

The Crowding Out Effect of Local Government Debt: Micro- and Macro-Estimates

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Abstract

Local government expenditures are increasingly financed by debt, mostly consisting of bank loans. I study the crowding out effect of these loans on corporate credit, investment, employment, and output, using French administrative data over 2006-2018. Exploiting plausibly exogenous variation in local government credit growth across banks, I show that when a local government borrows an additional €1 from a bank, this bank reduces corporate credit by €0.5, with significant effects on firm-level investment. Combining these reduced-form effects and a model, I show that crowding out reduces the output multiplier of debt-financed local government spending by 0.3. This is large compared to government spending multiplier estimates. Crowding out is driven by banks' limited ability to expand their credit supply. These results show that constraints on financing supply reduce the stimulus effect of debt-financed government spending.

Keywords: Government debt, Crowding out, Banks, Credit supply.

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1 Introduction

Local governments are key providers of public goods and services.¹ Their expenditures are increasingly debt-financed, driving a swell in local government debt. Over 1990-2019, local government debt-to-GDP increased from 11% to 22% in large developed and developing countries (Fig. 1). This debt may adversely affect the private sector via a crowding out effect. As per the standard theory, if the aggregate supply of loanable funds is imperfectly elastic, an increase in local governments' demand for debt will reduce the supply of debt to firms, hindering corporate investment and output.

Local—as opposed to central—government debt mostly consists of bank loans: in large developed and developing countries, bank loans account for 80% of local government debt (Fig. 1).² The output loss due to crowding out is plausibly high in the case of bank loans for two reasons. First, bank credit supply being notably constrained,³ an increase in local government lending should reduce *aggregate* corporate credit. Second, segmentation across banks—i.e., frictions preventing capital from flowing across banks and borrowers from switching banks—gives rise to an additional effect. Local government lending by a given bank may disproportionately crowd out credit to firms borrowing from that same bank. This effect on the *distribution* of credit across heterogeneous firms may lower the efficiency of input allocation, and through that channel, aggregate output.

Such crowding out would undermine the stimulus effect of debt-financed local government spending. More precisely, the debt-financed spending multiplier can be decomposed into a pure multiplier effect of government spending and a negative crowding out effect. The extent and the determinants of crowding out are thus essential inputs for the level and financing of public spending.

This paper studies the crowding out effect of local government bank debt on corporate credit and quantifies the implied reduction in local government spending multipliers. I focus on France over 2006-2018, exploiting rich credit registry data covering all bank loans to private firms and local governments, combined with corporate tax-filings.

The main challenge—and the reason why empirical evidence on crowding out is scant—is identification. First, local government debt is a policy tool and reacts endogenously to economic conditions. Second, even exogenous shocks to local government debt may affect firms via other channels than crowding out, notably via any stimulus effect of local government spending.

I tackle this challenge in two steps. First, I consider causal *relative* crowding out effects implied by bank segmentation: I ask whether an increase in local government

1. They represent 40% of public expenditures in large developed and developing countries (Fig. A.1).

2. The US is an outlier: loans represent only 5% of local government debt. This segment experienced a fivefold increase over 2000-2016 (Ivanov and Zimmermann, 2018).

3. See, e.g., Paravisini (2008) and others in the literature review.

lending by a given bank causes a disproportionate reduction in that bank’s corporate credit supply, and in investment and employment for its borrowers. Considering relative effects allows me to isolate the crowding out channel. Second, combining the estimated relative effects and a model, I quantify the drop in *aggregate* output due to crowding out. The quantification takes into account the effect on aggregate investment and employment, and that on the efficiency of input allocation across firms.

In outline, I find that crowding out reduces the output multiplier of debt-financed local government spending by 0.3. This is large since typical debt-financed multiplier estimates range from 0.5 to 1.9 (e.g., Ramey, 2019). The main determinant is banks’ limited ability to expand their credit supply. Overall, my paper provides the first evidence that local government debt crowds out aggregate output, an important finding given the surge in debt-financed local government spending. This is also the first causal evidence of aggregate crowding out by government debt in general, identification having proven elusive in the case of central government debt. By identifying a causal effect and studying its determinants, I therefore also test a theory relevant for any government debt.

I exploit bank lending to French local governments as an empirical setting.⁴ Bank loans represent 90% of local government debt. I observe all outstanding loans by 506 banks to 63,545 local governments and 1.6 million firms, which I can locate across France’s 2,081 municipalities distributed over the country’s 22 regions. Firms and local governments tend to borrow locally, typically from a bank branch in the same municipality.

The crowding out effect corresponds to the effect of a demand-driven increase in local government loans on corporate credit. I first identify a relative crowding out effect in the cross-section of banks, that is, I ask whether a larger increase in local government lending by a given bank causes a larger reduction in that bank’s corporate credit. My research design examines whether a given firm experiences lower credit growth from banks that increase their lending to local governments by a larger amount.⁵ I instrument the actual increase in local government loans by a demand shifter, exploiting the fact that banks’ pre-determined geographic implantation across municipalities generates heterogeneous exposure to local government debt growth. Identification relies on the fact that other endogenous relationships between local government debt and corporate credit (e.g., multiplier effects) affect *firm*-level demand for credit. The within-firm estimator (Khwaja and Mian, 2008) thus partials out these channels. By contrast, crowding out uniquely operates as a shock to the *bank*-specific supply of corporate credit, which depends on the bank-level demand-driven increase in local government loans. In the baseline analysis, I define the increase in local government loans at the bank×region level to study distributive effects

4. By local governments, I refer to the four layers of sub-national governments, the local public entities they control (public schools, public housing, etc.), and state-owned local public service operators.

5. 30% of firms borrow from multiple banks, they account for 70% of total corporate credit.

across firms borrowing from different banks and located in different regions. I conduct the analysis at the quarterly frequency in a stacked first differences setting.

This design yields the relative crowding out parameter under two identifying assumptions. First, the firm-level shocks that may be correlated with local government debt must be evenly spread across the firm's banks. Second, the bank-specific local government debt demand shocks I construct must be orthogonal to other bank-level determinants of credit supply. I run various tests and find support for these assumptions.

I find that when local governments borrow an additional €1 from a given bank, that bank lends €0.54 less to private firms located in the same region during the same quarter. Importantly, the crowding out effect is similar when excluding state-owned banks and does not vary with proxies for political pressure on banks. Hence, crowding out is orthogonal to political interference.⁶

My results are confirmed by a quasi-natural experiment: the bankruptcy of Dexia, the main lender to French local governments until it went into trouble during the 2008 crisis. Dexia's failure generated an exogenous increase in local government borrowing at other banks. In addition to providing a robustness check of my results, this design is better suited to assess long-run effects: I find a crowding out effect of €0.25 over five years.

Next, I turn to the question of why crowding out occurs—i.e., why banks do not increase total lending to accommodate local government debt demand while maintaining corporate credit supply. I find that crowding out is driven by the limited supply of deposits and by banks' capital and liquidity constraints, which limit banks' ability to expand their credit supply. While these constraints limit credit supply at the banking system level, I also find bank segmentation to matter: crowding out is stronger for banks with less access to the interbank market. I then provide evidence of relative crowding out *within* banks: for a given bank, higher local government debt demand in a region leads to lower corporate credit in this region relative to the other regions where the bank operates; and higher local government demand at a given branch reduces this branch's corporate credit relative to other branches of the same bank in the same region. This highlights the relevance of within-bank frictions, such as inefficient internal capital markets. Taken together, these results show that crowding out reflects the extent to which frictions prevent a bank (and each of its subdivisions) from increasing total credit supply.

The adjustment of corporate credit implied by the constrained credit supply occurs through both a reduction in quantities and an increase in interest rates, albeit to a lesser extent. In addition, banks mostly cut credit to small and unrated firms. I investigate different explanations and find my results to be most consistent with banks responding to a lending opportunity with safe local governments by downsizing the segments of their loan portfolio where information asymmetry is the highest.

6. I study the effect of the *marginal* euro of local government loans on corporate credit, not the *level* of local government loans which may reflect regulatory or political distortions.

Finally, I study whether the reduction in corporate credit by a bank has real effects on investment and employment for its corporate borrowers. I compare firms borrowing from banks exposed to local government debt shocks to firms borrowing from other banks. More precisely, I define firm-level exposure to crowding out as the credit-share weighted average of its banks' shocks. Importantly, I compare only firms located in the same municipality, and therefore subject to a similar local-level change in local government debt, but that differ in their exposure to crowding out because they borrow from different sets of banks. I restrict the comparison to firms matched to the same main bank (i.e., that with the largest credit share) to alleviate assortative firm-bank matching concerns. I also only compare firms in the same industry, and I directly control for an estimate of firm-level demand shocks obtained from the within-firm specification.⁷ The dependent variables are investment and wage bill growth, obtained from corporate tax-filings at the yearly frequency. The identifying assumption is that, conditional on controls, there are no shocks to real outcomes correlated with bank affiliation. I perform several checks and find support for this assumption.

I find that the reduction in corporate credit supply has real effects. An additional €1 in local government loans at one bank leads to a €0.23 reduction in investment and a €0.06 reduction in wages for firms borrowing from that bank in the same year. These effects are heterogeneous across firms, with more financially constrained firms exhibiting higher credit-to-investment and credit-to-employment sensitivities.

With these relative effects in hand, I turn to how crowding out affects aggregate output. I quantify the output loss relative to a counterfactual in which local government debt has no crowding out effect. One concrete example of such counterfactual is if local government debt is entirely financed by foreign investors.⁸ I consider two channels: the effect on aggregate investment and employment and that on allocative efficiency.

How does crowding out affect aggregate output through changes in aggregate investment and employment? The *relative* effects documented so far do not add up to the *aggregate* effect because they ignore any effect on non-exposed banks and firms. To obtain the aggregate effect, I develop a model of crowding out in a segmented banking system. Banks lend to firms and local governments, are funded via deposits, and can access the interbank market at a cost. Firms, local governments and depositors are assigned to a given bank. Together with the cost of accessing the interbank market, this implies that banks are (partially) segmented. I study the equilibrium response of corporate and local government lending to bank-specific local government debt demand shocks. This model allows me to define formally the relative crowding out coefficient—the

7. See Cingano, Manaresi, and Sette (2016) and Jiménez, Mian, Peydró, and Saurina (2019).

8. See Diamond (1965) or Broner, Clancy, Erce, and Martin (2021) for a recent treatment. The intuition is that if domestic firms are financed by domestic banks, and the government borrows from foreign investors, there is no domestic crowding out.

counterpart to my empirical estimates—as well as the aggregate crowding out coefficient that determines aggregate outcomes. The analysis shows that the coefficient for relative crowding out is a lower bound for aggregate crowding out. The intuition is that unless banks are fully segmented, the banks exposed to the local government debt shock will draw in capital from non-exposed banks, which therefore also reduce their corporate credit supply. I quantify this equilibrium effect by estimating the effect of local government debt demand shocks on interbank flows. I find that the drop in aggregate investment and employment attributable to crowding out generates an output loss of €0.18 per euro of local government loans.⁹

Crowding out may also affect aggregate output through an effect on allocative efficiency. Indeed, my reduced form results show that crowding out affects the distribution of investment and employment across borrowers of different banks and across more or less financially-constrained firms. I quantify the impact on allocative efficiency using the standard framework from the misallocation literature (Hsieh and Klenow, 2009). I find that crowding out reduces aggregate output by €0.12 per euro of local government debt via a decline in allocative efficiency. This is entirely driven by the fact that firms with higher marginal products of inputs—i.e., firms most constrained in their input usage—experience a similar reduction in credit but have investment and employment that are particularly sensitive to a credit cut. By contrast, the distributional inefficiencies induced by the dispersion in firm exposure to crowding out—i.e., the effect most specific to crowding out operating through banks—are negligible.

Aggregating these effects, an additional €1 of local government debt reduces output by €0.3 (0.18+0.12) through crowding out. This implies that the output multiplier of debt-financed local government spending would be higher by 0.3 absent crowding out. The output loss results from the aggregate reduction in corporate credit, which reflects banks' limited ability to increase credit supply, and from the differential effects of a credit cut on firms with heterogeneous returns to inputs.

This paper makes four main contributions. First, I identify a causal crowding out effect and quantify the reduction in spending multipliers attributable to crowding out in the case of local government bank debt. Second, I show that crowding out reflects the elasticity of banks' credit supply. Third, I uncover and quantify distributive effects of crowding out when lenders are segmented and firms are heterogeneous. Fourth, I provide a test of the standard crowding out theory and a framework to quantify the aggregate and distributive effects of crowding out, which apply to other forms of government debt. The paper also has a methodological contribution: I account for firms' substituting across

9. I consider extensions of the baseline model with firms substituting across banks, regulatory-type frictions on banks' total balance sheet size, and bank market power, and these features do not affect the aggregation result. Considering other forms of capital flows across banks than the interbank market implies that my quantification is a lower bound.

banks in the Khwaja and Mian (2008) framework and show how the effect of credit supply shocks can be identified separately.

There are two main policy implications from my results. First, crowding out is large, notably compared to estimates of spending multipliers.¹⁰ This may be especially problematic during crises, when local government debt tends to soar while banks are particularly constrained. Second, crowding out can be mitigated if local governments borrow from less constrained lenders. In this respect, my results highlight an important downside of transferring debt-taking to lower levels of government, since central government debt financed by bonds issued on international capital markets is likely to generate a lower crowding out effect on the domestic economy.

Related literature. This work contributes to four strands of the literature. First, I contribute to the large literature on government debt crowding out corporate financing and investment (see Hubbard (2012) for a review). Virtually all studies focus on government bonds and rely on time-series variation in the US.¹¹ No consensus has emerged, partly reflecting the challenge in establishing causality.¹² Closer to my focus, recent papers study the effect of loans to local governments on corporate credit and investment: Huang, Pagano, and Panizza (2020) in China, and Hoffmann, Stewen, and Stiefel (2021) in Germany. However, they focus on state-owned banks and political interference, and only consider relative effects.¹³ My work also relates to papers showing that banks' holdings of sovereign bonds—due to political pressure during the European sovereign debt crisis in Becker and Ivashina (2017) or to a home bias in holdings of Colombian sovereign debt in Williams (2018)—crowd out corporate credit and investment.

Second, this work feeds into the broader literature on the effects of (local) government debt on growth, and notably on the size of debt-financed fiscal multipliers (Clemens and Miran (2012), Adelino, Cunha, and Ferreira (2017), Dagostino (2018)). Importantly, large crowding out effects imply that the policy-relevant debt-financed multipliers will be

10. Debt-financed multipliers are difficult to estimate, but a reasonable range is 0.5-1.9 (Ramey, 2019). They would be higher absent crowding out. Transfer-financed multipliers (estimates ranging from 0.8 to 4) would be lower if the spending was financed by debt, notably because of crowding out.

11. Exceptions are Temin and Voth (2005) on British sovereign bonds in 1700-1850 and Huang, Panizza, and Varghese (2018) on the effect of sovereign debt on corporate investment in a cross-country setting.

12. Several papers have tested the refinement of the crowding out hypothesis by Friedman (1978) according to which government debt affects the relative prices of other securities depending on their substitutability with government debt. These papers show that government debt affects corporate leverage (Graham, Leary, and Roberts (2014), Demirci, Huang, and Sialm (2018), Akkoyun, Ersahin, and James (2020)), maturity (Greenwood, Hanson, and Stein, 2010), and short-term debt in the financial sector (Krishnamurthy and Vissing-Jorgensen, 2015), but have no direct implications for corporate investment.

13. Looking at crowding out outside of state-owned banks is critical. In most countries, state-owned banks account for a small share of credit. Besides, crowding out due to political pressure may have different implications for banks' health, if they are pressured to hold risky sovereign debt (Acharya, Drechsler, and Schnabl (2014), Ongena, Popov, and Van Horen (2019)) or make losses lending to governments at sub-competitive rates (Hoffmann, Stewen, and Stiefel, 2021).

lower than the transfer-financed multipliers of local government spending estimated in much of the recent literature.¹⁴ I also add to the broader literature on the effects of local government indebtedness on the real economy, e.g., Sauvagnat and Vallée (2021).

Third, this paper contributes to the empirical literature documenting that banks' funding constraints significantly limit their ability to expand their credit supply.¹⁵ In this respect, my paper is closest to Chakraborty, Goldstein, and MacKinlay (2018), Martín, Moral-Benito, and Schmitz (2021) and Greenwald, Krainer, and Paul (2021) who show how one segment of banks' loan portfolio may crowd out another one.¹⁶ By looking at the crowding out effect of local government loans, I provide a novel estimate of the elasticity of banks' loan portfolio to an increase in credit demand. In addition, I document the consequences of banks' funding constraints for the transmission of bank-financed government spending to the real economy.¹⁷ Finally, I add to the evidence on the real effects of credit supply shocks on investment and employment.¹⁸

Fourth, I contribute to the empirical literature on the effects of financing constraints on input misallocation.¹⁹ In this respect, my work is closest to Banerjee, Breza, Townsend, and Vera-Cossio (2019) and Bau and Matray (2020) who show that the heterogeneous effects of a uniform financing shock can significantly affect allocative efficiency.

Section 2 presents the data and provides institutional details. Section 3 provides a brief conceptual framework. Section 4 studies relative crowding out effects of local government loans on corporate credit across banks. Section 5 fleshes out the mechanism. Section 6 investigates the effects of crowding out on corporate investment and employment across firms. Section 7 quantifies aggregate implications. Section 8 concludes.

14. Cohen, Coval, and Malloy (2011) show that transfer-financed multipliers can themselves be reduced by *real* crowding out (independently of the mode of financing, if production factors are fully employed, government production can only occur at the expense of private sector activity).

15. See, e.g., Peek and Rosengren (1997), Kashyap and Stein (2000), Khwaja and Mian (2008), Paravisini (2008), Ivashina and Scharfstein (2010), Loutskina (2011), Cetorelli and Goldberg (2012), Jiménez, Ongena, Peydró, and Saurina (2012), Schnabl (2012), Iyer, Peydró, da-Rocha-Lopes, and Schoar (2014), Gilje, Loutskina, and Strahan (2016), Drechsler, Savov, and Schnabl (2017). Many of these studies analyze the effect of bank-specific shocks to funding constraints on bank-level credit supply, incidentally highlighting the important segmentation of the banking sector.

16. Chakraborty, Goldstein, and MacKinlay (2018) and Martín, Moral-Benito, and Schmitz (2021) show that commercial loans are crowded out by banks responding to opportunities in mortgage lending; Greenwald, Krainer, and Paul (2021) show that credit line drawdowns crowd out term loans.

17. A distinct literature has shown that banks' exposure to government debt lead to a contraction in corporate lending during the European sovereign debt crisis (Gennaioli, Martin, and Rossi (2014), Popov and Van Horen (2015), Acharya, Eisert, Eufinger, and Hirsch (2018), Bottero, Lenzu, and Mezzanotti (2020)). However, the mechanism is the impairment of the value of existing sovereign holdings which is different from the mechanism I describe.

18. See, e.g., Campello, Graham, and Harvey (2010), Chodorow-Reich (2014), Cingano, Manaresi, and Sette (2016), Amiti and Weinstein (2018), Huber (2018), Benmelech, Frydman, and Papanikolaou (2019), Caggese, Cuñat, and Metzger (2019), Jiménez, Mian, Peydró, and Saurina (2019), Bustos, Garber, and Ponticelli (2020), Greenstone, Mas, and Nguyen (2020).

19. See, e.g., Gopinath, Kalemli-Özcan, Karabarbounis, and Villegas-Sanchez (2017), Larrain and Stumpner (2017), Blattner, Farinha, and Rebelo (2020), Banerjee, Breza, Townsend, and Vera-Cossio (2019), Bau and Matray (2020), Sraer and Thesmar (2020), Schivardi, Sette, and Tabellini (2021).

2 Data and institutional setting

2.1 Data

My main data source is the French credit registry administered by Banque de France, which collects data on borrowers with total exposure (debt and guarantees) above 25,000 euros toward banks operating in France. For each borrower-bank pair, I recover outstanding credit for each month from 2006 to 2018. I focus on credit with initial maturity above one year to avoid measurement issues related to credit lines. Banks correspond to legal entities, not bank holding companies. I use this level to avoid bundling the different affiliates of the cooperative banking groups that dominate the French corporate credit market.²⁰ There are 506 unique banks. On the corporate credit side, I obtain 1,654,720 unique firms and 3,259,266 unique bank-firm relationships, close to the full population of French corporations. As for local governments, I have 63,545 unique local governments and 208,174 unique local government-bank relationships.

I complement this data with balance sheet and income statement information from the corporate tax-filings collected by Banque de France, which are the tax-filings for firms with revenues above €750,000. Finally, I obtain banks' balance sheets from regulatory filings. More details on the data can be found in Appendix G.

Figure 2 shows the aggregate time series of corporate credit and local government loans in my final dataset. Table 1 shows summary statistics of the variables of interest.

Geographic units. The credit registry provides the location of borrowers. I sort borrowers across intermunicipal cooperations, which I refer to as municipalities. There are 2,081 such municipalities. Each municipality belongs to one of the country's 22 regions. Unless otherwise stated, municipalities and regions correspond to geographical units, not to layers of subnational governments.

2.2 Institutional details

French banks. There are three important features of the French banking landscape. First, the size distribution of French banks is highly skewed, with a large number of mid-sized banks and a few very large banks. Second, a large share of banks are local banks: banks operating in 2 regions or less account for 30% of total corporate lending. In particular, banks belonging to cooperative networks are local banks, mostly following regional boundaries. These two features are depicted in Figure A.2. Third, lending markets are highly local and are well approximated by municipalities. For the average

20. These groups are networks of legally-independent banks that operate on designated geographical areas—mostly following regional boundaries—with a bottom-up ownership structure. While individual banks are linked by solidarity agreements that ensure their joint liquidity and solvency, all matters related to business operations, risk management, or supervision operate at the level of individual banks.

bank branch located in a given municipality, 72% of corporate loans and 86% of local government loans go to borrowers located in the same municipality.

Local government debt. French local governments obtain more than 90% of their external financing through bank loans. Therefore, bank loans to local governments are large: they amount to 14% of GDP in 2018. As can be seen from the aggregate time series on Figure 2, loans to government entities have grown at an average rate of 4% per annum on my sample period, but this average masks a dynamic growth until 2013, followed by a more subdued growth, with negative growth rates in 2016-2017.

I group under the term local governments all local government entities. Looking at the split by entity types on Figure A.3, local governments indeed represent the largest share, followed by public hospitals, state-owned public service operators, and public housing.²¹ Rules on subnational entities borrowing imply that local government debt finances investment expenditures, as opposed to operating expenditures. This is reflected in the relatively long maturity of local government loans (15 years on average). Local governments benefit from an explicit guarantee of the state, so that the risk on these loans is that of a sovereign default.

Loans to local governments are also large from the point of view of banks. They account for 40% of total credit to local governments and corporations combined (Fig. A.4 (a)).²² However, there is a large heterogeneity in banks' participation in this market. Looking across banks, only 42% of banks are active in this market and the banks that are active in this market tend to be the largest banks, accounting for 90% of corporate credit (Fig. A.4 (b)).

Finally, this market is characterized by highly local dynamics. First, local governments are scattered on the French territory and take their lending decisions in a decentralized manner. Second, as mentioned above, local governments borrow locally. These two facts induce a large geographical heterogeneity in the dynamics of local government debt across municipalities, which can be seen on the maps in Figure 3.

The combination of variation in banks' participation to this market, variation in local government debt dynamics across locations, and variation in banks' geographical implantation generates heterogeneity in local government debt dynamics across banks. Figure 4 displays this variation by plotting the distribution of changes in local government loans as a fraction of total loan portfolio at the bank×region level. My empirical strategy exploits (the plausibly exogenous part of) this heterogeneity.

21. The fact that these other entities borrow independently of the local governments that control them is very much country-specific, hence the bundling into a single local government term.

22. Using aggregate data to take into account loans to households (which are not observed in the credit registry), the share of local government loans is around 15%.

3 Conceptual framework

This paper investigates the crowding out effect of local government debt on private sector activity, operating via a reduced availability of corporate financing.²³ The textbook mechanism works as follows: an increase in local government loan demand raises the total demand for loans, which puts upwards pressure on interest rates, and leads to a reduction in corporate credit. From the point of view of firms, crowding out is akin to a shift in banks' residual credit supply curve. This mechanism is depicted on the simple supply and demand graph in Figure A.5. The mechanism is very general: it occurs as long as bank credit supply is not perfectly interest-elastic. In particular, it does not depend on banks having a preference for local government loans. The effect is stronger when bank credit supply is less elastic.

When banks are segmented, crowding out has a bank-specific dimension: a larger increase in local government debt demand directed at one bank leads to a larger drop in that bank's corporate credit supply. This occurs because frictions prevent capital from flowing across banks. Without such frictions, banks facing a higher demand for local government loans would draw in capital from other banks and any reduction in corporate credit would be uniform across banks. In addition, the bank-specific drop in corporate credit disproportionately affects the banks' existing borrowers because of frictions preventing borrowers from switching banks. Importantly, the hypothesis that banks are segmented is testable: if false, there should be no relative crowding out effect.²⁴

Finally, while the most basic crowding out mechanism fully operates through changes in the interest rate, crowding out can also operate through quantity rationing instead of prices, or a combination of both.

The goal of this paper is to quantify the aggregate crowding out effect, that is, the effect of a demand-driven aggregate increase in local government loans on aggregate corporate credit. To do so, I first document a causal relative crowding out effect across banks, and subsequently firms. The relative crowding out effect reflects both the aggregate crowding out effect and the degree of bank segmentation. While this relative effect is conceptually different from the aggregate effect, it is useful for two reasons. First, the well-identified relative crowding out effect can serve as an input to quantify the aggregate crowding out effect. Second, it allows me to investigate the distributive effects of crowding out operating through segmented intermediaries.

23. I study *financial* crowding out, independently of any *real* crowding out effect. Real crowding out refers to the fact that—independently of the mode of financing—government production can only occur at the expense of private sector activity when production factors are fully employed.

24. See the model in Appendix C for a formalization of these arguments.

4 Relative crowding out: corporate credit

4.1 Empirical strategy

I present the methodology to investigate the relationship between bank-level demand-driven increases in local government loans and corporate credit supply. To clarify the identification strategy, it is useful to examine the structural equations obtained from a simple model similar to Khwaja and Mian (2008):²⁵

$$\begin{aligned}\Delta C_{fbt} &= \theta_{ft} + \xi_{bt} + \beta \Delta C_{bt}^{gov} \\ \Delta C_{bt}^{gov} &= Z_{bt}^{gov} + \xi_{bt}\end{aligned}$$

ΔC_{fbt} is bank \times firm-level credit growth, ΔC_{bt}^{gov} is the bank-level increase in local government debt, θ_{ft} is a firm-level shock (e.g., a productivity shock), ξ_{bt} is a bank-level shock (e.g., a liquidity shock) and Z_{bt}^{gov} is a shock to the demand for local government loans addressed to bank b . t indexes time.

The first equation states that bank \times firm-level credit growth depends on firm-level shocks, bank-level shocks, and bank-specific increases in local government loans if $\beta \neq 0$. The second equation states that the bank-level increase in local government loans depends on the bank-specific demand for local government loans and on the bank-level shock. β is the structural relative crowding out parameter that we want to estimate.

The first hurdle to estimating β is the potential correlation between local government debt and firm-level shocks. If local government debt is used as a countercyclical policy tool, changes in local government debt will be negatively correlated to firm-level shocks. Conversely, multiplier effects of local government debt would induce a positive correlation with firm-level shocks. This correlation may exist not only in the time series, but also across banks. If banks have different geographical footprints, and if the correlation between local government debt and corporate credit operates at the local level, the firm-level shocks θ_{ft} will differ for banks experiencing different local government loan demand Z_{bt}^{gov} . Hence, ΔC_{bt}^{gov} and θ_{ft} are likely correlated. I address the identification problem by focusing on firms with multiple lending relationships and adding firm \times time fixed effects, which capture the firm-level determinants of credit flows that are common to all of its lenders (Khwaja and Mian, 2008). Provided that firm-level demand shocks—which may be correlated with changes in local government debt—are symmetric across lenders, they will be absorbed by the fixed effects.²⁶ Intuitively, the identification of crowding out relies on the fact that the aforementioned confounding channels predict a correlation between local government debt and *firm-level credit demand*, while crowding out operates as a

25. See for instance the model in Appendix C.

26. Credit demand must be interpreted in a broad sense: it captures a firm's propensity to receive a loan independently of its lenders. Focusing on credit with initial maturity above one year makes the symmetry assumption less demanding (Ivashina, Laeven, and Moral-Benito (2020)).

shock to the *bank-specific supply* of credit, which depends on the bank-level increase in local government loans.

Estimating β presents a second endogeneity issue: ΔC_{fbt} and ΔC_{bt}^{gov} are jointly determined in bank b 's optimization problem; therefore, they are both affected by bank-specific shocks ξ_{bt} . For instance, if a bank is hit by a negative liquidity shock, this will adversely affect both ΔC_{fbt} and ΔC_{bt}^{gov} . The solution to this problem is to instrument ΔC_{bt}^{gov} by the demand shifter Z_{bt}^{gov} , which affects ΔC_{bt}^{gov} but is orthogonal to ξ_{bt} . To proxy for Z_{bt}^{gov} , I exploit highly granular variation in local government debt dynamics across municipalities along with variation in banks' geographical footprints to define:

$$BankExposure_{bt} = \sum_m \omega_{bm,t-1}^{gov} \times \Delta C_{mt}^{gov} \quad (1)$$

where ΔC_{mt}^{gov} are municipality-level growth rates in local government loans and $\omega_{bm,t-1}^{gov}$ are shares that capture bank b 's exposure to local government debt dynamics in municipality m (defined below). $BankExposure_{bt}$ proxies for the demand pressure directed to bank b , attributable to the fact that banks' pre-determined geographic implantation across municipalities generates heterogeneous exposure to local government debt demand shocks. The shift-share structure abstracts from the potential correlation between ΔC_{fbt} and the bank-specific component of ΔC_{bmt}^{gov} . The key assumption is that $BankExposure_{bt}$ is orthogonal to other bank-level shocks ξ_{bt} .

It is ex-ante unclear what is the appropriate unit to analyze relative crowding out effects: the bank holding company, bank, bank \times location, or branch level. Identifying relative crowding out effects requires that units are segmented, which favors coarser aggregation levels. On the other hand, looking at finer levels allows to study distributive effects across narrower subpopulations. In my baseline analysis, I study crowding out at the bank \times region level, which balances these requirements.²⁷ My baseline specification is:

$$\Delta C_{fbt} = d_{ft} + \beta \Delta C_{brt}^{gov} + \Phi \cdot \mathbf{X}_{fbrt} + \varepsilon_{fbt} \quad (2)$$

where the additional subscript r indicates the region in which firm f is located. d_{ft} is a firm \times time fixed effect, and \mathbf{X}_{fbrt} is a vector of controls. Time corresponds to quarters. I define ΔC_{fbt} as the mid-point growth rate $\Delta C_{fbt} = \frac{C_{fbt} - C_{fb,t-1}}{0.5(C_{fbt} + C_{fb,t-1})}$ to account for both the intensive and extensive margins (Davis and Haltiwanger, 1992). The (endogenous) independent variable is the change in local government loans normalized by banks' lagged total loan portfolio: $\Delta C_{brt}^{gov} = \frac{C_{brt}^{gov} - C_{br,t-1}^{gov}}{C_{br,t-1}^{tot}}$, which captures the increase in lending to local governments relative to total lending capacity.²⁸ ΔC_{brt}^{gov} is instrumented by $BankExposure_{brt}$, defined accordingly as the weighted sum of ΔC_{mt}^{gov} for municipalities m

27. Many banks are regional banks and regions are typically the main operating subdivisions of national banks. Hence, the hypothesis that there is segmentation across bank \times regions is plausible. Section 5 investigates crowding out at the bank, bank \times region or bank branch level.

28. This is the relevant quantity to analyze crowding out as appears in the model in Appendix C. Moreover, it is well defined for banks that do not lend to local governments.

in region r , with weights $\omega_{bm,t-1}^{gov} = \frac{C_{bm,t-1}^{gov}}{C_{br,t-1}^{tot}}$ that capture exposure to the local government loan market in municipality m relative to total credit. I control for the sum of weights, $\omega_{br,t-1}^{gov}$, equal to the share of local governments in the banks' total loan portfolios, as recommended by Borusyak, Hull, and Jaravel (2021).²⁹ In robustness checks, I use alternative definitions of these variables. My main results present the reduced form effect of *BankExposure* on ΔC_{fbt} , and I use the IV to provide the relevant magnitudes.

Estimating specification (2) yields an unbiased estimate of β if the standard exclusion restriction is satisfied: $\mathbb{E}[BankExposure_{bri}\varepsilon_{fbt}|\mathbf{X}_{fbri}, d_{ft}] = 0$. Following the preceding discussion, two conditions need to be met. First, the firm-level shocks that may be correlated with local government debt must be evenly spread across the firm's lenders, so that they are absorbed by the firm \times time fixed effects. Second, *BankExposure* must not be systematically correlated with other bank-level shocks. The assumption is that banks do not sort into locations such that unobserved bank-level shocks are correlated to both a decline in corporate credit supply and increases in local government loans in the locations where the bank operates (Borusyak, Hull, and Jaravel, 2021).³⁰ Figure 5 tests whether *BankExposure* is systematically correlated with banks' observable characteristics. I report both unconditional correlations and correlations conditional on $\omega_{br,t-1}^{gov}$, the share of local government loans in banks' loan portfolios. The unconditional correlations show differences between exposed and non-exposed banks. However, these differences are mainly driven by differences between banks that do take part in the local government loan market, and are thus more likely to have high exposure, and non-participating banks: once we condition on $\omega_{br,t-1}^{gov}$, the differences essentially disappear. Two characteristics remain unbalanced, bank size and bank state-owned status, and are included as controls in my baseline specification. In robustness checks, I also control for all available characteristics.

Identification with shift-share instruments can rely on orthogonality conditions on shares or on shifters (Goldsmith-Pinkham, Sorkin, and Swift (2020), Borusyak, Hull, and Jaravel (2021)). In my setting, the natural assumption is identification based on shifters. The key element is that, provided that the shifters ΔC_{mt}^{gov} induce shocks to ΔC_{fbt} that are symmetric across lenders, ΔC_{mt}^{gov} is orthogonal to ε_{fbt} conditional on d_{ft} . Appendix D further discusses the shares and shifters views of identification in my setting and provides associated tests.

29. This isolates the variation stemming from banks' heterogeneous exposure to municipality-level shocks, partialling out the variation in banks' participation to the market for local government loans.

30. It is *not* a problem that banks sort into locations based on sectoral specialization, types of clienteles, business dynamism, so that banks with different exposure lend to firms with different credit demand. The firm \times time fixed effects control for these differences. What matters is that these geographical footprints are not correlated to other bank-level credit supply shocks, themselves correlated with banks' exposure to local government debt shocks.

4.2 Results

4.2.1 Baseline

Table 2 presents the results corresponding to equation (2). In the baseline results, controls include the sum of shares, the bank’s assets, equity ratio, a dummy indicating whether the bank is state-owned and the length of the bank-firm relationship. Standard errors are clustered at the bank-region level, which corresponds to the level of the shock.³¹ Section 4.2.2 presents results for alternative specifications.

In column (1), I investigate the effect of bank exposure to local government debt demand shocks on corporate credit without any controls or fixed effects. I do not find any significant effect. However, this coefficient confounds the crowding out channel and other endogenous relationships between local government debt and corporate credit. To address this concern, I augment my model with firm \times time fixed effects (column (2)). This specification only exploits within-firm variation, comparing changes in credit provided to the same firm by banks that are more or less exposed to increased demand for local government loans. I find that bank exposure to higher demand for local government debt significantly predicts lower corporate credit growth. My baseline specification is column (3), which includes firm \times time fixed effects as well as controls. The point estimate remains similar, slightly larger in absolute value. Interestingly, the comparison between column (1) and columns (2) and (3) suggests that the endogenous bias plays in a direction opposite to crowding out, as would occur if local government debt had positive multiplier effects.

Columns (4)-(5) show the IV results with the actual increase in local government loans ΔC_{brt}^{gov} instrumented by *BankExposure*, with and without controls.³² The point estimate implies that an increase in local government loans equal to 1% of total lending reduces corporate credit growth by 0.95 percentage point.³³ As a back-of-the-envelope computation, this implies that when local governments borrow an additional €1 from a given bank, that bank lends €0.54 less to private firms located in the same region.³⁴

These estimates isolate the crowding out effect of local government debt operating through reduction in corporate credit. They hold constant local demand effects of government debt, government debt endogenously responding to private sector financing conditions, and any “real” crowding out operating independently of the financing channel.

The crowding out parameter captures banks’ ability to increase their balance sheet size in response to a credit demand shock. Under the assumption that local government loan demand is interest-insensitive, it is equal to the sensitivity of corporate credit to

31. In robustness checks, I cluster standard errors at the municipality level to account for the correlation of residuals across banks that have similar municipality exposures, an issue raised by Adão, Kolesár, and Morales (2019) and Borusyak, Hull, and Jaravel (2021).

32. The first stage and the OLS of the IV are presented in Table D.2.

33. For the mid-point growth rate, or equivalently, reduces the standard growth rate by 0.96pp.

34. Equal to the euro loss for each firm $0.95 \frac{C_{fb,t-1}}{C_{br,t-1}^{tot}}$ multiplied by the number of firms by bank \times region.

a change in banks' total funding and can be compared to the existing evidence on this topic. The key contribution is Paravisini (2008), who estimates that a \$1 increase in Argentinian banks' access to external finance increases corporate credit by \$0.66 at the monthly horizon and \$0.82 at the yearly horizon. More recently, and in a developed country setting, Drechsler, Savov, and Schnabl (2017) show that a \$1 change in deposits leads to a \$0.57 change in corporate lending. My estimate is thus quantitatively consistent with existing evidence.

4.2.2 Robustness and further tests of the identifying assumption

Distortions in the market for local government lending and crowding out. I estimate the effect of a *marginal* €1 increase in local government loans on corporate credit. The market for local government loans may be subject to regulatory or political distortions that affect the *level* of local government lending. In theory, the marginal effect is independent of these level distortions and is only determined by banks' ability to expand their balance sheets.³⁵ I rule out one important case: that crowding out is only the result of political interference. It is important to exclude this specific case: the mechanism could be different (e.g., the reduction in corporate credit could be driven by banks making losses on coerced government lending as in Hoffmann, Stewen, and Stiefel (2021)) or the distortion in banks' objective function due to political interference could make credit supply artificially inelastic. To rule out this hypothesis, I use the fact that state-owned banks are more exposed to political interference. Columns (6) and (7) of Table 2 present the results of estimating equation (2) excluding state-owned banks from the sample. I find point estimates that are highly similar to my main results. As robustness checks, I also show that the crowding out coefficient is independent of other proxies for political pressure on banks and of proxies for abnormal profits on local government loans (Table B.1). Therefore, crowding out is not specific to state-owned banks or the associated political interference, and more generally, the crowding out coefficient does not depend on distortions that may affect the level of local government lending, in line with theory.

Discussion of identifying assumptions. This paragraph provides additional tests that further support the validity of my identifying assumptions: (1) firm×time fixed effects absorb firm-level demand shocks that are symmetric across the firm's banks; and (2) there are no other bank-level credit supply shocks that are systematically correlated with *BankExposure*.

Pre-trends: Regarding (1), one worry is that local government debt is correlated to firm×bank-level demand shocks. One story would be a form of reverse causality whereby

35. To take a simple example, assume total lending capacity is fixed and equal to 100. Distortions on the relative desirability of local government vs. corporate debt affect the split between x local government debt and $100 - x$ corporate debt. However, the euro for euro crowding out parameter will always be equal to -1, irrespective of x . See Appendix C.2.6 for a formal treatment.

local governments increase their borrowing when they observe that corporate demand directed toward the banks they typically borrow from is low. Regarding (2), one concern is that banks with high *BankExposure* have systematically lower corporate credit growth, independently of local government debt shocks. To alleviate these concerns, I show that bank exposure measured at the time of the shock is not correlated with corporate credit patterns before the shock. Figure 6 presents the result of including leads and lags of the independent variable in my specification and shows the absence of a significant pre-trend.

More granular fixed effects: A story that would violate assumption (1) is if, when local government debt rises, corporate demand shifts toward banks that are not active in the market for local government loans. Similarly, assumption (2) would be violated if banks lending to local governments receive different time-varying credit supply shocks. If these effects are time-varying, they are not controlled for by the share of local government loans in the bank’s loan portfolio. I alleviate this concern by including time fixed effects interacted with a dummy equal to 1 if the bank is active in lending to local governments $1[\omega_{br,t-1}^{gov} > 0]$. I further test the identifying assumption by including bank×region fixed effects that control for any time-invariant factor affecting local government and corporate credit at the bank×region level. Finally, I include bank×time fixed effects that control for any time-varying bank-level shocks that may be correlated to bank exposure to local government debt shocks. This specification is very conservative: it identifies whether *within* banks, higher local government debt demand in a region leads to lower corporate credit in this region relative to the other regions where the bank operates. These specifications produce coefficients very similar to my baseline result (Table B.2).

Heterogeneity by strength of demand effects: Assumption (1) does not hold if the firm×time fixed effects do not correctly control for the firm-level credit demand shocks that may be correlated to changes in local government debt. To alleviate this concern, I exploit the fact that some firms are more likely to experience a positive demand shock when local government debt increases. Local government debt finances public investment projects, which is likely to generate an increase in local public procurement contracts. I flag industries in which public procurement contracts account for more than 5% of total revenues as highly sensitive to local government debt shocks. If the firm×time fixed effects were unable to control for firm-level credit demand, we would observe relatively higher credit growth for these firms as local government debt increases. Table B.2 shows that this is not the case: the effect of exposure to local government debt shocks is not significantly different (and if anything slightly larger) for these firms.

Overall, this evidence provides strong support for the identifying assumptions behind my empirical strategy. Additional tests related to the shift-share structure of the instrument are presented in Appendix D.

Additional robustness checks. I perform a variety of additional robustness checks of my baseline results, detailed in Appendix B.1. First, Table B.3 presents the results when including additional controls, dropping banks who never participate in the market for local government debt, very small banks or observations in the first quarter when local government debt growth tends to be the largest. I also show the estimated coefficient when dropping any of the 100 largest banks, municipalities, or any year. Second, B.4 shows results for alternative definitions of the independent variable, such as defining ΔC_{brt}^{gov} as the standard growth rate. Third, Table B.5 shows results when looking at alternative outcomes, namely the log change or the change in firm-bank credit normalized by the firm’s total loans. The effect on log change is smaller, highlighting the importance of accounting for the extensive margin. Fourth, I show that my findings are robust to clustering standard errors at the municipality level or at the region level.

4.2.3 Addressing the bias due to firms substituting across banks

A limitation of the within-firm estimator is that if firms substitute across lenders in response to a lender’s shock, the estimated coefficient will be biased. Intuitively, if firms substitute toward less affected lenders when one of their lenders is shocked, it means that control banks are affected by the shock in a direction opposite to that of treated banks. Comparing the two, as done by the within-firm estimator, then overestimates the true effect. The existing literature does not provide a methodology to obtain an unbiased estimate of β when bank-level credit supply shocks are correlated with firm-level demand shocks—i.e., the within-firm estimator is essential—and firms substitute across their lenders.³⁶

I provide a methodology to separately identify the direct effect of the shock and substitution across banks, allowing us to obtain an unbiased estimate of β . In a methodological appendix (Appendix E), I describe the problem, provide results on the sign and the size of the bias, present the proposed method to address this concern, and establish the conditions for identification.

Appendix B.1 presents the obtained results (Table B.6). I find that firms do not substitute toward less affected banks and that accounting for this possibility only makes the effect larger in absolute value than my baseline effect (by roughly 20%). Consequently, omitting the substitution term is innocuous in the case at hand.

36. In particular, looking at firm-level effects—even controlling for firm-level demand by including the estimated fixed effects from the within-firm regression as proposed by Cingano, Manaresi, and Sette (2016) and Jiménez, Mian, Peydró, and Saurina (2019)—does not solve the issue (see Appendix E).

4.3 Exploiting the near failure of Dexia as a natural experiment

The identification strategy in Section 4.1 has the value of being general, in that it can be implemented at any date for which there is bank-firm data on credit granted. 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.”³⁷ Compared to my baseline strategy, this has two advantages: (i) it does not suffer from the identification concerns discussed above related to the shift-share instrument based on realized local government loan growth; and (ii) it allows me to investigate long-run effects.

Before 2008, the Franco-Belgian bank Dexia was the main lender to French local governments, with a market share above 30%. In 2008, Dexia was hit by severe credit losses in the US subprime market, forcing the French and Belgian governments to intervene. Unable to recover, the bank was eventually dismantled in 2013. These events led to a sharp decline in the market share of Dexia and forced local governments to turn to other lenders. I exploit this major market restructuring as an exogenous shock to local government debt demand directed toward other banks. I use the fact that the shock was likely larger for banks implanted in areas where Dexia was the most active. I define the variable *DexiaExposure* at the bank×region level in a manner similar to *BankExposure*:

$$DexiaExposure_{br} = \sum_{m \in r} \omega_{bm,2008} \times DexiaDependent_{m,2008} \quad (3)$$

where *DexiaDependent*_{*m*,2008} is a dummy equal to 1 if the municipality-level market share of Dexia in 2008 is above median and $\omega_{bm,2008}$ are 2008 exposure weights, defined as before.³⁸ I posit that banks that were subject to a higher increase in demand for local government loans driven by the failure of Dexia cut their corporate credit supply by a larger amount. To test this hypothesis, I estimate the following equation, similar to my baseline test (2):

$$\Delta C_{fb\tau} = d_f + \beta DexiaExposure_{br,2008} + \Phi \cdot \mathbf{X}_{fb\tau,2008} + \varepsilon_{fb} \quad (4)$$

where $\Delta C_{fb\tau}$ is the growth rate from 2008 to $\tau \in \{2013, 2014\}$. Table A.1 presents the results. Columns (1) and (2) show the first stage coefficient, which indicates that higher *DexiaExposure* indeed predicts higher local government loan growth $\Delta C_{br\tau}^{gov}$ after the failure of Dexia. Columns (3) and (4) show that a higher *DexiaExposure* predicts a lower corporate credit growth, at the 2013 and 2014 horizon. Columns (5) and (6) show the results of the IV where *DexiaExposure* is used as an instrument for $\Delta C_{br\tau}^{gov}$. Expressing this coefficient as a euro for euro effect, I find that an additional 1€ in local government loans translates into a 0.25€ reduction in corporate credit at the 5-year horizon. This is roughly half the effect estimated at the quarterly horizon. Hence, even though banks do manage to gradually adjust their balance sheet size, the effect persists

37. This shock is also used in Derrien, Mesonnier, and Vuillemeys (2019).

38. See Appendix G for details on the construction of Dexia market shares.

over a long horizon. To further support a causal interpretation of these results, I show the effect of *DexiaExposure* on the growth rate of local government and corporate loans *before* the failure of Dexia and I find no effect (Table A.2).

5 Mechanism

My results show that a demand-driven increase in local government lending causally leads to a reduction in banks' corporate credit supply. This raises two questions. First, why does crowding out occur, that is, what prevents banks from increasing total lending to maintain their corporate credit supply in the face of strong local government debt demand? Second, how do banks adjust their corporate credit portfolio in the face of a local government debt demand shock?

5.1 What prevents banks from increasing total credit supply?

Bank-level frictions on increasing balance sheet size. Ideally, banks should match the additional demand for credit by an additional supply of capital, by borrowing (from depositors or the interbank market) or by raising equity. However, banks only have a limited ability to attract more deposits or to raise equity, interbank markets are imperfect, and banking regulation may additionally constrain total lending. In theory, the severity of these constraints determines the extent of crowding out. To test this hypothesis, I examine whether, in the cross-section of banks, crowding out is stronger for banks that appear more constrained in their ability to increase credit supply.

Table 3 presents the results. First, column (1) shows that crowding out is more severe for smaller banks, which are likely to be more constrained overall.³⁹ Looking at specific constraints, I find that crowding out is stronger for banks with a higher deposit gap (column (2)) and is weaker for banks with better access to international financing sources (column (3)), emphasizing the importance of banks' access to a large pool of funding. Column (4) reveals that crowding out is less severe for banks that securitize their loan portfolio, in line with the idea that securitization allows banks to relax capital constraints. Similarly, column (5) shows that crowding out is less severe for banks that have a large share of their loan portfolio that can be pledged as collateral to the European Central Bank. Column (6) shows that crowding out is slightly less severe for banks with higher capital ratios, but the difference is statistically insignificant. One explanation is that capital ratios matter only when they are binding. In line with this idea, I find that during the implementation period of Basel III—i.e., when most banks had to increase their capital ratios—banks that were further away from the target exhibit a stronger

39. The definition of all characteristics is detailed in the notes of Table 3.

crowding out effect (column (7)).⁴⁰ Liquidity constraints also appear to matter, since cash-poor banks are more sensitive to crowding out (column (8)). Finally, column (9) shows that crowding out is weaker for banks that can easily borrow on the interbank market, suggesting that cross-sectional crowding out effects not only reflect constraints that apply to the aggregate banking system but also constraints preventing individual banks from drawing in capital from other banks in face of a demand shock.

Together, these results imply that crowding out is related to banks' limited ability to increase their total balance sheet size, in line with the standard theory. I explore two further implications. First, I document that the crowding out effect is asymmetric: increases in local government debt lead to a reduction in corporate credit, while reductions in local government debt do not increase corporate credit (Table A.3). This is in line with the mechanism proposed in this paper: when constrained banks increase their lending to local governments, they are forced to reduce corporate credit, while when the shock is negative banks have more leeway to adjust (e.g., by holding liquid assets instead of increasing credit), and the adjustment may take more time to materialize. Second, I look at the time-series of the effect across four subperiods: the pre-crisis (2006-2007), the crisis (2008-2009), the recovery and sovereign crisis (2010-2013) and the post-2013 period (Table A.4). The crowding out effect is significant in all subperiods, except for the last one. A first explanation is that local government debt growth has been lower since 2013 (even negative in 2016-17), while my effects are driven by increases in local government debt. Second, the post-2013 period is characterized by an accommodative monetary policy, which likely reduced banks' balance sheet constraints, and hence crowding out.

Frictions across and within banks. Each local government borrows from a given bank branch. Therefore, crowding out is ultimately determined by the ability of a specific branch to absorb the increased demand for credit. I now exploit the granularity of my data to further investigate the level at which frictions operate.

To do so, I compare the effects of local government debt shocks constructed at three levels: the bank level, the bank \times region level (as in the baseline) and the bank branch level. These three levels correspond to distinct sources of frictions. Bank-level frictions are related to banks' limited ability to expand their total balance sheet, explored in the previous paragraph. Within banks, there are constraints on the reallocation of capital across units due to inefficient internal capital markets or to the need to incentivize local managers.⁴¹ There are also constraints on the time of local loan officers, i.e. crowding out may also be driven by an inelastic supply of labor.

Table 4 presents the results. Column (1) looks at the effect of *BankExposure* defined

40. Besides, loans to local governments have low capital requirements (0 to 20% depending on the type of entity); hence regulatory capital ratios are unlikely to be the main driver of crowding out.

41. Consistent with models and empirical results on optimal delegation, e.g., Stein (2002), Liberti and Mian (2008), Liberti, Seru, and Vig (2016), Liberti (2018), Skrastins and Vig (2019).

at the bank level, and shows that higher exposure to demand for local government loans (averaged across regions) leads to a lower corporate credit supply. Column (2) considers *BankExposure* defined at the bank×region level, conditional on bank×time fixed effects. I find that, for a given bank, higher local government debt demand in a region leads to lower corporate credit in this region, relative to the other regions where the bank operates. Column (3) looks at the effect of shocks defined at the branch level, conditional on bank×region×time fixed effects.⁴² It shows higher local government debt demand at a given branch reduces this branch’s corporate credit relative to other branches of the same bank in the same region. All three coefficients have comparable sizes. Appendix C.2.3 shows that this can be interpreted as frictions on increasing credit supply being of similar size at the three levels. This is confirmed by including all three variables in the same specification: the lower-level variable subsumes the others, as predicted if the size of frictions is similar. These results thus highlight the quantitative significance of within-bank frictions.⁴³

There are two implications. First, within-bank spillovers across regions is limited. I confirm this insight by including in the same regression the bank×region-level shock and the average shock of the bank in other regions (column (6)). The effect of this indirect exposure term is small and statistically insignificant. Second, the presence of local banks and the existence of within-bank frictions imply that crowding out will have a local dimension: aggregating across banks, regions where local governments borrow more experience a stronger reduction in corporate credit supply.⁴⁴

5.2 How do banks adjust their lending portfolio?

Banks’ limited ability to expand their credit supply implies that they must adjust their corporate credit in response to a demand-driven increase in local government loans. First, which type of corporate loans are crowded out the most? Second, does the adjustment operate through prices or through quantity rationing?

Severity of crowding out and loans’ characteristics. Which corporate loans are crowded out the most? This is theoretically ambiguous. On the one hand, Friedman (1978) shows that crowding out should be stronger for assets that are closer substitutes to government debt. These would be loans to large, highly rated firms. On the other hand, banks may systematically favor safer lending opportunities. This could be the

42. A branch’s local government lending is typically concentrated in a few municipalities (86% of the total is in one municipality on average), which threatens the consistency of the branch-level shift-share instrument (see Appendix D). The result should be interpreted in light of this caveat.

43. Others have highlighted within-bank frictions, e.g., Scharfstein and Stein (2000) on inefficient internal capital markets and Chakraborty, Goldstein, and MacKinlay (2018) on personnel constraints.

44. Such causal effect could not be obtained by investigating the local-level relationship between local government debt and corporate credit because it would be confounded by local multiplier effects.

case because lending to safe borrowers allows banks to capture all the surplus instead of leaving an informational rent,⁴⁵ or because safer loans can be used to meet regulatory requirements. If banks are constrained by a limited supply of safe assets, additional lending opportunities to safe local governments will induce banks to disproportionately downsize the riskier part of their lending portfolio.

I investigate these competing hypotheses in Table 5. I find that banks selectively cut credit to the smallest firms, with an effect monotonic in firm size (columns (1) and (2)). Crowding out is also more severe for unrated firms (column (3)). Moreover, the effect is the same for firms in sectors that are heavily reliant on public procurement contracts (column (6)), that are likely to have risk profiles correlated to those of local governments. These results go against the predictions of Friedman (1978) and are in line with a preference for safer loans. Besides, if the preference for safer loans was driven by regulation, we should observe a differential effect by credit rating, since collateral eligibility and capital requirements depend on those ratings. In column (4), I show that, conditional on being rated, there is no differential crowding out effect for firms rated as safe or risky. On the other hand, column (5) shows that crowding out is less severe for banking relationships where banks are likely to have invested in information acquisition.⁴⁶ Overall, these results show that banks respond to a lending opportunity with safe local governments by downsizing the segments of their loan portfolio where information asymmetry is the highest.

Price vs. quantity adjustment. The results presented so far relate to corporate credit quantities. I now investigate how increases in local government debt demand affect interest rates, using the “New contracts” dataset collected by Banque de France, which includes information on interest rates for a representative sample of loans. I estimate the effect of local government debt shocks on interest rates using the within-firm specification (2), with the interest rate charged by bank b on new loans to firm f as a dependent variable. Details on the sample and on the specification are in Appendix B.2.

I find that the price effect is positive, but small compared to the quantity reaction. My results imply a price elasticity of corporate credit demand equal to 30. This is in line with the empirical evidence on loan price stickiness and on bank-level shocks inducing quantity rationing without price adjustments, as well as with structural estimations of the price elasticity of corporate credit demand.⁴⁷ This result is usually rationalized by

45. A similar mechanism is described in Manove, Padilla, and Pagano (2001) where safe government lending would disincentivize (lazy) banks’ screening activity, reducing lending to the risky private sector.

46. To proxy this dimension, I define a banking relationship as important from the perspective of the bank if lending to the firm is large compared to the size of the bank’s portfolio in the firm’s region.

47. For loan rates stickiness, see, e.g., Berger and Udell (1992). For bank-level shocks inducing quantity rationing without price adjustments, see, e.g., Khwaja and Mian (2008), Cingano, Manaresi, and Sette (2016), and Bentolila, Jansen, and Jiménez (2018). My results can be compared to the structural estimation in Diamond, Jiang, and Ma (2021), who find an extensive margin elasticity of 228. Note that

concerns about the adverse selection effects of higher interest rates (Stiglitz and Weiss, 1981), consistent with my finding of a credit cut concentrated on small, opaque firms. In sum, I find a price effect, but it remains unclear whether the quantity reaction corresponds to the sole adjustment of firms along their demand curve or to quantity rationing.⁴⁸

6 Relative crowding out: investment & employment

The previous results show that lenders exposed to increased demand for local government loans reduce their credit supply to firms. In addition, I show that this effect is not attenuated by firms substituting across lenders, so that crowding out will impact firm-level borrowing. How does the reduction in credit affect firms' investment and employment?

6.1 Empirical strategy

The key mechanism described so far operates at the bank level: banks subject to higher demand for local government loans disproportionately reduce their corporate credit supply. To investigate real effects on investment, I follow the literature and translate the bank-level effect into a firm-level effect by considering firms' exposure to the shock through their lenders. I present the strategy to investigate the firm-level effects on investment, and use the same strategy for employment. I estimate the following specification:

$$\Delta K_{ft} = \beta^K FirmExposure_{ft} + \Phi \cdot \mathbf{X}_{ft} + \alpha_{mt} + \alpha_{st} + \alpha_{b(f)t} + \varepsilon_{ft} \quad (5)$$

where *FirmExposure* is the average *BankExposure* across the lenders of firm f , weighted by bank shares in firms' total credit $\omega_{fb,t-1}$:

$$FirmExposure_{ft} = \sum_b \omega_{fb,t-1} BankExposure_{brt} \quad (6)$$

α_{mt} , α_{st} , and $\alpha_{b(f)t}$ are municipality \times time, two-digit industry \times time, and main bank \times time fixed effects, respectively. I define a firm's main bank as the bank with the largest share in the firm's credit. \mathbf{X}_{ft} is a vector of firm-level controls. *FirmExposure* _{ft} captures the extent to which a firm borrows from banks subject to increased demand for local government loans. Intuitively, the specification compares firms borrowing from banks subject to higher demand for local government loans to firms borrowing from other banks.

To understand the logic of the identification, it is useful to return to the firm \times bank-level model (2). Aggregating this specification at the firm level using bank shares, we obtain (omitting controls): $\Delta C_{ft} = d_{ft} + \beta FirmExposure_{ft} + \varepsilon_{ft}$. That is, firm-

the term elasticity is improper in case of quantity rationing.

48. These results incidentally attenuate concerns about the baseline results being driven by bank-specific credit demand shocks: in this case, we should find lower rates for more exposed banks. The combination of lower quantity and higher prices instead indicates a supply shock. In addition, the within-firm differential effect on interest rates further reflects firms' poor ability to substitute across lenders.

level credit growth depends on firm-level exposure to crowding out and on firm-level unobserved credit demand shocks. This equation highlights the identification challenge. If *BankExposure* was correlated to d_{ft} , then *FirmExposure* is also correlated to d_{ft} . Besides, the firm-level specification cannot include firm \times time fixed effects to absorb the firm-specific shocks that may be correlated with *FirmExposure*. Following Cingano, Manaresi, and Sette (2016) and Jiménez, Mian, Peydró, and Saurina (2019), I overcome this issue by including as controls the estimates of the firm-level shocks d_{ft} obtained from the within-firm specification. This procedure precisely controls for the correlation between *FirmExposure* and d_{ft} . Identification of β in the firm-level credit growth regression then follows from identification in the firm \times bank-level credit growth specification.⁴⁹

When looking at investment, the coefficient of interest β^K corresponds to $\beta \times \eta^K$, the effect on credit multiplied by the credit-to-investment sensitivity η^K . The identifying assumption is that the firm-level unobservable determinants of ΔK_{ft} are the same as those of ΔC_{ft} , so that they are properly controlled for by the estimated d_{ft} .

I further tighten my identification strategy by looking at the effect of *FirmExposure* *within* municipality \times time cells, that is, within firms experiencing a similar local-level increase in local government debt, but *across* firms differentially exposed to this increase through their banking relationships. This allows to partial out the local-level macroeconomic relationship between local government debt and private firms' prospects. I further add industry \times time fixed effects to account for time-varying industry-specific shocks. Finally, I include main bank \times time fixed effects to compare firms matched to the same main bank, alleviating concerns related to firm-bank matching patterns. Consistency with (2) requires that \mathbf{X}_{ft} contains the firm-level weighted average of \mathbf{X}_{fbrt} . I also include additional firm-level controls.

The dependent variables are defined as the mid-point growth rate of credit, the growth rate of fixed assets and the growth rate of the total wage bill. The last two variables are obtained from the corporate tax-filings, available for firms with annual turnover above €750,000. Hence, I cannot study firm entry and exit and consider only the intensive margin for these variables. Since the frequency of corporate tax-filings is annual, I construct *BankExposure* and *FirmExposure* at the yearly frequency. In the baseline specification, bank shares are defined as mid-point shares to properly aggregate the within-firm specification in mid-point growth rates. As in Alfaro, García-Santana, and Moral-Benito (2021), I recover firm-level demand shocks for both multi-bank and single-bank firms. The firm-level effects are thus estimated on the sample of all firms with tax-filings data.

49. This procedure is problematic if firms substitute across banks: since the within-firm coefficient is biased, the estimated d_{ft} are also biased, and including them in the firm-level regression produces a biased estimate. In the case at hand, I have shown that there is no such substitution, so this is not an issue. See Appendix E for more details and for a method to treat the opposite case.

Figure 5 tests whether firms with higher exposure to crowding out are systematically different on observed characteristics. I report unconditional correlations and correlations conditional on the fixed effects included in the firm-level specification as well as on the firm-level average of $\omega_{br,t-1}^{gov}$, the share of local governments in banks' loan portfolios. Figure 5 shows it matters to control for the average $\omega_{br,t-1}^{gov}$, i.e. for whether the firm borrows from banks that are active in local government lending. Conditional on this control, firms with high and low exposure to crowding out are essentially similar on size, leverage, asset tangibility, profitability, liquidity, interest coverage, and on estimates of the marginal products of capital and labor.

6.2 Results

6.2.1 Baseline results

I first repeat the within-firm estimation on yearly data to obtain the relevant magnitudes and recover the firm-level demand shocks used as controls. Table A.5 lists the results. I find that the bank-level crowding out parameter is equal to 0.42 at the yearly horizon.

Table 6 presents the firm-level effects obtained from estimating (5). Columns (1) to (3) report the effect of firm exposure to crowding out on credit, investment and wage growth. Column (1) confirms the within-firm results and shows that firms more exposed to crowding out receive less credit. Column (2) shows that firms more exposed to crowding out invest significantly less. Column (3) shows that the effect on wage growth is also negative, albeit smaller in magnitude. This last result indicates that credit frictions also matter for firms' employment decisions.⁵⁰

To gauge the quantitative significance of my results, I separately estimate η^K and η^L , the credit-to-investment and employment sensitivities. To do so, I use *FirmExposure* as an instrument for firm-level credit growth. The effects are reported in columns (4) and (5). I find a credit-to-investment sensitivity equal to 0.38 and a credit-to-wage growth sensitivity equal to 0.11. These estimates are consistent with previous evidence.

These estimates can be used to quantify the effect of an additional €1 in local government debt at one bank on investment and wages at firms borrowing from this bank. Starting from the effect on credit and using the credit-to-investment sensitivity η^K , I find that an additional €1 in local government debt at one bank leads to a €0.23 drop in corporate investment at firms borrowing from this bank. For the wage bill, the effect is €0.06. To obtain these euro for euro estimates, I can alternatively estimate the specification where *FirmExposure_{ft}* is used as an instrument for its “realized quantity” version $\Delta C_{ft}^{gov} = \sum_b \omega_{fb,t-1} \Delta C_{brt}^{gov}$, which is the average increase in local government loans at the lenders of firm *f*. Table B.8 provides these results. I obtain a €0.24 effect

50. See, e.g., Schoefer (2015) and Fonseca and Van Doornik (2021) for evidence on this channel.

on investment and €0.06 on the wage bill. Computation details are in Appendix B.3.

6.2.2 Further tests and robustness checks

Discussion of identifying assumptions. The main threat to identification is that, conditional on controls included, firms with low demand for inputs tend to borrow from high exposure banks. In particular, a threat is that the firm-level determinants of investment and employment are not the same as the firm-level determinants of credit and are not properly controlled for by the inclusion of the estimated \hat{d}_{ft} . This paragraph provides several additional tests that alleviate this concern.

More granular fixed effects: My specification crucially includes municipality×time fixed effects, which restrict the comparison to firms experiencing a similar local-level increase in local government debt. I can further tighten the identification by interacting location and industry fixed effects, to allow any local effect of local government debt to be industry-specific. Table B.9 report the results including region×industry×time fixed effects and municipality×industry×time fixed effects. These specifications yield point estimates very similar to my baseline. I also include firm fixed effects and lagged credit growth as a control, in order to control for firm-specific time invariant characteristics or to restrict the comparison to firms on a similar credit trend, and I again find very similar effects.

The magnitude of the coefficient is remarkably stable across these specifications, despite the fact that the inclusion of the finer grid of fixed effects drastically increases the R^2 . This finding reveals that, if any unobservable is affecting both exposure to crowding out and investment or employment, then it must be orthogonal to municipality-level industry-specific trends and to firm invariant characteristics. This is extremely unlikely. A formal econometric treatment of this argument is provided by Oster (2019). Applying this methodology to the investment specification, I find a value for the δ parameter equal to 3.76 when comparing the baseline specification with that including municipality×industry×time fixed effects, and equal to 4.33 when comparing the baseline with the specification with firm fixed effects, both well above the recommended value of 1.⁵¹ Consequently, correlated unobservables are unlikely to drive my results.

Pre-trends: Figure 7 presents pre-trends for the three different outcomes. The absence of pre-trends alleviates the concern that *FirmExposure* is systematically higher for firms with poor investment opportunities or declining labor demand.

Heterogeneity by strength of demand effects: I exploit the fact that firms in industries highly reliant on public procurement contracts are likely to experience a positive demand shock when local government debt increases. If my specification imperfectly controls for

51. The interpretation of this parameter is that the correlation of unobservables with the variable of interest must be ≈ 4 times larger than that of observables for a bounding set accounting for the presence of unobservables to include 0. See details in Appendix B.3.

the demand effects of local government debt, I would find that exposure to local government debt shocks has a less negative effect for these firms. Interacting *FirmExposure* with a dummy for industries highly reliant on public procurement contracts, I observe no differential effect for these firms (Table B.9).

Robustness checks. I perform a variety of robustness checks of my results, detailed in Appendix B.3. Table B.10 presents the results when dropping firm-level controls and when including additional firm-level controls. It also shows the results when dropping firms borrowing from state-owned banks, firms borrowing from banks that do not lend to local governments, or when restricting the sample to multibank firms. Finally, I show the results when firm-level averages are constructed using lagged bank shares instead of the mid-point shares that properly aggregate mid-point growth rates. In Table B.11, I show the reduced-form and IV results when credit growth is defined using the standard growth rate instead of the mid-point growth rate, and the results for employment growth defined as the growth in the number of full-time employees. My results go through with these different specifications.

Firm-level effects of the Dexia experiment. As a further validation of my results, I examine firm-level real effects using the alternative identification strategy outlined in Section 4.3. As for the baseline strategy, I aggregate the within-firm specification at the firm level. I examine the effect on credit growth, investment and employment over 2008-13 and 2008-14. Table A.1 reports the results. I find that banks' exposure to the local government debt demand shock generated by the failure of Dexia significantly reduced investment and wage growth for firms borrowing from these banks. To further support the causal interpretation of these results, I report placebo regressions where Dexia exposure is used to predict 2006-2007 credit growth, and 2001-2007 investment and employment growth (the tax-filings being available for previous years). I find no effect.

6.3 Heterogeneous effects

Heterogeneous crowding out effects across firms may arise from two channels. First, banks may disproportionately reduce credit to some types of firms, as shown by the within-firm results in Table 5. Second, firms may differ in their sensitivity of investment (employment) to a credit cut.

Regarding the first channel, the first panel of Table 7 shows that, even among the subpopulation of relatively large firms for which tax-filings are available, banks selectively cut credit to the smallest firms, in line with the evidence in Table 5. A high tangibles ratio also moderates the credit supply shock, although the effect is not statistically significant.

Panels B and C of Table 7 investigate the second channel. Proxies for firm dependence on external finance do not affect the size of the credit cut, but significantly affect the sen-

sitivity of input usage to the availability of bank financing, in line with intuition. Namely, firms in industries with a higher Rajan and Zingales (1998) index (i.e., industries that need more external funding to finance investment) exhibit a credit-to-investment sensitivity that is larger than the baseline by 50%. Similarly, firms with a high working capital-over-sales ratio—that are more likely to require external financing to pay workers—have a credit-to-labor sensitivity that is three times as large as the baseline.

I also find that small firms and firms in low tangibility industries, two typical proxies for capital constraints, have higher credit-to-investment sensitivities, in line with the idea that these firms have a lower ability to turn toward alternative sources of financing.

Finally, I investigate how the effect varies when sorting firms by revenues-over-capital or revenues-over-labor, which provide within-industry measures of firms' marginal product of inputs when the production function is Cobb-Douglas. Firms with higher marginal products are likely to be more constrained in their input acquisition decisions.⁵² I find that the effect on credit is not different for firms with higher marginal products. However, in line with the intuition that these firms are more constrained, I find that firms with higher Y/K have larger credit-to-investment sensitivity and firms with higher Y/L have larger credit-to-labor sensitivity.⁵³ Therefore, even though banks do not selectively cut credit to high marginal product firms, these higher sensitivities imply that crowding out generates a larger reduction in inputs for firms with higher marginal output gains from those inputs. This indicates that the shock reduces allocative efficiency. The next section quantifies the aggregate cost of this effect.

7 Aggregate effects

Thus far, I have documented *relative* crowding out effects: increases in local government loans at one bank reduce that bank's corporate credit relative to other banks, and adversely affect investment and employment at firms borrowing from this bank relative to other firms. However, these cross-sectional relationships do not yield the crowding out effect on *aggregate* corporate credit, investment, employment and ultimately output. This section develops a framework to bridge this gap.

Relating cross-sectional crowding out effects to aggregate output requires to consider two channels. First, crowding out may reduce output through a reduction in aggregate input usage. Second, by affecting the distribution of inputs across firms, crowding out may affect allocative efficiency. This would affect aggregate output through a change in aggregate total factor productivity (TFP). I quantify these effects in turn.

52. The first-best allocation requires equalization of marginal products across firms. Therefore, higher than average marginal products must reflect frictions that prevent this equalization from occurring. The advantage of looking at dispersion in marginal products is that it provides an agnostic way to study the effect of frictions on input acquisition (Hsieh and Klenow, 2009).

53. For capital, the coefficient is economically large but not statistically significant.

The counterfactual of interest is a situation where local government debt does not crowd out corporate credit. To take a concrete counterfactual, assume that the path of local government spending and debt is unchanged, but that local government debt is entirely financed by foreign investors. All other effects of local government debt are kept constant, but local government debt does not crowd out domestic credit.⁵⁴ This counterfactual corresponds to the situation where $\Delta C_{brt}^{gov} = 0$ for all (b, r) . I use the potential outcomes notation $X(\mathbf{0})$ to denote the counterfactual value of variable X .

7.1 Crowding out and aggregate input usage

How does crowding out affect aggregate credit, investment, and employment? The reduced-form analysis is silent on the causal effect of crowding out on non-exposed banks and firms. If crowding out also affects credit and input usage at non-exposed banks and firms, aggregate effects will differ from relative effects. I circumvent this issue by developing a simple model to link my relative estimates to aggregate effects. I provide the key elements in the main text and leave details to Appendix C.

Model. I construct a perfectly competitive model of the banking sector with three markets: the credit market, the deposit market and the interbank market. The model features local governments, firms, banks, and households. Banks lend to firms and local governments and are funded via household deposits. The key feature of the model is that banks are segmented. Firms, local governments, and households are assigned to a given bank and do not arbitrage across banks. Banks can undo this segmentation by trading (at a cost) on the interbank market.⁵⁵ Each bank solves the following problem:

$$\max_{\{C_b^{corp}, C_b^{gov}, S_b, B_b\}} r_b^c C_b^{corp} + r_b^g C_b^{gov} - r_b^s S_b - i B_b - \frac{\phi}{2} B_b^2$$

subject to a funding constraint: $C_b^{corp} + C_b^{gov} = S_b + B_b$. C_b^{corp} and C_b^{gov} are corporate and local government loans, S_b is deposits, B_b is net interbank borrowing. r_b^c , r_b^g , r_b^s , and i are the interest rates on the credit markets, the deposit market and the interbank market. ϕ indexes the degree of interbank frictions. I assume that households have an isoelastic upward-sloping deposit supply with elasticity ϵ^s . Firms have downward-sloping isoelastic credit demand curves with elasticity ϵ^c , which are shifted by mean-zero credit demand shocks θ_f . Local governments have downward-sloping isoelastic credit demand curves with elasticity ϵ^g . Local governments also have demand shocks which, combined with the fact that they are assigned to banks, generates bank-specific local government loans demand shocks Z_b^{gov} .

54. For the only difference to be the financing of government debt—i.e. to prevent simultaneous changes in the allocation of savings at home vs. abroad—one needs to assume some form of international capital markets segmentation. See for instance the model in Broner, Clancy, Erce, and Martin (2021).

55. The model is homothetic to having depositors partly arbitrage across banks. I also consider firms substituting across banks and the key results are unchanged (extension C.2.2).

The equilibrium of the model is defined by the solution of banks' maximization problem and by the market clearing conditions for the corporate credit market, the local government credit market, the deposit market and the interbank market. The equilibrium conditions determine the value of all endogenous variables as a function of the credit demand shocks Z_b^{gov} and θ_f . In particular, I obtain firm \times bank-level corporate credit C_{fb} , bank-level local government credit C_b^{gov} , and their aggregate counterparts C^{corp} and C^{gov} .

The object of interest is the effect of a change in local government lending induced by a local government debt demand shock on corporate credit, at the level of each bank and at the aggregate level. I obtain these relationships by log-linearizing the model around the deterministic equilibrium where all shocks are identically equal to 0. I denote \hat{X} the relative change of variable X with respect to its deterministic equilibrium value X^* . The effect of a demand-driven increase in local government loans on corporate credit is:

$$\begin{aligned}\hat{C}^{corp} &= -\chi\lambda\hat{C}^{gov} \\ \hat{C}_{fb} &= \theta_f - \chi(1 - \nu)\lambda\hat{C}^{gov} - \chi\nu\lambda\hat{C}_b^{gov}\end{aligned}$$

$\chi > 0$ is decreasing in the ratio of the elasticity of deposit supply on the elasticity of corporate credit demand $\frac{\epsilon^s}{\epsilon^c}$. ν is monotonically increasing in ϕ , $\nu = 0$ when $\phi = 0$ (perfect integration) and $\nu = 1$ when $\phi \rightarrow +\infty$ (full segmentation). λ is the share of local government loans in banks' loan portfolios.

The first relationship is the aggregate crowding out relationship. I call χ the aggregate crowding out parameter. It captures the ability of the aggregate banking system to expand its credit supply. The second relationship is the bank \times firm-level change in corporate credit, which depends on three terms. The first term is the firm-level demand shock. The two last terms correspond to the crowding out channel. The second term depends on the aggregate change in local government loans, while the last term depends on the bank-specific increase in local government loans.

The effect of a bank-specific increase in local government loans on corporate credit depends on ν , the degree of banking frictions. I call $\nu\chi$ the relative crowding out parameter. The intuition is the following. Assume that the banking sector is perfectly integrated, that is, $\nu = 0$. Then, a bank subject to a higher demand for local government debt than other banks draws in capital from other banks using the interbank market, up to the point where interest rates are equalized across banks. The reduction in corporate credit is uniform across banks, and there is no relative crowding out effect.

This equilibrium effect explains the existence of the second term: when segmentation is not perfect ($\nu < 1$), the pressure on rates related to an increased demand for local government debt at one bank is partly transmitted to other banks through the interbank market, so that non-exposed banks also reduce corporate credit. Therefore, in equilibrium, each bank's corporate credit supply is negatively affected by the aggregate amount of local government loans. The key implication is that the relative effect is smaller than

the aggregate effect: $\nu\chi \leq \chi$.

Interbank market frictions determine the relative effect but do not affect the aggregate crowding out parameter, which depends on $\frac{\epsilon^s}{\epsilon^c}$. I also consider the case where banks face balance sheet constraints (e.g., net worth or regulatory constraints). In this case, the aggregate crowding out parameter increases with the severity of the constraint. The key insight that the relative effect is smaller than the aggregate effect remains unchanged.⁵⁶

Link with the empirical specification. To link the static model to the panel setting of the main text, I assimilate log-deviations from the deterministic equilibrium to growth rates. Re-writing the model equations using the notations of my empirical specifications, I obtain:⁵⁷

$$\begin{aligned}\Delta C_t^{corp} &= -\chi\Delta C_t^{gov} \\ \Delta C_{fbt} &= \theta_{ft} - \chi(1 - \nu)\Delta C_t^{gov} - \chi\nu\Delta C_{bt}^{gov}\end{aligned}$$

The second equation is the theoretical counterpart to my firm×bank-level empirical specification (2). The coefficient that I identify in this analysis is the relative crowding out parameter $\chi\nu$.⁵⁸ I obtain the equivalent equations for investment (employment) by aggregating the firm×bank-equation at the firm-level and using the credit-to-investment (employment) pass-through coefficients:

$$\begin{aligned}\Delta K_t &= -\eta^K\chi\Delta C_t^{gov} \\ \Delta K_{ft} &= \eta^K\theta_{ft} - \eta^K\chi(1 - \nu)\Delta C_t^{gov} - \eta^K\chi\nu\Delta C_{ft}^{gov}\end{aligned}$$

where $\Delta C_{ft}^{gov} = \sum_b \omega_{fb,t-1}\Delta C_{bt}^{gov}$ is the average increase in local government loans at the lenders of firm f . $\eta^K\chi\nu$ corresponds to the coefficient obtained from specification (5), where $FirmExposure_{ft}$ is used as an instrument for ΔC_{ft}^{gov} (Table B.8).

Quantification of aggregate effects. The quantities of interest are the aggregate shortfalls in corporate credit, capital, and labor due to crowding out, defined as:

$$\begin{aligned}\mathcal{L}(C_t^{corp}) &= -\frac{C_t^{corp} - C_t^{corp}(\mathbf{0})}{C_t^{corp}(\mathbf{0})} = \chi\Delta C_t^{gov} \\ \mathcal{L}(K_t) &= -\frac{K_t - K_t(\mathbf{0})}{K_t(\mathbf{0})} = \eta^K\chi\Delta C_t^{gov} \\ \mathcal{L}(L_t) &= -\frac{L_t - L_t(\mathbf{0})}{L_t(\mathbf{0})} = \eta^L\chi\Delta C_t^{gov}\end{aligned}$$

56. These theoretical predictions are in line with the reduced-form evidence presented in Section 5.1.

57. Firm×bank credit growth ΔC_{fb} is approximately equal to \hat{C}_{fb} . The increase in local government lending normalized by banks' total loan portfolio ΔC_b^{gov} is approximately equal to the log-deviation in local government lending multiplied by the share of local government loans in the banks' portfolio $\lambda\hat{C}_b^{gov}$. Aggregate variables are defined accordingly.

58. The baseline model considers only banks and not regions within banks. To link this model to the empirical specification, I consider bank×regions as distinct banks. This is not inappropriate since I document that frictions across and within banks are of similar magnitudes. See extension C.2.3 for a model with different frictions across and within banks.

Under the assumption that production is Cobb-Douglas, I can then compute the output loss from the reduction in input usage as: $\mathcal{L}^{input}(Y_t) = \alpha\mathcal{L}(K_t) + (1 - \alpha)\mathcal{L}(L_t)$, where α is the capital share. To gauge the magnitude of these effects, the shortfalls can be translated into a euro for euro effect, comparable to government spending multipliers. For output, this yields $m_t^Y = \frac{Y_t - Y_t(\mathbf{0})}{C_t^{gov} - C_t^{gov}(\mathbf{0})}$.

Lower bound. From the model, the relative effects are lower bounds for the aggregate effects: $\mathcal{L}(C_t^{corp}) \geq \chi\nu\Delta C_t^{gov}$, $\mathcal{L}(K_t) \geq \eta^K\chi\nu\Delta C_t^{gov}$, and $\mathcal{L}(L_t) \geq \eta^L\chi\nu\Delta C_t^{gov}$. These quantities can be obtained using my reduced-form estimates.⁵⁹

The yearly corporate credit shortfall attributable to crowding out is equal to at least 0.43%. Equivalently, €1 of local government loans crowds out at least €0.46 of corporate credit. For capital and labor, I find lower bounds equal to 0.17% and 0.03%, respectively. This translates into an output loss due reduced inputs equal to 0.07%, or equivalently, a multiplier m^Y equal to -0.15 .

Estimating the equilibrium effect. These lower bounds miss the fact that all banks—even those not increasing their lending to local governments—reduce their corporate credit supply when aggregate local government debt increases. The size of this effect depends on ν , which determines the extent of the transmission of the shock across banks. This parameter can be separately identified by considering another prediction of the model: banks exposed to a higher local government debt demand shock borrow from other banks on the interbank market. Namely, the model predicts that:

$$\Delta B_{bt} = (1 - \nu)(\Delta C_{bt}^{gov} - \Delta C_t^{gov})$$

where ΔB_{bt} is the change in net interbank borrowing, normalized by total assets.

I estimate $1 - \nu$ by regressing the change in net interbank borrowing—observed in bank balance sheets data—on the increase in local government lending, instrumented by *BankExposure*. All details are in Appendix B.4. I find that the estimated coefficient is positive and statistically significant. In line with the prediction of the model, banks exposed to a higher local government debt demand borrow from other banks on the interbank market, and pre-trending tests show that this only happens at the time of the shock. I estimate $1 - \nu$ to be equal to 0.17. Since all my cross-sectional effects scale with ν , the lower bounds underestimate the aggregate effect by 17%.

This implies that the aggregate corporate credit loss due to crowding out is equal to on average 0.52%, or equivalently, €1 of local government loans crowds out €0.55 of corporate credit. The capital and labor shortfalls are equal 0.20% and 0.04%, respectively. The aggregate output loss from the reduction in input usage is equal to 0.08%, or equivalently, €1 of local government loans crowds out €0.18 of corporate output. The

59. Computation details are in Appendix B.4. The aggregate loss formulas hold in the model where all banks and firms are symmetric. My baseline computations take into account the distribution of firm and bank size (which yields more conservative estimates).

multipliers are summarized in Table 9. Figure 8 plots the time series of the output loss. The output loss is highest at the beginning of the sample when local government debt growth was the highest, and turns negative in 2016 and 2017 when local government debt recedes. Appendix B.4 reports robustness checks for the aggregate effects computations, which only produce larger estimates.

How credible is the quantification of equilibrium effects based on my simple model? If capital moves across banks in other forms than interbank debt (e.g., deposits moving across banks, holders of bank equity or bonds substituting across banks), we return to the case where my quantification is a lower bound. The next paragraph discusses equilibrium effects operating outside of the banking sector.

Other general equilibrium effects. χ is the aggregate crowding out parameter in the model presented above, which integrates equilibrium effects on the credit/deposit market but is not a general equilibrium model: it takes as given loan demand and deposit supply functions. Typical general equilibrium analysis suggests two additional channels that may cause unaffected firms to adjust their inputs. First, to the extent that the credit shock generates an increase in the cost of capital, the relative price of goods produced by the most exposed firms will tend to increase, triggering a reallocation of demand toward the least exposed firms. The magnitude of this effect depends on the substitutability of goods produced by different firms. Second, the shock generates a reduction in aggregate expenditure, which reduces input demand for non-exposed firms. Chodorow-Reich (2014) quantifies these general equilibrium responses to a credit supply shock and finds that for plausible parameter values, they either magnify the effects from the partial equilibrium exercise or have at most a modest attenuating effect.

Alternative counterfactual. I focus on the counterfactual that isolates the crowding out effect, i.e., the counterfactual where government debt is kept constant but does not crowd out domestic credit. For the sake of completeness, Appendix C.2.4 investigates the case in which local government debt increases by €1 to reduce lump-sum taxes by €1.

7.2 Crowding out and allocative efficiency

The reduced-form results presented above show that crowding out has distributive effects on firm-level input usage, which may affect aggregate output through a change in allocative efficiency. I now quantify this effect.

Framework. I start by describing the standard misallocation framework and then show how crowding out affects allocative efficiency. In the first-best allocation of resources, marginal products of inputs are equalized across firms. Input misallocation can thus be quantified by assessing the extent of deviations from marginal product equalization. I

follow the standard practice in the literature and model misallocation as wedges on the prices of inputs. Intuitively, the wedges can be thought of as explicit or implicit taxes that distort firms' input decisions. The allocative prices paid by firm f are $R(1 + \tau_f^K)$ and $w(1 + \tau_f^L)$ for capital and labor, respectively. The wedges correspond to frictions, such as distortionary regulation or taxation, financial constraints, or imperfect competition, that distort actual or shadow input prices. With a Cobb-Douglas production function, the presence of wedges implies the modified first-order conditions for firms' marginal revenue products of inputs (henceforth MRPX):

$$\begin{aligned} \text{MRPK}_{ft} &= \alpha \frac{P_{ft} Y_{ft}}{K_{ft}} = R_t (1 + \tau_{ft}^K) \\ \text{MRPL}_{ft} &= (1 - \alpha) \frac{P_{ft} Y_{ft}}{L_{ft}} = w_t (1 + \tau_{ft}^L) \end{aligned}$$

A higher capital wedge τ_{ft}^K induces firms to use a suboptimal amount of capital, reflected in a higher marginal product of capital MRPK. Let us denote τ_{ft} the average of the capital and labor wedges, $\tau_{ft} = \alpha \tau_{ft}^K + (1 - \alpha) \tau_{ft}^L$. Hsieh and Klenow (2009) show that aggregate productivity is a function of the dispersion in wedges:

$$\log(\text{TFP}_t) = \log(\text{TFP}_t^*) - \frac{\sigma - 1}{2} \text{Var}(\tau_{ft}) - \frac{\alpha}{2} \text{Var}(\tau_{ft}^K) - \frac{1 - \alpha}{2} \text{Var}(\tau_{ft}^L)$$

with σ the elasticity of substitution across products of different firms (see Appendix F for details). The first term corresponds to TFP under the optimal allocation of resources and the three last terms to misallocation. When wedges are highly dispersed, marginal products are not equalized; consequently, there are large gains from reallocation inputs away from firms with low marginal products toward firms with high marginal products. What matters is wedges dispersion: if wedges are high but equal across firms, there are no gains from reallocating inputs.

Crowding out and allocative efficiency. How do cross-sectional crowding out effects affect aggregate productivity? I take firms' exposure to the credit supply shock generated by crowding out as a positive shock to firms' wedges. The reduction in credit supply acts as an increase in the shadow cost of taking on credit, which is equivalent to an increase in the wedge of inputs financed by bank loans.⁶⁰ The observed reduction in firms' input usage (Table 6) is to be understood as the reaction to this shock to wedges.

How does the change in the distribution of wedges driven by firms' heterogeneous exposure to crowding out affect aggregate TFP? This depends on whether the variance of wedges increases or decreases. If wedges increase and inputs drop to a larger extent for firms with the highest marginal product of inputs (the highest ex-ante wedges), the variance goes up and misallocation worsens. Conversely, if wedges fall more for firms with higher ex-ante wedges, we get closer to marginal product equalization and TFP increases.

60. In considering a shock to financing conditions as a shock to wedges, I follow Larrain and Stumpner (2017) and Blattner, Farinha, and Rebelo (2020).

To quantify this effect, let us define the TFP loss due to crowding out as $\mathcal{L}(\text{TFP}_t) = -[\log(\text{TFP}_t) - \log(\text{TFP}_t(\mathbf{0}))]$. $\text{TFP}_t(\mathbf{0})$ is aggregate TFP in the no-crowding-out counterfactual, which depends on the counterfactual wedges $\tau_{ft}(\mathbf{0})$, $\tau_{ft}^K(\mathbf{0})$, and $\tau_{ft}^L(\mathbf{0})$.

Descriptive evidence on firm-level wedges. Before turning to the quantification of the TFP loss, I present descriptive statistics on firm-level wedges. A key assumption in the TFP loss computation is that wedges capture firm-level distortions or frictions that prevent firms from using the optimal amount of inputs. In practice, I find that firms with higher wedges tend to be smaller, to have a lower tangibles ratio and to be more dependent on external finance, suggesting that wedges partly reflect financing frictions that constrain firms' input acquisition decisions.⁶¹

Reduced-form effect of crowding out on wedges. Quantifying the TFP loss requires estimates of the counterfactual wedges $\tau_{ft}(\mathbf{0})$, $\tau_{ft}^K(\mathbf{0})$ and $\tau_{ft}^L(\mathbf{0})$. That is, we need to quantify the effect of firm exposure to crowding out on wedges. To do so, I estimate the effect of *FirmExposure* on wedges using the specification for firm-level inputs (equation (5)), with the change in wedges $\Delta\tau_{ft}^K$, $\Delta\tau_{ft}^L$ and $\Delta\tau_{ft}$ as dependent variables.⁶²

The results are reported in Table 8. The first panel shows that firms' exposure to the credit supply shock generated by crowding out generates a significant increase in the capital wedge, the labor wedge, and their average. The fact that wedges respond to firm-level credit supply shocks further verifies that wedges are partly driven by credit frictions, which supports considering firm exposure to crowding out as a shock to wedges. The effect is larger for the capital wedge, in line with the idea that credit frictions particularly affect firms' ability to invest.

I then estimate the same regressions on a sample splitted along firms' previous period wedge $\tau_{f,t-1}$ to investigate whether the size of the shock to wedges varies with the level of ex-ante constraints. The second panel of Table 8 presents the results. Columns (3) to (8) show that the credit supply shock corresponds to a larger increase in wedges for firms with higher ex-ante wedges. This is particularly true for the capital wedge. This differential effect is not driven by the fact that banks cut credit to a larger extent to high-wedge firms (if anything the effect on credit is slightly weaker for these firms). Rather, a given tightening of credit represents an increase in the cost of acquiring inputs that is larger for firms that are more constrained. Therefore, input usage will drop by a larger amount for firms with higher ex-ante marginal products of inputs, worsening misallocation. This corroborates the findings of Table 7, which showed that more constrained firms have higher credit-to-investment and credit-to-employment sensitivities.

61. See Table A.6. I also find that high-wedge firms are more profitable, in line with the idea that they have higher marginal products of inputs due to constraints. These firms also have higher credit ratings, suggesting that the higher marginal product of capital does not solely reflect the price of risk.

62. See Appendix F for details on definitions and estimation of wedges.

Aggregate TFP loss due to crowding out. I use the estimates of the causal effect of exposure to crowding out on wedges to obtain the aggregate TFP loss attributable to crowding out. From the fitted value of the regression, I obtain $\hat{\tau}_{ft}$ the predicted wedge given actual exposure to crowding out. I then predict the counterfactual wedge $\tau_{ft}(\mathbf{0}) = \hat{\tau}_{ft} - \hat{\beta}^\tau FirmExposure_{ft}$. I use the results of the regressions where the effect is allowed to differ for firms with higher ex-ante wedges. The TFP loss is given by:⁶³

$$\begin{aligned} \mathcal{L}(\text{TFP}_t) = & \frac{\sigma - 1}{2} [\text{Var}(\hat{\tau}_{ft}) - \text{Var}(\tau_{ft}(\mathbf{0}))] \\ & + \frac{\alpha}{2} [\text{Var}(\hat{\tau}_{ft}^K) - \text{Var}(\tau_{ft}^K(\mathbf{0}))] + \frac{1 - \alpha}{2} [\text{Var}(\hat{\tau}_{ft}^L) - \text{Var}(\tau_{ft}^L(\mathbf{0}))] \end{aligned} \quad (7)$$

I compute the TFP loss for each industry and aggregate across industries using industry shares in value added. I find that the misallocation effect of crowding out reduces aggregate TFP, and thus output, by 0.06% per year on average. The time series of the output loss is depicted on Figure 8. It is not as dependent on the change in local government loans as the output loss due to reduced inputs. This is because this effect is not linear in the change in local government debt but depends on the distribution of exposure to crowding out across banks and firms. The loss turns slightly negative in 2016 when local government debt recedes, because a reduction in credit constraints induces a larger increase in inputs for high marginal product firms, reducing misallocation. Over the sample period, the output loss corresponds to a multiplier m^Y equal to -0.12 . These results are summarized in Table 9.

Segmentation across banks vs. heterogeneous effect of the shock. Crowding out may increase the dispersion in wedges through two channels. First, a uniform credit shock may increase misallocation if it generates a larger drop in inputs for firms with higher ex-ante wedges. Second, there is an effect specific to crowding out operating through banks: the distribution of local government lending across banks generates variation in credit supply shocks across firms, and hence affects the distribution of firm-level wedges. The misallocation effect of this second channel depends on the variance of firm-level credit shocks and on the covariance between firm-level shocks and ex-ante wedges. To assess the relative importance of these channels, I decompose the TFP loss as:

$$\mathcal{L}(\text{TFP}_t) = - \underbrace{[\log(\text{TFP}_t) - \log(\text{TFP}_t(\bar{\mathbf{F}}_t))]}_{\text{Segmentation}} - \underbrace{[\log(\text{TFP}_t(\bar{\mathbf{F}}_t)) - \log(\text{TFP}_t(\mathbf{0}))]}_{\text{Heterogeneous effects}}$$

where $\bar{\mathbf{F}}_t$ denotes the counterfactual where changes in local government debt are equal at all banks—or equivalently there is no segmentation across banks—so that firm-level shocks are equal at all firms. The first term is the TFP loss due to the dispersion in credit supply shocks. The second term is the loss due to the heterogeneous effect of a

63. This equation holds under the assumption that $\log(\text{TFP}_t^*)$ is unaffected. I discuss this point below.

uniform shock.

I find that the increase in misallocation is entirely driven by heterogeneous firm-level effects. Segmentation has a small, positive effect on aggregate TFP, equal to €0.01 per €1 of local government loans, because high-wedge firms are slightly less exposed to the shock. In addition, the heterogeneous effects channel is potent not because banks selectively cut credit to high-wedge firms, but because high-wedge firms are more sensitive to a given credit cut. This decomposition is important for two reasons. First, even if the credit cut is not larger for firms with high marginal products of inputs, the fact that high marginal product-constrained firms tend to experience a larger reduction in inputs from a given reduction in credit can induce a large misallocation effect.⁶⁴ Second, the aggregate cost of the distributive effects induced by bank segmentation is negligible.

Limitations and robustness. This computation is subject to several caveats. First, Hsieh and Klenow (2009)’s framework only quantifies the losses from misallocation within industries, a limitation common to most of the misallocation literature. Since the shock under study causes a reallocation of inputs both within and across industries, within-industry misallocation is likely a lower bound on the total misallocation effect. Second, the previous computation is correct under the assumption that $\log(\text{TFP}_t^*)$ is unaffected by the shock. This assumption would be violated if the shock affects firm-level productivity A_{ft} . Unfortunately, this cannot be tested in the absence of data on firm-level product quantities.⁶⁵ Since there is no strong theoretical prior for expecting credit frictions to affect A_{ft} , this assumption is reasonable. Third, measurement error in wedges is a prevalent issue in the misallocation literature. Attributing all cross-sectional dispersion in the observed marginal returns to misallocation may overstate the extent of misallocation. However, focusing on *within firm* changes in wedges largely alleviates this concern (Bau and Matray, 2020). Finally, my TFP loss computation is exact only under the functional form restriction on the effect of *FirmExposure* on wedges implied by my empirical specification. To check the robustness of my results, I perform alternative quantifications of the TFP loss (i) using the estimation methodology proposed by Sraer and Thesmar (2020), also based on Hsieh and Klenow (2009), and (ii) using the alternative framework of Petrin and Levinsohn (2012). These methods provide estimates consistent with my baseline quantification (see details in Appendix F).

64. This complements Banerjee, Breza, Townsend, and Vera-Cossio (2019) who find that a credit expansion program that uniformly targets the population induces misallocation when the returns to credit are larger for more constrained entrepreneurs. Similarly, Bau and Matray (2020) find that foreign capital liberalization reduces misallocation because it generates a larger reduction in wedges for high-wedge firms. In contrast, Blattner, Farinha, and Rebelo (2020) quantify misallocation induced by a credit shock concentrated on high-wedge firms.

65. I observe only revenues, which can be used to compute *revenue* productivity TFPR_{ft} , which is not equal to A_{ft} and is instead a function of wedges.

7.3 Discussion

Crowding out and multipliers of local government spending. Aggregating these results, I find that an additional €1 in local government loans reduces aggregate output by €0.30 via crowding out. Debt-financed multipliers are notoriously hard to estimate, but a reasonable range is 0.5-1.9.⁶⁶ My results imply that these multipliers would be higher by 0.3 in the absence of crowding out, a quantitatively significant effect.

The existence of substantial crowding out effects shows that the source of financing matters when interpreting local government spending multipliers. In particular, an active strand of the fiscal multipliers literature exploits geographic variation in transfer-financed government spending to estimate relative multipliers across locations. My results suggest that debt-financed fiscal multipliers may be substantially smaller than the transfer-financed multipliers estimated in this literature.⁶⁷ While one must be cautious when comparing estimates relying on different sources of variation, estimates of debt-financed multipliers (ranging from 0.5 to 1.9) tend to be lower than estimates of transfer-financed multipliers (ranging from 0.8 to 4), in line with this reasoning.⁶⁸

External validity. My results have the greatest external validity for other countries where local governments heavily rely on bank debt. As shown on Figure 1, this represents a large sample of countries.

Do my results teach us something about crowding out generated by central or local government bonds? I show that the output loss due to crowding out reflects the elasticity of the supply of loanable funds, which is likely to be higher in the case of bonds traded on international capital markets than in the case of bank loans.⁶⁹ In that case, my quantification is an upper bound for the crowding out effect of government bonds. A specific case is when local or central government bonds are acquired by banks. This is notably frequent in the U.S. municipal bonds markets, as documented in Dagostino (2018). In this case, similar crowding out effects can be expected.

In addition to magnitudes, this paper provides a framework to quantify aggregate and distributive crowding out effects in segmented markets, which could be applied to sovereign bonds issued on capital markets segmented by maturities (Greenwood, Hanson, and Stein, 2010) or by currencies (Schreger and Du, 2021).

66. From the literature review in Ramey (2019).

67. I thereby provide empirical evidence confirming the theoretical insights of Clemens and Miran (2012) and Farhi and Werning (2016).

68. See the literature reviews in Ramey (2019) and Chodorow-Reich (2019).

69. By contrast, inefficiencies due to segmentation across banks, which is specific to the case of bank loans, play a minor role.

8 Conclusion

This paper investigates one potential adverse effect of increasing levels of local government bank debt: crowding out effects on corporate credit, and subsequently investment, employment, and output.

I first document relative crowding out effects across banks, and then firms. I show that a larger increase in local government debt at one bank disproportionately reduces that bank's corporate credit supply, with real effects on investment and employment for its borrowers. My identification strategy isolates the crowding out channel operating through a reduction in credit supply, holding constant any other effect that local government debt may have on the real economy. In a second step, I build a simple model that shows how these relative effects implied by bank segmentation feed into aggregate effects. I quantify that an additional €1 in local government loans reduces aggregate output by €0.18 via the crowding out-induced reduction in input usage. I also show that relative crowding out effects affect the distribution of inputs across firms. This affects allocative efficiency and leads to a €0.12 output loss per euro of local government loans.

Aggregating these results, crowding out reduces the output multiplier of debt-financed local government spending by 0.3. This is large, as typical debt-financed multiplier estimates range from 0.5 to 1.9. The output loss is driven by the aggregate reduction in corporate credit, which reflects banks' limited ability to increase credit supply when faced with a demand shock, and by the differential effect of the credit cut on firms with heterogeneous returns to inputs. Distributional inefficiencies due to heterogeneous bank and firm exposure to crowding out—the effect most specific to crowding out operating through banks—are negligible.

My results show that constraints on financing supply undermine the ability of debt-financed government spending to stimulate the economy. In addition, they highlight an important downside of transferring debt-taking to lower levels of government, since central government debt financed by bonds issued on international capital markets is likely to generate a lower crowding out effect on the domestic economy.

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Tables and Figures

Table 1: Summary statistics

Panel A: Firm×bank-level variables (quarterly frequency)

	All					Multibank				
	mean	sd	p10	p50	p90	mean	sd	p10	p50	p90
Outstanding credit $C_{f_{bt}}$ (K€)	216.1	473.9	16	77	445	334.3	678.8	17	99	772
Credit growth $\Delta C_{f_{bt}}$ (MPGR)	-0.011	0.69	-0.21	-0.045	0.26	-0.011	0.69	-0.24	-0.050	0.32
Credit growth $\Delta C_{f_{bt}}$ (log diff.)	-0.053	0.099	-0.15	-0.047	0	-0.061	0.11	-0.17	-0.053	0
Bank exposure $BankExposure_{brt}$ (%)	0.14	0.44	-0.13	0.0073	0.64	0.11	0.39	-0.097	0.00042	0.52
Change in local govt debt ΔC_{brt}^{gov} (%)	0.18	1.22	-0.59	-0.0035	1.26	0.16	1.16	-0.52	0	1.03
Local gvt credit C_{brt}^{gov} (K€)	433157	639095	0	120616	1256333	331009	585894	0	31586	1078466
Total credit C_{brt}^{tot} (K€)	1919850	2903060	113746	1129607	3600693	1569226	2648616	29985	820236	3285471
Observations	41,895,794					12,803,109				

Panel B: Firm-level variables (yearly frequency)

	Tax-filings sample				
	mean	sd	p10	p50	p90
Outstanding credit C_{ft} (K€)	588	729.8	4	117	1109.7
Credit growth ΔC_{ft} (MPGR)	-0.067	0.97	-1.31	-0.16	1.56
Credit growth ΔC_{ft} (std)	-0.10	0.66	-0.89	-0.19	0.57
Firm Exposure $FirmExposure_{ft}$ (%)	0.62	1.23	-0.080	0.11	2.33
Change in local govt loans ΔC_{ft}^{gov} (%)	0.72	2.88	-1.41	0	3.92
Capital growth	0.026	0.44	-0.32	-0.078	0.50
Wage bill growth	0.037	0.16	-0.12	0.026	0.20
Assets (K€)	5862.2	16331.6	573.0	1626.0	10319
Fixed assets (K€)	845	2729.6	18	139	1371
Wage bill (K€)	761.1	1345.4	104	361	1474
Value added (K€)	1486	2786.2	220	659	2871
Debt (fin)/Assets	0.27	0.25	0.035	0.20	0.61
Debt (all)/Assets	0.64	0.25	0.34	0.64	0.92
Tangibles/Assets	0.32	0.24	0.052	0.28	0.67
EBIT/Sales	0.049	0.099	-0.020	0.035	0.14
ROA	0.051	0.093	-0.026	0.046	0.15
Cash/Assets	0.092	0.11	0.0019	0.054	0.23
EBIT/Interests	19.4	42.4	-3	6.20	59.7
Observations	1,457,423				

Note: This table reports the summary statistics of the relationship-specific (panel a), and firm-specific (panel b) variables used in the analysis. Credit growth is defined either as the mid-point growth rate (MPGR), the log difference (log diff.) or the standard growth rate (std). Debt (fin) refers to bank debt and bonds. Debt (all) also includes accounts payable. Multibank firms refers to firms with at least two active banking relationships in t or $t - 1$. The weighted average of firm×bank-level and firm-level credit growth are consistent with the aggregate time series.

Table 2: Crowding out effect on corporate credit

	Credit growth						
	Full sample					Excl. state-owned banks	
	RF (1)	RF (2)	RF (3)	IV (4)	IV (5)	RF (6)	IV (7)
Bank Exposure	-0.411 (0.345)	-0.882*** (0.265)	-1.098*** (0.266)			-1.061*** (0.273)	
Change in local govt loans ΔC_{brt}^{gov}				-0.822*** (0.247)	-0.954*** (0.230)		-0.916*** (0.235)
Controls	-	-	✓	-	✓	✓	✓
Firm×Time FE	-	✓	✓	✓	✓	✓	✓
Observations	12,360,042	12,360,042	12,360,042	12,360,042	12,360,042	11,580,020	11,580,020
R-squared	0.00	0.47	0.52	-	-	0.52	-
F stat.	-	-	-	588.8	576.6	-	569.2

Note: This table examines the crowding out effect of local government debt on corporate credit at the bank×firm-level. It reports the results of estimating specification (2). The outcome variable is the mid-point growth rate of credit granted to firm f by bank b . The main independent variable is exposure to local government debt demand shocks defined at the bank×region×time level as the sum of municipality-level local government debt growth, weighted by exposure shares equal to the bank's local government credit in each municipality as a fraction of bank×region-level total credit (equation (1)). In columns labeled IV, *BankExposure* is used as an instrument for the actual increase in bank×region-level local government loans ΔC_{brt}^{gov} . Controls include the sum of shares, the bank's assets, equity ratio, a dummy indicating whether the bank is state-owned, and the length of the bank-firm relationship. In columns (6) and (7), state-owned banks are excluded. All regressions are estimated on the sample of firms with multiple credit relationships. Standard errors are clustered at the region×bank level. ***, ** and * indicate significance at the 1%, 5% and 10% levels, respectively.

Table 3: Severity of crowding out by banks' characteristics

	Credit growth								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Bank Exposure	-2.118*** (0.389)	-1.927*** (0.382)	-1.030*** (0.313)	-1.095*** (0.270)	-1.749*** (0.600)	-1.319* (0.742)	-1.179** (0.573)	-0.933** (0.433)	-2.322*** (0.379)
Large×Bank Exposure	1.577*** (0.475)								
High deposit surplus×Bank Exposure		1.146*** (0.425)							
High international×Bank Exposure			1.041** (0.512)						
Securitize×Bank Exposure				2.332* (1.220)					
High collateral×Bank Exposure					1.309** (0.602)				
High capital×Bank Exposure						0.422 (0.772)			
High 2010 capital×Bank Exposure							1.296** (0.661)		
High cash×Bank Exposure								0.970** (0.450)	
High interbank×Bank Exposure									1.952*** (0.432)
Controls×Bank char.	✓	✓	✓	✓	✓	✓	✓	✓	✓
Firm×Time FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
Bank char.×Time FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
Observations	12,365,482	12,128,737	8,284,872	12,365,482	12,347,764	12,365,482	3,925,697	8,284,856	12,365,482
R-squared	0.52	0.52	0.52	0.52	0.52	0.52	0.53	0.52	0.52

Note: This table examines the severity of the crowding out effect by banks' characteristics. It reports the results of estimating specification (2), where all independent variables are interacted with a dummy for the bank characteristic. The outcome variable is the mid-point growth rate of credit granted to firm f by bank b . The main independent variable is exposure to local government debt demand shocks defined at the bank×region×time level as the sum of municipality-level local government debt growth, weighted by exposure shares equal to the bank's local government credit in each municipality as a fraction of bank×region-level total credit (equation (1)). Large is a dummy equal to 1 if bank's assets are above median. High deposit surplus is a dummy equal to 1 if the difference between deposit growth and loan growth over the last 4 quarters is above median. High international is a dummy equal to 1 if the share of bank liabilities held by non-residents is above median. Securitize is a dummy equal to 1 if the bank is active in securitizing corporate or local government loans. High collateral is a dummy equal to 1 if the share of the loan portfolio eligible as collateral by ECB rules is above median. High capital is a dummy equal to 1 if the equity ratio is above the first quartile. High 2010 capital is a dummy equal to 1 if the 2010 equity ratio is above median. High cash is a dummy equal to 1 if the 2010 cash ratio is above median. High interbank is a dummy equal to 1 if the bank's net creditor position on the interbank market, which proxies for ability to easily increase net interbank borrowing, is above median. Controls include the sum of shares, the bank's assets, equity ratio, a dummy indicating whether the bank is state-owned and the length of the bank-firm relationship. The variables used in columns (3) and (8) are available only since 2010. In column (7) the sample is restricted to 2010-2014. All regressions are estimated on the sample of firms with multiple credit relationships. Standard errors are clustered at the region×bank level. ***, ** and * indicate significance at the 1%, 5% and 10% levels, respectively.

Table 4: Crowding out at different scales

	Credit growth					
	(1)	(2)	(3)	(4)	(5)	(6)
Bank-level exposure	-0.686** (0.324)			0.368 (0.396)	0.483 (0.624)	
Bank×region-level exposure		-1.021*** (0.236)		-1.210*** (0.361)	0.0456 (0.500)	-1.041*** (0.279)
Branch-level exposure			-0.978** (0.484)		-1.308** (0.611)	
Bank×region-level indirect exposure						-0.158 (0.468)
Controls	✓	✓	✓	✓	✓	✓
Firm×Time FE	✓	✓	✓	✓	✓	✓
Bank×Time FE	–	✓	–	–	–	–
Bank×Region×Time FE	–	–	✓	–	–	–
Observations	12,147,895	11,301,553	9,729,656	12,094,234	12,496,413	12,041,821
R-squared	0.52	0.54	0.49	0.52	0.46	0.52

Note: This table examines the crowding out effect of local government debt on corporate credit at various levels. The outcome variable is the mid-point growth rate of credit granted to firm f by bank b . When the shock is defined at the bank branch level, the outcome variable is the mid-point growth rate of credit granted to firm f by bank b 's branch o . The main independent variable is exposure to local government debt demand shocks measured either at the bank×time level, at the bank×region×time level, or at the bank branch×time level. In all cases, exposure is defined as a shift-share with municipality-level local government debt shocks as shifters weighted by the share of each municipality within banks' (or the branch's) loan portfolio in the preceding period, as in (1). Bank×region-level indirect exposure is defined for bank b in region r as the exposure of bank b leaving region r out. Controls include the sum of shares, the bank's assets, equity ratio, a dummy indicating whether the bank is state-owned and the length of the bank-firm relationship. All regressions are estimated on the sample of firms with multiple credit relationships. In column (2), the sample is restricted to banks that are present in multiple regions, defined as banks with less than 95% of observations in a single region. In column (3), the sample is restricted to banks with more than 2 branches in the region of interest. Standard errors are clustered at the region×bank level, except for the specification with the branch-level shock (3) where clustering is at the branch level. ***, ** and * indicate significance at the 1%, 5% and 10% levels, respectively.

Table 5: Severity of crowding out by firms' characteristics

	Credit growth					
	(1)	(2)	(3)	(4)	(5)	(6)
Bank Exposure	-1.335*** (0.283)	-1.826*** (0.364)	-1.968*** (0.378)	-1.968*** (0.378)	-1.792*** (0.325)	-1.090*** (0.268)
Large×Bank Exposure	1.989*** (0.474)					
SME×Bank Exposure		1.270*** (0.320)				
Large×Bank Exposure		2.480*** (0.525)				
Rated×Bank Exposure			1.695*** (0.334)			
Rated safe×Bank Exposure				1.662*** (0.346)		
Rated risky×Bank Exposure				1.957*** (0.400)		
Strategic firm×Bank Exposure					1.946*** (0.356)	
Public procurement×Bank Exposure						-0.163 (0.359)
Controls×Firm char.	✓	✓	✓	✓	✓	✓
Firm×Time FE	✓	✓	✓	✓	✓	✓
Firm char.×Time FE	✓	✓	✓	✓	✓	✓
Observations	12,357,133	12,365,482	12,365,482	12,365,482	12,177,423	12,365,482
R-squared	0.52	0.52	0.52	0.52	0.54	0.52

Note: This table examines the severity of the crowding out effect by firms' characteristics. It reports the results of estimating specification (2) where all independent variables are interacted with a dummy for the firm characteristic. The outcome variable is the mid-point growth rate of credit granted to firm f by bank b . The main independent variable is exposure to local government debt demand shocks defined at the bank×region×time level as the sum of municipality-level local government debt growth, weighted by exposure shares equal to the bank's local government credit in each municipality as a fraction of bank×region-level total credit (equation (1)). In column (1), Large is a dummy equal to 1 if the firm has > 250 employees. In column (2), bank exposure is interacted with a variable equal to 0 if the firm has ≤ 10 employees, 1 if the firm has 11-250 employees and 2 if the firm has > 250 employees (the size categories appearing in the credit registry). In column (3), Rated is a dummy equal to 1 if the firm has a Banque de France rating. In column (4), bank exposure is interacted with a variable equal to 0 if the firm is not rated, 1 if the firm is rated as safe, 2 if the firm is rated as risky (rating threshold equal to 5). In column (5), Strategic firm is a dummy equal to 1 if the share of the firm in the bank×region's portfolio is above median. In column (6), Public Procurement is a dummy equal to 1 if the firm belongs to industries where more than 5% of industry revenues are accounted for by government contracts. 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 (data from *Observatoire économique de la commande publique*). Controls include the sum of shares, the bank's assets, equity ratio, a dummy indicating whether the bank is state-owned and the length of the bank-firm relationship. All regressions are estimated on the sample of firms with multiple credit relationships. Standard errors are clustered at the region×bank level. ***, ** and * indicate significance at the 1%, 5% and 10% levels, respectively.

Table 6: Firm-level real effects

	Effect of exposure to local government debt shocks			Credit-to-inputs sensitivities	
	gr(credit)	gr(capital)	gr(wage bill)	gr(capital)	gr(wage bill)
	RF (1)	RF (2)	RF (3)	IV (4)	IV (5)
Firm Exposure	-0.577*** (0.124)	-0.232*** (0.087)	-0.063** (0.030)		
gr(credit)				0.376*** (0.142)	0.111** (0.053)
Controls	✓	✓	✓	✓	✓
Municipality×Time FE	✓	✓	✓	✓	✓
Industry×Time FE	✓	✓	✓	✓	✓
Main bank×Time FE	✓	✓	✓	✓	✓
Observations	1,134,323	1,093,439	1,081,736	1,093,439	1,081,736
R-squared	0.88	0.12	0.075	–	–
F stat.	–	–	–	26.4	20.1

Note: This table examines the crowding out effect of local government debt on corporate credit, investment and employment. It reports the results of estimating specification (5). The outcome variable is the firm-level mid-point growth rate of credit, the growth rate of fixed assets and the growth rate of the total wage bill. The main independent variable is firm exposure to crowding out, defined in (6) as the firm-level average of banks' exposure to local government debt shocks weighted by the share of each bank in the firm's total credit. Columns (4) and (5) show the credit-to-input sensitivities, obtained by instrumenting firm-level credit growth by *FirmExposure*. Controls include the firm-level weighted average of the bank-specific controls included in Table 2, the firms' assets, leverage, ROA, and the estimate of the firm-level credit demand shock. Standard errors are clustered at the region×main bank level. ***, ** and * indicate significance at the 1%, 5% and 10% levels, respectively.

Table 7: Firm-level real effects: heterogeneity

Panel A: Credit

<i>Interaction with D_{ft}</i>	gr(credit)					
	Large (1)	High Tang/A (2)	High RZ (3)	High WC/Sales (4)	High Y/K (5)	High Y/L (6)
Firm Exposure	-0.725*** (0.155)	-0.619*** (0.142)	-0.537*** (0.136)	-0.530*** (0.157)	-0.627*** (0.120)	-0.662*** (0.155)
Firm Exposure $\times D_{ft}$	0.274* (0.167)	0.197 (0.150)	-0.159 (0.193)	-0.063 (0.161)	0.127 (0.160)	0.157 (0.159)
Controls $\times D_{ft}$	✓	✓	✓	✓	✓	✓
FE $\times D_{ft}$	✓	✓	✓	✓	✓	✓
Observations	1,125,558	1,124,393	1,123,887	1,119,122	1,104,418	1,084,341
R-squared	0.88	0.88	0.88	0.88	0.88	0.88

Panel B: Investment

<i>Interaction with D_{ft}</i>	gr(capital)									
	Large		High Tang/A		High RZ		High WC/Sales		High Y/K	
	RF (1)	IV (2)	RF (3)	IV (4)	RF (5)	IV (6)	RF (7)	IV (8)	RF (9)	IV (10)
Firm Exposure	-0.499*** (0.143)		-0.326*** (0.102)		-0.118 (0.099)		-0.202 (0.158)		-0.140 (0.094)	
Firm Exposure $\times D_{ft}$	0.462*** (0.174)		0.436** (0.181)		-0.464** (0.189)		-0.035 (0.178)		-0.218 (0.171)	
gr(credit)	0.633*** (0.189)		0.485*** (0.163)		0.210 (0.170)		0.326 (0.256)		0.221 (0.139)	
gr(credit) $\times D_{ft}$	-0.555* (0.297)		-0.742* (0.424)		0.570* (0.322)		0.057 (0.293)		0.405 (0.307)	
Controls $\times D_{ft}$	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
FE $\times D_{ft}$	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Observations	1,084,703	1,084,703	1,083,554	1,083,554	1,082,965	1,082,965	1,081,923	1,081,923	1,083,356	1,083,356
R-squared	0.14	0.060	0.14	0.040	0.14	0.056	0.14	0.078	0.15	0.051

Panel C: Employment

<i>Interaction with D_{ft}</i>	gr(wage bill)									
	Large		High Tang/A		High RZ		High WC/Sales		High Y/L	
	RF (1)	IV (2)	RF (3)	IV (4)	RF (5)	IV (6)	RF (7)	IV (8)	RF (9)	IV (10)
Firm Exposure	-0.055 (0.048)		-0.069* (0.036)		-0.043 (0.033)		0.062 (0.062)		-0.003 (0.042)	
Firm Exposure $\times D_{ft}$	-0.012 (0.062)		0.030 (0.069)		-0.112 (0.088)		-0.168** (0.070)		-0.130** (0.063)	
gr(credit)	0.071 (0.061)		0.111* (0.061)		0.083 (0.064)		-0.111 (0.118)		0.004 (0.065)	
gr(credit) $\times D_{ft}$	0.095 (0.128)		-0.012 (0.165)		0.135 (0.143)		0.292** (0.140)		0.263* (0.137)	
Controls $\times D_{ft}$	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
FE $\times D_{ft}$	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Observations	1,073,098	1,073,098	1,071,835	1,071,835	1,071,193	1,071,193	1,070,932	1,070,932	1,072,364	1,072,364
R-squared	0.096	-0.016	0.093	0.0041	0.093	-0.010	0.092	-0.052	0.10	-0.076

Note: This table examines the crowding out effect of local government debt on corporate credit, investment and employment by firms' characteristics. It reports the results of estimating specification (5) where the independent variable is interacted with dummies for firms' characteristics. The outcome variable is the firm-level mid-point growth rate of credit, the growth rate of fixed assets and the growth rate of the total wage bill. The main independent variable is firm exposure to crowding out, defined in (6) as the firm-level average of banks' exposure to local government debt shocks weighted by the share of each bank in the firm's total credit. The columns labeled IV show the credit-to-input sensitivities, obtained by instrumenting firm-level credit growth by *FirmExposure*. Large is a dummy equal to 1 is the firms' assets are above median. High Tang/A is a dummy equal to 1 if the industry tangibles ratio is in the upper quartile. High RZ is a dummy equal to 1 if the industry Rajan-Zingales index is in the upper quartile. High WC/Sales is a dummy equal to 1 if the firms' working capital-over-sales ratio is above the first quartile. High Y/K (Y/L) is a dummy equal to 1 if Y/K (Y/L) is above median. Controls include the firm-level weighted average of the bank-specific controls included in Table 2, the firms' assets, leverage, ROA, and the estimate of the firm-level credit demand shock. Standard errors are clustered at the region \times main bank level. FE are municipality \times time, industry \times time, and main bank \times time fixed effects. All controls and fixed effects are interacted with the D_{ft} dummy. ***, ** and * indicate significance at the 1%, 5% and 10% levels, respectively.

Table 8: Effect on firm-level wedges

Panel A: Full sample

	gr(credit) (1)	Capital wedge $\Delta\tau_{ft}^K$ (2)	Labor wedge $\Delta\tau_{ft}^L$ (3)	Combined wedge $\Delta\tau_{ft}$ (4)
Firm Exposure	-0.577*** (0.124)	0.168* (0.098)	0.070* (0.037)	0.090** (0.044)
Controls	✓	✓	✓	✓
Municipality×Time FE	✓	✓	✓	✓
Industry×Time FE	✓	✓	✓	✓
Main bank×Time FE	✓	✓	✓	✓
Observations	1,134,323	1,082,517	1,059,756	1,049,164
R-squared	0.88	0.11	0.088	0.11

Panel B: Sample splitted by ex-ante wedge $\tau_{f,t-1}$

	gr(credit)		Capital wedge $\Delta\tau_{ft}^K$		Labor wedge $\Delta\tau_{ft}^L$		Combined wedge $\Delta\tau_{ft}$	
	Low (1)	High (2)	Low (3)	High (4)	Low (5)	High (6)	Low (7)	High (8)
Firm Exposure	-0.578*** (0.121)	-0.488** (0.227)	0.133 (0.109)	0.491* (0.273)	0.075* (0.042)	0.115 (0.076)	0.058 (0.050)	0.235** (0.107)
Controls	✓	✓	✓	✓	✓	✓	✓	✓
Municipality×Time FE	✓	✓	✓	✓	✓	✓	✓	✓
Industry×Time FE	✓	✓	✓	✓	✓	✓	✓	✓
Main bank×Time FE	✓	✓	✓	✓	✓	✓	✓	✓
Observations	815,412	248,346	808,095	241,033	800,395	243,856	803,122	241,326
R-squared	0.86	0.91	0.13	0.15	0.098	0.11	0.12	0.13

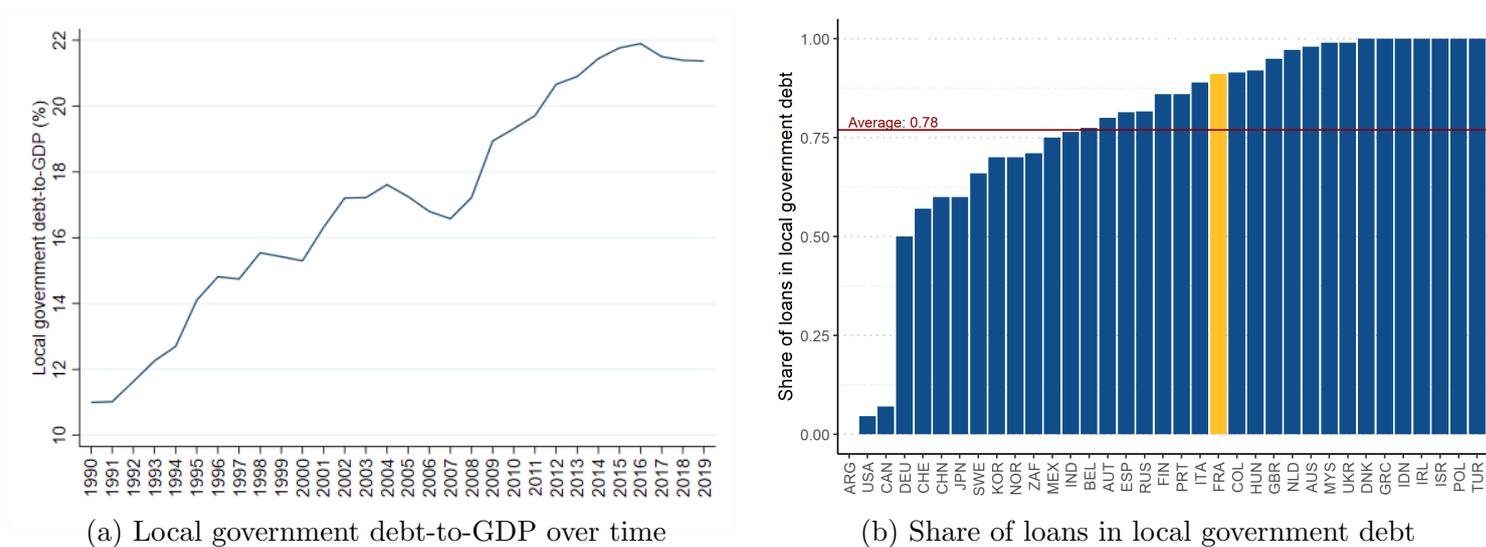
Note: This table examines the crowding out effect of local government debt on corporate credit and input wedges. It reports the results of estimating specification (5). The outcome variables are the firm-level growth rate of credit, and the change in the capital wedge, the labor wedge and the combined wedge, as defined in the main text. Details on the construction of wedges can be found in Appendix F. The main independent variable is firm exposure to crowding out, defined in (6) as the firm-level average of banks' exposure to local government debt shocks weighted by the share of each bank in the firm's total credit. In the second panel, the sample is splitted along a dummy equal to 1 if the ex-ante combined wedge is in the upper quartile. Controls include the firm-level weighted average of the bank-specific controls included in Table 2, the firms' assets, leverage, ROA, and the estimate of the firm-level credit demand shock. Standard errors are clustered at the region×main bank level. ***, ** and * indicate significance at the 1%, 5% and 10% levels, respectively.

Table 9: Aggregate effects

	Multiplier
<hr/>	
Aggregate credit & inputs	
Corporate credit	-0.55
Capital	-0.30
Labor	-0.04
Aggregate output	
Input usage channel (A)	-0.18
TFP channel (B)	-0.12
Output (A+B)	-0.30

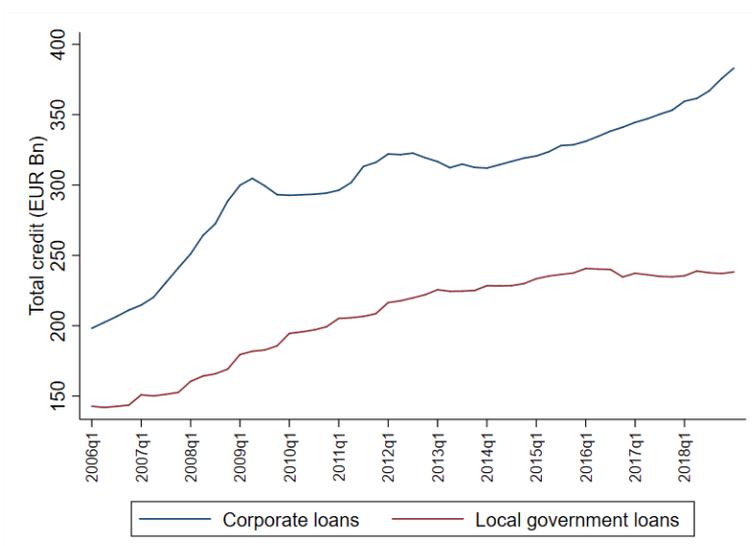
Note: This table reports the effects of crowding out on aggregate variables. The reported quantities are the multipliers, defined as the euro change in the quantity of interest with respect to the no-crowding-out counterfactual, per euro of local government loans. Line 1 is the aggregate corporate credit loss. Line 2 is the loss in the stock of capital (fixed assets). Line 3 is the total labor compensation loss. Line 4 is the output loss due to a change in input usage. Line 5 is the output loss due to a change in allocative efficiency. Line 6 is the sum and yields the total output loss. The reported multipliers are the averages of yearly multipliers.

Figure 1: Local government debt in large developed and developing economies



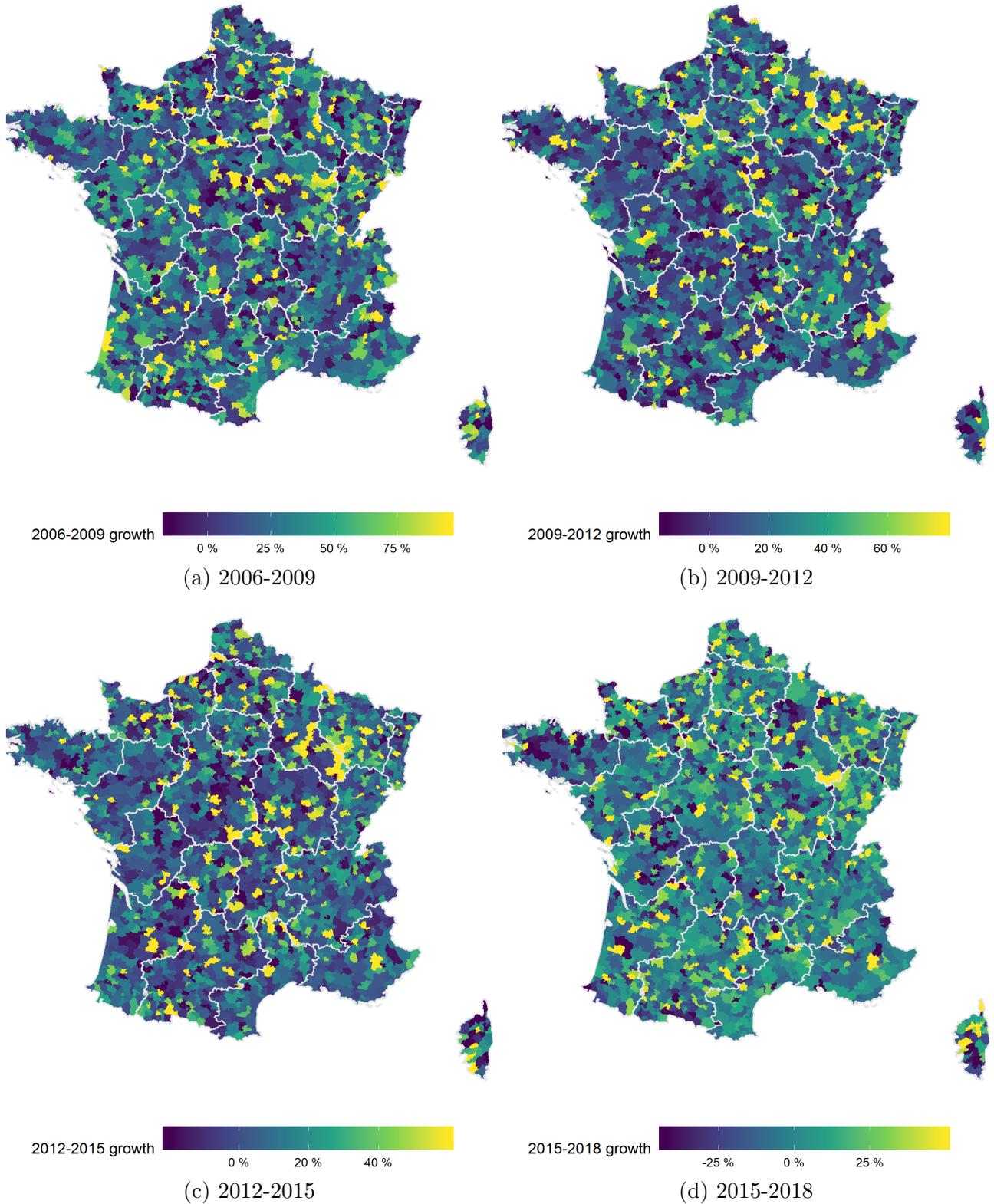
Note: Subfigure (a) shows the average local government debt-to-GDP ratio over time. Subfigure (b) shows the share of loans in local government debt in 2016. Sample of countries with government debt higher than \$75bn in 2016. Data from OECD/UCLG World Observatory on Subnational Government Finance and Investment and IMF Government Finance Statistics. See Appendix G for details on sources.

Figure 2: Aggregate credit to corporations and local governments in France



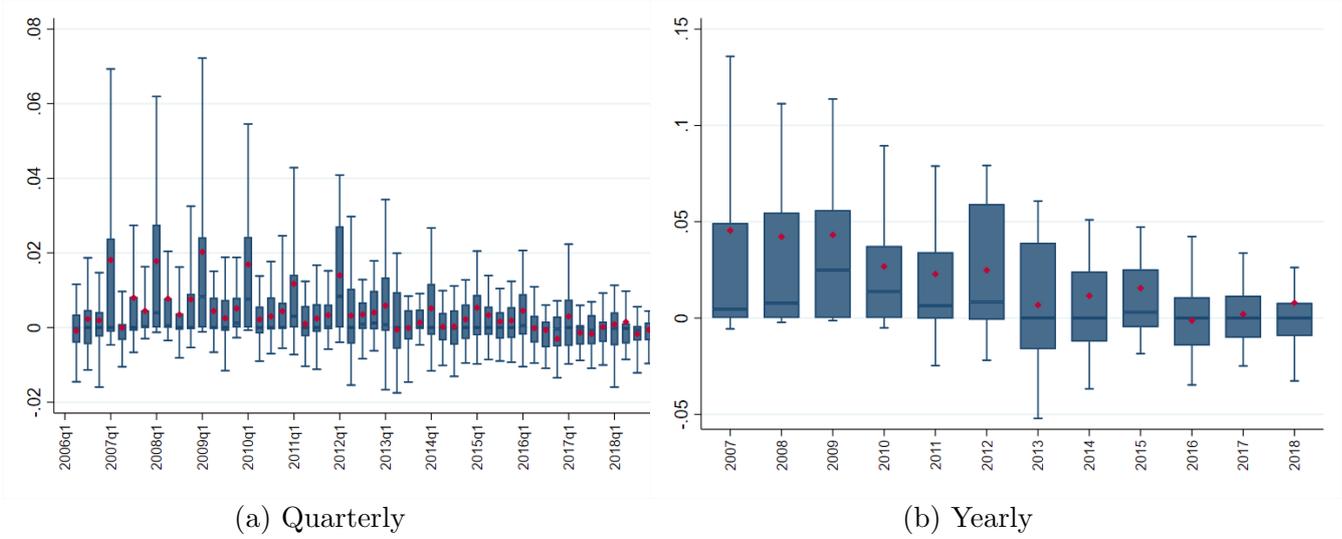
Note: This figure plots the aggregate time series obtained from the Banque de France credit registry. See details on data filtering in Section 2 and Appendix G.

Figure 3: Growth rate of local government loans by municipality



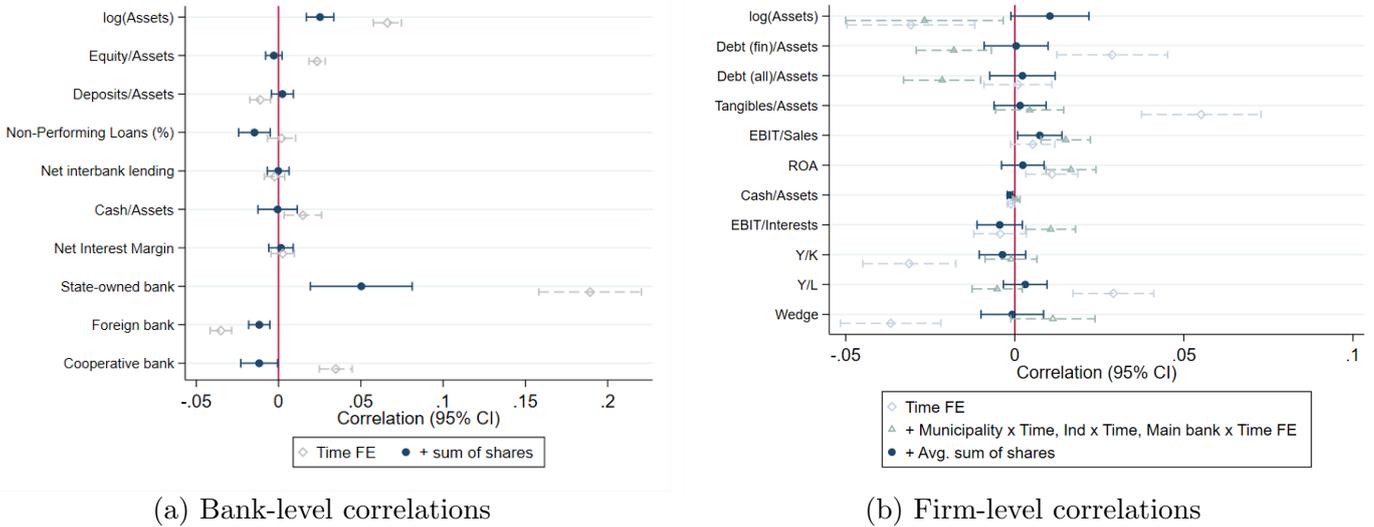
Note: These maps depict the growth rate of bank lending to local government entities across municipalities for four equal subperiods. The more toward bright yellow (dark blue), the higher (lower) the growth rate of local government loans. Regional boundaries appear in light gray.

Figure 4: Variation in local government debt dynamics across banks \times regions



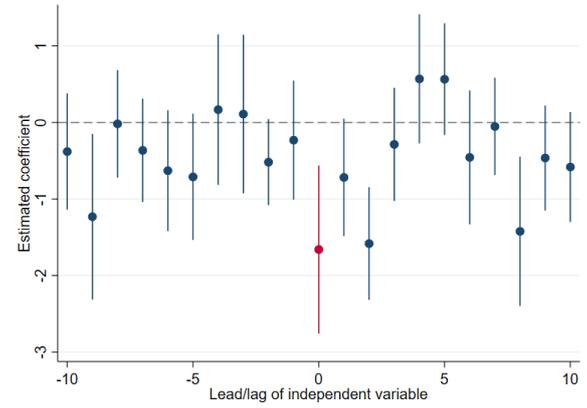
Note: This figure shows the distribution of the bank \times region-level increase in local government lending (change in local government loans normalized by lagged loan portfolio) by time period. The bars indicate the median and the interquartile range. The whiskers indicate the 10th and 90th percentiles. The red dot indicates the mean.

Figure 5: Correlation between exposure to local government debt demand shocks and pre-determined characteristics



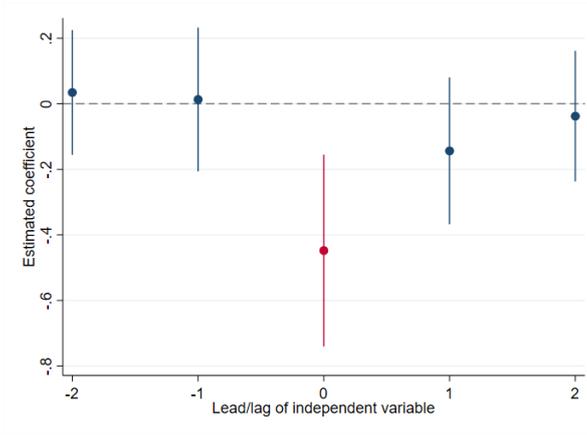
Note: Panel (a) shows the correlation between bank exposure to local government debt demand and bank characteristics measured at $t - 1$. Bank exposure is defined at the bank \times region \times time level as the sum of municipality-level local government debt growth, weighted by exposure shares equal to the bank's local government credit in each municipality as a fraction of bank \times region-level total credit (equation (1)). Regressions are weighted by lagged bank \times region corporate credit volume (approximately equal to the weight of each bank \times region in the firm \times bank-level data). All regressions include time fixed effect. “+ sum of shares” includes the sum of municipality-level exposure shares $\omega_{br,t-1}^{gov}$ as a control, as recommended by Borusyak, Hull, and Jaravel (2021). Standard errors are clustered at the bank \times region level. Panel (b) shows the correlation between firm exposure to crowding out and firm characteristics measured at $t - 1$. Firm exposure is defined in (6) as the firm-level average of banks' exposure to local government debt shocks, weighted by the share of each bank in the firm's total credit. All regressions include time fixed effect. “+ Municipality \times Time, Ind \times Time, Main bank \times Time FE” includes the fixed effects of my baseline specification. “+ Avg. sum of shares” includes the firm-level average of $\omega_{br,t-1}^{gov}$ as a control. Standard errors are clustered at the main bank \times region level. All variables are normalized. The dot is the point estimate and the bar is the 95% confidence interval.

Figure 6: Pre-trends for firm \times bank-level effect on credit growth (quarterly frequency)

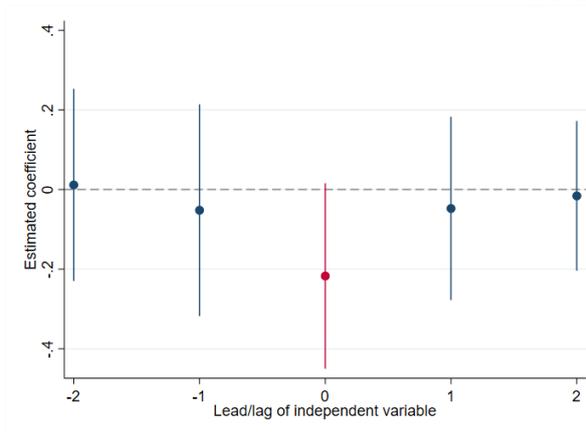


Note: This figure reports the point estimate and 95% confidence interval obtained estimating specification (2), including 10 leads and lags of bank exposure to local government debt demand shocks (defined in (1)). The outcome variable is the mid-point growth rate of credit granted to firm f by bank b . Controls include the sum of shares, the bank's assets, equity ratio, a dummy indicating whether the bank is state-owned and the length of the bank-firm relationship. I include leads and lags of the sum of shares. The sample is firms with multiple credit relationships. Standard errors are clustered at the region \times bank level. The coefficients are identified under the assumption that the treatment effect before -10 quarters and after 10 quarters is zero.

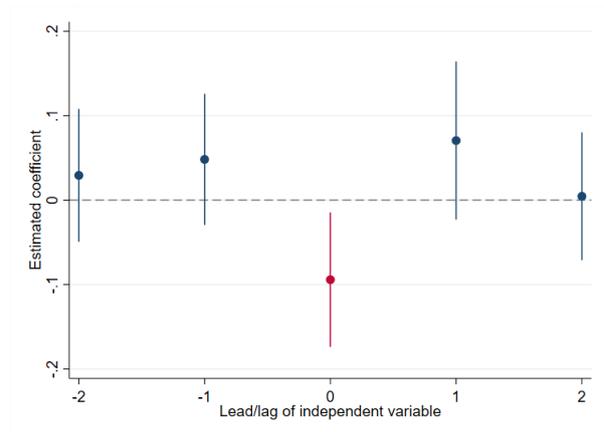
Figure 7: Pre-trends for firm-level effects (yearly frequency)



(a) gr(credit)



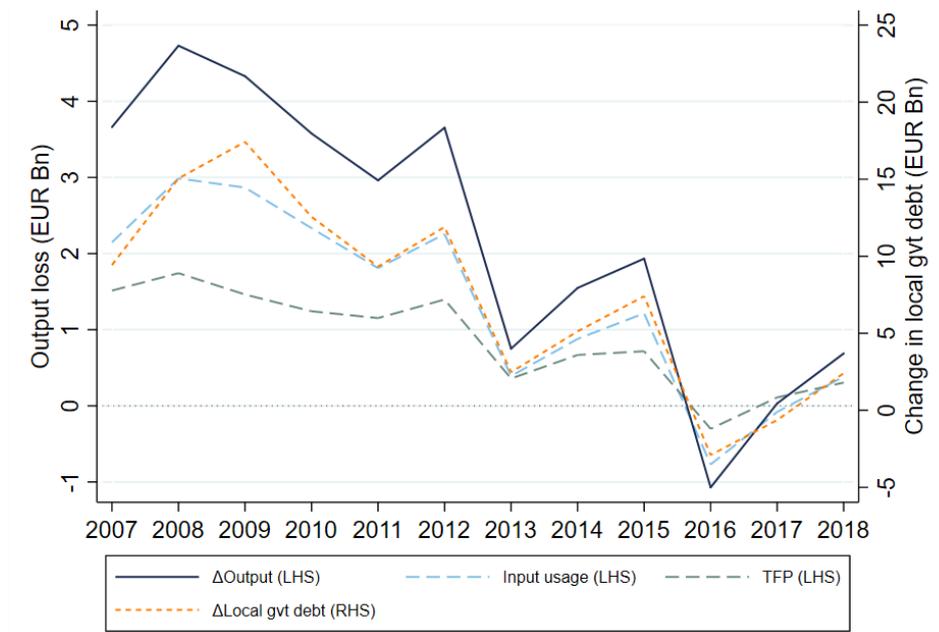
(b) gr(capital)



(c) gr(wage bill)

Note: This figure reports the point estimate and 95% confidence interval obtained estimating specification (5), including 2 leads and lags of firm exposure to crowding out (defined in (6)). The outcome variable is the firm-level mid-point growth rate of credit, the growth rate of fixed assets and the growth rate of the total wage bill. Controls include the firm-level weighted average of the bank-specific controls included in Table 2, the firms' assets, leverage, ROA, and the estimate of the firm-level credit demand shock. I include leads and lags of the firm-level weighted average of the sum of shares. Standard errors are clustered at the region \times main bank level. The coefficients are identified under the assumption that the treatment effect before -2 years and after 2 years is zero.

Figure 8: Aggregate effects



Note: This figure plots the time series of the aggregate output loss. The left-side scale measures the euro output loss. The right-side scale measures the euro change in local government loans. The left-right ratio is 20%. Δ Output loss refers to the total output loss. Input usage refers to the output loss through the input usage channel. TFP refers to the output loss through the aggregate total factor productivity channel.

APPENDIX

The Crowding Out Effect of Local Government Debt: Micro- and Macro-Estimates

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A Additional tables and figures

Table A.1: Using the near-failure of Dexia as a natural experiment

Panel A: Firm \times bank-level effect

	First stage		Credit growth			
			Reduced form		IV	
	2013 (1)	2014 (2)	2013 (3)	2014 (4)	2013 (5)	2014 (6)
Dexia Exposure	1.287*** (0.428)	1.303*** (0.434)	-0.604*** (0.163)	-0.553*** (0.155)		
Change in local govt. loans					-0.481** (0.208)	-0.440** (0.195)
Controls	✓	✓	✓	✓	✓	✓
Firm \times Time FE	✓	✓	✓	✓	✓	✓
Observations	520,503	520,503	459,018	479,301	459,018	479,301
R-squared	0.46	0.45	0.76	0.78	0.49	0.51
F stat.					9.19	9.06

Panel B: Firm-level effect

	Reduced form					
	gr(credit)		gr(capital)		gr(wage bill)	
	2013 (1)	2014 (2)	2013 (3)	2014 (4)	2013 (5)	2014 (6)
Dexia Exposure	-0.556*** (0.140)	-0.418*** (0.147)	-0.539** (0.224)	-0.465** (0.226)	-0.103* (0.056)	-0.097 (0.065)
Controls	✓	✓	✓	✓	✓	✓
Municipality \times Time FE	✓	✓	✓	✓	✓	✓
Industry \times Time FE	✓	✓	✓	✓	✓	✓
Main bank \times Time FE	✓	✓	✓	✓	✓	✓
Observations	64,860	65,075	61,578	57,417	62,485	58,555
R-squared	0.91	0.92	0.19	0.19	0.086	0.091

Note: This table examines the crowding out effect of local government debt, using the near-failure of Dexia as a natural experiment. Panel A reports the results of estimating specification (4). The main independent variable is exposure to the local government debt demand shock triggered by the near-failure of Dexia, measured at the bank \times region level. It is equal to the average municipality-level dependence on Dexia (a dummy equal to 1 if the 2008 market share of Dexia is above median) weighted by exposure shares equal to the bank's local government credit in each municipality as a fraction of bank \times region-level total credit (equation (3)). Columns (1) and (2) show the effect of Dexia exposure on the bank \times region-level increase in local government lending from 2008 to 2013 and 2014. In columns (3)-(6) the outcome variable is the mid-point growth rate of credit granted to firm f by bank b . Columns (3) and (4) show the effect of Dexia exposure on firm \times bank-level credit growth. Columns (5) and (6) show the IV coefficient where Dexia exposure is used as an instrument for the bank \times region-level increase in local government lending. Controls include the sum of shares, the bank's assets, equity ratio, a dummy indicating whether the bank is state-owned and the length of the bank-firm relationship. All regressions are estimated on the sample of firms with multiple credit relationships. Standard errors are clustered at the region \times bank level. Panel B reports the results of the firm-level specification. The outcome variable is the firm-level growth rate of credit, fixed assets and total wage bill. The main independent variable is firm exposure to crowding out, defined as the firm-level average of banks' Dexia exposure weighted by the share of each bank in the firm's total credit. The specification is otherwise similar to (5). Controls include the firm-level weighted average of bank-specific controls, the firms' assets, leverage, ROA, and the estimate of the firm-level credit demand shock. Standard errors are clustered at the region \times main bank level. ***, ** and * indicate significance at the 1%, 5% and 10% levels, respectively.

Table A.2: Using the near-failure of Dexia as a natural experiment: placebo tests

	Within		Between		
	gr(local gvt. loans)	gr(credit)	gr(credit)	gr(capital)	gr(wage bill)
	2006-07	2006-07	2006-07	2001-07	2001-07
	(1)	(2)	(3)	(4)	(5)
Dexia Exposure (bank)	0.012 (0.090)	0.027 (0.085)			
Dexia Exposure (firm)			0.099 (0.122)	0.159 (0.252)	-0.075 (0.093)
Controls	✓	✓	✓	✓	✓
FE	✓	✓	✓	✓	✓
Observations	171,088	171,088	72,190	38,578	39,108
R-squared	0.59	0.72	0.095	0.091	0.10

Note: This table presents placebo tests for the results exploiting the near-failure of Dexia as a natural experiment presented in Table A.1. Columns (1)-(2) are placebo tests for the results of panel A of Table A.1. The main independent variable is bank \times region-level exposure to Dexia. In column (1), the dependent variable is the 2006-07 bank \times region-level increase in local government lending. In column (2), the dependent variable is the 2006-07 firm \times bank-level credit growth. Controls and fixed effects are as in panel A of Table A.1. Standard errors are clustered at the region \times bank level. Columns (3)-(5) are placebo tests for the results of panel B of Table A.1. The main independent variable is firm-level exposure to Dexia. The dependent variables are firm-level credit growth over 2006-07 (column (3)), and firm-level fixed assets growth and wage bill growth over 2001-07 (column (4) and (5)). Controls and fixed effects are as in panel B of Table A.1. Standard errors are clustered at the region \times main bank level. ***, ** and * indicate significance at the 1%, 5% and 10% levels, respectively.

Table A.3: Crowding out: asymmetric effect

<i>Sample split by:</i>	Credit growth			
	Region-level growth		Bank Exposure	
	Positive (1)	Negative (2)	Positive (3)	Negative (4)
Bank Exposure	-1.272*** (0.377)	0.021 (0.489)	-1.354*** (0.300)	0.453 (0.644)
Controls	✓	✓	✓	✓
Firm \times Time FE	✓	✓	✓	✓
Observations	8,691,342	3,674,140	11,028,383	1,337,099
R-squared	0.53	0.51	0.52	0.53

Note: This table presents the baseline crowding out coefficient, distinguishing between increases and reductions in local government debt. It reports the results of estimating specification (2) separately when local government debt rises or falls. The outcome variable is the mid-point growth rate of credit granted to firm f by bank b . The main independent variable is exposure to local government debt demand shocks defined at the bank \times region \times time level as the sum of municipality-level local government debt growth, weighted by exposure shares equal to the bank's local government credit in each municipality as a fraction of bank \times region-level total credit (equation (1)). In columns (1)-(2), I split the sample according to the sign of the regional local government debt growth rate. In columns (3)-(4), I split the sample according to the sign of within-firm maximum *BankExposure*. Column (3) investigates within-firm credit growth across banks if at least one of the banks experiences a positive local government debt demand shock, and column (4) investigates within-firm credit growth across banks experiencing negative local government debt demand shocks. Controls include the sum of shares, the bank's assets, equity ratio, a dummy indicating whether the bank is state-owned and the length of the bank-firm relationship. All regressions are estimated on the sample of firms with multiple credit relationships. Standard errors are clustered at the region \times bank level. ***, ** and * indicate significance at the 1%, 5% and 10% levels, respectively.

Table A.4: Time-series variation in baseline coefficient

	Credit growth			
	Quarterly		Yearly	
	(1)	(2)	(3)	(4)
Bank Exposure	-1.098*** (0.266)		-2.548*** (0.460)	
2006-07 \times Bank Exposure		-2.431*** (0.701)		-3.205** (1.255)
2008-09 \times Bank Exposure		-2.430*** (0.426)		-3.577*** (0.585)
2010-13 \times Bank Exposure		-0.910** (0.460)		-2.675*** (0.524)
Post 2013 \times Bank Exposure		-0.00897 (0.322)		-0.675 (0.518)
Controls	✓	✓	✓	✓
Firm \times Time FE	✓	✓	✓	✓
Observations	12,360,042	12,360,030	3,667,808	3,667,808
R-squared	0.52	0.52	0.59	0.59

Note: This table presents the baseline crowding out coefficient across different time periods. It reports the results of estimating specification (2), at the quarterly and at the yearly frequency. The outcome variable is the mid-point growth rate of credit granted to firm f by bank b . The main independent variable is exposure to local government debt demand shocks defined at the bank \times region \times time level as the sum of municipality-level local government debt growth, weighted by exposure shares equal to the bank's local government credit in each municipality as a fraction of bank \times region-level total credit (equation (1)). In even columns, this variable is interacted with 4 dummies for 4 subperiods. Controls include the sum of shares, the bank's assets, equity ratio, a dummy indicating whether the bank is state-owned and the length of the bank-firm relationship. All regressions are estimated on the main sample of firms with multiple credit relationships. Standard errors are clustered at the region \times bank level. ***, ** and * indicate significance at the 1%, 5% and 10% levels, respectively.

Table A.5: Firm \times bank-level effect on credit: yearly frequency

	Credit growth			
	Full sample		Tax-filings sample	
	RF (1)	IV (2)	RF (3)	IV (4)
Bank Exposure	-2.548*** (0.460)		-0.666** (0.294)	
Change in local govt loans ΔC_{brt}^{gov}		-1.603*** (0.346)		-0.408** (0.196)
Controls	✓	✓	✓	✓
Firm \times Time FE	✓	✓	✓	✓
Observations	3,667,808	3,662,495	1,414,858	1,413,042
R-squared	0.59	0.21	0.54	0.17
F stat.		179.0		230.7

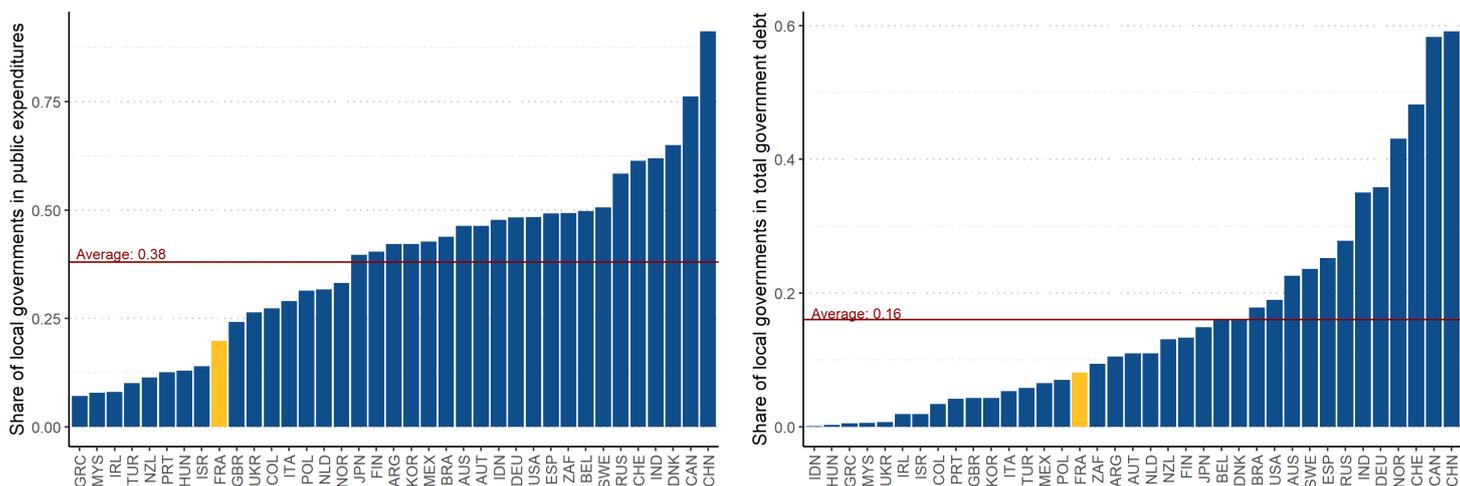
Note: This table examines the crowding out effect of local government debt on corporate credit at the bank \times firm-level, at the yearly frequency. It reports the results of estimating specification (2) at the yearly frequency. The outcome variable is the mid-point growth rate of credit granted to firm f by bank b . The main independent variable is exposure to local government debt demand shocks defined at the bank \times region \times time level as the sum of municipality-level local government debt growth, weighted by exposure shares equal to the bank's local government credit in each municipality as a fraction of bank \times region-level total credit (equation (1)). In columns labeled IV, *BankExposure* is used as an instrument for the actual increase in bank \times region-level local government lending ΔC_{brt}^{gov} . In the last two columns, the sample is restricted to firms for which tax-filings are available. Controls include the sum of shares, the bank's assets, equity ratio, a dummy indicating whether the bank is state-owned and the length of the bank-firm relationship. Standard errors are clustered at the region \times bank level. ***, ** and * indicate significance at the 1%, 5% and 10% levels, respectively.

Table A.6: Who are the high-wedge firms?

	log(Assets) (1)	log(Revenues) (2)	Tang/A (3)	RZ (4)	WC/Sales (5)	ROA (6)	D/A (7)	Rating (8)
High wedge	-0.132*** (0.012)	-0.021 (0.019)	-0.183*** (0.004)	0.003*** (0.001)	0.005*** (0.001)	0.066*** (0.002)	-0.045*** (0.007)	0.136*** (0.011)
Time FE				✓				
Industry×Time FE	✓	✓	✓		✓	✓	✓	✓
Observations	1,216,031	1,216,015	1,216,031	1,216,031	1,216,015	1,216,031	1,216,031	1,060,995
R-squared	0.14	0.10	0.42	0.10	0.21	0.13	0.19	0.053

Note: This table provides descriptive evidence on firm-level wedges. I regress the firm-level combined wedge τ_{ft} defined in the main text on various firm characteristics. RZ is the industry-level Rajan-Zingales index. Rating is the credit rating delivered by Banque de France. I invert the scale of Banque de France ratings, so that a higher value indicates lower credit risk. Standard errors are clustered at the industry×time level. ***, ** and * indicate significance at the 1%, 5% and 10% levels, respectively.

Figure A.1: Local government expenditures and debt in large developed and developing economies

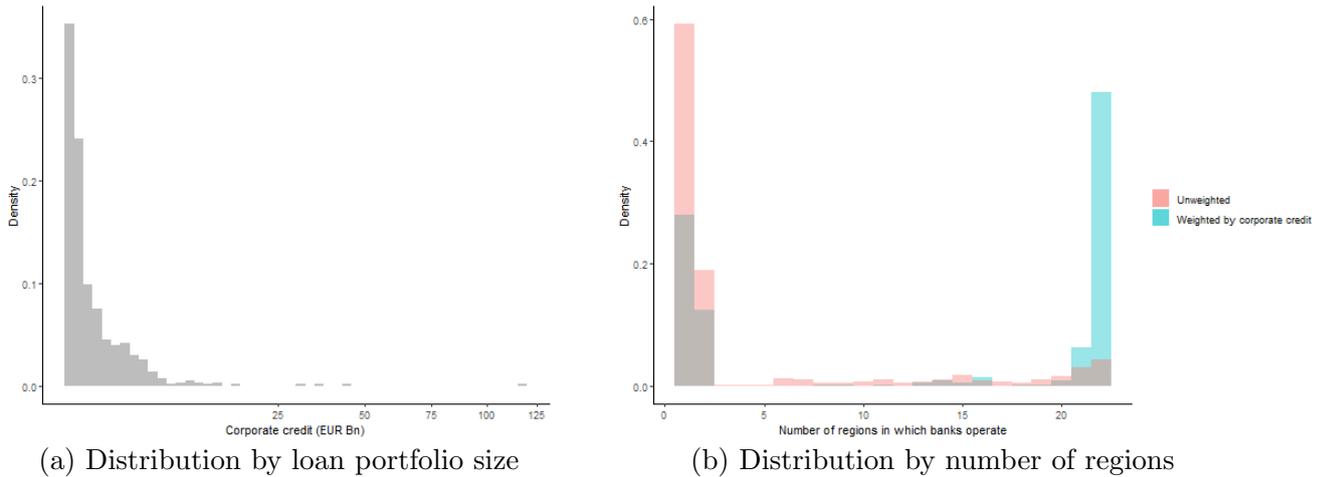


(a) Share of local governments in public expenditures

(b) Share of local governments in total government debt

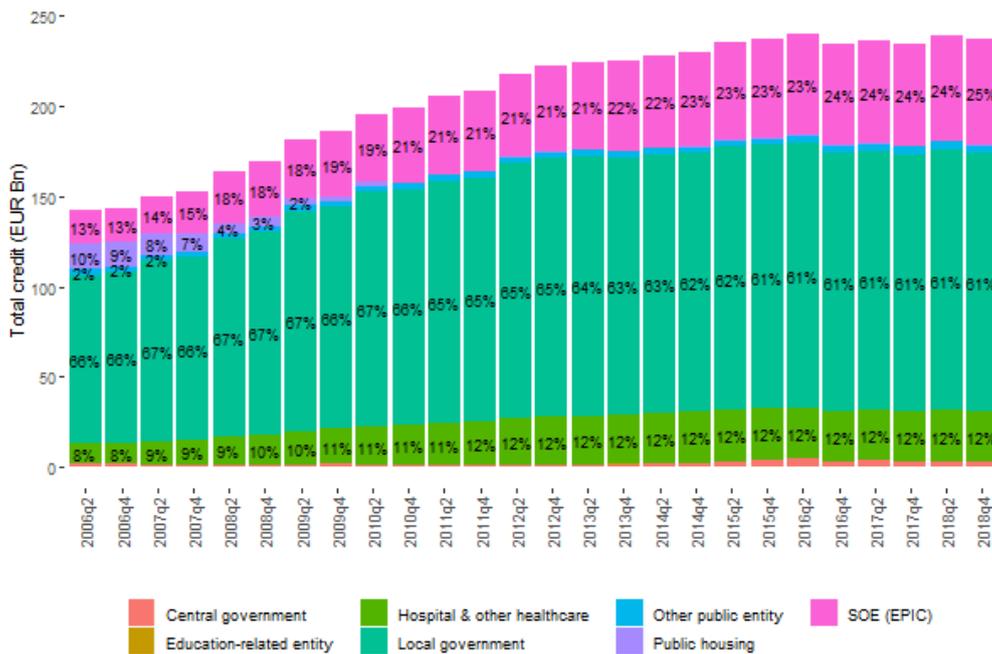
Note: Subfigure (a) shows the share of local governments in total government expenditures. Subfigure (b) shows the share of local governments in total government debt. OECD/UCLG World Observatory on Subnational Government Finance and Investment data for 2016. Sample of countries with government debt higher than \$75bn in 2016. See Appendix G for details.

Figure A.2: Population of French banks



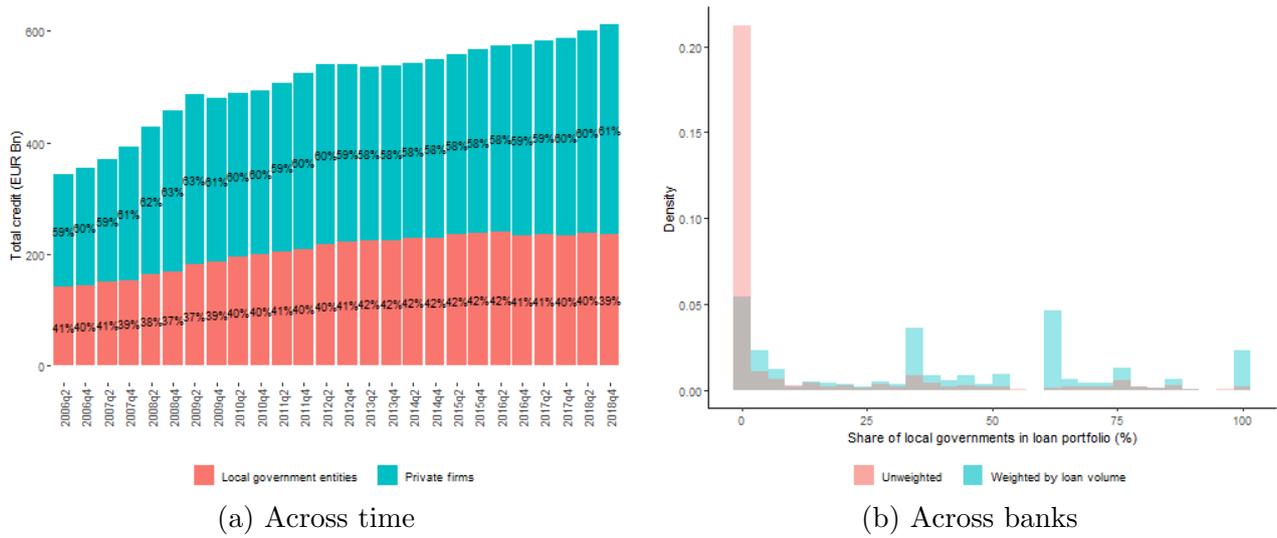
Note: Panel (a) shows the distribution of bank size, as defined by banks' corporate credit portfolios. Panel (b) shows the fraction of banks by bins defined by the number of regions in which a given bank operates (unweighted and weighted by corporate credit volume). If a single region accounts for more than 75% of the bank's total credit, I define the number of regions as 1, if two regions accounts for more than 80% of the bank's total credit, I define the number of regions as 2.

Figure A.3: Loans to local government entities by category



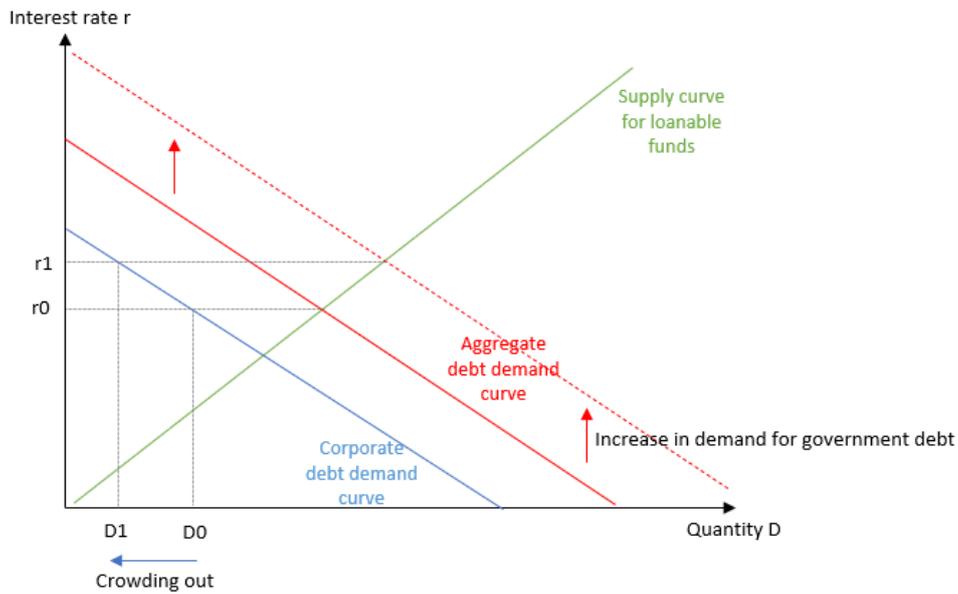
Note: This figure shows the evolution of bank lending to local government entities by type of entity. Local government refers to the four layers of local governments (regions, departements, intermunicipal cooperations, communes). SOE (EPIC) refers to state-owned public service operators. Central government refers to local entities under the direct control of the central government.

Figure A.4: Local government loans in banks' loan portfolios



Note: This figure shows the share of local government loans in bank credit in the time series and in the cross-section of banks. Banks' total loan portfolio is computed from the credit registry, i.e. excluding loans to households, after the data filtering detailed in Appendix G.

Figure A.5: Crowding out: simple supply and demand graph



Note: This figure depicts the crowding out mechanism on a simple supply and demand graph.

B Additional details and robustness checks

B.1 Cross-sectional effects on credit

Distortions in the market for local government lending and crowding out. Table B.1 shows that the crowding out coefficient does not vary along a number of proxies for political interference with banks. I first exploit the fact that political interference is more likely if local politicians are more powerful. Powerful politicians are likely better able to exert coercion on banks. I look at two type of politicians: members of parliaments (MPs, *députés*), the most prominent local political figures, and mayors, who head *communes*, the largest borrower category within local governments. I define a politician as powerful if she is influential in her own party and well-connected to other local politicians.⁷⁰ I also exploit the fact that political interference is more likely when electoral incentives are strongest. Politicians could for instance coerce banks into lending to local governments before elections to fund public investment projects. I use upcoming contested elections as a proxy for electoral incentives. I also look at whether these variables matter specifically for local banks, which may be more responsive to local political pressure, or if they matter when combined. The results in Table B.1 show that the crowding out coefficient is not driven by instances where political interference is likely potent (if anything, some of these proxies are associated to lower crowding out).

One distortion in the market for local government loans is that these loans are profitable for banks: the risk is the same as that on French sovereign bonds, and yet they earn a 150-200bps spread on these bonds on average (Delatte, Matray, and Pinardon-Touati (2019)). This likely induces a supra-optimal *level* of lending to local governments. To show that this distortion does not affect the crowding out coefficient, I exploit the fact that this spread only exists for actual local governments, and not for local public service operators (*EPIC*). I use the share of *EPIC* in the regional local government loan market as a proxy for the profitability of local government lending in the region. Interacting banks' exposure to local government debt shocks with this proxy, I find no difference in the crowding out coefficient. I also repeat the construction of *BankExposure* using only lending to *EPIC* instead of total local government loans. That is, I restrict the focus to crowding out due to increases in lending to *EPIC*. I find a similar crowding out effect.

Additional tests of identifying assumptions. Table B.2 presents further tests that support the identifying assumptions of my main results, described in the main text.

Robustness checks. Table B.3 shows the results when including additional controls (column (1)): the bank's deposit ratio, share of non-performing loans, net interbank lending position, and dummies equal to 1 if the bank is a cooperative bank or a foreign bank. Column (2) restricts the sample to banks with total loan portfolio (corporates and local governments combined) above €10 millions. Columns (3) and (4) drop banks that are never active in local government lending, globally and in the region of interest. Columns (5) drops first-quarter observations. In Figure B.1, I further test the sensitivity of my results to the definition of the sample by dropping any of the 100 largest banks, any of the 100 largest municipalities, or any year of data.

Table B.4 shows results for alternative definitions of the independent variable. In columns (1) and (2), I show the results when the shift-share instrument is constructed using 2006 shares for all periods. This avoids having exposure shares affected by previous period shocks, the downside being that the instrument loses predictive power for the most recent periods. In columns (3) and (4), I show the results when $\Delta C_{br,t-1}^{gov}$ is the standard growth rate and the shift-share is defined using weights normalized by $C_{br,t-1}^{gov}$ instead of $C_{br,t-1}^{tot}$. In columns (5) and (6), I show results using a design not relying on municipality-level variation: bank exposure is defined as the product of the region-level local government debt growth rate times the market share of the bank in the given region. All these specifications yield a negative and significant effect, with quantitative implications in line with my baseline result (except for the last specification, which yields a larger estimate).

70. Details on variables definitions are in the table notes.

Table B.1: Crowding out and distortions in the market for local government loans

	MP characteristics					Mayor characteristics					Tests with EPIC	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Bank Exposure	-1.112*** (0.336)	-1.244*** (0.309)	-1.032*** (0.284)	-0.990*** (0.281)	-1.304*** (0.287)	-1.062*** (0.290)	-1.008*** (0.259)	-1.027*** (0.281)	-1.043*** (0.266)	-0.903*** (0.262)	-1.203*** (0.321)	
×Powerful	0.276 (0.563)											
×(Powerful×Local)		0.951 (0.598)										
×Contested			-0.222 (0.595)									
×(Contested×Local)				0.110 (0.718)								
×(Contested×Powerful)					1.557** (0.688)							
×Powerful						-0.143 (0.507)						
×(Powerful×Local)							0.559 (0.536)					
×Contested								-0.404 (0.673)				
×(Contested×Local)									0.105 (0.735)			
×(Contested×Powerful)										0.358 (1.108)		
× Share EPIC											0.532 (0.443)	
Bank Exposure (EPIC)												-1.646*** (0.520)
Controls	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Controls×Dummy	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Firm×Time FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Dummy×Time FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Observations	12,365,482	12,365,482	12,365,482	12,365,482	12,365,482	12,365,482	12,365,482	12,365,482	12,365,482	12,365,482	12,365,482	12,638,611
R-squared	0.52	0.52	0.52	0.52	0.52	0.52	0.52	0.52	0.52	0.52	0.52	0.52

Note: This table shows that the crowding out coefficient does not vary along a number of proxies for distortions in local government lending. Columns (1)–(10) test heterogeneity by local politicians' characteristics. Columns (1)–(5) relate to MPs' and columns (6)–(10) to mayors' characteristics. *Powerful* is a region×time-level dummy equal to 1 if the share of powerful politicians in the region is above the sample mean. A politician is powerful if influential in her own-party and well-connected. Influential: has been in office at least three times since 1993 or has ever been a minister of the Fifth Republic. Well-connected: from the same party as the national government or the regional council or—in the case of MPs—from the same party as more than 50% of mayors in the constituency. *Contested* is a region×time-level dummy equal to 1 if the share of incumbents in contested races in the region is above the sample mean. I use the legislative election cycle for MPs and the municipal cycle for mayors. An election is contested if the office was held by the other party prior to the incumbent's election or if based on subsequent actual election results the number of votes for the incumbent differs by less than 6% from the number for her closest rival. The variable is 0 if there is no election in the next 4 quarters. These definitions follow Delatte, Matray, and Pinardon-Iouati (2019). These variables are further interacted with a dummy equal to 1 if the bank is *Local*, i.e. operating in as most two regions. Columns (9) tests if the effect is different if the share of local public service operators (EPIC) in total local government loans in the region is above median. In column (10), the independent variable is bank exposure defined as in in (1) but restricting the focus to loans to local public service operators (EPIC). All regressions are estimated on the sample of firms with multiple credit relationships. Standard errors are clustered at the region×bank level. ***, ** and * indicate significance at the 1%, 5% and 10% levels, respectively.

Table B.2: Firm×bank-level effects: Tests of identifying assumptions

	Credit growth				
	(1)	(2)	(3)	(4)	(5)
Bank Exposure	-1.156*** (0.267)	-0.969*** (0.267)	-0.931*** (0.224)	-1.021*** (0.236)	-1.090*** (0.268)
Pub. Proc. × Bank Exposure					-0.163 (0.359)
Controls	✓	✓	✓	✓	✓
Firm×Time FE	✓	✓	✓	✓	✓
Lends to local govt. (national)×Time FE	✓				
Lends to local govt. (regional)×Time FE		✓			
Bank×Region FE			✓		
Bank×Time FE				✓	
Observations	12,365,482	12,365,482	12,365,231	11,301,553	12,365,482
R-squared	0.52	0.52	0.53	0.54	0.52

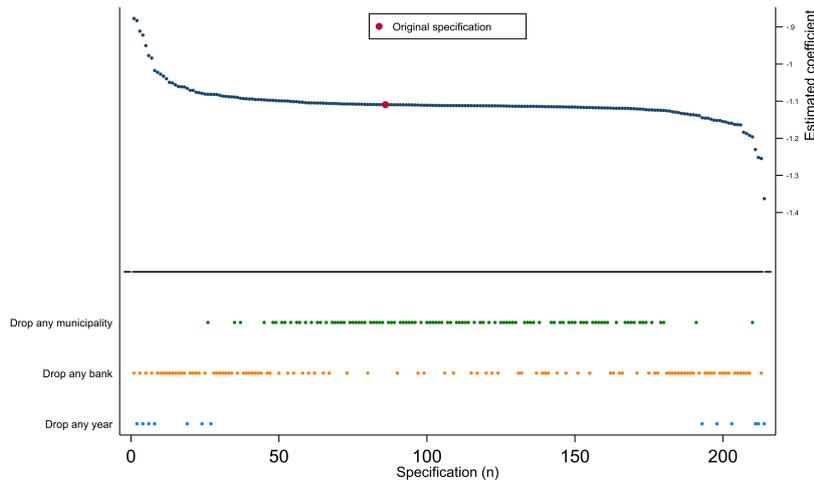
Note: This table presents tests of the assumptions that uphold a causal interpretation of the results presented in Table 2. It reports the results of estimating variations of specification (2). The outcome variable is the mid-point growth rate of credit granted to firm f by bank b . The main independent variable is exposure to local government debt demand shocks defined at the bank×region×time level as the sum of municipality-level local government debt growth, weighted by exposure shares equal to the bank's local government credit in each municipality as a fraction of bank×region-level total credit (equation (1)). In columns (1) and (2), I interact a dummy equal to 1 if the bank lends to local governments globally (respectively in the considered region) with time fixed effects. In column (4), the sample is restricted to banks that are present in multiple regions, defined as banks with less than 95% of observations in a single region. In column (5), *BankExposure* is interacted with a dummy equal to 1 if the firm's industry has more than 5% of its revenues coming from public procurement contracts. 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 (data from *Observatoire économique de la commande publique*). Controls include the sum of shares, the bank's assets, equity ratio, a dummy indicating whether the bank is state-owned and the length of the bank-firm relationship. All regressions are estimated on the sample of firms with multiple credit relationships. Standard errors are clustered at the region×bank level. ***, ** and * indicate significance at the 1%, 5% and 10% levels, respectively.

Table B.3: Firm×bank-level effects: Robustness checks (1)

	Credit growth				
	(1)	(2)	(3)	(4)	(5)
Bank Exposure	-1.243*** (0.261)	-1.205*** (0.273)	-1.089*** (0.262)	-0.972*** (0.272)	-1.142*** (0.305)
Controls	✓	✓	✓	✓	✓
Add. controls	✓				
Firm×Time FE	✓	✓	✓	✓	✓
Sample	Full	≥ 10€M	Active (all)	Active (region)	Excl. Q1
Observations	12,338,738	11,219,306	11,776,089	10,422,055	9,337,396
R-squared	0.52	0.53	0.52	0.53	0.52

Note: This table presents robustness checks of the main results presented in Table 2. Column (1) adds additional controls: the bank's deposit ratio, share of non-performing loans, net interbank lending position, and dummies equal to 1 if the bank is a cooperative bank or a foreign bank. Column (2) restricts the sample to banks with total loan portfolio (corporates and local governments combined) above €10 millions. Columns (3) and (4) drop banks that are never active in local government lending, globally and in the region of interest, respectively. Column (5) drops first-quarter observations. Controls include the sum of shares, the bank's assets, equity ratio, a dummy indicating whether the bank is state-owned and the length of the bank-firm relationship. All regressions are estimated on the sample of firms with multiple credit relationships. Standard errors are clustered at the region×bank level. ***, ** and * indicate significance at the 1%, 5% and 10% levels, respectively.

Figure B.1: Firm×bank-level effects: Robustness to dropping any bank, municipality, year



Note: This figure shows the coefficient obtained from estimating specification (2). The red dot is the baseline estimate, corresponding to column (3) in Table 2. The blue dots correspond to the estimated coefficients when dropping any of the 100 largest banks, any of the 100 largest municipalities, or any year of data. All coefficients are significant at the 5% level.

Table B.4: Firm×bank-level effects: Robustness checks (2)

	Credit growth					
	RF (1)	IV (2)	RF (3)	IV (4)	RF (5)	IV (6)
<i>BankExposure</i> (2006 shares)	-0.770*** (0.288)					
Change in local govt loans $\Delta C_{br,t}^{gov}$		-0.805*** (0.300)				
<i>BankExposure</i> (norm. $C_{br,t-1}^{gov}$)			-0.191*** (0.055)			
$\frac{C_{br,t}^{gov} - C_{br,t-1}^{gov}}{C_{br,t-1}^{gov}}$				-0.206** (0.091)		-0.362** (0.168)
$\frac{C_{br,t-1}^{gov}}{C_{rt,t-1}^{gov}} \Delta C_{rt}^{gov}$					-5.395*** (1.565)	
Controls	✓	✓	✓	✓	✓	✓
Firm×Time FE	✓	✓	✓	✓	✓	✓
Observations	12,299,103	12,299,103	12,533,478	12,437,146	12,155,593	12,063,196
R-squared	0.52	0.093	0.52	0.090	0.52	0.088
Euro for euro crowding out		0.46		0.52		0.91

Note: This table presents robustness checks of the main results presented in Table 2. It reports the results of estimating specification (2). The outcome variable is the mid-point growth rate of credit granted to firm f by bank b . The main independent variable is exposure to local government debt demand shocks measured at the bank×region×time level. Odd columns are reduced form regressions using bank exposure as an independent variable, and even columns show IV results when bank exposure is used as an instrument for the actual change in local government lending. In columns (1) and (2), *BankExposure* is defined as in (1) but I use 2006 exposure shares. In columns (3) and (4), *BankExposure* is defined as in (1) but exposure shares are normalized by $C_{br,t-1}^{gov}$. In columns (5) and (6), bank exposure is defined as the product of the region-level local government debt growth rate times the market share of the bank in the given region. In columns (4) and (6), the instrumented variable is the standard growth rate of local government lending at the bank×region-level. I assign a 0 growth rate and exposure when $C_{br,t-1}^{gov}$ is 0. Controls include the sum of shares, the bank's assets, equity ratio, a dummy indicating whether the bank is state-owned and the length of the bank-firm relationship. All regressions are estimated on the sample of firms with multiple credit relationships. Standard errors are clustered at the region×bank level. ***, ** and * indicate significance at the 1%, 5% and 10% levels, respectively.

Table B.5 shows the results for different assumptions about the covariance structure of the errors. I then show the results when the dependent variable is the log change in firm×bank credit and when the dependent variable is the change in firm×bank credit normalized by the firm’s total borrowing in the previous period.

Table B.5: Firm×bank-level effects: Robustness checks (3)

	Credit growth				
	MPGR (1)	MPGR (2)	MPGR (3)	Log change (4)	Norm. diff (5)
<i>BankExposure</i>	-1.109*** (0.139)	-1.109*** (0.306)	-1.109*** (0.334)	-0.068* (0.037)	-0.153*** (0.030)
Controls	✓	✓	✓	✓	✓
Firm×Time FE	✓	✓	✓	✓	✓
Cluster	Municipality	Municipality and bank	Region	Baseline	Baseline
Observations	12,365,482	12,365,482	12,365,482	9,772,005	11,366,841
R-squared	0.52	0.52	0.52	0.51	0.51

Note: This table presents robustness checks of the main results presented in Table 2. In columns (1)-(3), the outcome variable is the mid-point growth rate of credit granted to firm f by bank b . In column (4), the outcome variable is the log change in credit granted to firm f by bank b . In column (5), the outcome variable is the change in credit granted to firm f by bank b , normalized by firm f total borrowing in the previous period. Controls include the sum of shares, the bank’s assets, equity ratio, a dummy indicating whether the bank is state-owned and the length of the bank-firm relationship. All regressions are estimated on the sample of firms with multiple credit relationships. Standard errors are clustered at the municipality-level in column (1), two-way clustered at the municipality and bank level in (2), clustered at the region level in (3), and clustered and at the bank-region level (my baseline specification) in (4) and (5). ***, ** and * indicate significance at the 1%, 5% and 10% levels, respectively.

Addressing the bias due to firms substituting across banks. If firms can substitute across banks, model (2) is misspecified and the true data-generating process is of the form:

$$\Delta C_{fbt} = d_{ft} + \beta \text{BankExposure}_{b,rt} + \gamma \text{BankExposure}_{-b,rt} + \Phi \cdot \mathbf{X}_{b,rt} + \varepsilon_{fbt} \quad (8)$$

where $\text{BankExposure}_{-b,rt}$ captures the shocks of the other banks f borrows from. If there is substitution, β and γ have opposite signs, i.e. a shock at firm f ’s other banks induces f to borrow more from bank b . In this case the within-firm estimator overestimates the true β .

It is in general not possible to separately identify β and γ in (8) since, with firm×time fixed effects, $\text{BankExposure}_{b,r,t}$ and $\text{BankExposure}_{-b,r,t}$ are collinear. This problem is not solved by looking at firm-level effects. Propositions 6 and 7 in Appendix D establish the conditions under which β and γ are separately identified, relying on either variation in the number of banks per firm or variation in bank shares within firms.

The results are reported in Table B.6. In my preferred specification, the substitution term is defined as the shock of the main substitute of bank b (the main lender of firm f , or the second main lender if b is firm f ’s main lender). I find that the main effect β is larger in absolute value than my baseline effect by roughly 20% and statistically significant, while the coefficient on the substitution term γ is also negative. I repeat the exercise with the substitution term defined as the simple or weighted average of the shocks of the other banks and find similar patterns.⁷¹ 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. A plausible explanation is that banks interpret credit cuts at others bank as a negative signal on borrowers’ quality, inducing them to further cut credit (Darmouni, 2020). Consequently, omitting the substitution term is conservative. In the remainder of the text, I abstract from this term.

⁷¹ In this case, the coefficients are less precisely estimated. This is because within-firm collinearity of $\text{BankExposure}_{b,rt}$ and $\text{BankExposure}_{-b,rt}$ is more of an issue in these two cases.

Table B.6: Recovering β if firms substitute across banks

	Variation in n_f and ω_{bf}		Variation in n_f
	Max wgt. (1)	Value wgt. (2)	Equal wgt. (3)
β	-1.339*** (0.306)	-4.571*** (0.766)	-1.924*** (0.374)
γ	-1.035*** (0.292)		
γ		-5.486*** (0.854)	
γ			-1.195** (0.485)
Controls	✓	✓	✓
Firm×Time FE	✓	✓	✓
Observations	12,161,576	11,317,535	12,156,794
R-squared	0.52	0.48	0.52

Note: This table presents robustness checks of the main results reported in Table 2. I check that the within-firm estimator does not overstate the true effect because firms substitute across lenders. I implement the methodology described in Appendix E to disentangle the direct effect of the shock (β) from substitution across banks (γ). The estimated equation is (8). The outcome variable is the mid-point growth rate of credit granted to firm f by bank b . Columns (1) to (3) correspond to different definitions of $BankExposure_{brt}$. In column (1), $BankExposure_{brt}$ is the shock of firm f 's main lender (second main lender if bank b is the main lender). In column (2), $BankExposure_{brt}$ is the mean of the other banks' shocks weighted by bank shares. In column (3), $BankExposure_{brt}$ is the equal-weighted mean of the other banks' shocks. Controls include the sum of shares, the bank's assets, equity ratio, a dummy indicating whether the bank is state-owned and the length of the bank-firm relationship. All regressions are estimated on the sample of firms with multiple credit relationships. Standard errors are clustered at the region×bank level. ***, ** and * indicate significance at the 1%, 5% and 10% levels, respectively.

B.2 Cross-sectional effects on interest rates

The “New contracts” dataset collected by Banque de France is a representative sample accounting for approximately 75% of the total new lending amount in each quarter. I use the data on new loans to private corporations. In the baseline sample, I focus on loans with maturity longer than 12 months, exclude credit lines, exclude loans benefiting of any form of subsidy and firms that take on more than 5 different investment loans in the same year. I also present results corresponding to different filtering. The empirical specification is:

$$\dot{i}_{lft} = d_{ft} + \beta BankExposure_{brt} + \Phi \cdot \mathbf{X}_{fbrt} + \Lambda \cdot \mathbf{W}_l + \varepsilon_{lft}$$

where the additional subscript l indexes loans. Loan-level controls \mathbf{W}_l are the size of the loan, and maturity bucket×index×type of loan×time fixed effects to absorb changes in the yield curve.

This specification tests whether the same firm borrowing from different banks borrows at a higher interest rates from the relatively more exposed ones. The estimation requires that the firm takes on new loans of the same type from two different banks in the same period, which is mechanically less likely than having a same firm with ongoing relationships with two banks at the same time. In order to circumvent this issue, I estimate this equation with firm×year fixed effects instead of firm×quarter fixed effects.

The results are presented in Table B.7. Columns (1) to (3) present the results without controls, with the controls used in the baseline specification, and with the additional loan-level controls. The coefficient is positive but imprecisely estimated once we add the granular loan characteristics fixed effects. Columns (4) to (6) explore alternative definitions of the sample. Finally, column (7) adds the more restrictive firm×quarter fixed effects. Overall, the coefficients are positive, but not always significant. This is due to a combination of relatively small treatment effect and lack of statistical power due to the highly granular fixed effects structure.

Table B.7: Crowding out effect on interest rates

	Interest rate						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Bank Exposure	0.0565*	0.0560*	0.0374	0.0171	0.0341*	0.0410*	0.0237
	(0.0323)	(0.0324)	(0.0233)	(0.0249)	(0.0196)	(0.0212)	(0.0548)
Controls	–	✓	✓	✓	✓	✓	✓
Loan-level chars	–	–	✓	✓	✓	✓	✓
Firm×Year FE	✓	✓	✓	✓	✓	✓	–
Firm×Quarter FE	–	–	–	–	–	–	✓
Observations	310301	310286	309303	217577	424223	404765	189945
R-squared	0.883	0.884	0.934	0.928	0.934	0.926	0.949

Note: This table examines the crowding out effect of local government debt on interest rates at the bank×region-level. The outcome variable is the interest rate on loan l granted to firm f by bank b . The main independent variable is exposure to local government debt demand shocks defined at the bank×region×time level as the sum of municipality-level local government debt growth, weighted by exposure shares equal to the bank’s local government credit in each municipality as a fraction of bank×region-level total credit (equation (1)). Controls include the sum of shares, the bank’s assets, equity ratio, a dummy indicating whether the bank is state-owned and the length of the bank-firm relationship. Loan-level characteristics are the size of the loan, and maturity bucket×index×type of loan×time fixed effects. Column (4) restricts the sample to fixed rate loans, column (5) removes the filter on the number of loans per year, column (6) includes subsidized loans. All regressions are estimated on the sample of firms with multiple credit relationships. Standard errors are clustered at the region×bank level. ***, ** and * indicate significance at the 1%, 5% and 10% levels, respectively.

B.3 Cross-sectional effects on real variables

Effect of the firm-level average of bank-level changes in local government lending. Table B.8 presents the firm-level effects obtained from estimating (5), when $FirmExposure_{ft}$ is used as an instrument for its “realized quantity” version $\Delta C_{ft}^{gov} = \sum_b \omega_{fb,t-1} \Delta C_{brt}^{gov}$.

Table B.8: Firm-level real effects: IV results

	gr(credit)	gr(capital)	gr(wage bill)
	IV	IV	IV
	(1)	(2)	(3)
Change in local govt loans ΔC_{ft}^{gov}	-0.467***	-0.189***	-0.051**
	(0.102)	(0.069)	(0.024)
Controls	✓	✓	✓
Municipality×Time FE	✓	✓	✓
Industry×Time FE	✓	✓	✓
Main bank×Time FE	✓	✓	✓
Observations	1,134,323	1,093,439	1,081,736
R-squared	0.87	0.089	0.034
F stat.	366.9	359.4	350.5

Note: This table presents the firm-level effects obtained from estimating (5), when $FirmExposure_{ft}$ is used as an instrument for ΔC_{ft}^{gov} . The outcome variable is the firm-level mid-point growth rate of credit, the growth rate of fixed assets and the growth rate of the total wage bill. Controls include the firm-level weighted average of bank-specific controls, the firms’ assets, leverage, and ROA as well as the estimate of the firm-level credit demand shock. Standard errors are clustered at the region×main bank level. ***, ** and * indicate significance at the 1%, 5% and 10% levels, respectively.

Additional details on quantification. The quantification provided in the main text starts from the bank-level crowding out parameter (0.42). Since firms do not substitute across banks, the reduction in credit by a bank is equal to the reduction in credit for the borrowers of this bank. To obtain the effect on investment, I then use $d\bar{K}_{ft} = \eta^K \frac{\bar{K}_{ft}}{C_{ft}} d\bar{C}_{ft}$, where upper bar denotes sample mean. I proceed similarly for labor.

Another method is to use the IV coefficients in Table B.8. When bank b lends one extra euro to local governments in region r , the capital shortfall at the borrowers of bank b in region r , denoted \mathcal{F}_{br} , is: $\beta^K \sum_{f \in \mathcal{F}_{br}} \frac{\omega_{fb,t-1}}{C_{brt}^{tot}} K_{f,t-1} = \beta^K \sum_{f \in \mathcal{F}_{br}} \frac{1}{C_{brt}^{tot}} C_{fb,t-1} \frac{K_{f,t-1}}{C_{f,t-1}} \approx \beta^K \text{card}(\mathcal{F}_{br}) \frac{\bar{C}_{fb,t-1}}{C_{brt}^{tot}} \frac{\bar{K}_{f,t-1}}{C_{f,t-1}}$.

Additional tests of identifying assumptions. Table B.9 presents further tests that support the identifying assumptions of my main results. Columns (1)-(4) add additional fixed effects and column (5) looks at the differential effect of exposure to crowding out for firms in industries highly reliant on public procurement. When I interact industry and location fixed effects, I use the 38 industries *NES* classification instead of the standard 88 industries 2-digit classification to keep a sufficient number of observations.

Unobservable selection and coefficient stability (Oster, 2019): Let us define \tilde{R} and $\tilde{\beta}$ the R^2 and the coefficient of interest of the unrestricted regression (full set of fixed effects) and R_0 and β_0 their restricted counterpart (the baseline specification). Oster (2019) provides bounds on the treatment effect accounting for unobservable selection: $\beta^* = \tilde{\beta} - \delta(\beta_0 - \tilde{\beta}) \frac{R_{max} - \tilde{R}}{\tilde{R} - R_0}$. R_{max} is the maximum R^2 that a regression including all observable and unobservable variables can attain. I set R_{max} equal to 1, the most conservative value. δ is the relative importance of unobservable variables with respect to the observable controls. I obtain δ by setting $\beta^* = 0$.

Robustness checks. Table B.10 presents the results when dropping firm-level controls and when including additional firm-level controls. I show the results when dropping firms borrowing from state-owned banks, firms borrowing from banks that do not lend to local governments, or when restricting the sample to multibank firms. Finally, I show the results when firm-level averages are constructed using lagged bank shares instead of the mid-point shares that properly aggregate mid-point growth rates. Table B.11 shows the reduced-form results for employment growth defined as the growth in the number of full-time employees (column (1)), and the reduced-form and IV results when credit growth is defined using the standard growth rate instead of the mid-point growth rate (column (2)-(4)).

Table B.9: Firm-level real effects: tests of identifying assumptions

	gr(credit)				
	(1)	(2)	(3)	(4)	(5)
Firm Exposure	-0.586*** (0.121)	-0.564*** (0.135)	-0.495*** (0.126)	-0.581*** (0.126)	-0.566*** (0.126)
Pub. Proc. \times Firm Exposure					-0.208 (0.248)
Controls	✓	✓	✓	✓	✓
Baseline FE	✓	✓	✓	✓	✓
Region \times Ind \times Time FE	✓	-	-	-	-
Municipality \times Ind \times Time FE	-	✓	-	-	-
Firm FE	-	-	✓	-	-
Lagged credit growth	-	-	-	✓	-
Observations	1,134,561	1,035,812	1,090,981	1,061,522	1,129,580
R-squared	0.88	0.89	0.89	0.88	0.88
	gr(capital)				
	(1)	(2)	(3)	(4)	(5)
Firm Exposure	-0.221*** (0.085)	-0.226** (0.103)	-0.219** (0.097)	-0.228*** (0.088)	-0.214** (0.092)
Pub. Proc. \times Firm Exposure					-0.337 (0.272)
Controls	✓	✓	✓	✓	✓
Baseline FE	✓	✓	✓	✓	✓
Region \times Ind \times Time FE	✓	-	-	-	-
Municipality \times Ind \times Time FE	-	✓	-	-	-
Firm FE	-	-	✓	-	-
Lagged credit growth	-	-	-	✓	-
Observations	1,093,698	995,459	1,051,187	1,023,135	1,088,625
R-squared	0.11	0.20	0.31	0.12	0.14
	gr(wage bill)				
	(1)	(2)	(3)	(4)	(5)
Firm Exposure	-0.047* (0.028)	-0.062* (0.034)	-0.085** (0.035)	-0.043 (0.030)	-0.064* (0.033)
Pub. Proc. \times Firm Exposure					0.050 (0.104)
Controls	✓	✓	✓	✓	✓
Baseline FE	✓	✓	✓	✓	✓
Region \times Ind \times Time FE	✓	-	-	-	-
Municipality \times Ind \times Time FE	-	✓	-	-	-
Firm FE	-	-	✓	-	-
Lagged credit growth	-	-	-	✓	-
Observations	1,081,943	985,334	1,042,405	1,013,156	1,076,946
R-squared	0.060	0.15	0.32	0.075	0.090

Note: This table presents tests of the assumptions that uphold a causal interpretation of the results presented in Table 6. It reports the results of estimating variations of specification (5). The outcome variable is the firm-level mid-point growth rate of credit, the growth rate of fixed assets and the growth rate of the total wage bill. The main independent variable is firm exposure to crowding out, defined in (6) as the firm-level average of banks' exposure to local government debt shocks weighted by the share of each bank in the firm's total credit. Columns (1)-(3) include additional fixed effects. Column (4) controls for lagged credit growth. In column (5), *FirmExposure* is interacted with a dummy equal to 1 if the firm's industry has more than 5% of its revenues coming from public procurement contracts. 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 (data from *Observatoire économique de la commande publique*). Controls include the firm-level weighted average of the bank-specific controls included in Table 2, the firms' assets, leverage, ROA, and the estimate of the firm-level credit demand shock. Standard errors are clustered at the region \times main bank level. ***, ** and * indicate significance at the 1%, 5% and 10% levels, respectively.

Table B.10: Firm-level real effects: Robustness checks (1)

	gr(credit)								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Firm Exposure	-0.559*** (0.122)	-0.589*** (0.119)	-0.619*** (0.093)	-0.590*** (0.123)	-0.493*** (0.144)	-0.391** (0.185)	-0.577*** (0.076)	-0.577*** (0.086)	
Firm Exposure (alt.)									-0.533*** (0.084)
Firm controls	-	✓	✓	✓	✓	✓	✓	✓	✓
Add. firm controls	-	✓	✓	-	-	-	-	-	-
Bank rel. controls	-	-	✓	-	-	-	-	-	-
FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
Sample	Full	Full	Full	Excl. SOB	Active banks	Multibank	Full	Full	Full
Cluster	Baseline	Baseline	Baseline	Baseline	Baseline	Baseline	Municipality	Region	Baseline
Observations	1272853	1097517	1097517	1061926	1041366	451075	1134323	1134323	1134433
R-squared	0.88	0.87	0.89	0.88	0.87	0.66	0.88	0.88	0.84
	gr(capital)								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Firm Exposure	-0.208** (0.087)	-0.232*** (0.087)	-0.210** (0.082)	-0.267*** (0.089)	-0.206** (0.090)	-0.203 (0.136)	-0.232*** (0.083)	-0.232*** (0.074)	
Firm Exposure (alt.)									-0.152* (0.080)
Firm controls	-	✓	✓	✓	✓	✓	✓	✓	✓
Add. firm controls	-	✓	✓	-	-	-	-	-	-
Bank rel. controls	-	-	✓	-	-	-	-	-	-
FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
Sample	Full	Full	Full	Excl. SOB	Active banks	Multibank	Full	Full	Full
Cluster	Baseline	Baseline	Baseline	Baseline	Baseline	Baseline	Municipality	Region	Baseline
Observations	1093439	1062330	1062330	1024157	1004556	435600	1093439	1093439	1093540
R-squared	0.11	0.12	0.15	0.12	0.13	0.17	0.12	0.12	0.12
	gr(wage bill)								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Firm Exposure	-0.061** (0.030)	-0.067** (0.030)	-0.062** (0.030)	-0.065** (0.031)	-0.047 (0.031)	-0.075 (0.048)	-0.063** (0.030)	-0.063* (0.031)	
Firm Exposure (alt.)									-0.049 (0.031)
Firm controls	-	✓	✓	✓	✓	✓	✓	✓	✓
Add. firm controls	-	✓	✓	-	-	-	-	-	-
Bank rel. controls	-	-	✓	-	-	-	-	-	-
FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
Sample	Full	Full	Full	Excl. SOB	Active banks	Multibank	Full	Full	Full
Cluster	Baseline	Baseline	Baseline	Baseline	Baseline	Baseline	Municipality	Region	Baseline
Observations	1081736	1050531	1050531	1014719	996926	429518	1081736	1081736	1081808
R-squared	0.056	0.078	0.078	0.075	0.077	0.11	0.075	0.075	0.074

Note: This table presents robustness checks of the main results presented in Table 6. It reports the results of estimating variations of specification (5). The outcome variable is the firm-level mid-point growth rate of credit, the growth rate of fixed assets and the growth rate of the total wage bill. The main independent variable is firm exposure to crowding out, defined in (6) as the firm-level average of banks' exposure to local government debt shocks weighted by the share of each bank in the firm's total credit. All specification include the firm-level weighted average of bank-specific controls included in my firm×bank specification as well as the estimate of the firm-level credit demand shock. "Firm controls" indicates that I include the baseline firm-level controls: firms' assets, leverage, and ROA. "Add. firm controls" indicates that I include the interest coverage ratio, the tangibles ratio, the EBIT-to-sales ratio, and the cash ratio. "Bank rel. controls" indicates that I include the number of banks the firm borrows from, the Herfindahl index of bank shares, and dummies equal to 1 if the firm adds or drops a lending relationship in the current period. FE are municipality×time, industry×time, and main bank×time fixed effects. In column (4), I drop firms borrowing from state-owned banks. In column (5), I drop firms borrowing from banks that do not lend to local governments in the region of interest. Standard errors are clustered at the region×main bank level. ***, ** and * indicate significance at the 1%, 5% and 10% levels, respectively.

Table B.11: Firm-level real effects: Robustness checks (2)

	Effect of exposure to local government debt shocks		Credit-to-input sensitivities	
	gr(emp) RF	gr(credit) RF	gr(capital) IV	gr(wage bill) IV
	(1)	(2)	(3)	(4)
Firm Exposure	-0.065** (0.029)	-0.410*** (0.139)		
gr(credit) (std.)			0.407** (0.185)	0.148* (0.087)
Controls	✓	✓	✓	✓
Municipality×Time FE	✓	✓	✓	✓
Industry×Time FE	✓	✓	✓	✓
Main bank×Time FE	✓	✓	✓	✓
Observations	1,049,841	1,105,360	1,069,502	1,053,796
R-squared	0.050	0.62	0.049	-0.13
F stat.			10.8	7.72

Note: This table presents robustness checks of the main results presented in Table 6. It reports the results of estimating variations of specification (5). In column (1), the outcome variable is the growth rate of the number of end-of-year full-time employees. In column (2), the outcome variable is the standard growth rate of credit. In column (3), the outcome variable is the growth rate of fixed assets. In column (4), the outcome variable is the growth rate of the wage bill. The main independent variable is firm exposure to crowding out, defined in (6) as the firm-level average of banks' exposure to local government debt shocks weighted by the share of each bank in the firm's total credit. Columns (3) and (4) show the credit-to-input sensitivities, obtained by instrumenting firm-level standard credit growth by *FirmExposure*. Controls include the firm-level weighted average of the bank-specific controls included in Table 2, the firms' assets, leverage, ROA, and the estimate of the firm-level credit demand shock. Standard errors are clustered at the region×main bank level. ***, ** and * indicate significance at the 1%, 5% and 10% levels, respectively.

B.4 Aggregate input usage

This section details the computations to obtain the aggregate effects on credit, inputs, as well as the effect on output through the reduction in inputs. I detail equations in the case of capital. The methodology is the same for aggregate credit and labor. The model predicts that:

$$\Delta K_{ft} = \eta^K \theta_{ft} - \eta^K \chi (1 - \nu) \Delta C_t^{gov} - \eta^K \chi \nu \Delta C_{ft}^{gov}$$

In the case where all firms and banks have equal size, the capital shortfall is simply equal to $\mathcal{L}(K_t) = \eta^K \chi \Delta C_t^{gov}$. In the case where the distribution of firm and bank size is non-degenerate, the capital shortfall is given by:

$$\begin{aligned} \mathcal{L}(K_t) &= \eta^K \chi (1 - \nu) \Delta C_t^{gov} + \eta^K \chi \nu \sum_f \frac{K_{ft}(0)}{K_t(0)} \Delta C_{ft}^{gov} \\ &\geq \eta^K \chi \nu \sum_f \frac{K_{ft}(0)}{K_t(0)} \Delta C_{ft}^{gov} \end{aligned}$$

Lower bound. I compute the lower bound as follows. $\eta^K \chi \nu$ is obtained using $\hat{\beta}_{IV}^K = -\eta^K \chi \nu$, where $\hat{\beta}_{IV}^K$ is the coefficient of the IV regression in Table B.8. $K_{ft}(0)$ is obtained using the predicted value of the regression.

This quantity can greatly differ from $\eta^K \chi \nu \Delta C_t^{gov}$, and even have an opposite sign, when the variance of ΔC_{ft}^{gov} is high. To avoid this issue, I perform the baseline quantification using the coefficient on *FirmExposure*_{ft}, which has a lower variance. I provide the quantification using the coefficient on ΔC_{ft}^{gov} as a robustness check. To go from inputs to output, I compute the industry-level output loss using industry-specific capital shares before aggregating across industries. This yields the estimates presented in the main text.

Robustness checks. This computation depends on the joint distribution of the shock and of firm size, which may not be the result of an invariant economic mechanism but rather of luck. I also

provide the quantification of the output shortfall based on the assumption that all firms and banks are symmetric, which neutralizes this effect. To obtain the lower bounds, I multiply the estimated coefficients by the average ΔC_t^{gov} , equal to 0.0145. I obtain lower bounds equal to 0.68%, 0.27%, and 0.07% for corporate credit, capital, and labor, respectively. The output loss is then equal to 0.14%. The equivalent output multiplier is -0.28 .

The lower bound for the output multiplier can also be recovered from the back-of-the-envelope computations from the reduced-form results in Section 6. The relationship is $dY = \alpha \frac{Y}{K} dK + (1 - \alpha) \frac{Y}{L} dL$. Using sample mean values of the different variables, I obtain a multiplier equal to -0.21 . Finally, I find similar estimates using the IV specification with ΔC_{ft}^{gov} instead of $FirmExposure_{ft}$. I find a bound for the output multiplier equal to 0.17, but the standard deviation of this multiplier is high (0.2) compared to that of my baseline estimate (0.03).

Adjusted for the equilibrium effect, these lower bound computations also provide robustness checks for the total aggregate effects.

Estimation of the equilibrium effect on interbank flows. I estimate $1 - \nu$ by regressing the change in net interbank borrowing on the increase in local government lending, instrumented by $BankExposure$. The specification is:

$$\Delta B_{bt} = \alpha_t + \beta \Delta C_{b_{rt}}^{gov} + \Phi \cdot \mathbf{X}_{b_{rt}} + \varepsilon_{b_{rt}}$$

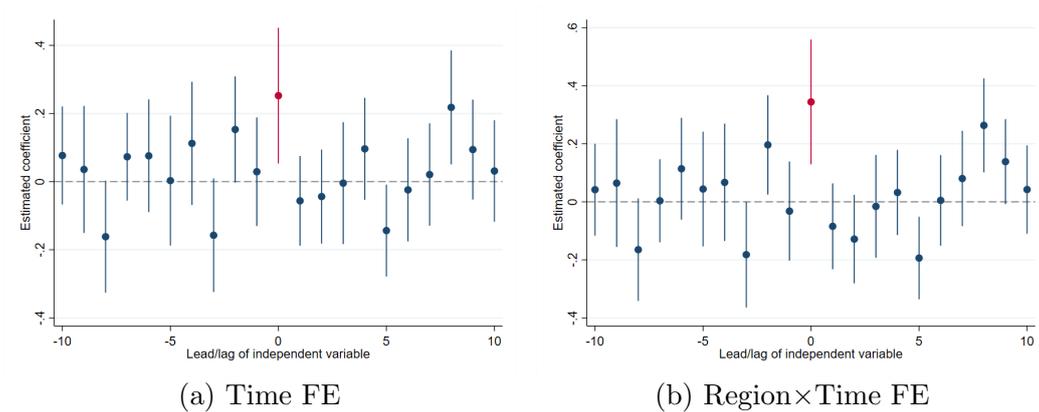
where ΔB_{bt} is the change in net interbank borrowing normalized by lagged total assets and α_t are time fixed effects. Since the outcome is at the bank level, I investigate this relationship using the increase in local government lending defined at the bank \times region level (the shock in my baseline specification) or at the bank level. I can additionally include region \times time fixed effects to only compare banks present in the same region. I include the same bank controls as in my baseline specification. Table B.12 presents the results. The main specifications are columns (4)-(6). Taking the most conservative of these coefficients, I use $\hat{\beta} = 0.17$. Figure B.2 shows pre-trending tests.

Table B.12: Effect on interbank borrowing

	Change in net interbank borrowing							
	Bank \times region-level					Bank-level		
	RF (1)	RF (2)	RF (3)	IV (4)	IV (5)	IV (6)	RF (7)	IV (8)
$BankExposure_{b_{rt}}$	0.187*** (0.058)	0.205*** (0.056)	0.250*** (0.063)					
Change in local govt loans $\Delta C_{b_{rt}}^{gov}$				0.167*** (0.053)	0.184*** (0.052)	0.224*** (0.058)		
$BankExposure_{bt}$							0.283*** (0.068)	
Change in local govt loans ΔC_{bt}^{gov}								0.278*** (0.067)
Controls		✓	✓		✓	✓	✓	✓
Time FE	✓	✓	✓	✓	✓	✓	✓	✓
Region \times Time FE			✓			✓		
Observations	191,910	191,550	191,550	191,910	191,550	191,550	17,560	17,552
R-squared	0.040	0.041	0.067				0.051	
F stat.				363.8	360.1	306.0		1867.5

Note: This table examines the effect of banks' exposure to increased demand for local government loans on interbank borrowing. The outcome variable is the change in net interbank borrowing normalized by lagged total assets. The main independent variable is exposure to local government debt demand shocks defined at the bank \times region \times time level as the sum of municipality-level local government debt growth, weighted by exposure shares equal to the bank's local government credit in each municipality as a fraction of bank \times region-level total credit (equation (1)). In columns labeled IV, $BankExposure$ is used as an instrument for the actual increase in bank \times region-level local government lending. In columns (6) and (7), the independent variable is defined at the bank \times time level. All columns include the share of local government loans in the bank portfolio as control. Other controls include the bank's assets, equity ratio, a dummy indicating whether the bank is state-owned. The regressions are weighted by the lagged corporate credit volume. ***, ** and * indicate significance at the 1%, 5% and 10% levels, respectively.

Figure B.2: Effect on interbank borrowing: pre-trending tests



Note: This figure reports the point estimate and 95% confidence interval obtained when estimating the effect of 10 leads and lags of bank exposure to increased demand for local government loans (defined in (1)) on interbank borrowing. Controls include the sum of shares, the bank's assets, equity ratio, a dummy indicating whether the bank is state-owned and the length of the bank-firm relationship. I include leads and lags of the sum of shares. The sample is firms with multiple credit relationships. Standard errors are clustered at the region \times bank level. The regressions are weighted by the lagged corporate credit volume.

With the model accounting for both interbank and intrabank frictions. The decomposition presented in the main text is exact when considering a bank \times region unit as a bank. This amounts to considering that all regions within a bank have the same value for ΔB_{brt} . As shown in the two-layer model in Appendix C.2.3, not considering within-bank transfers and making this approximation is innocuous when within-bank and across-banks frictions have a similar size, as shown in Section 5.1.⁷²

72. Otherwise, if frictions on internal capital markets are lower than interbank frictions, which is the natural assumption, omitting within-bank transfers implies that I estimate a lower bound.

C Model

C.1 Baseline model

The model contains four sectors: households that saves in the form of bank deposits; firms and local governments that borrow to finance projects; and banks that are funded via deposits and lend to firms and local governments. There is a continuum of banks of mass 1, indexed by $b \in [0, 1]$. Banking relationships enter the model through the assumption that firms and local governments are assigned to a given bank. Let us denote \mathcal{F}_b the set of firms borrowing from bank b and \mathcal{G}_b the set of local governments borrowing from bank b . \mathcal{F}_b and \mathcal{G}_b have mass 1 for all b . Imperfect capital mobility across banks enters the model through the assumption that there is an identical depositor assigned to each bank that does not arbitrage across banks. An interbank market can be accessed at a cost. All agents are price-takers.⁷³

Local governments. Local governments operate on a unit square, with $b \in [0, 1]$ indexing banks and $m \in [0, 1]$ indexing local governments borrowing from a bank. Each local government has the following demand for loans:

$$C_{mb}^{gov} = g e^{z_m} (r_b^g)^{-\epsilon^g}$$

with $\epsilon^g \geq 0$. z_m is demand shifter. Total demand for local government loans directed to bank b is given by:

$$C_b^{gov} = \int_{m \in \mathcal{G}_b} e^{z_m} (r_b^g)^{-\epsilon^g} dm$$

I define $Z_b^{gov} = \int_{m \in \mathcal{G}_b} z_m dm$ and $Z^{gov} = \int_b \int_{m \in \mathcal{G}_b} z_m dm db$.

Firms. Firms operate on a unit square, with $b \in [0, 1]$ indexing banks and $f \in [0, 1]$ indexing firms borrowing from a bank. Each firm has the following demand for loans:

$$C_{fb} = c e^{\theta_f} (r_b^c)^{-\epsilon^c}$$

with $\epsilon^c \geq 0$.⁷⁴ θ_f denotes firm-level idiosyncratic demand shocks (e.g. technological shocks). I assume that the firm-level shocks aggregate to 0 at all banks: $\int_{f \in \mathcal{F}_b} \theta_f df = 0$ for all b . This assumption does not affect crowding out.

Households. Households save in the form of deposits. The supply of deposits is given by:

$$S_b = s (r_b^s)^{\epsilon^s}$$

with $\epsilon^s \geq 0$.⁷⁵

Banks. Banks maximize the revenues from lending minus the cost of funds. Banks are funded via deposits and can borrow on the interbank market at rate i . Let B_b be net interbank borrowing. To model imperfect functioning of the interbank market, I assume that banks face a quadratic cost. The problem of the bank is:

$$\max_{\{C_b^{corp}, C_b^{gov}, S_b, B_b\}} r_b^c C_b^{corp} + r_b^g C_b^{gov} - r_b^s S_b - i B_b - \frac{\phi}{2} B_b^2$$

subject to: $C_b^{corp} + C_b^{gov} = S_b + B_b$. The equilibrium prices consistent with the first-order condition of banks are $r_b^c = r_b^g = r_b^s = r_b$ and $r_b = i + \phi B_b$.

73. Introducing monopolistic banks leaves all key results unchanged.

74. In the model where firms use credit to finance investment and maximize $k^\alpha - r^c k$, ϵ^c is $\frac{1}{1-\alpha}$.

75. If the savings function is derived from a consumption savings trade-off with CRRA preferences with parameter γ and discount factor β , we obtain $\epsilon^s = \frac{\gamma-1}{1+\beta}$, which for typical parameter values is approximately equal to 1.

Market clearing. For each bank b , demand must equal supply for C_b^{corp} , C_b^{gov} , S_b . The interbank market must clear: $\int_0^1 B_b db = 0$.

Solution. I solve the model by log-linearisation around the deterministic equilibrium (DE), characterized by $\theta_f = 0$ for all f and $z_m = 0$ for all m . I denote \hat{x} the relative change of variable x with respect to its DE value x^* . In the DE, all quantities are the same for all firms, local governments and banks. Therefore, in the DE, net interbank borrowing $B_b^* = 0$ for all b .

Solving this model, we obtain:

$$\hat{i} = \frac{\lambda Z^{gov}}{\epsilon^s + (1-\lambda)\epsilon^c + \lambda\epsilon^g}$$

$$\hat{r}_b = \frac{\lambda Z^{gov}}{\epsilon^s + (1-\lambda)\epsilon^c + \lambda\epsilon^g} + \frac{\lambda(Z_b^{gov} - Z^{gov})}{\epsilon^s + (1-\lambda)\epsilon^c + \lambda\epsilon^g + \frac{i^*}{\phi S^*}}$$

where λ is the share of local government loans in the bank loan portfolio in the DE, equal for all banks. The interbank rate, which is also the average of the bank-specific interest rates, depends on the aggregate local government debt shock. The bank-specific interest rate \hat{r}_b depends on the interbank rate and on the deviation of the bank-specific local government debt demand Z_b^{gov} from the aggregate shock Z^{gov} . The extent to which \hat{r}_b depends on Z_b^{gov} depends on ϕ .⁷⁶ When ϕ tends to 0, banks that receive a larger shock draw funds from other banks using the interbank market up to the point where interest rates are equalized across banks, so that the \hat{r}_b does not depend on Z_b^{gov} but only on Z^{gov} . Conversely, when ϕ tends to $+\infty$, banks cannot use the interbank market, and \hat{r}_b only depends on Z_b^{gov} , not on Z^{gov} .

Having solved for the interest rates, we obtain the quantities of interest using:

$$\begin{aligned} \hat{C}_{fb} &= \theta_f - \epsilon^c \hat{r}_b & \text{and} & & \hat{C}_b^{gov} &= Z_b^{gov} - \epsilon^g \hat{r}_b \\ \hat{C}^{corp} &= \theta - \epsilon^c \hat{i} & & & \hat{C}^{gov} &= Z^{gov} - \epsilon^g \hat{i} \end{aligned}$$

Aggregate and relative crowding out. I define crowding out as the effect of a demand-driven change in local government lending on corporate credit. It is the relationship between two endogenous variables (corporate and local government credit) in response to the exogenous local government credit demand shock.⁷⁷

Proposition 1 Let $\chi = \frac{\epsilon^c}{\epsilon^s + (1-\lambda)\epsilon^c}$. The aggregate crowding out relationship is given by:

$$\hat{C}^{corp} = -\chi \lambda \hat{C}^{gov}$$

χ is the aggregate crowding out parameter, which is decreasing in the relative elasticity of the supply and demand of loans $\frac{\epsilon^s}{\epsilon^c}$. It does not depend on interbank market frictions. We would obtain the same aggregate crowding out parameter if the economy was composed of a single bank.

At the bank \times firm level, we obtain:

$$\hat{C}_{fb} = \theta_f - \frac{\epsilon^c \frac{i^*}{\phi S^*}}{(\epsilon^s + (1-\lambda)\epsilon^c)(\epsilon^s + (1-\lambda)\epsilon^c + \frac{i^*}{\phi S^*})} \lambda \hat{C}^{gov} - \frac{\epsilon^c}{\epsilon^s + (1-\lambda)\epsilon^c + \frac{i^*}{\phi S^*}} \lambda \hat{C}_b^{gov}$$

Let $\nu = \frac{\epsilon^s + (1-\lambda)\epsilon^c}{\epsilon^s + (1-\lambda)\epsilon^c + \frac{i^*}{\phi S^*}}$. $\nu \in [0, 1]$ is monotonically increasing in ϕ . When $\phi \rightarrow 0$ (no interbank frictions), $\nu = 0$, and when $\phi \rightarrow +\infty$ (complete segmentation), $\nu = 1$.

76. $\frac{i^*}{S^*}$ is a model parameter that depends only on the parameters of the supply and demand functions. i^* solves $s i^{\epsilon^s} = c i^{-\epsilon^c} + g i^{-\epsilon^g}$. S^* solves $S^* = s(i^*)^{\epsilon^s}$.

77. The crowding out parameter is given by $\frac{\partial \hat{C}^{corp}}{\partial Z^{gov}}$ (at the aggregate and at the bank level). In the case where $\epsilon^g = 0$, this equals $\frac{\partial \hat{C}^{corp}}{\partial Z^{gov}}$.

Proposition 2 *The bank×firm-level crowding out relationship writes:*

$$\hat{C}_{fb} = \theta_f - \chi(1 - \nu)\lambda\hat{C}^{gov} - \chi\nu\lambda\hat{C}_b^{gov}$$

$\chi\nu$ is the relative crowding out parameter. When banks are perfectly integrated, corporate credit by bank b does not depend on the bank-specific increase in local government loans, but only on the aggregate increase. Conversely, when banks are fully segmented, corporate credit by bank b only depends on the bank-specific increase in local government loans, and not on the aggregate increase. As long as $\nu < 1$, banks not directly exposed to increased demand for local government loans lend to other banks and corporate credit also falls at these banks.

Proposition 3 *The relative crowding out parameter is equal to $\chi\nu$ and is a lower bound on the aggregate crowding out parameter χ .*

Equilibrium effect on the interbank market. The difference between the relative and the aggregate parameter is determined by the extent of the transmission of the shock across banks, which is determined by $1 - \nu$. This parameter drives the response of interbank flows to the shock.

Proposition 4 *Net interbank borrowing of bank b is given by:*

$$\frac{B_b}{S^*} = (1 - \nu)(\lambda\hat{C}_b^{gov} - \lambda\hat{C}^{gov})$$

Relationship with the empirical strategy. To link the static model with the panel setting of the main text, I consider that in each period the economy starts from the deterministic equilibrium, so that I can assimilate observed growth rates to log-deviations from the deterministic equilibrium. Therefore, firm×bank credit growth ΔC_{fb} is approximately equal to \hat{C}_{fb} . The increase in local government lending normalized by banks' total loan portfolio ΔC_b^{gov} is approximately equal to the log-deviation in local government lending multiplied by the share of local government loans in the banks' portfolio $\lambda\hat{C}_b^{gov}$. Aggregate variables are defined accordingly.

To link the model and the identification strategy of Section 4, I assume that each firm borrows from several banks and that the demand for loan directed at each bank is independent from what happens at other banks (the standard Khwaja-Mian assumption).⁷⁸ Second, I add a mean-zero bank-specific liquidity shock ξ_b that affects the budget constraint of the bank: $C_b^{corp} + C_b^{gov} = S_b + B_b + \xi_b$. Adding time-subscripts, I can re-write the bank×firm-level relationship and the bank-level increase in local government lending as:

$$\begin{aligned}\Delta C_{fbt} &= \theta_{ft} - \chi(1 - \nu)\Delta C_t^{gov} - \chi\nu\Delta C_{bt}^{gov} + \iota^c \xi_{bt} \\ \Delta C_{bt}^{gov} &= \kappa Z_{bt}^{gov} + \varkappa Z_t^{gov} + \iota^g \xi_{bt}\end{aligned}\tag{9}$$

where ι^c , ι^g , κ and \varkappa are parameters.⁷⁹ The coefficient we want to identify is $\beta = -\chi\nu$. In Section 4.1, to simplify the exposition, I repeat the preceding equation omitting the variables that are constant within each time period and the coefficients κ , ι^c and ι^g .

Estimation of the effect on net interbank borrowing. In the model, net interbank borrowing is zero for all banks in the deterministic equilibrium. Therefore $\frac{B_b}{S^*}$ is to be understood as the change in interbank borrowing normalized by the banks' total assets.⁸⁰ I denote this variable ΔB_b . Therefore, the relationship implied by the model is:

$$\Delta B_b = (1 - \nu)(\Delta C_b^{gov} - \Delta C^{gov})$$

78. The model does not require that the sets \mathcal{F}_b are disjoint.

79. $\iota^c = \frac{\lambda}{S^*(\epsilon^s + (1-\lambda)\epsilon^c + \frac{i^*}{\phi S^*})}$, $\kappa = \frac{\lambda(\epsilon^s + (1-\lambda)\epsilon^c + \frac{i^*}{\phi S^*})}{\epsilon^s + (1-\lambda)\epsilon^c + \lambda\epsilon^g + \frac{i^*}{\phi S^*}}$, $\varkappa = \frac{-\epsilon^g \frac{i^*}{\phi S^*} \lambda^2}{(\epsilon^s + (1-\lambda)\epsilon^c + \lambda\epsilon^g)(\epsilon^s + (1-\lambda)\epsilon^c + \frac{i^*}{\phi S^*})}$, and $\iota^g = \frac{\lambda\epsilon^g}{S^*(\epsilon^s + (1-\lambda)\epsilon^c + \lambda\epsilon^g + \frac{i^*}{\phi S^*})}$.

80. If we add bank equity to the model, the denominator is total assets and not total deposits.

C.2 Extensions

In extensions, I make the simplifying assumption $\epsilon^g = 0$. Hence, $\hat{C}_b^{gov} = Z_b^{gov}$ and $\hat{C}^{gov} = Z^{gov}$.

C.2.1 Introducing frictions on increasing total balance sheet size

Assume that banks can borrow and lend to each other freely on the interbank market, but that it is costly for banks to increase the size of their balance sheet. Reasons for this include: leverage constraints combined with a high cost of raising new equity, frictions due to the time to process new loans, etc. Banks now maximize:

$$\max_{\{C_b^{corp}, C_b^{gov}, S_b, B_b\}} r_b^c C_b^{corp} + r_b^g C_b^{gov} - r_b^s S_b - i B_b - \frac{\varphi}{2} (S_b + B_b)^2$$

subject to: $C_b^{corp} + C_b^{gov} = S_b + B_b + E_b$. I include a fixed equity amount per bank $E_b = E$ so that the problem makes sense in the limit $\varphi \rightarrow +\infty$. Solving for this model, we find:

$$\hat{C}_{fb} = \theta_f - \chi(1 - \nu)\lambda\hat{C}^{gov} - \chi\nu\lambda\hat{C}_b^{gov}$$

where the aggregate crowding out parameter and the interbank friction parameter are:

$$\chi = \frac{\epsilon^c}{\frac{i^* + \varphi S^*}{i^* + \varphi \epsilon^s S^*} \frac{S^*}{S^* + E^*} \epsilon^s + (1 - \lambda)\epsilon^c} \quad \nu = \frac{\frac{\varphi \epsilon^s S^*}{i^* + \varphi \epsilon^s S^*} + (1 - \lambda)\epsilon^c (S^* + E^*) \frac{\varphi}{i^* + \varphi S^*}}{1 + (1 - \lambda)\epsilon^c (S^* + E^*) \frac{\varphi}{i^* + \varphi S^*}}$$

S^* and i^* depend on φ . The aggregate crowding out parameter is now a function of φ . When $\varphi = 0$ and $E^* = 0$, we recover $\chi = \frac{\epsilon^c}{\epsilon^s + (1 - \lambda)\epsilon^c}$. When $\varphi \rightarrow +\infty$, the aggregate supply of lending is essentially fixed and determined by the amount of equity. In this case, $\chi = \frac{1}{1 - \lambda}$, i.e. the euro increase in local government loans equals the euro reduction in corporate lending.⁸¹

As before, ν is in $[0, 1]$, $\nu = 0$ when $\varphi = 0$ and $\nu = 1$ when $\varphi \rightarrow +\infty$. In the frictionless case, we do not have bank-specific crowding out. In the full segmentation case, the bank-level crowding out parameter equals its aggregate counterpart. The key insight that the cross-sectional parameter is a lower bound to the aggregate parameter remains unchanged.

C.2.2 Substitution across banks

Assume that firms borrow from multiple banks and can substitute across banks. Each firm borrows from a set of banks \mathcal{B}_f . All firms borrows from an equal number of banks M . Firms optimize the allocation of loans across banks:

$$\min_{C_{fb}} \sum_{b \in \mathcal{B}_f} r_b^c C_{fb} \quad \text{subject to} \quad \left(\sum_{b \in \mathcal{B}_f} C_{fb}^{\frac{\sigma-1}{\sigma}} \right)^{\frac{\sigma}{\sigma-1}} \geq C_f$$

The first-order condition writes:

$$C_{fb} = \left(\frac{r_b^c}{r_f^c} \right)^{-\sigma} C_f \quad \text{where} \quad r_f^c = \left(\sum_{b \in \mathcal{B}_f} r_b^{c1-\sigma} \right)^{\frac{1}{1-\sigma}}$$

C_f is still given by $C_f = ce^{\theta_f} (r_f^c)^{-\epsilon^c}$. Log-linearizing, the solution writes:

$$\hat{C}_{fb} = -\sigma(\hat{r}_b^c - \hat{r}_f^c) + \hat{C}_f = \theta_f - \sigma\hat{r}_b^c + (\sigma - \epsilon^c)\hat{r}_f^c$$

If loans granted by the different banks as highly substitutable, $\sigma > \epsilon^c$. Then, the demand directed toward a given bank is decreasing in this banks' interest rate, but increasing in the interest rate

81. Assuming a realistic $\epsilon^s \approx 1$, χ is monotonically increasing in φ .

charged by other banks, captured by the term \hat{r}_f^c .

Banks solve the same problem as before. The problem is analytically intractable for a generic firm-bank network. To obtain closed-form solutions, I make additional assumptions. Each bank lends to only one firm. Therefore, the sets \mathcal{B}_f form a partition of the sets of all banks $[0, 1]$. In addition, the firm demand shock is the same for all firms and equal to 0. Let us denote $Z_f^{gov} = \frac{1}{M} \sum_{b \in \mathcal{B}_f} Z_b^{gov}$. Using these assumptions, we obtain:

$$\hat{C}_{fb} = -\frac{\epsilon^c \frac{i^*}{\phi S^*}}{(\epsilon^s + (1-\lambda)\epsilon^c)(\epsilon^s + (1-\lambda)\epsilon^c + \frac{i^*}{\phi S^*})} \lambda Z^{gov} + \frac{(\sigma - \epsilon^c)(\epsilon^s + \frac{i^*}{\phi S^*})}{(\epsilon^s + (1-\lambda)\epsilon^c + \frac{i^*}{\phi S^*})(\epsilon^s + (1-\lambda)\sigma + \frac{i^*}{\phi S^*})} \lambda Z_f^{gov} - \frac{\sigma}{\epsilon^s + (1-\lambda)\sigma + \frac{i^*}{\phi S^*}} \lambda Z_b^{gov}$$

$$\hat{C}_f = -\frac{\epsilon^c \frac{i^*}{\phi S^*}}{(\epsilon^s + (1-\lambda)\epsilon^c)(\epsilon^s + (1-\lambda)\epsilon^c + \frac{i^*}{\phi S^*})} \lambda Z^{gov} - \frac{\epsilon^c}{\epsilon^s + (1-\lambda)\epsilon^c + \frac{i^*}{\phi S^*}} \lambda Z_f^{gov}$$

If $\sigma > \epsilon^c$, the coefficient of the within-firm relationship overestimates the firm-level effect. The coefficient of the firm-level relationship is the same as that of the firm \times bank-level relationship when firms do not substitute across banks, and remains a lower bound on the aggregate crowding out parameter. Hence, the correctly estimated firm-level regression coefficient yields a lower bound on the aggregate crowding out parameter.

C.2.3 Regions within banks

Let us assume that all banks operate with a mass 1 of regional subdivisions, indexed by r . Firms and local governments are now located on a unit cube, with an additional dimension for regions. Firms, local governments and depositors do not arbitrage across regions within banks. Banks have an internal capital market and can move funds across regions. These transfers are denoted T_{br} . I capture the imperfect functioning of this market by including a cost ψ . I assume that funds borrowed from the interbank market are shared equally across regions. The banks' optimization problem becomes:

$$\max_{\{C_{br}^{corp}, C_{br}^{gov}, S_{br}, T_{br}\}_{r, B_b}} \int (r_{br}^c C_{br}^{corp} + r_{br}^g C_{br}^{gov} - r_{br}^s S_{br} - \frac{\psi}{2} T_{br}^2) dr - i B_b - \frac{\phi}{2} B_b^2$$

subject to: $C_{br}^{corp} + C_{br}^{gov} = S_{br} + T_{br} + B_b$ and $\int T_{br} dr = 0$. Define $\nu^r = \frac{\epsilon^s + (1-\lambda)\epsilon^c}{\epsilon^s + (1-\lambda)\epsilon^c + \frac{i^*}{\psi S^*}}$ and $\nu^b = \frac{\epsilon^s + (1-\lambda)\epsilon^c}{\epsilon^s + (1-\lambda)\epsilon^c + \frac{i^*}{\phi S^*}}$. Solving this problem, we obtain that for all f in \mathcal{F}_{br} ,

$$\hat{C}_{fb} = z_f - \chi(1 - \nu^b) \lambda \hat{C}^{gov} - \chi(\nu^b - \nu^r) \lambda \hat{C}_b^{gov} - \chi \nu^r \lambda \hat{C}_{br}^{gov}$$

In this case, corporate credit at bank b in region r depends on the aggregate local government debt shock \hat{C}^{gov} , on the bank-level shock \hat{C}_b^{gov} provided that $\nu^b \neq 0$ and $\nu^b \neq \nu^r$, and on the bank \times region-level shock \hat{C}_{br}^{gov} provided that $\nu^r \neq 0$. The conditions $\nu^r \neq 0$ and $\nu^b \neq 0$ have the same interpretation as before: without frictions, the idiosyncratic shocks are perfectly smoothed within/across banks. Besides, if $\nu^b = \nu^r$, corporate credit depends on \hat{C}_{br}^{gov} but not on \hat{C}_b^{gov} : the bank layer is irrelevant since it is equally costly to smooth shocks across and within banks.

The aggregate crowding out parameter is unchanged and equal to χ . In addition, as long as $\nu^r \neq 0$, there will be region-level crowding out:

$$\hat{C}_r^c = -\chi(1 - \nu^r) \lambda \hat{C}^{gov} - \chi \nu^r \lambda \hat{C}_r^{gov}$$

so that local-level corporate credit depends on local-level local government debt. I can repeat the same analysis introducing the layer of bank branches within regions.

C.2.4 Introducing a Ricardian response

What if instead we want to quantify the output loss when increasing local government debt by €1 to reduce lump-sum taxes by €1? In this case the crowding out effect will be dampened by the Ricardian effect: if agents increase their savings in response to the increase in government debt, this

constitutes additional supply of savings which offsets the increased demand for government debt. In the neoclassical-Ricardian equivalence benchmark, savings increase 1 for 1 with government debt and there is no crowding out. I now provide a back-of-the-envelope quantification of this offsetting effect based on estimates of households Ricardian response.

With segmented banks, Ricardian equivalence does not hold. The reason is that the interest rate on household b 's deposit will be different from the average interest rate paid on government debt. To quantify the potential magnitude of the Ricardian effect, I assume the following ad-hoc deposit supply function $S_b = \kappa C^{gov} + s(r_b^s)^{\epsilon^s}$ where κ drives the Ricardian response, $\kappa = 1$ being the case of fully Ricardian agents.⁸² Solving my model with this deposit supply function, I find that:

$$\hat{C}_{fb} = \theta_f + \chi \kappa \lambda \hat{C}^{gov} - \chi(1 - \nu) \lambda \hat{C}^{gov} - \chi \nu \lambda \hat{C}_b^{gov}$$

where $\chi = \frac{\epsilon^c}{(1-\kappa\lambda)\epsilon^s + (1-\lambda)\epsilon^c}$. If $\kappa = 1$, the aggregate crowding out effect, given by $(\kappa - 1)\chi$, is zero.

κ is the euro response of deposits to a one euro increase in local government debt that is used to reduce current taxes. I estimate κ as follows. First, I take a savings response equal to 0.59 (the average of the most recent estimates).⁸³ Second, only a fraction of increased savings takes the form of deposits that may be used by banks to finance loans. I use the share of bank deposits in French households' assets (6%, French statistical office data). I provide an upper bound using the share of financial assets (20%), i.e. considering that an increase in any type of financial assets would ultimately lead to an increase in corporate credit.

Accounting for the Ricardian effect predicts a credit shortfall equal to $(1 - \kappa)$ times the shortfall not accounting for Ricardian effects. This implies that with $\kappa^{baseline}$, the credit (and as a result the output) shortfall is reduced by 3.5% (12% using κ^{upper}).

C.2.5 Introducing household loans

Assume that household also demand loans, with an isoelastic demand function with elasticity ϵ^h . The aggregate crowding out parameter is then given by:

$$\chi = \frac{\epsilon^c}{\epsilon^s + \lambda_c \epsilon^c + \lambda_h \epsilon^h}$$

where λ_c and λ_h are the shares of corporate and household loans in banks' credit portfolio, respectively. The key result that the relative crowding out parameter is a lower bound for the aggregate one is unchanged.

C.2.6 Introducing a differential cost for local government loans

Assume that banks face a different marginal cost of lending to private corporations vs. local governments. This could be the case if lending to local governments is less costly for banks, for instance for regulatory reasons, or if banks enjoy private benefits of lending to local governments. Banks maximize:

$$\max_{\{C_b^{corp}, C_b^{gov}, S_b, B_b\}} r_b^c C_b^{corp} + r_b^g C_b^{gov} - r_b^s S_b - i B_b - \frac{\phi^g}{2} C_b^{gov2} - \frac{\phi^c}{2} C_b^{corp2}$$

The aggregate crowding out parameter depends only on ϕ^c , not on ϕ^g or on the difference between the two parameters. Hence, the crowding out parameter does not reflect distortions that make local government loans more profitable.

82. This parametric departure of Ricardian equivalence is in the spirit of Abel (2017).

83. Considering the limitations of the early studies, I rely on estimates of the reaction of savings to a tax cut from the most recent literature, namely Barczyk (2016) (0.61), Hayo and Neumeier (2017) (0.36) and Meissner and Rostam-Afschar (2017) (0.79), taking the upper bound of estimated results.

D Identification with the shift-share instrument

I repeat the baseline specification (2) omitting controls and the time and region subscripts (the following discussion applies to the bank-level shock defined at any scale):

$$\Delta C_{fb} = d_f + \beta \Delta C_b^{gov} + \varepsilon_{fb}$$

ε_{fb} is by construction orthogonal to the firm-level fixed effects, hence it captures the firm \times bank-specific unobservable determinants of credit flows, in particular due to bank-specific supply shocks (ξ_b in the structural equations from the main text). I instrument ΔC_b^{gov} by the shift-share instrument $BankExposure_b = \sum_m \omega_{bm}^{gov} \Delta C_m^{gov}$. The standard exclusion restriction writes:

$$\mathbb{E} \left[\sum_c \omega_{bm}^{gov} \Delta C_m^{gov} \varepsilon_{fb} \middle| d_f \right] = 0 \quad (10)$$

Identification based on shocks. Condition (10) is immediately satisfied if the shocks ΔC_m^{gov} are exogenous, but does not require it. The less restrictive requirement is that municipality-level shocks are uncorrelated with the average bank-level determinants of corporate credit for the banks most exposed to each municipality (Borusyak, Hull, and Jaravel, 2021). I follow these authors and write the full-data orthogonality condition. Since my specification includes firm \times time fixed effects, I write the orthogonality condition in terms of deviations from the within-firm average, denoted with a tilde:

$$\mathbb{E} \left[\sum_m \Delta C_m^{gov} \sum_{f,b} \tilde{\omega}_{bm}^{gov,f} \varepsilon_{fb} \right] = 0 \quad (11)$$

ΔC_m^{gov} must be orthogonal to the bank-specific shocks ε_{fb} aggregated using the (within-firm deviations in) exposures of banks to municipality m . Put differently, it must not be the case that banks experiencing negative bank-specific shocks ε_{fb} have systematically higher exposure to municipalities where ΔC_m^{gov} is high. Note that including firm \times time fixed effect is critical for the orthogonality condition to plausibly hold. Otherwise, this condition would write:

$$\mathbb{E} \left[\sum_m \Delta C_m^{gov} \left(\sum_f \bar{\omega}_{bm,-1}^{gov,f} d_f + \sum_{f,b} \omega_{bm}^{gov} \varepsilon_{fb} \right) \right] = 0$$

where $\bar{\omega}_{bm,-1}^{gov,f}$ is the sum of ω_{bm}^{gov} for the set of banks b lending to f . Since ΔC_m^{gov} is correlated to d_f , the correlation coming for the first term in the parenthesis is unlikely to be zero.⁸⁴

The main threat to condition (11) is if municipality-level local government debt shocks are systematically correlated to bank-level credit supply shock through other channels than crowding out. It would be problematic if (i) local government debt were correlated to some variable X_m (e.g., firm characteristics in m), (ii) X_m affects banks' ability to lend through the same exposure weights ω_{bm}^{gov} . Two potential channels may be problematic: non-performing loans and deposits. If local government debt increases in municipalities where bankruptcies are high, then $\sum_m \omega_{bm}^{gov} \Delta C_m^{gov}$ is correlated to $\sum_m \omega_{bm}^{gov} Bankruptcy_c$. If local government debt exposure weights are similar to corporate credit exposure weights, the latter will likely affect $\Delta NonPerformingLoans_b$, which may in turn enter ε_{fb} . A similar concern would arise if changes in local government debt are correlated to local deposit withdrawals and local government debt weights and deposit weights are the same.

A generic way to address this concern is to show that municipality-level changes in local gov-

84. Notably since ΔC_m^{gov} and d_f are likely to be more correlated when $\bar{\omega}_{bm}^f$, that is when the banks lending to f have large exposure weights to m , which given the local nature of lending markets indicates that f is likely to be located in or close to municipality m .

ernment debt are not correlated to other municipality-level variables. Although local government debt is endogenous to local economic outcomes, this relationship is unlikely to operate at the municipality level: municipalities are very narrowly-defined units, smaller than the relevant economic scale for stimulus spending effects, and there is high dispersion in local government debt shocks across neighbouring municipalities. Figure D.1 shows that ΔC_m^{gov} is in fact not correlated with the municipality-level GDP growth, private credit growth, change in the number of banked firms or bankruptcy rate.⁸⁵ While reassuring, these municipality-level orthogonality conditions are not necessary. What matters is that other municipality-level shocks do not generate bank-level shocks correlated to *BankExposure*. Figure 5 shows that banks with high and low *BankExposure* are relatively similar on observed variables, which limits this concern. I directly test for the correlation between *BankExposure* and the bank-level changes in deposits and in non-performing loans. Figure D.2 shows no correlation patterns between these variables.⁸⁶

Finally, I address an additional concern: ΔC_m^{gov} is a realized quantity that aggregates the shocks of the banks lending to municipality m , which also directly enter the residual of my regression, introducing a mechanical bias.⁸⁷ First, this concern is alleviated is the market shares of banks in municipalities are not too concentrated. In the case at hand, the average Herfindahl index is 0.17. Second, as suggested by Borusyak, Hull, and Jaravel (2021), a solution is to use a leave-one-out estimator (LOO) or to follow the Amiti and Weinstein (2018) procedure (AW) to filter out bank-specific shocks from municipality-level growth rates.^{88,89} Table D.3 reports similar results using these definitions of *BankExposure*. Third, this would induce a mechanical bias of the IV in the direction of the OLS coefficient (in the extreme case where there is only one bank per municipality, the shift-share instrument and the endogenous variable are equal). Table D.2 shows that the OLS coefficient has an opposite sign. This positive bank-level correlation between local government and corporate credit corresponds to the expected sign of the bias if banks are hit by shocks affecting their ability to lend to both segments, as clarified by equation 9.

Consistency: Exposure to common municipality-level shocks induce dependencies across banks with similar exposure shares, so that the setting is not *iid*. Borusyak, Hull, and Jaravel (2021) show that the conditions for consistency are that there is a sufficiently large number of shocks with sufficient shock-level variation and that shocks exposure is not too concentrated.

Panel A of Table D.1 documents a large dispersion in ΔC_m^{gov} , which persists when residualizing on time, region \times time or municipality fixed effects. Figure 3 additionally shows a large degree of variation even within very narrowly defined geographic clusters. Besides, exposure shares are not too concentrated. Define municipality-level weights as $s_{mt} = \sum_b e_{bt} \omega_{bm,t-1}^{gov}$ where e_{bt} are bank-level weights.⁹⁰ Panel B shows that the largest weight is very small (0.1%) and the inverse Herfindahl index is large: 6,297. I report the same statistics when exposure weights are aggregated at the municipality-level, and there is sufficient municipality-level dispersion even when shocks are allowed to be serially correlated.⁹¹

85. I show the dynamic correlations to show there is no degradation in the municipality-level bankruptcy rate following the increase in local government borrowing.

86. There is a drop in non-performing loans at $t + 2$, which may *positively* affect bank health.

87. The preceding point would apply if we observed the true municipality-level local government debt demand shocks. This point relates to the fact that I use realized quantities as a proxy for the underlying shock.

88. For the LOO, let $\Delta C_{m,-b}^{gov} = \sum_{b' \neq b} \frac{C_{b',m,-1}^{gov} - 1}{C_{m,-1}^{gov}} \Delta C_{b'm}^{gov}$ and $BankExposure_b = \sum_m \omega_{bm}^{gov} \Delta C_{m,-b}^{gov}$. For the AW, obtain municipality-time fixed effects in the following regression: $\Delta C_{bmt}^{gov} = \alpha_{mt} + \alpha_{bt} + \varepsilon_{bct}$. Define $BankExposure_b$ using the estimated α_{mt} instead of ΔC_m^{gov} .

89. Note that if what is truly exogenous is the municipality-level increase in local government debt but the split across banks is not, these alternative definitions may be more problematic than the initial definition.

90. My baseline firm-bank regression is unweighted. When running tests at the bank-level, I weight bank-level by the lagged corporate loan portfolio of each bank.

91. To provide a benchmark, Borusyak, Hull, and Jaravel (2021) show that their methodology is relevant in the Autor setting where the inverse Herfindahl is 58.4 and the largest share is 6.5%.

Identification based on shares. A correlation between ΔC_m^{gov} and any other municipality-level variable is problematic only to the extent that this other variable affects banks through the same exposure shares, i.e. that shares are correlated to bank-level credit supply shocks. As shown by Goldsmith-Pinkham, Sorkin, and Swift (2020), $\mathbb{E}[\varepsilon_{fb}\omega_{bm}^{gov}|d_f] = 0$ for all m with $\Delta C_m^{gov} \neq 0$ is a sufficient condition for the shift-share IV to be unbiased and consistent. This assumption is credible in my setting, but shares exogeneity is a less intuitive source of identification.

First, the variable used to define the shares, local government loans, is specific to the mechanism under study. As a placebo test, I repeat the construction of *BankExposure* with corporate credit exposure weights. Table D.3 shows that this alternative variable does not predict a decline in corporate credit. This further alleviates concerns that *BankExposure* is picking up local shocks occurring on the corporate credit market and correlated to ΔC_m^{gov} that reduce banks' credit supply.⁹²

Second, there are many municipalities, so that the correlation between bank-level shocks and banks' exposure to any given municipality is likely small. I find that the municipality-level Rotemberg weights—which summarize the identifying variation used by the shift-share instrument—are very dispersed. The 5 largest Rotemberg weights account for roughly 25% of the positive weight in the estimator.^{93,94} Dispersed Rotemberg weights reduce the sensitivity of the Bartik instrument to non-random exposure to a given municipality. On the other hand, it makes it harder to interpret the identifying variation. The fact that the intuition of the identification does not rely on comparing local government debt dynamics in a handful of “extreme” municipalities but instead relies on banks being exposed to a large number of municipalities justifies the favored interpretation of identification as coming for shocks.

Table D.1: Shock-level summary statistics

Panel A: Summary statistics on municipality-level shocks

	count	mean	sd	p25	p50	p75
Municipality-level growth ΔC_{mt}^{gov}	108,062	0.010	0.092	-0.019	-0.004	0.024
Residualized on time FE	108,062	0.000	0.091	-0.029	-0.012	0.013
Residualized on region \times FE	108,062	0.000	0.090	-0.029	-0.011	0.015
Residualized on municipality FE	108,062	-0.000	0.091	-0.030	-0.013	0.015

Panel B: Summary statistics on exposure shares

	Across municipalities and dates	Across municipalities
Inverse HHI	6,297	124
Largest weight (%)	0.001	0.037

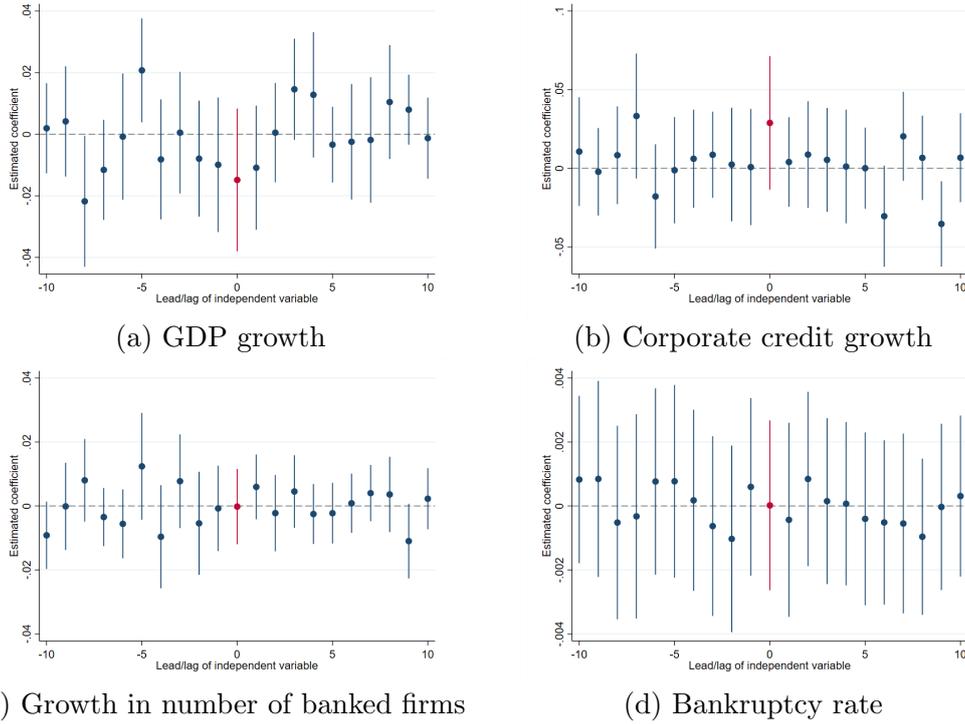
Note: This table presents descriptive statistics relevant for the shift-share design. Panel A presents summary statistics of the municipality-level shocks, that is the municipality-level local government loans growth rates. Panel B presents summary statistics of municipality-level weights $s_{mt} = \sum_b e_{brt}\omega_{bm,t-1}^{gov}$ where $\omega_{bm,t-1}^{gov}$ is defined as in my baseline analysis relative to bank \times region total lending and e_{brt} is bank \times region-level corporate credit. Weights are normalized to sum to 1 for the whole sample. I compute the municipality-level inverse Herfindahl index $1/\sum_{m,t} s_{mt}^2$ and the largest s_{mt} weight, and then the same quantities when weights are aggregated across time for a given municipality.

92. This test is quite demanding since corporate and local government exposure weights—which are both largely determined by the banks' branch network—are significantly correlated.

93. All examples in Goldsmith-Pinkham, Sorkin, and Swift (2020) yield a number larger than 40%.

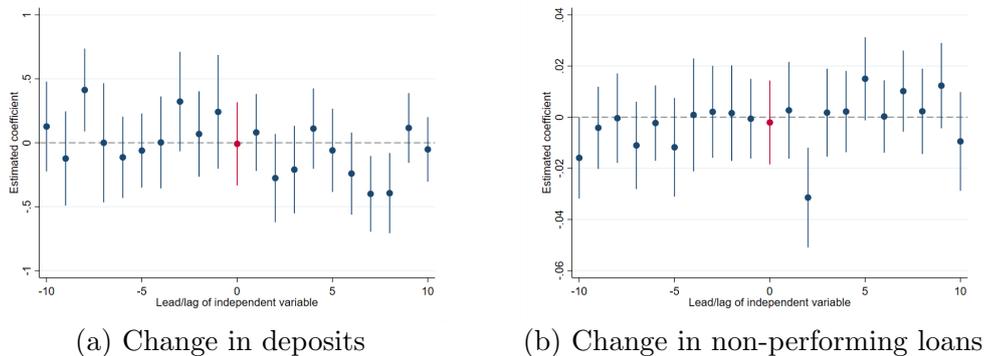
94. These 5 instruments are the municipalities of Strasbourg, Ajaccio, Toulouse, Dijon and Bordeaux, five mid-size French municipalities located in different regions of France. Repeating the analysis at the municipality \times time-level shows that these highest weight municipalities vary across time. Repeating the analysis by region, the top 5 weights by region account for on average 4% of the region-level positive weight.

Figure D.1: Municipality-level balance tests



Note: These figures show the coefficients of the regression of municipality-level economic variables on leads and lags of municipality-level local government loans growth and time fixed effects. In panel (a), the dependent variable is municipality-level GDP growth, defined as the growth in value added of firms headquartered in the municipality for which I have tax-filings. In panel (b), the dependent variable is municipality-level corporate credit growth. In panel (c), the dependent variable is municipality-level growth rate in the number of banked firms appearing in the credit registry. In panel (d), the dependent variable is the municipality-level bankruptcy rate, defined as the number of firms entering bankruptcy proceedings normalized by the number of banked firms in the municipality. As recommended by Borusyak, Hull, and Jaravel (2021), the regressions are weighted by $s_{mt} = \sum_b e_{bt} \omega_{bm,t-1}^{gov}$ where e_{bt} is the lagged corporate loan portfolio of each bank \times region. The dot is the point estimate and the bar is the 95% confidence interval.

Figure D.2: Bank-level balance tests



Note: These figures show the coefficients of the regression of bank-level variables on leads and lags of bank exposure to local government debt shocks. Exposure to local government debt demand shocks is measured at the bank \times region \times time level and is defined in (1) as the sum of municipality-level increases in local government debt weighted by municipalities' shares in the bank's loan portfolio. In panel (a), the dependent variable is bank-level deposit growth. In panel (b), the dependent variable is bank-level growth in non-performing loans. All regressions control for the sum of shares and time fixed effects. The regressions are weighted by e_{bt} is the lagged corporate loan portfolio of each bank \times region. The dot is the point estimate and the bar is the 95% confidence interval.

Table D.2: Baseline specification: first stage, OLS, reduced form and IV

	Change in local govt loans		Firm×bank credit growth	
	FS	OLS	RF	IV
	(1)	(2)	(3)	(4)
Bank Exposure	1.151*** (0.048)		-1.098*** (0.266)	
Change in local govt loans		0.696*** (0.138)		-0.954*** (0.230)
Controls	✓	✓	✓	✓
Firm×Time FE	✓	✓	✓	✓
Observations	12,360,042	12,360,042	12,360,042	12,360,042
R-squared	0.51	0.52	0.52	–
Kleibergen-Paap rk Wald F stat.	–	–	–	576.6

Note: This table reports the results of the first stage (FS), OLS, reduced form (RF), and IV of specification (2). In column (1), the outcome variable is the realized increase in bank×region-level local government loans ΔC_{brt}^{gov} . In columns (2)-(4), the outcome variable is the mid-point growth rate of credit granted to firm f by bank b . In columns (1) and (3), the independent variable is exposure to local government debt demand shocks defined at the bank×region×time level as the sum of municipality-level local government debt growth, weighted by exposure shares equal to the bank's local government credit in each municipality as a fraction of bank×region-level total credit (equation (1)). In columns (2) and (4), the independent variable is the realized increase in bank×region-level local government loans ΔC_{brt}^{gov} (instrumented by Bank Exposure in column (4)). Controls include the sum of shares, the bank's assets, equity ratio, a dummy indicating whether the bank is state-owned, and the length of the bank-firm relationship. All regressions are estimated on the sample of firms with multiple credit relationships. Standard errors are clustered at the region×bank level. ***, ** and * indicate significance at the 1%, 5% and 10% levels, respectively.

Table D.3: Additional robustness checks

	Credit growth			
	LOO (1)	AW (2)	Placebo with corporate shares (3) (4)	
Bank Exposure (LOO)	-1.388*** (0.383)			
Bank Exposure (AW)		-0.754*** (0.119)		
Bank Exposure (corporate)			0.083 (0.126)	0.046 (0.126)
Controls	✓	✓	✓	✓
Firm×Time FE	✓	✓	✓	✓
Active Bank×Time FE				✓
Observations	12,360,042	12,169,465	12,237,052	12,237,052
R-squared	0.52	0.52	0.52	0.52

Note: This table presents tests of the robustness of my main results to concerns related to the shift-share structure of the shock. In column (1), I define bank exposure using the leave-one-out methodology: bank b 's exposure is defined as the sum of municipality-level increases in local government debt weighted by municipalities' local government loan shares in the bank's loan portfolio, where the municipality-level growth rates are computed by leaving out bank b . In column (2), I define bank exposure using the Amiti-Weinstein methodology: I first regress bank×municipality×time-level growth rates in local government loans on bank×time and municipality×time fixed effects. I then define bank exposure as the sum of the municipality×time fixed effects weighted by municipalities' local government loan shares in the bank's loan portfolio. In columns (3) and (4), bank exposure is defined as the sum of municipality-level increases in local government debt weighted by municipalities' corporate credit shares in the bank's loan portfolio. Bank exposure remains defined at the bank×region level. Controls include the sum of shares, the bank's assets, equity ratio, a dummy indicating whether the bank is state-owned and the length of the bank-firm relationship. All regressions are estimated on the sample of firms with multiple credit relationships. Standard errors are clustered at the region×bank level. ***, ** and * indicate significance at the 1%, 5% and 10% levels, respectively.

E Substitution effects in the Khwaja-Mian framework

This Appendix details the method to disentangle the direct effect of credit supply shocks from substitution across banks in the Khwaja and Mian (2008) framework (KM). To simplify the exposition, I omit the time subscript and I abbreviate the *BankExposure_{br}* variable as B_b . All proofs are stacked at the end.

E.1 The standard KM framework (no substitution)

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 is $\Delta C_{fb} = \frac{C_{b,f} - C_{b,f,-1}}{C_{b,f,-1}}$.⁹⁵ Besides, let $B_f = \sum_{b=1}^{n_f} \omega_{bf} B_b$ where ω_{bf} are the bank shares $\omega_{bf} = \frac{C_{bf,-1}}{C_{f,-1}}$. As shown by KM, the basic credit channel equation can be written as:

$$\Delta C_{fb} = \beta B_b + d_f + \varepsilon_{fb} \quad (12)$$

The key issue is that firm- and bank-shocks may be correlated. Let $\rho_{bd} = \text{cov}(B_b, d_f)$.⁹⁶ Besides, let $\text{Var}(B_b) = \sigma_b^2$. 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).

As shown by KM, firm fixed effects allow to abstract from the correlation between B_b and d_f : while the OLS 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 :

$$\begin{aligned} \beta_{OLS} &= \beta + \frac{\rho_{bd}}{\sigma_b^2} \\ \beta_{FE} &= \beta \end{aligned}$$

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 (12) at the firm-level using the bank shares as weights yields:

$$\Delta C_f = \beta B_f + d_f + \varepsilon_f \quad (13)$$

However, in the cross-sectional model (13), 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 estimate and the comparison with the within-firm coefficient is not informative. Under assumption (A1), the expression for $\bar{\beta}_{OLS}$ is:

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

To circumvent this issue, Cingano, Manaresi, and Sette (2016) and Jiménez, Mian, Peydró, and Saurina (2019) have proposed to use the estimated fixed effects in (12) to correct for this bias. Including \hat{d}_f in the estimation of (13), we get:

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

One then compares $\bar{\beta}_{OLS, \hat{d}}$ to β_{FE} to assess the existence of substitution across banks: $\bar{\beta}_{OLS, \hat{d}} = \beta_{FE}$ would suggest there is no substitution. The rest of this appendix shows that this reasoning is incorrect.

⁹⁵ My results are unaffected if ΔC_{fb} is defined as the mid-point growth rate.

⁹⁶ A more rigorous notation for the bank shock variable would be $B_{bf} = B_b \mathbb{1}_{[b \in \mathcal{B}_f]}$, since this variable is defined in the bank \times firm data only when bank b lends to firm f . Likewise, $\rho_{bd} = \text{cov}(B_{bf}, d_f) = \text{cov}(B_b, d_f | b \in \mathcal{B}_f)$. In the rest of the text, I keep the simple notation B_b .

E.2 Introducing substitution in the KM framework

If there are spillovers across banks, equation (12) is misspecified and the true model is:

$$\Delta C_{fb} = \beta B_b + \gamma B_{-bf} + d_f + \varepsilon_{fb} \quad (14)$$

where B_{-bf} 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_{-bf} is:

$$B_{-bf} = \frac{1}{n-1} \sum_{\substack{b' \in \mathcal{B}_f \\ b' \neq b}} B_{b'}$$

One cannot run a within-firm estimation of equation (14) because B_{-bf} and B_b are collinear conditional on the firm fixed effects. If we estimate equation (14) omitting the term B_{-bf} , we obtain:

$$\beta_{FE} = \beta - \frac{1}{n-1} \gamma \quad (15)$$

In the case where β and γ have opposite signs, the estimated coefficient in the standard KM regression overestimates the true effect. The KM estimator is akin to a within-firm difference-in-differences 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.

The between-firm coefficient is also biased. Summing equation (14) at the firm level, we obtain:

$$\Delta C_f = (\beta + \gamma) B_f + d_f + \varepsilon_f \quad (16)$$

Estimating this equation omitting d_f , we get:

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

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

$$\bar{\beta}_{OLS, \hat{d}} = \beta - \frac{1}{n-1} \gamma \quad (17)$$

The intuition is that since β_{FE} in (15) 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. Moreover, equation (17) 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 $\bar{\beta}_{OLS, \hat{d}} \neq \hat{\beta}_{FE}$ is because assumption (A1) does not hold in general, not because the difference captures substitution effects. Hence, with substitution effects neither the standard KM estimator nor the procedure of Cingano, Manaresi, and Sette (2016) and Jiménez, Mian, Peydró, and Saurina (2019) allows to recover the true β .

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

Let us again assume that the true data-generating process is given by:

$$\Delta C_{fb} = \beta B_b + \gamma B_{-bf} + d_f + \varepsilon_{fb} \quad (18)$$

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

$$B_{-bf} = \sum_{\substack{b' \in \mathcal{B}_f \\ b' \neq b}} \frac{\omega_{b'f}^\phi}{\left(\sum_{j \neq b} \omega_{jf}^\phi\right)} B_{b'}$$

where ϕ is a parameter. Taking a generic functional form allows to make assumptions on the extent to which each bank's shock affects the firm, depending on the bank shares ω_{bf} . It nests all the intuitive forms for B_{-bf} : the equal-weighted mean of other banks' shocks, their bank-share weighted mean, the shock of the bank with the highest bank share.⁹⁷

Proposition 5 *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 variation in n_f across firms; (ii) using variation in ω_{bf} within firms. For simplicity, I focus on the case without control variables, but adding controls does not affect any of the results.

Using variation in n_f across firms. A first avenue to identify β and γ is using variation in the number of banks per firm n_f . To clarify the intuition, I assume that ω_{bf} is constant within firm, or equivalently, B_{-bf} is defined using $\phi = 0$.

Proposition 6 *If n_f varies across firms, then equation (18) is identified and $(\beta_{FE}, \gamma_{FE}) = (\beta, \gamma)$.*

The intuition for identification when n_f varies (while the coefficients are not identified with constant n) is the following: the size of the bias related to the substitution effect in β_{FE} 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, enabling to disentangle the effects of γ and β . This method requires no additional assumption, but requires sufficient variation in n_f across firms.

Using variation in ω_{bf} within firms. Let us now assume that n_f is constant and equal to n . In this case, we can use within-firm variation in ω_{bf} along with a specific functional form for B_{-bf} to separately identify β and γ .

Proposition 7 *If $n > 2$, ω_{bf} not constant within firms, and $\phi \neq 0$, equation (18) is identified and $(\beta_{FE}, \gamma_{FE}) = (\beta, \gamma)$.*

When these conditions are satisfied, B_b and B_{-bf} are not collinear conditional on the firm fixed effects, so that we can estimate equation (18). Intuitively, we disentangle the direct effect from the substitution term by assuming that the substitution effect from bank b' toward bank b is related to the share of bank b' in the firm total credit. The advantage of this identification strategy is that it works for n_f constant. There are nevertheless limitations to this method. First, it requires a substantial number of firms with $n > 2$. Second, it requires sufficient variation in ω_{bf} within firms.

Note that here—contrary to the constant ω_{bf} case— $B_{-bf} \perp \varepsilon_{fb}$ is not a direct implication of $B_b \perp \varepsilon_{fb}$. To show that this orthogonality condition holds, one must rely on the argument for identification with shift-share instruments with exogenous shocks, as stated in Borusyak, Hull, and Jaravel (2021).⁹⁸

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

98. Namely, the full-data orthogonality condition can be rewritten as $\mathbb{E} \left[\sum_b B_b \left(\sum_f \sum_{b' \neq b} \frac{\omega_{bf}}{1-\omega_{b'f}} \varepsilon_{fb'} \right) |d_f \right] = 0$.

Effect on firm-level credit. The two procedures above allow to obtain unbiased estimates of the firm-level demand shocks \hat{d}_f . With this estimates in hand, one can use the methodology outlined in Cingano, Manaresi, and Sette (2016) and Jiménez, Mian, Peydró, and Saurina (2019) to obtain unbiased estimates of the between-firm coefficient. Namely, including the estimated \hat{d}_f in the firm-level regression yields an unbiased estimate of $\bar{\beta}$.⁹⁹

Implementation. I test these methods on simulated data. I simulate 100 datasets with 180,000 bank-firm observations, with either a distribution of the number of banks per firm or a within-firm dispersion in bank shares similar to that of my true data. For each of these simulated datasets, I implement the methods outlined above. Table E.1 reports the average estimated coefficient as well as its standard error across the 100 simulations. Columns (1) and (2) correspond to the method relying on variation in n_f , in the case where n_f is random and in the case where n_f is correlated to d_f . Columns (3) to (6) correspond to the method relying on variation in bank shares, with $\phi = 1$ or $\phi = +\infty$, and ω_{bf} random or correlated to ε_{bf} . The upper panel shows that the naive estimates

Table E.1: Estimation of β and γ : simulation results

	Estimation results for $(\beta, \gamma) = (-0.5, 0.3)$					
	Variation in n_f		Variation in ω_{bf}			
	$\phi = 0$		$\phi = 1$		$\phi = +\infty$	
	Random n_f	Corr. n_f	Random ω_{bf}	Corr. ω_{bf}	Random ω_{bf}	Corr. ω_{bf}
<u>Naive estimators</u>						
$\hat{\beta}_{FE}$	-0.695 (0.003)	-0.695 (0.004)	-0.650 (0.003)	-0.650 (0.003)	-0.650 (0.004)	-0.650 (0.003)
$\hat{\kappa}_d, \hat{d}$	1.138 (0.003)	1.138 (0.002)	1.126 (0.003)	1.126 (0.003)	1.126 (0.003)	1.126 (0.003)
$\hat{\beta}_{OLS}$	0.390 (0.005)	0.750 (0.007)	0.345 (0.007)	0.344 (0.007)	0.371 (0.007)	0.370 (0.007)
$\hat{\beta}_{OLS}, \hat{d}$	-0.695 (0.003)	-0.695 (0.004)	-0.653 (0.004)	-0.653 (0.004)	-0.658 (0.005)	-0.658 (0.004)
<u>Correct method</u>						
$\hat{\beta}_{FE}$	-0.499 (0.007)	-0.499 (0.007)	-0.500 (0.006)	-0.500 (0.006)	-0.501 (0.004)	-0.499 (0.004)
$\hat{\gamma}_{FE}$	0.301 (0.010)	0.301 (0.010)	0.300 (0.011)	0.300 (0.011)	0.300 (0.004)	0.300 (0.005)
$\hat{\kappa}_d, \hat{d}$	1.000 (0.005)	1.000 (0.005)	1.000 (0.005)	1.000 (0.005)	1.001 (0.003)	1.000 (0.003)
$\hat{\beta}_{OLS}, \hat{d}$	-0.199 (0.017)	-0.198 (0.017)	-0.251 (0.015)	-0.251 (0.015)	-0.222 (0.008)	-0.220 (0.008)

Note: This table shows summary statistics of the results of 100 estimations of the parameters of interests on simulated data. In columns (1) and (2), I simulate 202,600 firms, the number of bank per firms to follow a geometric law with success probability 0.65, I keep firms with two banks or more ($\mathbb{E}[N] = 180,000$), ω_{bf} is constant and equal to $1/n_f$. In column (1) B_b and d_f are jointly normally distributed with mean 0, variance 1 and covariance $\rho_{bd} = 0.28$. In column (2), d_f is instead n_f plus a normal noise and $\rho_{bd} = 0.67$. In both columns, ε_{bf} is a normal noise. In columns (3)-(6), I simulate 60,000 firms, the number of banks per firm is equal to 3 ($N=180,000$). B_b and d_f are jointly normally distributed with mean 0, variance 1 and covariance $\rho_{bd} = 0.28$, ω_{bf} follows a uniform distribution and is normalized to sum to 1 for each firm. I columns (3) and (6), ε_{bf} is a normal noise. In column (4) and (6), ε_{bf} is equal to ω_{bf} plus a normal noise. The term B_{-b} is defined as per formula (E.3), the value of ϕ being indicated in the table header. I then generate ΔC_{bf} as in (14) with $\beta = -0.5$ and $\gamma = 0.3$.

can be far from the true parameters. The within-firm coefficient (line 1) overestimates the true β , as predicted. The second line shows that the \hat{d}_f are biased. Line 3 shows the naive between-firm coefficient, which is biased due to the positive correlation between B_b and d_f . Finally, including the wrongly estimated \hat{d}_f also leads to a biased coefficient (line 4). In the lower panel, I show the estimates for β and γ obtained from my method. They are very close to the true parameters. The

99. Under assumption (A1), $\bar{\beta} = \beta + \gamma$. When (A1) does not hold, this equality is not true anymore, but the intuition is similar: the between firm coefficient is lower (in absolute value) when substitution offsets the direct effect of the shock.

regression coefficient of \hat{d}_f on the true d_f is equal to 1. Including the unbiased estimates of \hat{d}_f in the between-firms regression yields an unbiased estimate of $\bar{\beta}$.¹⁰⁰ The standard deviation of the estimates I recover tend to be higher than that of the standard KM estimates, but remain in the same order of magnitude.

In practice, the two sources of variation can be combined. A limitation of the proposed approach is that it requires specifying a functional form for the substitution term B_{-bf} , leading to potential errors due to misspecification. Table E.2 shows the estimated coefficients when introducing a misspecified substitution term i.e. if the model is estimated using B_{-bf} defined in one way while the true data-generating process depends on B_{-bf} defined in another way. I find that estimating (18) with a misspecified substitution term reduces the bias compared to omitting this term.

Table E.2: Robustness to misspecification

True DGP	Parameter	Random n_f and ω_{bf}	Corr. n_f	Corr. ω_{bf}
$\phi = 0$	Misspecified $\hat{\beta}$	-0.500 (0.004)	-0.500 (0.004)	-0.501 (0.004)
	Misspecified $\hat{\gamma}$	0.300 (0.006)	0.300 (0.006)	0.299 (0.006)
$\phi = 1$	Misspecified $\hat{\beta}$	-0.503 (0.004)	-0.503 (0.003)	-0.503 (0.004)
	Misspecified $\hat{\gamma}$	0.295 (0.005)	0.296 (0.005)	0.295 (0.006)
$\phi = +\infty$	Misspecified $\hat{\beta}$	-0.578 (0.003)	-0.577 (0.002)	-0.578 (0.003)
	Misspecified $\hat{\gamma}$	0.180 (0.003)	0.181 (0.003)	0.181 (0.004)

Note: This table shows summary statistics of the results of 100 estimations of the parameters of interests on simulated data. I simulate 202,600 firms, the number of bank per firms to follow a geometric law with success probability 0.65, I keep firms with two banks or more ($\mathbb{E}[N] = 180,000$). In column (1), B_b and d_f are jointly normally distributed with mean 0, variance 1 and covariance $\rho_{bd} = 0.28$, ω_{bf} follows a uniform distribution and is normalized to sum to 1 for each firm, ε_{bf} is a normal noise. In column (2), d_f is instead n_f plus a normal noise and $\rho_{bd} = 0.67$. In column (3), ε_{bf} is instead equal to ω_{bf} plus a normal noise. B_{-bf} is defined as per formula (E.3) with $\phi = 0$ in line 1, $\phi = 1$ in line 2 and $\phi = +\infty$ in line 3. I generate ΔC_{bf} as if B_{-b} were defined with $\phi = 0$, $\phi = 1$ and $\phi = +\infty$, always with $\beta = -0.5$ and $\gamma = 0.3$. The coefficients in line (1) are the averages of the estimated coefficient when the true DGP is $\phi = 0$ but I run the regression with the 3 alternative definitions of ΔC_{bf} . Lines (2) and (3) follow the same logic.

Proof of Proposition 5. The KM estimator is equal to:

$$\beta_{FE} = \beta + \gamma \frac{\text{Cov}(B_{-bf}, B_b - \overline{B_b})}{\text{Var}(B_b - \overline{B_b})}$$

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

$$\text{Cov}(B_{-bf}, B_b - \overline{B_b}) = -\mathbb{E} \left[\sum_{\substack{b' \in \mathcal{B}_f \\ b' \neq b}} \frac{\lambda_{bb'f}}{n_f} (\mathbb{E}[B_b^2 | n_f, \Lambda] - \mathbb{E}[B_b B_{b'} | n_f, \Lambda]) \right]$$

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, $\frac{\lambda_{bb'f}}{n_f} \geq 0$. Hence, $\text{Cov}(B_{-bf}, B_b - \overline{B_b}) \leq 0$. Hence when β and γ have opposite (equal) signs, we obtain $|\beta_{FE}| \geq |\beta|$ ($|\beta_{FE}| \leq |\beta|$). ■

Proof of Proposition 6. In this case,

$$B_{-bf} = \frac{1}{n_f - 1} \sum_{\substack{b' \in \mathcal{B}_f \\ b' \neq b}} B_{b'}$$

100. Note that for varying ω_{bf} , the true $\bar{\beta}$ is no longer exactly equal to $\beta + \gamma$.

Let us use the upper bar denotes within-firm averages. First, let us show that $B_b \perp \varepsilon_{bf}|d_f \Rightarrow B_{-bf} \perp \varepsilon_{bf}|d_f$. Write B_{-bf} as

$$B_{-bf} = \frac{n_f}{n_f - 1} \overline{B_b} - \frac{1}{n_f - 1} B_b = \frac{n_f}{n_f - 1} \mathbb{E}[B_b|d_f] - \frac{1}{n_f - 1} B_b$$

and note that $\mathbb{E}[B_{-bf}|d_f] = \mathbb{E}[B_b|d_f]$. We can then write:

$$\begin{aligned} \mathbb{E}[B_{-bf}\varepsilon_{bf}|d_f] &= \mathbb{E}\left[\left(\frac{n_f}{n_f - 1} \mathbb{E}[B_b|d_f] - \frac{1}{n_f - 1} B_b\right)\varepsilon_{bf}|d_f\right] \\ &= \frac{n_f}{n_f - 1} \mathbb{E}[B_b|d_f]\mathbb{E}[\varepsilon_{bf}|d_f] - \frac{1}{n_f - 1} \mathbb{E}[B_b\varepsilon_{bf}|d_f] \quad \text{using } n_f \text{ constant conditional on } d_f \\ &= \mathbb{E}[B_b|d_f]\mathbb{E}[\varepsilon_{bf}|d_f] \quad \text{using } \mathbb{E}[B_b|d_f]\mathbb{E}[\varepsilon_{bf}|d_f] = \mathbb{E}[B_b\varepsilon_{bf}|d_f] \\ &= \mathbb{E}[B_{-bf}|d_f]\mathbb{E}[\varepsilon_{bf}|d_f] \quad \text{using } \mathbb{E}[B_{-bf}|d_f] = \mathbb{E}[B_b|d_f] \end{aligned}$$

I then use the equivalence between the least-square dummy variable and the within-firm estimators. The within-firm version of (14) writes:

$$\Delta C_{fb} - \overline{\Delta C_{fb}} = \beta(B_b - \overline{B_b}) + \gamma(B_{-bf} - \overline{B_{-bf}}) + (\varepsilon_{fb} - \overline{\varepsilon_{fb}})$$

Using the definition of B_{-bf} , one obtains that $B_{-bf} - \overline{B_{-bf}} = -\frac{B_b - \overline{B_b}}{n_f - 1}$. Therefore,

$$\begin{pmatrix} \beta_{FE} \\ \gamma_{FE} \end{pmatrix} = \mathbb{E}[\mathbf{X}'\mathbf{X}]\mathbb{E}[\mathbf{X}'\mathbf{Y}]$$

where $\mathbf{X} = \begin{pmatrix} B_b - \overline{B_b} & -\frac{B_b - \overline{B_b}}{n_f - 1} \end{pmatrix}$ and $\mathbf{Y} = \Delta C_{fb}$. The determinant of $\mathbb{E}[\mathbf{X}'\mathbf{X}]$ is equal to

$$d = \mathbb{E}[(B_b - \overline{B_b})^2]\mathbb{E}\left[\left(\frac{B_b - \overline{B_b}}{n_f - 1}\right)^2\right] - \mathbb{E}\left[\frac{(B_b - \overline{B_b})^2}{n_f - 1}\right]^2$$

which is not generically equal to 0 when n_f is not constant. In the case where $n_f \perp B_b$, d is proportional to $d \propto \mathbb{E}\left[\frac{n_f - 1}{n_f}\right]\mathbb{E}\left[\frac{1}{n_f(n_f - 1)}\right] - \mathbb{E}\left[\frac{1}{n_f^2}\right] = -\text{Cov}\left(\frac{n_f - 1}{n_f}, \frac{1}{n_f(n_f - 1)}\right)$. Hence,

$$\begin{pmatrix} \beta_{FE} \\ \gamma_{FE} \end{pmatrix} = \begin{pmatrix} \beta \\ \gamma \end{pmatrix}$$

■

Proof of Proposition 7. I detail the proof of identification in the case $B_{-bf} = \frac{1}{1 - \omega_{bf}} \sum_{j \neq b} \omega_{jf} B_j$, that is $\phi = 1$. Then the within firm estimation of (14) is:

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

$B_{-bf} - \overline{B_{-bf}}$ collinear to $B_b - \overline{B_b}$ implies that all the ω_{bf} are equal to $1/n$. By contrapositive, as long as not all the ω_{bf} are equal to $1/n$, we obtain that $B_{-bf} - \overline{B_{-bf}}$ 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 $\beta_{FE} = \beta$. ■

F Misallocation

This Appendix details Hsieh and Klenow (2009) used to perform the quantification of the effect on aggregate TFP. I omit time subscripts whenever possible.

Set-up. Consumers consume an aggregate output of S sectors $Y = \prod_s Y_s^{\theta_s}$ implying constant expenditure shares $\theta_s = \sum P_s Y_s$ (defining the final good as the numeraire). A fixed number of M_s firms produce in each sector, and goods of different firms are imperfectly substitutable. Real output in sector s is given by the CES aggregator:

$$Y_s = \left(\sum_{f=1}^{M_s} Y_{fs}^{\frac{\sigma-1}{\sigma}} \right)^{\frac{\sigma}{\sigma-1}}$$

which yields the following first-order condition: $P_{fs} Y_{fs} = P_s Y_{fs}^{\frac{\sigma-1}{\sigma}} Y_s^{\frac{1}{\sigma}}$. Each firm s produces using a Cobb-Douglas production function: $Y_{fs} = A_{fs} K_{fs}^{\alpha_s} L_{fs}^{1-\alpha_s}$ and faces wedges τ_{fs}^K and τ_{fs}^L on capital and labor, respectively. The firm's first-order conditions write

$$\begin{aligned} \text{MRPK}_{fs} &= \frac{\sigma-1}{\sigma} \alpha_s \frac{P_{fs} Y_{fs}}{K_{fs}} = r(1 + \tau_{fs}^K) \\ \text{MRPL}_{fs} &= \frac{\sigma-1}{\sigma} (1 - \alpha_s) \frac{P_{fs} Y_{fs}}{L_{fs}} = w(1 + \tau_{fs}^L) \end{aligned}$$

Define $\text{TFPR}_{fs} = P_{fs} A_{fs}$. We can show that:

$$\begin{aligned} \text{TFPR}_{fs} &= \tilde{\kappa}_s \text{MRPK}_{fs}^{\alpha_s} \text{MRPL}_{fs}^{1-\alpha_s} \\ &= \kappa_s (1 + \tau_{fs}^K)^{\alpha_s} (1 + \tau_{fs}^L)^{1-\alpha_s} \end{aligned}$$

where $\tilde{\kappa}_s = \frac{\sigma}{\sigma-1} \alpha_s^{-\alpha_s} (1 - \alpha_s)^{\alpha_s-1}$ and $\kappa_s = \frac{\sigma}{\sigma-1} \frac{r^{\alpha_s} w^{1-\alpha_s}}{\alpha_s^{\alpha_s} (1-\alpha_s)^{1-\alpha_s}}$ are constant within sectors.

Write sector-level output as $Y_s = \text{TFP}_s K_s^{\alpha_s} L_s^{1-\alpha_s}$ where $K_s = \sum_f K_{fs}$ and $L_s = \sum_f L_{fs}$. Sector-level TFP is given by:

$$\text{TFP}_s = \frac{\left(\sum_f \frac{A_{fs}^{\sigma-1}}{(1+\tau_{fs}^K)^{\alpha_s(\sigma-1)} (1+\tau_{fs}^L)^{(1-\alpha_s)(\sigma-1)}} \right)^{\frac{\sigma}{\sigma-1}}}{\left(\sum_f \frac{A_{fs}^{\sigma-1}}{(1+\tau_{fs}^K)^{1+\alpha_s(\sigma-1)} (1+\tau_{fs}^L)^{(1-\alpha_s)(\sigma-1)}} \right)^{\alpha_s} \left(\sum_f \frac{A_{fs}^{\sigma-1}}{(1+\tau_{fs}^K)^{\alpha_s(\sigma-1)} (1+\tau_{fs}^L)^{\sigma-\alpha_s(\sigma-1)}} \right)^{1-\alpha_s}}$$

Using a second order approximation or a log-normality assumption on $\log(A_{fs})$, τ_{fs}^K and τ_{fs}^L , we obtain:

$$\log \text{TFP}_s = \log \text{TFP}_s^* - \frac{\sigma-1}{2} \text{Var}(\log(\text{TFPR}_{fs})) - \frac{\alpha}{2} \text{Var}(\log(\text{MRPK}_{fs})) - \frac{1-\alpha}{2} \text{Var}(\log(\text{MRPL}_{fs}))$$

where the variance is taken over all firms within each sector and $\text{TFP}_s^* = \left(\sum A_{fs}^{\sigma-1} \right)^{\frac{1}{\sigma-1}}$. Define $\tau_{fs} = \alpha_s \tau_{fs}^K + (1 - \alpha_s) \tau_{fs}^L$. Using the fact that wedges are small, we can rewrite this as:

$$\log \text{TFP}_s = \log \text{TFP}_s^* - \frac{\sigma-1}{2} \text{Var}(\tau_{fs}) - \frac{\alpha}{2} \text{Var}(\tau_{fs}^K) - \frac{1-\alpha}{2} \text{Var}(\tau_{fs}^L)$$

I repeatedly use the approximations $\log(\text{TFPR}_{fs}) = \tau_{fs}$, $\log(\text{MRPK}_{fs}) = \tau_{fs}^K$, and $\log(\text{MRPL}_{fs}) = \tau_{fs}^L$. They are innocuous since the sector-level constants do not affect the variance.

Data and definitions. I work with the administrative firm-level data described in Section 2 and further detailed in Appendix G.

Definitions. Nominal output $P_{fs}Y_{fs}$ is defined as value added (gross sales minus intermediate input costs). Labor is defined as the wage bill. The capital stock is defined as the value of tangible assets, net of depreciation. MRPK and MRPL are defined as $MRPK_{fs} = \alpha_s \frac{P_{fs}Y_{fs}}{K_{fs}}$ and $MRPL_{fs} = (1 - \alpha_s) \frac{P_{fs}Y_{fs}}{L_{fs}}$. Omitting the multiplicative factor $\frac{\sigma-1}{\sigma}$ is innocuous. From MRPK and MRPL, I compute TFPR as $TFPR_{fs} = MRPK_{fs}^{\alpha_s} MRPL_{fs}^{1-\alpha_s}$, again omitting the sector-level constant $\frac{\sigma}{\sigma-1} \alpha_s^{-\alpha_s} (1-\alpha_s)^{\alpha_s-1}$.

Estimation of the production function. I estimate industry-specific Cobb-Douglas production functions at the 2-digit level using the cost shares method, as in Osootimehin (2019) and Blattner, Farinha, and Rebelo (2020). Namely, I define the labor share as the ratio of sectoral labor compensation over value added.¹⁰¹

Baseline estimation of the TFP loss. I estimate the following regression:

$$\Delta\tau_{ft} = \beta_0 FirmExposure_{ft} + \beta_1 FirmExposure_{ft} \times \mathbb{1}[High \tau_{f,t-1}] + \Phi \cdot \mathbf{X}_{ft} \otimes \mathbb{1}[High \tau_{f,t-1}] + \varepsilon_{ft}$$

The outer product denotes that I include all interacted and non-interacted terms. I define $\hat{\tau}_{ft} = \tau_{f,t-1} + \hat{\Delta}\tau_{ft}$ where $\hat{\Delta}\tau_{ft}$ is the fitted value from the regression. $\hat{\tau}_{ft} - \tau_{ft}(0) = \hat{\beta}_0 FirmExposure_{ft} + \hat{\beta}_1 FirmExposure_{ft} \mathbb{1}[High \tau_{f,t-1}]$ yields $\tau_{ft}(0)$. I proceed similarly for τ^K and τ^L . I can then compute the TFP loss in (7).

Alternative quantification based on Sraer and Thesmar (2020). I provide an alternative quantification of the TFP loss relying on the same framework but using the alternative estimation strategy proposed in Sraer and Thesmar (2020). The focus is on capital misallocation and omits labor misallocation. This method directly estimates the effect of the shock on the moments of interest, by comparing changes in the mean wedge, the variance of the wedge and the covariance between the wedge and sales, across exposed (treated) and non-exposed (control) firms. To compute these moments, I discretize the treatment by defining 20 quantiles of $FirmExposure$, indexed by q . For each date×industry×quantile cell, I compute the the mean $\log(MRPK)$ $\mu(qst)$, the variance of $\log(MRPK)$ $\sigma^2(qst)$, and the covariance between $\log(MRPK)$ and $\log(\text{sales})$ $\sigma_{lpy,lmrpk}(qst)$ at time t and $t-1$. I take the first difference and call these variable ΔM_{qst} , where M stands for “moments”. I then collapse the data at the date×industry×quantile level, taking the average of $FirmExposure_{ft}$ and firm-level controls \mathbf{X}_{ft} . I estimate the following regression:

$$\Delta M_{qst} = \beta FirmExposure_{qst} + \Phi \cdot \mathbf{X}_{qst} + \varepsilon_{qst}$$

It is important to include the average of the firm-level controls since the orthogonality condition that supports the causal interpretation of β is conditional on these controls. The fixed effects of the baseline regression cannot be absorbed here. To circumvent this issue, I run the firm-level specification with $\Delta\tau_{ft}^K$ as outcome, store the estimated fixed effects, take their average by date×industry×quantile and use these as controls. By construction, estimating this regression with $\Delta\mu(qst)$ as the firm-level regression with $\Delta\tau_{ft}^K$ as the outcome. For the other moments, the assumption is that the city, industry and bank effects affect $\Delta\sigma^2(qst)$ and $\Delta\sigma_{lpy,lmrpk}(qst)$ in the same way as $\Delta\mu(qst)$.

Using this specification, I can predict the counterfactual change in the three moments M_{qst} in the absence of crowding out. I define $\widehat{\Delta\sigma^2}(qst) = \beta^{\sigma^2} FirmExposure_{qst}$, $\widehat{\Delta\mu}(qst) = \beta^\mu FirmExposure_{qst}$ and $\widehat{\Delta\sigma_{lpy,lmrpk}}(qst) = \beta^{\sigma_{lpy,lmrpk}} FirmExposure_{qst}$. Sraer and Thesmar (2020) show that the change

101. This implicitly assumes that there is no misallocation across sectors.

in aggregate TFP is given by:

$$\begin{aligned} \Delta \log \text{TFP}_t \approx & -\frac{\alpha^*}{2} \sum_{s,q} \kappa_{qst} (1 + \alpha_s(\sigma - 1)) \widehat{\Delta \sigma^2}(qst) \\ & - \sum_{s,q} (\alpha_s \phi_{qst} - \alpha^* \kappa_{qst}) \left(\widehat{\Delta \mu}(qst) + \Delta \widehat{\sigma_{lpy,lmrpk}}(qst) + \frac{1}{2} \alpha_s (\sigma - 1) \widehat{\Delta \sigma^2}(qst) \right) \end{aligned}$$

where κ_{qst} is the share of cell $q \times s$ in total capital, ϕ_{qst} is the share of cell $q \times s$ in total sales, α_s are industry-specific capital shares and α^* is the sales-weighted capital share.

I find that crowding out reduces allocative efficiency, and through this channel, aggregate output by 0.03% each year, on average. This is equivalent to an output loss of 8 cents per euro of local government loans. This method only accounts for capital misallocation. Repeating my baseline computation accounting for capital misallocation only, I find an output loss equal to 0.04%, or equivalently, 8 cents per euro of local government loans. Hence, the two quantification strategies yield very similar results.

Alternative quantification based on Petrin and Levinsohn (2012). I provide an alternative quantification of the TFP loss due to misallocation using the decomposition of TFP growth in Petrin and Levinsohn (2012). Petrin and Levinsohn (2012) show that in general a first order approximation of the change in the Solow residual is given by:

$$\Delta \log \text{TFP} = \sum_f D_f \Delta \log A_f + \sum_f D_f \sum_{x \in K, L, M} (\varepsilon_{fx} - s_{fx}) \Delta \log X_f$$

where D_f is the ratio of firm f sales to total net output, K, L, M are capital, labor and intermediate inputs, ε_{fx} are production function elasticities and s_{fx} are income shares. The reallocation component corresponds to the second term. This expression does not require any assumptions about returns to scale, cross-good aggregation, or the shape of input-output networks. Note that we can equivalently write $\varepsilon_{fx} - s_{fx} = \varepsilon_{fx} \frac{\tau_{Xf}}{1 + \tau_{Xf}}$ using input wedges. This formula thus says that TFP increases if we reallocate input X from firms with a low τ_{Xf} to firms with a high τ_{Xf} . As highlighted by Osotimehin (2019) and Baqaee and Farhi (2020), this framework, used in the growth accounting literature, is conceptually different from the previous one. Hsieh and Klenow (2009) quantify the TFP loss from a change in wedges, holding constant other factors affecting the allocation of inputs. Petrin and Levinsohn (2012) quantify the effect of a change in the allocation of inputs given ex-ante wedges. The former is more appropriate in my setting, since the shock to wedges (the credit supply shock) is the first element of the causal chain and causes the reallocation of inputs. I provide this quantification as a further robustness check.

To implement this methodology, I use estimates of the effect of *FirmExposure* on $\Delta \log K_f$, $\Delta \log L_f$, $\Delta \log M_f$, where I allow the effect to depend on ex-ante wedges. To compute ε_{fx} , I estimate gross output production function using the Cobb-Douglas constant returns to scales assumption with sectoral cost shares. I compute cost shares as wages over revenues for L , intermediates over revenues for M and I compute the cost share of K as one minus the cost shares of L and M . I proceed similarly for firm-level cost shares. With all these in hand, I can compute the second term in the equation above.

I find a TFP loss equal to 0.03% per year, or equivalently, a loss of 12 cents per euro of local government loans.

G Data

This paper uses data collected from Banque de France. The data was accessed through the Banque de France virtual Open Data Room.¹⁰²

Disclaimer: The data on firms, households and financial institutions made available to researchers in the Banque de France Open Data Room are anonymized granular data and aggregate series collected or produced by the Banque de France. These data are not marketable. Any use and processing of these data, by any method or on any medium whatsoever, carried out as part of the research work with a view to publication or otherwise, is the sole responsibility of the author. The results of the research work carried out using the data made available in the Open Data Room belong to the author and cannot be considered as representing any opinion or position of the Banque de France. Under no circumstances can the Banque de France be held liable for the consequences—financial or otherwise—resulting from the use of the data or information provided in its Open Data Room.

Credit registry. The main data source used in this work is the French credit registry administered by Banque de France. The credit registry collects data on borrowers with total exposure (debt and guarantees) above 25,000 euros toward banks operating in France. For each entity-bank pair, I recover outstanding credit for each month from 2006 to 2018.

I focus on borrowers located in mainland France. I exclude borrowing by financial institutions (industry K) to exclude inter-bank lending. I implement a number of filters based on firms' legal status (*Code Categorie Juridique*). I exclude legal forms implying public-private partnerships (legal status 5415, 5515, 5615, 5546, 5547, 5646, 5647) as well as non-standard legal forms (e.g. non-profits, foundations, unions, etc. corresponding to legal status 8xxx and 9xxx). I exclude real estate investment trusts (legal status 6540, 6541 and size code 7). Finally, I exclude sole proprietorships (legal status 1xxx) due to a change in the reporting of these loans in the credit registry in 2012.

The French banking sector experienced a significant consolidation over the sample period, which is reflected by the number of banks decreasing from 506 in 2006 to 409 in 2018. In the period in which the merger and/or acquisition takes place, this induces large errors in the bank-level growth rates. I circumvent this issue by excluding observations for which the bank-level growth rate of total lending is equal to -1 (bank exit) or larger than +1 (proxy for the bank acquiring another bank).

I define credit as total credit with initial maturity above 1 year (variable *Tot MLT* in the credit registry). I classify entities as local government entities or private corporations based on their legal status. All entities with legal status 4xxx and 7xxx are classified as local government entities. All other entities (after applying the filters described above) are considered private corporations. Unless stated otherwise, all locations correspond to the geographical identifier of the borrower. The credit registry provides the location at the commune level. Based on this information, I assign each borrower to a given municipality and region, using time-invariant commune-to-municipality and commune-to-region mappings. I use regions before the 2015 redistricting. For the quarterly analysis, I keep all beginning-of-quarter months. In the yearly analysis, I take the average credit over the last 3 months of the calendar year.

In Section 4.3, I construct municipality-level shares of Dexia. I identify Dexia from the anonymized data using the fact that Dexia has the largest aggregate market share in 2008. I check that yearly aggregate lending by this bank matches the figures from Dexia annual reports.

Corporate tax-filings. I obtain firms' balance sheet and income statements from the corporate tax-filings collected by Banque de France, which are the tax-filings for firms with revenues above 750,000 euros (*FIBEN*).

102. The application procedure is detailed at <https://www.banque-france.fr/en/statistics/access-granular-data/open-data-room>

Banks' regulatory filings. I obtain banks' financial information from the financial reporting system used by Banque de France for financial institutions (*BAFI* until 2010, *SURFI* afterwards). I obtained *BAFI* time-series for 2006-2017 and *SURFI* for 2010-2018. *BAFI* and *SURFI* have slightly different definitions, and the *BAFI* data obtained through the data room has only broad balance sheet aggregates. To build consistent time series, I predict the 2018 *BAFI* variables using the corresponding item in *SURFI*. To avoid having missing values for my control variables, I interpolate the *BAFI* time series in case of missing values.

International statistics on local government expenditures and debt. The data for the share of local governments in total government expenditures and debt comes from the OECD/UCLG World Observatory on Subnational Government Finance and Investment (SNG-WOFI). The data is for 2016, for all countries with government debt higher than \$75bn in 2016 (except Lebanon, New Zealand and Pakistan due to data unavailability). The data for local government debt-to-GDP over time comes from the IMF Government Finance Statistics database. The sample is composed of all countries with government debt higher than \$75bn in 2016 for which data exists since 1990 in the IMF data (Australia, Belgium, Canada, Denmark, Germany, Hungary, Italy, Japan, Netherlands, Norway, Russia, South Africa, Spain, Sweden, Switzerland, UK, US), to which I added China (NAO and National Bureau of Statistics, 2019 estimates from S&P Global Ratings and Rhodium Group), India (Reserve Bank of India), Brazil (Banco Central do Brasil), and France (INSEE). SNG-WOFI and IMF-GFS provide cross-country data harmonized on a best efforts basis and do not always corresponds to official national sources.