974**	-0.0496*	-0.0671**	-0.0731**	0.0413**	-0.0315***	-0.0260	-0.0150
	(0.0293)	(0.0316)	(0.0410)	(0.0286)	(0.0293)	(0.0285)	(0.0205)	(0.0112)	(0.0159)	(0.0105)
Large bank-region × Bank Exposure	0.112***							0.0235*		
	(0.0301)							(0.0130)		
Large bank × Bank Exposure		0.0585*							-0.00610	
		(0.0347)							(0.0181)	
High Capital Ratio × Bank Exposure			0.114***							
			(0.0431)							
Net interbank creditor × Bank Exposure				0.0331						
				(0.0286)						
High NPL × Bank Exposure					0.0679**					
					(0.0300)					
High cash ratio × Bank Exposure						0.0706**				
						(0.0334)				
High Deposits/Loans × Bank Exposure							-0.108***			
							(0.0371)			
Cooperative bank × Bank Exposure										-0.0795
										(0.0581)
Bank controls	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Firm×Time FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Bank×Time FE	–	–	–	–	–	–	–	✓	✓	✓
Observations	5239499	5135899	4239319	4786185	4341083	2921514	4591714	5765023	5534613	4963044
R^2	0.506	0.505	0.506	0.506	0.508	0.511	0.498	0.528	0.523	0.504

This table examines the heterogeneity of the crowding out effect of government debt via the bank lending channel as a function of banks' characteristics. The outcome variable is the increase in firm f's borrowing from bank b, normalized by firm f's total borrowing in the previous period. The main independent variable is $BankExposure$. All regressions are estimated on the sample of firms with multiple credit relationships in regions with positive public sector entity debt growth. Standard errors are clustered at the region×bank level. ***, ** and * indicate significance at the 1%, 5% and 10% levels, respectively.

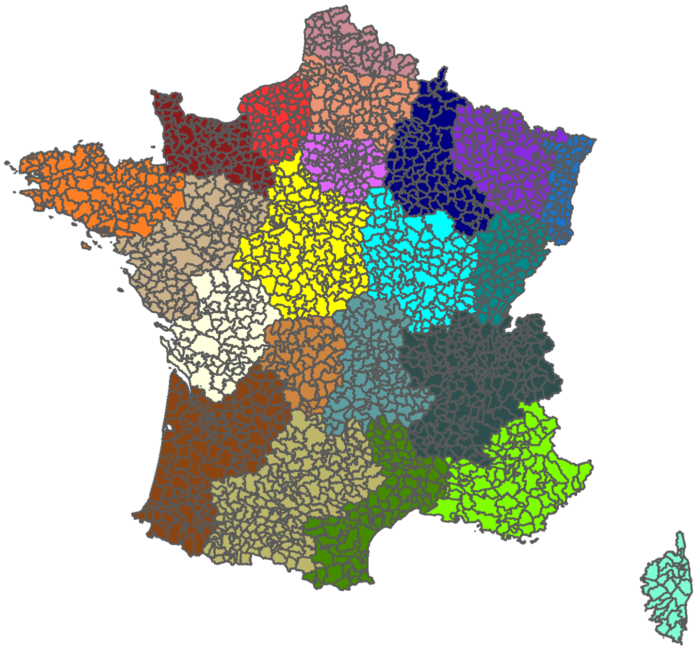
Table 7: Firm-level real effects

	$gr(Emp)$				$gr(Assets)$			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Firm Exposure	0.00336	-0.00163	-0.0234	-0.00421	-0.00728**	-0.00484	-0.0125**	-0.00914*
	(0.0213)	(0.0242)	(0.0283)	(0.0336)	(0.00355)	(0.00392)	(0.00535)	(0.00516)
$\hat{d}_{f,t}$	✓	✓	✓	✓	✓	✓	✓	✓
Firm controls	✓	✓	✓	✓	✓	✓	✓	✓
Wt avg bank controls	✓	✓	✓	✓	✓	✓	✓	✓
Ind×County×Time FE	✓	✓	✓	✓	✓	✓	✓	✓
Lead bank×Time FE	✓	✓	✓	✓	✓	✓	✓	✓
Lagged credit growth	–	✓	–	–	–	✓	–	–
Lagged outcome	–	–	✓	–	–	–	✓	–
Firm FE	–	–	–	✓	–	–	–	✓
Observations	200509	179994	109243	181919	217877	194957	122475	197349
R^2	0.238	0.242	0.262	0.468	0.334	0.337	0.376	0.566

***, ** and * indicate significance at the 1%, 5% and 10% levels, respectively.

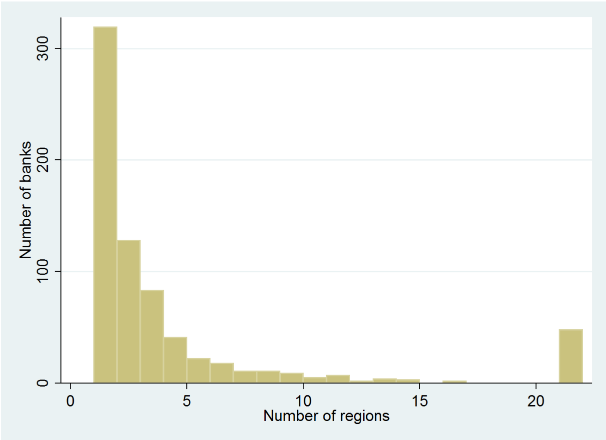
A Additional figures

Figure A.1: Geographic subdivisions

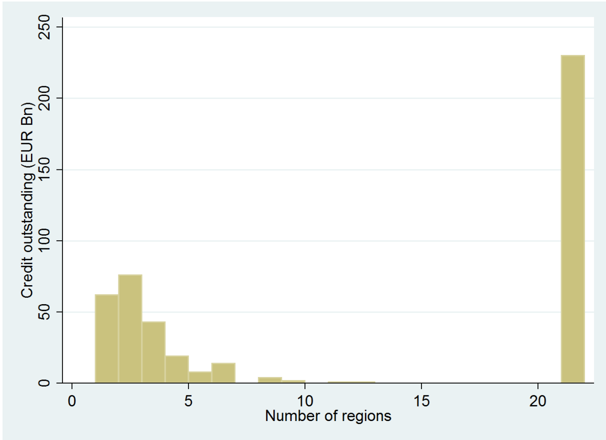


This map shows the geographical subdivisions used in the paper. Grey boundaries delineate municipality groupings (counties). Blocks of different colors distinguish the 22 French regions.

Figure A.2: Geographic footprint of French banks: number of regions in which banks operate



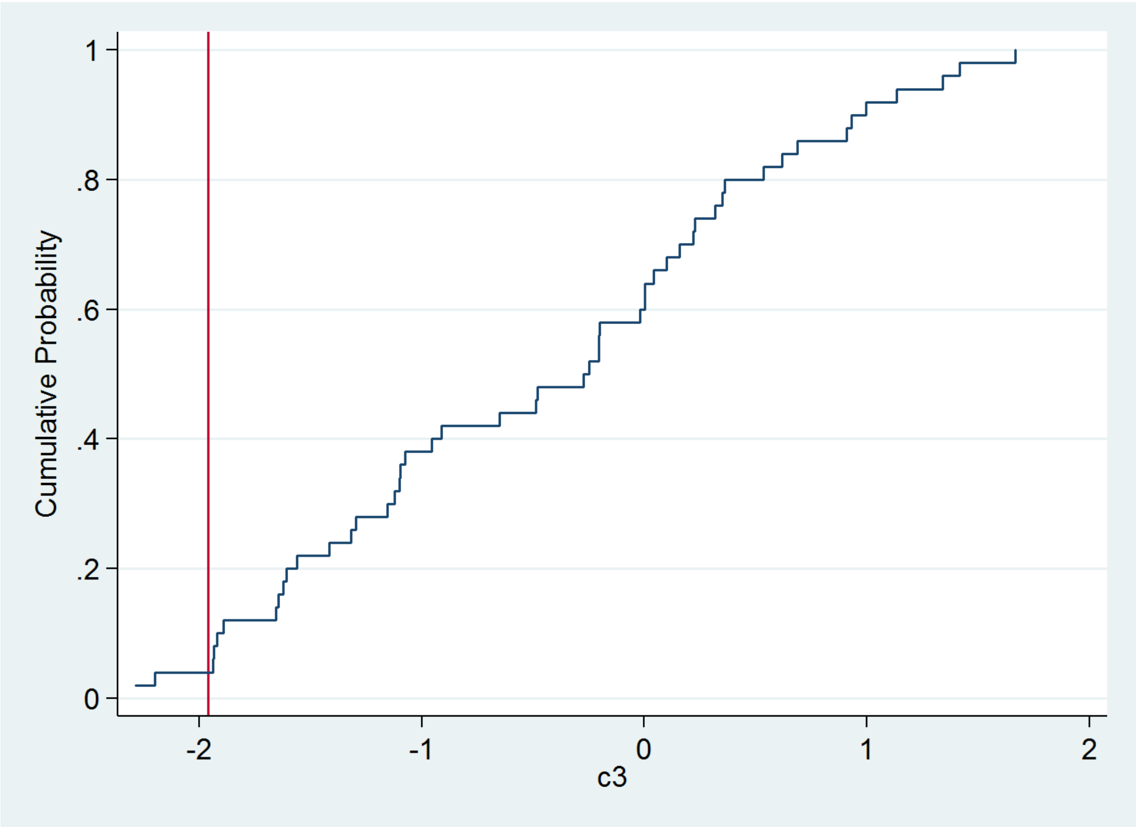
(a) Number of banks



(b) Volume of credit

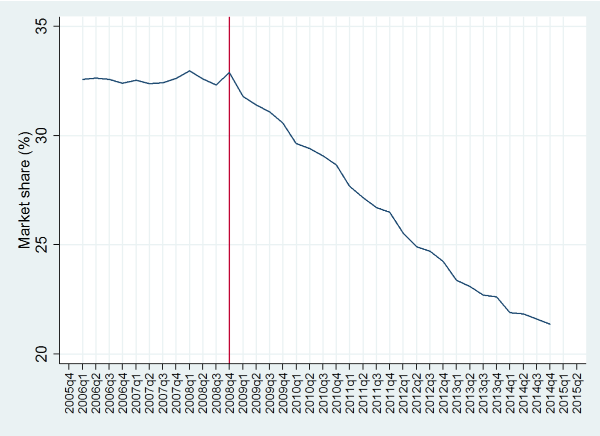
Panel (a) shows the number of banks by bins defined by the number of regions in which a given bank operates. Panel (b) shows outstanding credit by bins defined by the number of regions in which a given bank operates. I do not count regions which represent less than 2.5% in the banks' total loan portfolio.

Figure A.3: Placebo tests for the baseline specification

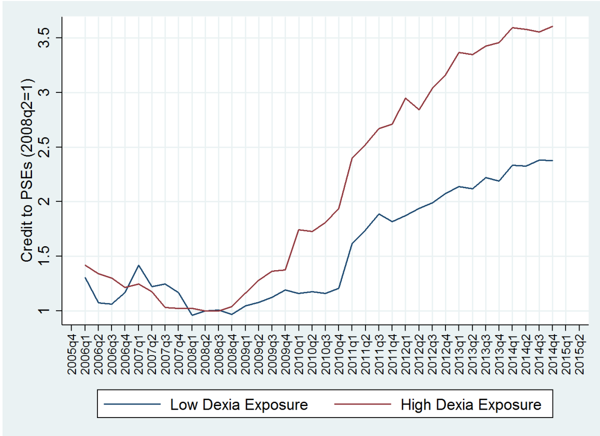


This figure shows the cumulative distribution function of the t-statistics relative to the estimation of β in 100 placebo regressions in which I estimate the effect of a shift-share regressor constructed with randomly generated PSE debt shocks and actual bank shares.

Figure A.4: The near failure of Dexia as a shock to other banks' lending to public sector entities



(a) Market share of Dexia in the public entity debt market



(b) Lending to public sector entities by banks with high and low $DexiaExposure$

Notes:

B Additional tables

Table B.1: Robustness checks

Alternative wrt.	Outcome var.		*BankExposure*		Cluster		Sample	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Bank Exposure								
Firm×Time FE	✓	✓	✓	✓	✓	✓	✓	✓
Bank×Time FE	–	–	–	–	✓	✓	✓	✓
Observations								
R-squared								

This table presents robustness checks of my main results.

Table B.2: Firm-level effect on quarterly credit growth

	Quarterly credit growth					
	(1)	(2)	(3)	(4)	(5)	(6)
Firm Exposure						
$\hat{d}_{f,t}$	–	✓	✓	✓	✓	✓
Bank controls	–	–	✓	✓	✓	✓
Firm controls	–	–	–	–	✓	✓
Ind.×County×Time FE	–	–	–	✓	✓	✓
Lead bank×Time FE	–	–	–	✓	✓	✓
Firm FE	–	–	–	–	–	✓
Observations						
R-squared						

This table examines the crowding out effect of public sector entity debt shocks at the firm level. The outcome variable is the quarterly firm-level growth rate of outstanding credit. The main independent variable is $FirmExposure$. $\hat{d}_{f,t}$ is the vector of firm-level dummies estimated in the credit Regression (1), see Table 2. ***, ** and * indicate significance at the 1%, 5% and 10% levels, respectively.

Table B.3: Sensitivity of bank lending to the macroeconomic cycle

Panel A: Correlation between government debt growth and GDP growth

	National level		Regional level
	Total debt	Bank debt	Bank debt
Nat. GDP growth	-1.564***	0.0960	
	(0.469)	(0.582)	
Reg. GDP growth			-0.218*
			(0.111)
Observations	12	11	198
R^2	0.527	0.003	0.019

Panel B: GDP beta of corporate credit for banks sorted by exposure to government debt shocks

	Corporate credit growth					
	(1)	(2)	(3)	(4)	(5)	(6)
Nat. GDP growth	1.675	2.455***	2.374***			
	(3.148)	(0.412)	(0.399)			
Nat. GDP growth × BankExp<0	0.780					
	(3.121)					
Nat. GDP growth × BankExp>0	-0.168					
	(3.281)					
Nat. GDP growth × BankExp≠0		-1.817*				
		(1.046)				
Nat. GDP growth × HighSharePSE			-2.056			
			(1.267)			
Reg. GDP growth				-1.582	1.303***	1.297***
				(2.649)	(0.345)	(0.335)
Reg. GDP growth × BankExp<0				2.884		
				(2.626)		
Reg. GDP growth × BankExp>0				-2.402		
				(2.770)		
Reg. GDP growth × BankExp≠0					-0.593	
					(0.905)	
Reg. GDP growth × HighSharePSE						-0.898
						(1.104)
Observations	44460	44460	44460	36745	36745	36745
R^2	0.001	0.001	0.001	0.001	0.001	0.000

Table B.4: At which level the constraint operates? Bank-region vs. bank-branches analysis

	Growth rate of credit					
	(1)	(2)	(3)	(4)	(5)	(6)
Bank-branch Exposure	-0.0139	-0.0293**	-0.00843	-0.0274**	-0.00902	-0.0276**
	(0.0183)	(0.0147)	(0.0124)	(0.0133)	(0.0124)	(0.0133)
Bank-region Exposure			-0.0277***	-0.0216***	-0.0158*	-0.0170***
			(0.00865)	(0.00609)	(0.00855)	(0.00589)
Bank-branch controls	✓	✓	✓	✓	✓	✓
Bank-region controls	–	–	–	–	✓	✓
Firm×Time FE	✓	✓	✓	✓	✓	✓
Bank×Time FE	–	✓	–	✓	–	✓
Observations	7404737	8284567	7345077	8212676	7344445	8211690
R^2	0.424	0.446	0.424	0.446	0.424	0.446

This table examines whether the crowding out effect of government debt shocks is stronger when banks' exposure is defined at the bank-region level or at the bank-branch level. The outcome variable is the increase in firm f's borrowing from bank b, normalized by firm f's total borrowing in the previous period. The main independent variable is either the bank-region exposure variable $BankExposure_{b,r,t}$ of the bank-branch exposure variable. I construct the bank-branch exposure variable $BankExposure_{b,o,t}$ – where o denote branches within banks – in the same way as the bank-region exposure variable $BankExposure_{b,r,t}$ defined in (2), except that I take the sum on $c \in o$ instead of $c \in r$. All regressions are estimated on the sample of firms with multiple credit relationships in regions with positive public sector entity debt growth. Standard errors are clustered at the region×bank level. ***, ** and * indicate significance at the 1%, 5% and 10% levels, respectively.

Table B.5: Using the near-failure of Dexia as a natural experiment

	Growth rate of credit					
	Full sample			$\Delta GovDebt_{r,t} > 0$ subsample		
	(1)	(2)	(3)	(4)	(5)	(6)
Post× DexiaExposure	0.000199	-0.00499***	-0.00327***	0.000106	-0.00543***	-0.00376***
	(0.000551)	(0.00112)	(0.00115)	(0.000635)	(0.00129)	(0.00131)
DexiaExposure	-0.00277***	0.00107	0.000925	-0.00246**	0.00145	0.00118
	(0.00101)	(0.000885)	(0.00119)	(0.00104)	(0.000998)	(0.00131)
Bank controls	–	–	✓	–	–	✓
Firm×Time FE	–	✓	✓	–	✓	✓
Observations	7226566	6993872	6040618	5785633	5600755	4800919
R^2	0.000	0.479	0.489	0.000	0.477	0.487

C Bank shocks vs. substitution effects in the Khwaja-Mian framework

This Appendix details the methods outlined in Section 4.3 to disentangle the direct effect of credit supply shocks from substitution across banks in the KM framework. To simplify the exposition, I omit the time subscript and I abbreviate the $BankExposure_{b,r}$ variable as B_b. All proofs are stacked at the end.

C.1 The KM estimator

The economy experiences two shocks: a firm-level demand shock d_f that proxies for firm-level (unobserved) fundamentals and a bank-specific credit supply shock B_b. Each firm borrows from a set of banks $\mathcal{B}_f$ counting n_f banks. The outcomes of interest are $\Delta D_{b,f} = \frac{D_{b,f}-D_{b,f,-1}}{D_{b,f,-1}}$ and $\Delta D_f = \frac{D_f - D_{f,-1}}{D_{f,-1}}$.[20] Besides, let $B_f = \sum_{b=1}^{n_f} \omega_{bf} B_b$ where ω_{bf} are the bank shares $\omega_{bf} = \frac{D_{bf,-1}}{D_{f,-1}}$. The basic credit channel equation can be written as:

$$\Delta D_{fb} = \beta B_b + d_f + \varepsilon_{fb} \tag{9}$$

The key issue is that firm- and bank-shocks may be correlated. Let $\rho_{bd} = cov(B_b, d_f)$.[21] Besides, let $Var(B_b) = \sigma_b^2$.

To obtain closed form expressions, I repeatedly use the assumption that each firm borrows the same amount from a constant number of banks: $n_f = n \ \forall f$ and $\omega_{fb} = 1/n_f \ \forall b, f$ (Assumption A1).

The baseline model (9) assumes that firms facing heterogeneous credit supply shocks from their banks do not substitute across lenders. In this case, as shown by KM, including firm FE allows to abstract from the correlation between B_b and d_f: while the OLS

[20] My main results are unaffected if $\Delta D_{b,f}$ is defined as the mid-point growth rate or the change in $D_{b,f}$ normalized by $D_{f,-1}$. I provide the alternative formulas in the proof section.

[21] A more rigorous notation for the bank shock variable would be $B_{bf} = B_b \mathbb{1}_{[b \in \mathcal{B}_f]}$ where $\mathcal{B}_f$ is the set of banks of firm f since this variable is defined in our bank×firm data only when bank b lends to firm f. Likewise, a more rigorous notation for ρ_{bd} would be $\rho_{bd} = cov(B_{bf}, d_f) = cov(B_b, d_f | b \in \mathcal{B}_f)$. In the rest of the text, I keep the simple notation B_b.

estimator β_{OLS} is biased because of the correlation between B_b and d_f, the within-firm KM estimator β_{FE} yields an unbiased estimate of β:

$$\beta_{OLS} = \beta + \frac{\rho_{bd}}{\sigma_b^2} \tag{10}$$

$$\beta_{FE} = \beta \tag{11}$$

The standard procedure in the literature is to then study firm-level effects and compare the within-firm to the firm-level coefficient to gauge the extent of substitution across banks. Summing (9) at the firm-level using the bank shares as weights yields:

$$\Delta D_f = \beta B_f + d_f + \varepsilon_f \tag{12}$$

However, in the cross-sectional model (12), the firm-specific demand shock d_f cannot be absorbed so that the correlation between d_f and B_b again leads to a biased estimator and the comparison with the within-firm coefficient is not informative.Under assumption (A1), the expression for $\bar{\beta}_{OLS}$ is:

$$\bar{\beta}_{OLS} = \beta + \frac{\rho_{bd}}{Var(B_f)} \tag{13}$$

To circumvent this issue, Cingano et al. (2016) and Jiménez et al. (2019) have proposed to use the estimated fixed effects in (9) to correct for this bias. Including $\hat{d_f}$ in the estimation of (12), we get:

$$\bar{\beta}_{OLS,\ \hat{d}} = \beta \tag{14}$$

Papers in this literature usually compare $\bar{\beta}_{OLS,\ \hat{d}}$ to β_{FE} to assess the existence of substitution across banks: $\bar{\beta}_{OLS,\ \hat{d}} = \beta_{FE}$ would suggest there is no substitution. However, the rest of this paper shows that this reasoning is incorrect.

C.2 Introducing substitution in the KM framework

If there are spillovers across banks, equation (9) is misspecified and the true model is:

$$\Delta D_{fb} = \beta B_b + \gamma B_{-b} + d_f + \varepsilon_{fb} \tag{15}$$

where B_{-b} captures the shocks of the other banks f borrows from. In the constant n equal bank-shares case (A1), an intuitive functional form for B_{-b} is

$$B_{-b} = \frac{1}{n-1} \sum_{b' \neq b} B_{b'} \tag{16}$$

One cannot run a within-firm estimation of equation (15) because B_{-b} and B_b are collinear conditional on the firm FE. If we estimate equation (15) omitting the term B_{-b}, we obtain:

$$\beta_{OLS} = \beta + \gamma \frac{\rho_{bb'}}{\sigma_b^2} + \frac{\rho_{bd}}{\sigma_b^2} \tag{17}$$

$$\beta_{FE} = \beta - \frac{1}{n-1}\gamma \tag{18}$$

where $\rho_{bb'} = cov(B_b, B_{-b}) = cov(B_b, B_{b'})\ \forall b' \neq b$. In the case where β and γ have opposite signs, the estimated coefficient in the standard KM regression (18) overestimates the true effect (the next section generalizes this result). The KM estimator is akin to a within-firm diff-in-diff and substitution implies that the control group is affected by the shock in a direction opposite to that of the treated group, so that taking the difference overestimates the true effect. The size of the bias is decreasing in n, the number of banks per firm. Substitution effects mean that a firm can partially offset a negative shock from bank b by increasing its demand to its $n-1$ other banks. If there are many such banks (n large), then each one of the other banks will receive only a small share of this increased demand.

Estimating instead (15) omitting the FE but including the B_{-b} term does not help recover the true β as we obtain:

$$\beta_s = \beta + \frac{1}{\sigma_b^2 + (n-1)\rho_{bb'}} \rho_{bd} \tag{19}$$

Note that the bias in β_s depends on ρ_{bd} with a coefficient that is different from that of the most naive OLS coefficient (17). Namely, the coefficient is smaller and inversely related to n. The reason is that $\rho_{bb'}$ which is the correlation between B_b and B_{-b} – i.e. the correlation between bank shocks hitting the same firm – already partly reflects the correlation between B_b and d_f.[22]

The between-firm coefficient is also biased. Summing equation (15) at the firm level, we obtain:

$$\Delta D_f = (\beta + \gamma) B_f + d_f + \varepsilon_f \tag{20}$$

Estimating this equation omitting d_f, we get:

$$\bar{\beta}_{OLS} = (\beta + \gamma) + \frac{\rho_{bd}}{Var(B_f)} \tag{21}$$

Besides, including the estimated $\hat{d}_f$ does not solve the issue:

$$\bar{\beta}_{OLS,\ \hat{d}} = \beta - \frac{1}{n-1}\gamma \tag{22}$$

The intuition is that since β_{FE} in (18) is biased, the estimated $\hat{d}_f$ are biased as well so that including them in the between-firm estimation leads to a biased coefficient as well. Moreover, equation (22) shows that comparing the FE and the between-firm coefficients tells us nothing: even with substitution effects, the between-firm coefficient is equal to the FE one. The reason why we may empirically find $\hat{\bar{\beta}}_{OLS,\ \hat{d}} \neq \hat{\beta}_{FE}$ is because assumption (A1) does not hold in general, not because the difference captures substitution effects (see next section). Hence, with substitution effects neither the standard KM estimator

[22] Take a simple model where each bank lends to one firm only. Then a simple way to generate a correlation between supply and demand shocks equal to ρ_{bd} is to take $B_b = \frac{\rho_{bd}}{Var(d_f)} d_f + \varepsilon_b$. This leads to $\rho_{bb'} = \rho_{bd}^2 / Var(d_f)$ which shows how $\rho_{bb'}$ conveys information on ρ_{bd}.

nor the procedure of Cingano et al. (2016) and Jiménez et al. (2019) allows to recover the true β. Finally, under assumption (A1) every subset of 3 equations among (17), (18), (19), (21) and (22) is collinear so that we cannot combine these equations to recover β, γ and ρ_{bd}.

C.3 Recovering the true β in the presence of substitution

Let us allow for variation in n_f across firms as well as for variation in ω_{bf} within firms. Besides, let us take a very general functional form for the substitution term B_{-b}:

$$B_{-b} = \sum_{b' \neq b} \frac{\omega_{b'f}^{\phi}}{(\sum_{j \neq b} \omega_{jf}^{\phi})} B_{b'} \tag{23}$$

where ϕ is a parameter. Taking a generic functional form allows to make assumptions on the extent to which each banks' shock affects the firm, depending on the bank shares ω_{bf}. It nests all the intuitive forms for B_{-b}: the equal-weighted mean of other banks' shocks, their bank-share weighted mean, the shock of the bank with the highest bank share.[23] The fact that the KM estimator is biased in the presence of substitution effects is very general, as shown in the following proposition.

Proposition 1 *If $\gamma \neq 0$, the within-firm estimator β_{FE} is biased. If γ and β have opposite (equal) signs, β_{FE} over-estimates (under-estimates) the true effect.*

I show that there are two ways to identify separately β and γ: (i) using the variation in n_f across firms ; (ii) using the variation in ω_{bf} within firms.

C.3.1 Using variation in n_f across firms

To grasp the intuition, assume that the substitution term B_{-b} depends on an equal-weighted mean of the $B_{b'}$ so that as in section C.2, equation (15) including both B_{-b} and

[23] B_{-b} is $\frac{1}{n_f-1}\sum_{b' \neq b} B_{b'}$ for ϕ=0; $\frac{1}{1-\omega_{bf}}\sum_{b' \neq b} \omega_{b'f} B_{b'}$ for ϕ=1; $\sum_{b' \neq b} \mathbb{1}_{[b' = \text{argmax}_{i \neq b}\{\omega_{if}\}]} B_{b'}$ for ϕ=+∞.

the firm FE is not identified. This is true: (i) for B_{-b} defined with any ϕ if there is no within-firm dispersion in ω_{bf}; or (ii) if B_{-b} is defined with $\phi = 0$.

Proposition 2 *Assume that n_f varies across firms and that $B_b d_f \perp n_f$. Then the equivalents of moments* (17), (18) *and* (19) *yield independent equations that allow to recover the parameters of interest. A simplifying sufficient condition is if additionally $B_b \perp n_f$ and $d_f \perp n_f$.*

The intuition for why the system is invertible in this case while it was not with constant n relies on the interpretation of β_{FE}. The size of the bias in β_{FE} is related to the substitution effect depends on n. Therefore cross-sectional variation in n introduces cross-sectional variation in the size of the bias relative to the size of the true effect, allowing us to disentangle the effects of γ and β. γ and β are identified by comparing β_{FE} and β_{OLS}, while the remaining equation serves to pin down ρ_{bd} which also enters the expression of β_{OLS}.

I test the implementation of this method on simulated data. I simulate 100 datasets with a number of firms and a distribution of the number of banks per firm n_f similar to that of my true data (see Figure 5) and with $\Delta D_{f,b}$ defined as either the standard growth rate or the change in $D_{f,b}$ normalized by lagged D_f. For each of these simulated datasets, I implement the method outlined above. Table C.1 reports the average estimated coefficient as well as its standard deviation across the 100 simulations. I perform the exercise both under the sufficient condition $B_b \perp n_f$ and $d_f \perp n_f$ (columns labelled SC) and under the necessary condition $B_b d_f \perp n_f$ (columns labelled NC). The upper panel shows that the naive estimates can be very far off the true parameters. In the lower panel, I solve for the true β, γ and ρ_{bd}. In the sufficient condition case, the three parameters are recovered with very good precision. In the necessary condition case, I find that the standard deviation of the estimates is very large (around 40 times higher than in the sufficient condition case). Since in practice I will perform this exercise only once on the true dataset, using this methodology in the NC case may lead to very large errors. I thus consider that this methodology is practically suitable only in the SC case.

Table C.1: Recovering β using variation in n_f

	Estimation results for $(\beta, \gamma, \rho_{bd}) = (-0.5, 0.3, 0.28)$							
	Panel A: $\Delta D_{b,f} = \frac{D_{b,f} - D_{b,f,-1}}{D_{f,-1}}$				Panel B: $\Delta D_{b,f} = \frac{D_{b,f} - D_{b,f,-1}}{D_{b,f,-1}}$			
	SC		NC		SC		NC	
	Mean	S.D.	Mean	S.D.	Mean	S.D.	Mean	S.D.
Naive estimators								
$\hat{\beta}_{OLS}$	-0.197	0.002	7.663	0.044	-0.197	0.002	7.663	0.044
$\hat{\beta}_{FE}$	-0.650	0.001	-0.679	0.047	-0.650	0.001	-0.679	0.047
$\hat{\beta}_s$	-0.254	0.001	3.885	0.036	-0.254	0.001	3.885	0.036
$\hat{\hat{\beta}}_{OLS}$	1.399	0.008	78.332	0.389	0.444	0.003	8.507	0.043
$\hat{\hat{\beta}}_{OLS,\ \hat{d}}$	-1.721	0.005	45.030	0.305	-0.650	0.001	-0.679	0.047
Solving the system								
$\hat{\beta}$	-0.500	0.003	-0.509	0.113	-0.500	0.004	-0.509	0.113
$\hat{\gamma}$	0.300	0.006	0.280	0.160	0.300	0.007	0.280	0.160
$\hat{\rho_{bd}}$	0.279	0.004	0.280	0.007	0.280	0.005	0.280	0.007

This table shows summary statistics of the results of 100 estimations of the parameters of interests on simulated data. In columns (SC), I simulate the data under the sufficient condition $B_b \perp n_f$ & $d_f \perp n_f$. In columns NC, I simulate the data under the necessary condition $B_b d_f \perp n_f$. I simulate 650,000 firms. The number of bank per firms to follow a geometric law with success probability 0.5. ω_{bf} is constant and equal to $1/n_f$. In the SC columncs, B_b and d_f are jointly normally distributed with mean 0, variance 1 and covariance $\rho_{bd} = 0.28$. In the NC columns, I generate $d_f = n_f + \varepsilon_d$ and $B_b = \varepsilon_b / d_f$ with $(\varepsilon_d, \varepsilon_b)$ normally distributed to obtain the desired correlation structure. I then generate ΔD_{bf} as in (15) with $\beta = -0.5$ and $\gamma = 0.3$. In Panel A, $\Delta D_{b,f}$ is taken to be the increase in $D_{b,f}$ normalized by the firm total borrowing in the previous period, in which case ΔD_f is the sum of $\Delta D_{b,f}$. In Panel B, $\Delta D_{b,f}$ is taken to be the mid-point growth rate in $D_{b,f}$, in which case ΔD_f is the weighted mean of $\Delta D_{b,f}$.

C.3.2 Using the variation in ω_{bf} within firms

A second avenue to identify β and γ separately is to use the within-firm variation in ω_{bf} along with a specific functional form for B_{-b}. To use this method we need to have within firm-variation in ω_{bf}. Otherwise, any functional form for B_{-b} simplifies to the $\phi = 0$ equal-weighted mean expression and we are back to the preceding case. Fortunately, as shown in Figure 5, the true data displays significant within-firm variation in ω_{bf}.

Proposition 3 *For $n_f > 2$, ω_{bf} not constant within firms and $\phi \neq 0$, equation* (15) *is identified and $\beta_{FE} = \beta$.*

The advantage of this identification strategy is that we don't need to make any assumption on n_f: it works for n_f constant as well as for any correlation pattern between n_f, B_f and d_f. It also works for any correlation pattern between ω_{bf}, B_f and d_f. The downside is that it relies on the choice of a specific functional form for B_{-b}, leading to potential errors due to misspecification.

Table C.2 shows estimation results on simulated data. Lines 4-6 show that β and γ are recovered with very good precision with a dispersion in ω_{bf} similar to that of the true data (the mean of $|\omega_{b,f,t} - \frac{1}{n_{f,t}}|$ is equal to 0.14 compared to xx in the true data) and for three different definitions of B_{-b}: (1) $\phi = 1$, (2) $\phi = 2$, and (3) $\phi = +\infty$. On the other hand, lines 1-3 show that omitting the B_{-b} (as in the standard KM estimator) term leads to large errors. Lines 7-9 show that the estimation of the parameters is quite sensitive to misspecification in B_{-b}.[24] but that averaging coefficients across definitions of B_{-b} gives much more reasonable estimates than when omitting B_{-b}.

[24]I.e. if the model is estimated using B_{-b} defined in a way while the true data-generating process depends on B_{-b} defined in another way.

Table C.2: Recovering β using variation in ω_{bf}

		Estimation results for $(\beta, \gamma, \rho_{bd}) = (-0.5, 0.3, 0.28)$							
		Panel A: constant n_f				Panel B: n_f correlated to B_b and d_f			
		β		γ		β		γ	
	Model	Mean	StdDev	Mean	StdDev	Mean	StdDev	Mean	StdDev
1	KM w/o B_{-b} (1)	-0.650	0.001			-0.710	0.001		
2	KM w/o B_{-b} (2)	-0.584	0.002			-0.962	1.820		
3	KM w/o B_{-b} (3)	-0.650	0.001			-0.710	0.001		
4	KM w/ B_{-b} (1)	-0.500	0.002	0.300	0.004	-0.500	0.003	0.300	0.004
5	KM w/ B_{-b} (2)	-0.500	0.001	0.300	0.000	-0.500	0.001	0.300	0.000
6	KM w/ B_{-b} (3)	-0.500	0.001	0.300	0.002	-0.500	0.002	0.300	0.002
7	Missspecified B_{-b} (1)	-0.582	0.001	0.136	0.002	-0.604	0.002	0.152	0.002
8	Missspecified B_{-b} (2)	-0.462	0.002	0.288	0.003	-0.917	3.013	-0.056	2.728
9	Missspecified B_{-b} (3)	-0.513	0.001	0.274	0.002	-0.555	0.002	0.221	0.002

This table shows summary statistics of the results of 100 estimations of the parameters of interests on simulated data. I simulate 650,000 firms. In panel A, $n_f = 3$ and B_b and d_f are drawn independently of n_f as jointly normally distributed with mean 0, variance 1 and covariance $\rho_{bd} = 0.28$. In panel B, the number of bank per firms follows a geometric law with success probability 0.5. B_b and d_f are defined as $an_f + \varepsilon$ such that $\rho_{bd} = 0.28$. I generate ΔD_{bf} as in (15) with $\beta = -0.5$ and $\gamma = 1.5$ Lines 1-3 shows the estimate of $\hat{\beta}_{FE}$ when omitting the B_{-b} term while ΔD_{bf} is generated with B_{-b} as in (1) definition (23) with $\phi = 1$; (2) definition of footnote 23 with $\phi = 1$; (3) definition (23) with $\phi = +\infty$. Lines 4-6 show the estimates of $\hat{\beta}_{FE}$ and $\hat{\gamma}_{FE}$ when including B_{-b}, defined in the same way. Lines 7-9 show the estimates of $\hat{\beta}_{FE}$ and $\hat{\gamma}_{FE}$ when the model is misspecified. In line 5, I take ΔD_{bf} generated as if B_{-b} were as in (1), and I estimate model (15) with B_{-b} alternatively defined as in (2) or (3). The average of the three coefficients yields an idea of the extent of the potential error due to misspecification. Line 6 and 7 follow the same method with ΔD_{bf} generated as if B_{-b} were as in (2) and (3).

C.4 Estimating the effect of credit supply shocks on firm-level outcomes

We may want to investigate the effect of bank-level credit supply shocks on outcomes that are observed only at the firm level. For instance, we may want to study the elasticity of investment to credit supply shocks. Let us denote these outcomes Y_f. In this case, we cannot resort to a within-firm estimator, since there is no within-firm variation in Y_f. Besides, a simple OLS regression of ΔY_f on B_f would lead to a biased coefficient: since credit supply shocks are taken to be correlated to firm-specific credit demand shocks, it is also quite natural to assume that they are correlated to firm-specific shocks to investment. However, we can use the logic of Cingano et al. (2016) and Jiménez et al. (2019) along with the method outlined above to recover the true β to obtain an unbiased estimate of the effect of B_f on ΔY_f.

Assume that the true model is:

$$\Delta Y_f = \eta \Delta D_f + \Phi \cdot \mathbf{X}_f + \delta_f + \nu_f \tag{24}$$

This says that the outcome ΔY_f is determined by the change in credit ΔD_f, a set of observable variables $\mathbf{X}_f$, an unobserved firm-specific shock δ_f and a white noise ν_f. Using equation (20), this equation rewrites as:

$$\Delta Y_f = \eta(\beta + \gamma) B_f + \Phi \cdot \mathbf{X}_f + \delta_f + \eta d_f + \nu_f + \eta \varepsilon_f \tag{25}$$

The preceding section showed how to recover unbiased estimates of β and γ. These estimates allow us to recover unbiased estimates of d_f, the unobservable firm-specific shock in the within-firm model (15).[25] Let us call these estimates $\tilde{d}_f$.

Moreover, assume that the unobservable determinants of outcome Y_f are the same as the unobservable determinants of credit outcomes, that is $d_f = \delta_f$. Then, using the same logic as Cingano et al. (2016), Jiménez et al. (2019) (as summarized in section C.1 of this document), equation (25) can be estimated including the estimated $\tilde{d}_f$, which yields an unbiased estimate of the coefficient of interest.

[25] In the case where β and γ where recovered using variation in n_f (and constant weights $\omega_{b,f}$), we obtain $d_f = \hat{d}_f - \frac{\gamma B_f}{\mathbb{E}\left[\frac{n_f - 1}{n_f}\right]}$ where $\hat{d}_f$ are the fixed-effects computed when estimating β_{FE}. In the case where β and γ where recovered using variation in $\omega_{b,f}$, the fixed-effects recovered when estimating equation (15) are unbiased estimates of d_f.

Proofs

Proof of Proposition 1. The KM estimator is equal to:

$$\beta_{FE} = \beta + \gamma \frac{cov(B_{-b}, B_b - \overline{B_b})}{Var(B_b - \overline{B_b})}$$

where the upper bar denotes within-firm averages. Define the random variables $\lambda_{bf} = \omega_{bf}^{\phi} / \sum_{j \neq b} \omega_{jf}^{\phi}$ and $\Lambda = \{\lambda_{bf}\}_{b,f}$. We can write:

$$cov(B_{-b}, B_b - \overline{B_b}) = -\mathbb{E}\left[\frac{1-\lambda_{bf}}{n_f}\left(\mathbb{E}\left[B_b^2 | n_f, \Lambda\right] - \mathbb{E}\left[B_b B_{b'} | n_f, \Lambda\right]\right)\right]$$

By the Cauchy-Schwarz inequality, $\mathbb{E}[B_b^2 | n_f, \Lambda] - \mathbb{E}[B_b B_{b'} | n_f, \Lambda] \geq 0$ for all (n_f, Λ). Besides, $\frac{1-\lambda_{bf}}{n_f} \geq 0$. Hence, $cov(B_{-b}, B_b - \overline{B_b}) \leq 0$. Hence when β and γ have opposite (equal) signs, we obtain $|\beta_{FE}| \geq |\beta|$ ($|\beta_{FE}| \leq |\beta|$). ■

Proof of Proposition 2. Under the necessary condition $n_f \perp B_b d_f$, we have 3 equations to identify the 3 parameters of interest:

$$\hat{\beta}_{OLS} = \beta + \gamma \frac{\rho_{bb'}}{\sigma_b^2} + \frac{\rho_{bd}}{\sigma_b^2} \tag{26}$$

$$\hat{\beta}_{FE} = \beta + \gamma \frac{cov(B_{-b}, B_b - B_f)}{Var(B_b - B_f)} \tag{27}$$

$$\hat{\beta}_s = \beta + \frac{1-\kappa}{Var(\tilde{B}_b)} \rho_{bd} \tag{28}$$

where κ and $\tilde{B}_b$ are respectively defined as the coefficient and the residual of the regression of B_b on B_{-b}.

If additionally we assume that $B_b \perp n_f$ and $d_f \perp n_f$, we obtain four equations:

$$\beta_{OLS} = \beta + \gamma\frac{\rho_{bb'}}{\sigma_b^2} + \frac{\rho_{bd}}{\sigma_b^2} \tag{29}$$

$$\beta_{FE} = \beta - \frac{\mathbb{E}\left[\frac{1}{n_f}\right]}{\mathbb{E}\left[\frac{n_f-1}{n_f}\right]}\gamma \tag{30}$$

$$\beta_s = \beta + \frac{\mathbb{E}[\frac{1}{n_f-1}]}{\mathbb{E}[\frac{1}{n_f-1}]\sigma_b^2 + \rho_{bb'}}\rho_{bd} \tag{31}$$

$$\bar{\beta}_{OLS} = (\beta+\gamma) + \frac{\rho_{bd}}{\mathbb{E}\left[\frac{1}{n_f}\right]\sigma_b^2 + \mathbb{E}\left[\frac{n_f-1}{n_f}\right]\rho_{bb'}} \tag{32}$$

$\bar{\beta}_{OLS,\ \hat{d}}$ is still proportional to β_{FE} so that again included the estimated $\hat{d_f}$ does not add any information. ■

Proof of Proposition 3. I detail the proof of identification in the case $B_{-b} = \frac{1}{1-\omega_{bf}}\sum_{j\neq b}\omega_{jf}B_j$, that is $\phi = 1$. Then the within firm estimation of (15) corresponds to

$$\begin{aligned}\Delta D_{bf} - \overline{\Delta D_{bf}} &= \beta(B_b - \overline{B_b}) + \gamma(B_{-b} - \overline{B_{-b}}) + \varepsilon_{bf} - \overline{\varepsilon_{bf}} \\ &= \beta(B_b - \frac{1}{n_f}\sum_j B_j) + \gamma\left(\sum_{b'\neq b}\frac{\omega_{b'f}}{1-\omega_{bf}}B_{b'} - \frac{1}{n_f}\sum_j\sum_{b'\neq j}\frac{\omega_{b'f}}{1-\omega_{jf}}B_{b'}\right) + \varepsilon_{bf} - \overline{\varepsilon_{bf}}\end{aligned}$$

$B_{-b} - \overline{B_{-b}}$ collinear to $B_b - \overline{B_b}$ implies that all the ω_{bf} are equal to $1/n_f$. By contrapositive, as long as not all the ω_{bf} are equal to $1/n_f$, we obtain that $B_{-b} - \overline{B_{-b}}$ is not collinear to $B_b - \overline{B_b}$ so that β and γ can be separately identified. By the regression anatomy formula, we obtain $\beta_{FE} = \beta$. ■