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

Noémie Pinardon-Touati *

November 4, 2021

Abstract

Local government expenditures are increasingly financed by debt, mostly consisting in 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 debt dynamics 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 relative 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, as typical multiplier estimates range from 0.5 to 1.9. The severity of crowding out depends on the elasticity of banks' credit supply. These results show that constraints on financing supply shape the real effects of debt-financed government spending.

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

*HEC Paris. Contact: noemie.pinardon-touati@hec.edu. I am grateful to Denis Gromb, Johan Hombert, Adrien Matray, David Sraer, and Boris Vallée for their guidance and support. Thanks to Paul Beaumont, Efraim Benmelech, Markus Brunnermeier, Jean-Edouard Colliard, François Geerolf, Vicente Cuñat, Anne-Laure Delatte, Victoria Ivashina, Augustin Landier, Christine Parlour, Julien Sauvagnat, Christophe Spanjaers, and Vikrant Vig for helpful comments and discussions. I thank conference participants at the Swiss Winter Conference on Financial Intermediation 2021, Dauphine Finance PhD workshop, and and seminar participants at the University of Zurich, Dauphine Macro-Finance seminar series, Toulouse School of Economics Finance PhD workshop, HEC Paris brownbag, and Rennes University for valuable comments. I am grateful to Banque de France for granting access to their data. The views expressed herein are those of the author and do not necessarily reflect those of Banque de France.

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 this 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.

[Figure 1 about here.]

Local—as opposed to central—government debt mostly consists in bank loans: in large developed and developing countries, bank loans account for 80% of local government debt (Fig. 1).[2] Crowding out is plausible in the case of bank loans: banks' credit supply being notably constrained,[3] an increase in local government loans may indeed reduce aggregate corporate credit. Moreover, another effect arises from the banking system being fragmented, i.e. from frictions preventing capital from flowing across banks and borrowers from switching banks. 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 firms may also lower aggregate output.

This paper empirically 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 register data covering all bank loans to private firms and local governments combined with corporate tax-filings.

The main challenge—and the reason why little is known about crowding out—is identification. First, local government debt reacts endogenously to firms' prospects. Second, even exogenous shocks to local government debt would 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 fragmentation: I ask whether an increase in local government lending by a given bank disproportionately crowds out that bank's corporate credit supply, and investment and employment for its borrowers. Second, combining the estimated relative effects and a model, I quantify the drop in *aggregate* output due to crowding out. I take into account the effect on aggregate investment and employment, and that on the distribution of inputs across firms.

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

In a nutshell, 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 credit supply elasticity. That is, crowding out is more severe with more constrained banks. Distributional inefficiencies due to bank fragmentation are negligible. All these effects are orthogonal to political interference. 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 for central government debt. By identifying a causal effect and studying its determinants, I therefore also test a theory relevant for all government debt.

I investigate crowding out by bank loans to French local governments, by which I mean all local government entities.[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. 30% of firms borrow from multiple banks, and they account for 70% of the total corporate credit volume. The distribution of bank size is highly skewed: there is a large number of medium-sized local banks (operating in at most two regions) and three large national banks present in all regions.

I first document relative crowding out effects across banks. The parameter of interest is the effect of a bank-level demand-driven change in local government lending on bank-level corporate credit supply. For identification, I rely on the fact that crowding out predicts a bank-level relationship, while the other endogenous relationships between local government debt and corporate credit operate at the firm level with no bank-specific dimension. My research design compares, for a given firm, changes in corporate credit across banks subject to different local government debt demand shocks. The within-firm estimator (Khwaja and Mian, 2008) removes any endogenous relationship between local government debt and firm-level determinants of credit demand. I construct bank×region-specific local government debt demand shocks, exploiting the fact that banks' pre-determined geographic implantation across municipalities generates heterogeneous exposure to local government debt growth. 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 its 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.

4. I.e. 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.

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.[5] Importantly, my results are similar when excluding state-owned banks, suggesting that crowding out is not solely due to political pressure on banks.[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 a positive local government debt demand shock at other banks. In addition to providing a robustness check of my results, this study allows me 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. of why banks do not increase total lending to accommodate local government debt demand while maintaining their 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 aggregate credit supply at the banking system level, I also find bank fragmentation 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 debt 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, e.g. 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.

This constrained credit supply implies that banks must adjust their corporate credit in response to a demand-driven increase in local government loans. I find the adjustment to occur through both a reduction in quantities and an increase in interest rates, albeit to a lesser extent. Besides, banks mostly cut credit to small and unrated firms. Investigating different explanations, I 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.

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

5. The magnitude is in line with evidence on banks' constraints e.g. Paravisini (2008) or Chakraborty, Goldstein, and MacKinlay (2018).

6. A distinct question is whether banks take on a supra-optimal *level* of local government loans e.g. because they are advantageous for regulatory or political reasons. However, I study the effect of the *marginal* euro of local government loans on corporate credit.

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 model. The identifying assumption is that, conditional on controls, there are no shocks to real outcomes correlated with bank affiliation. I perform 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 corporate investment and a €0.06 reduction in wages for firms borrowing from this bank. 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.[7] 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 fragmented 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) fragmented. This model allows me to define formally the relative crowding out coefficient—the 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 fragmented, 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

7. 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.

loss of €0.18 per euro of local government loans.[8]

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 Hsieh and Klenow (2009)'s framework. I find that crowding out reduces aggregate output by 0.04% each year due to a decline in allocative efficiency, equivalent to an output loss of €0.12 per euro of local government debt. 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. The dispersion in firm-level exposure to crowding out due to bank fragmentation has a negligible effect.

Aggregating these effects, an additional €1 of local government debt reduces corporate output by €0.3 (0.18+0.12) through debt crowding out. This implies that the output multiplier of debt-financed local government spending would be higher by 0.3 absent crowding out. This loss is the result of an imperfectly elastic aggregate supply of credit and of heterogeneity in firms' marginal product of inputs. Distributional inefficiencies due to fragmentation—i.e. the effect most specific to crowding out operating through banks—are negligible.

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, or more generally of the supply of loanable funds. Third, I uncover and quantify distributive effects of crowding out when lenders are fragmented 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 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 compared to estimates of debt-financed multipliers, that range from 0.5 to 1.9 (Ramey, 2019).[9] This may be especially problematic during crises, when local government debt tends to soar while banks are particularly constrained. Second, crowding out can be

8. 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 output loss quantification is again a lower bound.

9. The literature has estimated both debt-financed and transfer-financed fiscal multipliers. The former would be higher absent crowding out. The latter (estimates ranging from 0.8 to 4) would be lower if this spending was financed by debt, notably because of crowding out.

mitigated if local governments borrow from less constrained lenders. This argues for alleviating constraints on banks' credit supply, e.g. by limiting regulatory constraints.[10] If bond markets have a more elastic supply of funds, bond financing of local government debt would reduce crowding out. Finally, 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 Elmendorf and Mankiw (1999) or Hubbard (2012) for reviews). Virtually all of it focuses on government bonds and relies 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 an increase in banks' holdings of sovereign bonds—due to political pressure during the European sovereign debt crisis in Becker and Ivashina (2017) or to a strong home bias in holdings of Colombian sovereign debt in Williams (2018)—crowds 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 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

10. This also argues for a reduction in banks' market power which effectively reduces the the elasticity of supply of deposits (Drechsler, Savov, and Schnabl (2017)).

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 substituability 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)). Firm-level effects may also differ, with politically-connected firms shielded from crowding out (Huang, Pagano, and Panizza (2020)).

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 indebtedness on the real economy, e.g., Sauvagnat and Vallée (2021).

Third, this paper adds to the literature documenting that banks' funding constraint significantly limited their ability to expand their credit supply.[15,16] In this resoect, 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.[17] 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. Besides, I document the consequences of banks' funding constraints for the transmission of government spending financed by banks.[18]

Fourth, I add to the empirical literature on the role of financing constraints as a driver of input misallocation (Gopinath, Kalemli-Özcan, Karabarbounis, and Villegas-Sanchez (2017), Larrain and Stumpner (2017), Blattner, Farinha, and Rebelo (2019), Banerjee, Breza, Townsend, and Vera-Cossio (2019), Bau and Matray (2020), Schivardi, Sette, and Tabellini (2021)).

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

2 Data and institutional setting

2.1 Data

My main data source is the French credit register administered by Banque de France and collecting data on borrowers with total exposure (debt and guarantees) above 25,000 euros towards 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

government production can only occur at the expense of private sector activity).

15. Key contributions include Gertler and Gilchrist (1994), Kashyap et al. (1994), Peek and Rosengren (1997), Kashyap and Stein (2000), Campello (2002), Khwaja and Mian (2008), Paravisini (2008), Loutskina (2011), Schnabl (2012), Cetorelli and Goldberg (2012).

16. Many of those studies do so by analyzing the effect of bank-specific shocks to funding constraints on bank-level credit supply, incidentally highlighting the important fragmentation of the banking sector.

17. 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.

18. 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 very different from the mechanism I describe.

one year to avoid measurement issues related to credit lines. Banks correspond to legal entities, and 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.[19] 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 files collected by Banque de France, which are the tax files 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 register provides the location of borrowers. The paper sorts borrowers into geographic units using two levels of geographic subdivisions of France. First, regions are the largest administrative division of France. There are 22 regions in mainland France. Second, I use the level of intermunicipal cooperations, which I refer to as municipalities. There are 2081 such municipalities. Figure A.2 shows these subdivisions on a map.

[Figure 2 about here.]

[Table 1 about here.]

2.2 Institutional details

French banks. There are two 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 (Fig. A.3). Second, a large share of banks are local banks: banks operating in 2 regions or less account for 50% of total corporate lending. In particular, banks belonging to cooperative networks are local banks, mostly following regional boundaries.

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

19. 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.

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 very dynamic growth at the beginning of the period, followed by a much more subdued growth since 2014, 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.4, local governments indeed represent the largest share, followed by public hospitals, state-owned public service operators, and public housing.[20,21] Rules on subnational entities borrowing imply that local government entity debt finances mostly investment expenditures, as opposed to operating expenditures. This is reflected in the relatively long maturity of local government loans (15 years on average).

Loans to local governments are also large from the point of view of banks. Loans to local governments account for 40% of the total credit to local governments and corporations combined (30% when excluding state-owned banks), as depicted on panels (a) and (b) of Figure A.5.[22] However, as shown on panels (c) and (d), this hides 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.

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, local governments tend to borrow from local banks: local governments located in one municipality have a 0.97 probability of borrowing from a bank branch located in the same municipality.[23] Combined, these two facts induce a large geographical heterogeneity in the dynamics of local government debt, 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 a large degree of 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 to identify crowding out exploits (the plausibly exogenous part of) this heterogeneity.

[Figure 3 about here.]

[Figure 4 about here.]

20. 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.

21. When I refer to local governments in a given municipality or region, I include all local governments in that geographical unit and not only the municipality or the region as a local government.

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%.

23. This number is the same for corporates.

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.[24] The basic mechanism is that if banks' aggregate credit supply is imperfectly elastic, an increase in local governments' loan demand will reduce the aggregate supply of credit available to private firms. This mechanism is depicted on the simple supply and demand graph in Figure A.1. From the point of view of private firms, crowding out is akin to a shift in banks' residual supply curve, after local government demand is satisfied. The aggregate crowding out effect reflects the elasticity of banks' credit supply and the elasticity of firms' credit demand. Namely, crowding out is more severe when banks' credit supply is less elastic and when corporate credit demand is more elastic.

When banks are fragmented, crowding out has a bank-specific dimension: a larger increase in local government debt demand directed to 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 local government debt demand would draw in capital from other banks and any reduction in corporate credit would be uniform across banks. Besides, 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 fragmented is testable: if false, there would be no relative crowding out effect.[25]

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 (e.g. as in Temin and Voth (2005)).

The goal of this paper is to quantify the aggregate crowding out effect. To do so, I first document causal relative crowding out effects across banks, and subsequently firms. The relative crowding out parameters that I estimate jointly capture the crowding out effect and the degree of bank fragmentation. This relative effect—which is conceptually different from the aggregate effect—is useful for two reasons. First, as shown in Section 7, the properly identified relative crowding out effect can serve as an input to quantify the aggregate crowding out effect. That is, relative crowding out is informative of the extent of aggregate crowding out. Second, it allows me to investigate the distributive effects of crowding out operating through fragmented intermediaries.

24. The focus is on *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.

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

4 Cross-sectional crowding out: corporate credit

4.1 Empirical strategy

I present the methodology to investigate the relationship between bank-level increases in local government loans and corporate credit supply. To clarify the identification strategy, it is useful to look at the structural equations obtained from a simple perfectly competitive model of fragmented banks:[26]

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

ΔC_{fbt} is bank×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 or a demand 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 says that bank×firm-level credit growth depends on the firm-level shock, the bank-level shock, and the bank-specific increase in local government loans if $\beta \neq 0$. The second equation says that the bank-level increase in local government loans depends on the demand addressed to this specific bank and on the bank-level shock. β is the structural relative crowding out parameter that we want to estimate.

The first hurdle to obtain an unbiased estimate of β is the endogeneity of local government debt with respect to firm-level shocks θ_{ft}. If local government debt is used as a countercyclical policy tool, changes in local government debt will be negatively correlated to θ_{ft}. Conversely, if local government debt generates positive demand shocks for private firms through multiplier effects, this would induce a positive correlation between θ_{ft} and ΔC_{bt}^{gov}. I address the identification problem by focusing on firms with multiple lending relationships and adding firm×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 to changes in local government debt—are symmetric across lenders, they will be absorbed by the fixed effects.[27] Intuitively, the identification of crowding out relies on the fact that these other endogenous relationships predict a correlation between local government debt and *firm*-level credit *demand*, while crowding out operates as a shock to the *bank*-specific *supply* of credit that 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 and are therefore both affected by bank-specific

26. See equation (15) in Appendix C.

27. 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)).

shocks ξ_{bt}. For instance, if the 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:

$$BankExposure_{bt} = \sum_{m} \omega_{bm,t-1}^{gov} \times \Delta C_{mt}^{gov} \tag{1}$$

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 driven by the fact that b is ex-ante located in places where local governments happen to borrow more or less in the current period. The shift-share structure abstracts from the potential correlation between ΔC_{fbt} and the bank-specific component of the 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, the bank, the bank×location, or the branch. Identifying relative crowding out effects requires that units are fragmented, 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×region level.[28,29] My baseline specification is:

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

with r the region where firm f is located. d_{ft} is a firm×time fixed effect and $\mathbf{X}_{fbrt}$ is a vector of controls. 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 margin (Davis and Haltiwanger (1992)). The 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.[30] $BankExposure_{brt}$ is defined accordingly as the weighted sum of ΔC_{mt}^{gov} for municipalities m in region r, with weights $\omega_{bm,t-1}^{gov} = \frac{C_{bm,t-1}^{gov}}{C_{br,t-1}^{tot}}$ normalized by banks' total loan portfolio. I control for the sum of weights $\omega_{br,t-1}^{gov}$, equal to the share of local government loans in the banks' total loan portfolio, as recommended by Borusyak, Hull, and Jaravel (2020).[31] In robustness checks, I use alternative definitions of these variables. My main results present the reduced form

28. For regional banks, this coincides with the bank level. For national banks, regions are typically the main operating subdivision. The hypothesis that there is fragmentation across bank×regions is testable.

29. Section 5 investigates crowding out at the bank, bank×region or bank branch level.

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

31. 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.

effect of $BankExposure$ on ΔC_{fbt} and I use the IV to provide the relevant magnitudes.

Estimating specification (2) will yield an unbiased estimate of β if the standard exclusion restriction is satisfied: $\mathbb{E}[BankExposure_{brt}\varepsilon_{fbt}|\mathbf{X}_{fbrt}, d_{ft}] = 0$. Following the preceding discussion, this equation will be satisfied if two conditions are met. First, the firm-level shocks that may be correlated with local government debt must be evenly spread across its lenders, so that they are absorbed by the fixed effects. Second, $BankExposure$ must not be systematically correlated to other bank-level shocks. The assumption is that banks do not sort into locations such that unobserved bank-level shocks are correlated to both increases in local government loans in the locations in which the bank operates and a decline in corporate credit supply (Borusyak, Hull, and Jaravel (2020)).[32] Figure 5 tests whether $BankExposure$ is systematically correlated to banks' observable characteristics. I report both unconditional correlations and correlations conditional on $\omega^{gov}_{br,t-1}$ the share of local government loans in the banks' total loan portfolio. The unconditional correlations highlight large 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 debt market—and thus are more likely to have high exposure—and others: once we condition on $\omega^{gov}_{br,t-1}$, the differences essentially disappear. This alleviates the concern that $BankExposure$ is systematically correlated to other bank-level shocks. In Appendix D, I present further tests related to the shift-share structure of the instrument.[33] Section 4.2.2 additionally presents extensive evidence that supports my two identifying conditions.

[Figure 5 about here.]

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. Section 4.2.2 presents results for alternative specifications.

32. It is *not* a problem that banks sort into locations based on sectoral specialization, types on clientele, business dynamism, so that banks with high and low $BankExposure$ lend to firms with different credit demand. The firm$\times$time fixed effects precisely control for these differences. What matters is that these heterogeneous geographical footprints are not correlated to other bank-level credit supply shocks that are themselves correlated with the bank's exposure to increases in local government debt.

33. Identification with shift-share instruments can be based on exogenous shares or exogenous shocks (Borusyak, Hull, and Jaravel (2020), Goldsmith-Pinkham, Sorkin, and Swift (2020)). In my setting, the natural assumption is exogenous shocks: since local government debt shocks induce shocks to ΔC_{fbt} that are symmetric across lenders, ΔC^{gov}_{mt} is orthogonal to ε_{fbt} conditional on d_{ft}. Appendix D discusses in further details the shares vs. shocks-based view of identification in my setting.

[Table 2 about here.]

In the first column of Table 2, I investigate the relationship between exposure to government debt shocks and credit supply without any controls or fixed effects, and I find a coefficient equal to 0. 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×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 exposure to higher demand for local government debt significantly predicts lower corporate credit growth. My baseline specification is column (3) which includes firm×time fixed effects as well as controls to ensure that the coefficient of interest is not driven by variation in other banks' characteristics. The point estimate remains similar, slightly larger in absolute value.

The comparison between column (1) and column (2) and (3) suggest that the endogenous bias plays in a direction opposite to the crowding out effect, which would typically arise if local government debt has significant multiplier effects.

Columns (4) and (5) provide the result of the estimation when $BankExposure$ is used as an instrument for the actual actual increase in loans to local governments ΔC^{gov}_{brt}, with and without controls. The point estimate suggests that on average, an increase in local government debt equal to 1% of total portfolio size in a given bank-region reduces credit growth for borrowers of this bank located in this region by 0.96%.[34] To further gauge the magnitude of this effect, I perform a back of the envelope calculation to express it as a euro for euro crowding out parameter. If one additional euro of government debt must be absorbed by the average bank×region, then ΔC^{gov}_{brt} would increase by 1 divided by the average C^{tot}_{brt} (€1,927M). The decline in ΔC_{fbt} then equals $0.954 \times \frac{1}{1{,}927 \times 10^6}$, which for the median firm represents $0.954 \times \frac{235 \times 10^3}{1{,}927 \times 10^6} = 0.012$ cents. Considering that the average bank×region lends to 4678 firms, this leads to a decline in lending by this bank×region of €0.54.

Importantly, these effects isolate the crowding out effect of local government debt operating through the imperfectly elastic supply of bank credit. They hold constant local demand effects of government debt, government debt endogenously responding to private sector financing conditions, and real crowding out (i.e. the fact that when production factors are fully employed an increase in government spending can only happen at the expense of reduced private sector production).

The crowding out parameter estimated here captures banks' ability to increase their balance sheet size. Under the assumption that local governments' demand for loans is not

34. The effect on the mid-point growth rate is 0.954% and I use the transformation $gr = \frac{2MPGR}{2-MPGR}$ to express the result as a standard growth rate.

interest sensitive, the crowding out coefficient is equal to the sensitivity of corporate credit to a change in banks' total funding. A contribution of the paper is therefore to produce a novel estimate of this parameter. Besides, the number that I obtain is consistent with the existing evidence on this topic. The key contribution on this topic 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.

Distortions in the market for local government lending and crowding out. The market for local government loans may be subject to distortions due to political interference, regulation, or a preference of banks for safe assets. If lending to local governments is particularly advantageous for banks, this would lead to a socially supra-optimal *level* of local government loans on banks' balance sheets. However, the crowding out parameter that I estimate is the effect of a *marginal* €1 increase in local government loans on corporate credit. In theory, crowding out is independent of these 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 affect the level of local government lending, in line with the standard theory.

35. See Appendix C.2.7. 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. That said, the answer to this question affects the policy implications of my results: if banks take on excessive local government debt because of some specific distortion, solving this distortion would reduce the aggregate crowded out amount.

4.2.2 Robustness and further tests of the identifying assumption

Discussion of identifying assumptions. This paragraph provides additional tests that further support the validity of my identifying assumptions: (1) firm$\times$time fixed effects absorb firm-level demand shocks that are symmetric across 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$\times$*bank*-level demand shocks. One story would be a form of reverse causality whereby local governments increase their borrowing when they observe that corporate demand directed towards the banks they typically borrow from is low. Regarding (2), one concern is that banks with high $BankExposure$ are systematically subject to negative shocks that put them on a declining trend, independently of local government debt shocks. To alleviate these concerns, I include leads and lags of the independent variable in my baseline specification to show that bank exposure measured at the time of the shock is not correlated with the patterns of corporate credit before the shock. Figure 6 shows the absence of a significant pre-trend.[36]

[Figure 6 about here.]

More granular fixed effects: A story that would violate (1) is if when local government debt rises, corporate demand shifts towards banks that are not active in the market for local government loans. Similarly, (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 banks' loan portfolio. I alleviate this concern by including time fixed effects interacted with a dummy indicating whether the bank is active in the market for local government loans $1[\omega^{gov}_{br,t-1}] > 0$. These results are in Table B.2. I provide further tests of the identifying assumption by including bank$\times$region and bank$\times$time fixed effects. By including bank$\times$region, I control for any time-invariant correlation between $BankExposure$ and corporate credit. While I lose all the variation related to cross-sectional heterogeneity across regions, I find a coefficient in the same order of magnitudes. I then include bank$\times$time fixed effects that control for any time-varying bank-level shocks that may be correlated to bank's exposure to local government debt shocks. This specification is very conservative: it identifies whether *within* bank, regions that see a larger increase in the demand for local government debt experience a larger reduction in bank lending to private firms. While it restricts the focus to within-banks effects, it is useful to show that my effect persists.

36. Estimating the lead and lag coefficients assumes that the treatment effect before -10 quarters and after 10 quarters is zero, otherwise these coefficients are not identified.

Heterogeneity by strength of demand effects: Assumption (1) would be violated 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. Namely, local government debt is used to finance 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.[37] If the firm×time fixed effects were unable to control for firm-level credit demand, we would observe relatively higher credit growth for those 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 smaller) for these firms.

All in all, this evidence provides strong support for the identifying assumptions behind my empirical strategy.

Robustness checks. I perform a variety of 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 or municipalities or any year. Second, B.4 shows results for alternative definitions of the independent variable, e.g. 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 different assumptions about the covariance structure of the errors.

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 towards less affected lenders when one of their lender is shocked, it means that control banks are affected by the shock in a direction opposite to that of treated banks, so that comparing the two—as done by the within-firm estimator—overestimates the true effect. The existing literature does not provide a methodology to obtain an unbiased estimate of β in the case where bank-level credit supply shocks are correlated

37. 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.

with firm-level demand shocks—i.e. the within-firm estimator is essential—and firms may substitute across their lenders.[38,39]

I provide a methodology to separately identify the direct effect of the shock and substitution across banks, allowing 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.

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

4.3 Exploiting the near failure of Dexia as a natural experiment

The *BankExposure* measure defined in Section 4.1 has the value of being general, in that it can be attributed to all firms and measured at any date for which there is bank-firm data on credit granted, which allows me to investigate the crowding out phenomenon in a systematic manner. 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." 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 debt shocks; (ii) it allows me to investigate long-run effects. Namely, I exploit the failure of Dexia—a Franco-Belgian bank specialized in lending to local governments—as an exogenous shock forcing municipalities which relied heavily on Dexia to turn to other lenders, thereby creating a large demand shock of local government debt addressed to other banks. Before 2008, 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 the Belgian government to intervene. Unable to recover, the bank was eventually dismantled in 2013. These events, which came unexpectedly both to lenders and to borrowers, led to a sharp decline in the market share of Dexia. Concurrently, they generated a large shock to the demand for local government loans directed towards other banks, in particular for banks implanted in areas where Dexia was most active.[40] I define the variable *DexiaExposure* at the

38. The within-firm estimator relies on the fact that credit demand shocks are evenly spread across lenders i.e. that from the firm's point of view, credit from different banks are substitutes. It does not imply that firms indeed manage to substitute across banks when some face a shock, but it highlights the importance of accounting for this possibility.

39. 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).

40. This shock was first used in Derrien, Mesonnier, and Vuillemey (2019) and I heavily borrow from this paper's strategy.

bank×region level in a manner very similar to my baseline $BankExposure$ variable:

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

where $DexiaDependent_m$ is a dummy equal to 1 if the market share of Dexia in 2008 is above median and $\omega_{bm,2008}$ are as before the share of each municipality within region r in bank b's total loan portfolio. I posit that banks which 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):

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

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 $DexiaExposure$ indeed predicts the growth rate of local government loans after the failure of Dexia. Columns (3) and (4) show the results of estimating (4) and show that a higher $DexiaExposure$ predicts a lower corporate credit growth, at the 2013 and 2014 horizon. To gauge the magnitude of this effect, columns (5) and (6) show the results of the IV where $DexiaExposure$ is used as an instrument for $\Delta C^{gov}_{br\tau}$. Transforming this coefficient into a euro for euro crowding out parameter, I find that an additional 1€ of local government debt at the median bank-region 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. This shows that even though banks do manage to gradually adjust their balance sheet size, the effect persists over a very long horizon. To further support the 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.

5 Mechanism

I have documented that banks that increase their lending to local governments reduce their supply of credit to the corporate sector. This raises two questions. First, why does crowding out occur i.e. 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 when faced with a local government debt shock?

5.1 What prevents banks from increasing total credit supply?

In theory, the severity of crowding out is determined by the elasticity of the supply of capital of the banks acquiring government debt. This section investigates the various frictions that underlie the low elasticity of the supply of bank credit.

Bank-level frictions on increasing balance sheet size. I first show that at the bank-level, crowding out is more severe for banks that appear more constrained in their ability to increase their supply of credit. Ideally, banks should be able to match the additional demand of credit by an additional supply of capital, by borrowing more (from depositors or from the interbank market) or by raising additional equity. However, banks only have a limited ability to attract more deposits or to raise equity, interbank markets are not frictionless, and banking regulation also constrains the total amount of lending banks can do. To test whether these balance sheet constraints determine the severity of crowding out, I interact $BankExposure$ with various proxies for banks' balance sheet constraints.

The results are presented in Table 3. First, column (1) shows that crowding out is more severe for smaller banks, which are likely to be overall more constrained.[41] Looking at specific constraints, I find that the crowding out effect is stronger for banks with a higher deposit gap (column (2)) and is weaker for banks with a better access to international financing sources (column (3)), emphasizing the importance of banks' access to a large pool of long-term funding. In column (4), I show 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 with the European Central Bank. Column (6) shows that crowding out is slightly less severe for banks with higher capital ratios, but the difference is economically small and not statistically significant.[42] 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 the cross-sectional crowding out effects documented here not only reflect constraints that apply to the aggregate banking system but also constraints on individual banks who have a limited ability to draw in capital from other banks in face of a demand shock.

[Table 3 about here.]

Together, these results are in line with the theory and suggest that crowding out is indeed related to banks' inability to increase their total balance sheet size. I explore two

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

42. One explanation is that leverage ratios significantly matter only when they become binding. In line with this reasoning, I find that during the implementation period of Basel III—i.e. at a time when most banks had to significantly increase their capital ratios—banks that were further away from the target exhibit a stronger crowding out effect (column (7)). Besides, loans to local governments have low capital requirements (between 0 and 20%) depending on the type of entity, so that regulatory capital ratios are unlikely to be the main driver of crowding out.

further implications of this mechanism. 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 my key mechanism: when constrained banks increase their lending to local governments, they are forced to reduce their corporate lending, while when the shock is negative banks have more leeway to adjust. Second, I look at the time-series of the effect by reporting my results across 4 subperiods: the pre-crisis period (2006-2007), the crisis period (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 the last one. A first explanation is that local government debt growth is 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 a very accommodative monetary policy stance 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. Crowding out is therefore 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 go beyond the bank level and 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×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, as shown in the previous paragraph. Within banks, there are constraints on the reallocation of capital across units due to poorly functioning internal capital markets or to the need to incentivize local managers.[43] There are also constraints on the time of 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 at the bank level and shows that when a bank lends more to local governments (averaged across all regions), it disproportionately reduces its corporate credit supply. Column (2) looks at the effect of shocks defined at the bank×region level, as in the baseline specification, conditional on bank×time fixed effects. The negative coefficient shows that within a given bank, corporate credit is lower in regions where lending to local government increases by a larger amount. Column (3) looks at the effect of shocks defined at the branch level conditional on bank×region×time fixed effects and shows that within a given bank×region, branches that increase their lending to local government by a larger amount disproportionately reduce corporate credit. All three coefficients have comparable sizes.

43. 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).

Appendix C.2.3 shows that this can be interpreted as the three levels facing similarly sized frictions to increase the supply of credit. I confirm this by including all three variables in the same specification: the lower-level variable subsumes the other coefficients, which is what is predicted if the size of frictions is similar. These results thus highlight the quantitative significance of within-bank frictions.[44]

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 economically and statistically insignificant: when bank b increases its lending to local governments in regions $r' \neq r$ by €1, it reduces corporate credit in region r by €0.06. Second, the presence of local banks and the existence of frictions across locations within banks imply that crowding out will have a local dimension: aggregating across banks, regions where local governments borrow relatively more will experience a relatively stronger reduction in their corporate credit supply.[45]

[Table 4 about here.]

5.2 How do banks adjust their lending portfolio?

This section investigates how banks adjust their lending portfolio when faced with an increased demand for local government debt. First, does the adjustment operate through prices or through quantity rationing? Second, which type of corporate loans are crowded out the most?

Price vs. quantity adjustment. To investigate the first point, I use the "New contracts" dataset collected by Banque de France, which collects information on interest rates from a representative subset of banks (accounting for around 75% of the total new loan amount in each quarter). I estimate the effect of local government debt shocks on the price of credit using the within-firm specification (2) with the interest rate charged by bank b on new loans to firm f as a dependent variable:

$$i_{lfbt} = d_{ft} + \beta BankExposure_{brt} + \Phi \cdot \mathbf{X}_{fbrt} + \Lambda \cdot \mathbf{W}_l + \varepsilon_{lfbt} \tag{5}$$

where the additional subscript l indexes loans, since a firm can take up several loans from the same bank at the same time. This specification additionally includes loan-level controls: the size of the loan, and maturity bucket×index×type of loan×time fixed effects to absorb changes in the yield curve.

44. Other contributions have similarly highlighted the importance of within-bank frictions, e.g. Scharfstein and Stein (2000) and Cremers, Huang, and Sautner (2011) on inefficiencies on banks' internal capital markets and Chakraborty, Goldstein, and MacKinlay (2018) on banks' personnel constraints.

45. I formally show this in Appendix C.2.3. We could not obtain such causal effect looking at the local-level relationship between local government debt and corporate credit.

This 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 much 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. In the baseline sample, I 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 results of estimating this model are presented in Table A.5. 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: restricting the sample to fixed rate loans, removing the filter on the number of loans per year, and including subsidized loans. Finally, column (7) adds the more restrictive firm×quarter fixed effects. The coefficients are again 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.

The price effect is small compared to the quantity reaction, which would imply a price elasticity of corporate credit demand of the order of magnitude of 30. This is in line with the empirical evidence on loan price stickiness and on bank-level shocks inducing quantity rationing without price adjustments.[46] It is also in line with structural estimations of the price elasticity of corporate credit demand.[47] While this result is usually rationalized by concerns about the adverse selection effects of higher interest rates (Stiglitz and Weiss (1981)), it remains an open question in the literature which is outside the scope of my paper. To summarize, I find that higher demand for local government loans does increase the bank-specific interest rate, but it remains unclear whether the reduction in quantity corresponds to the sole adjustment of firms along their demand curve or is also driven by quantity rationing.[48]

46. On loan rates stickiness, see Berger and Udell (1992). On bank-level shocks inducing quantity rationing without price adjustments, see for instance Khwaja and Mian (2008), Cingano, Manaresi, and Sette (2016) or Bentolila, Jansen, and Jiménez (2018).

47. Note that the elasticity is not-well defined in case of quantity rationing. This is rather a quantity-to-price response ratio. The coefficient estimated here takes into account the extensive margin since it is computed with results on mid-point growth rates and can be compared to Diamond, Jiang, and Ma (2021) who finds an extensive margin "elasticity" of 228.

48. The effect on interest rates further attenuates concerns about the baseline results being driven by a bank-specific demand for credit. In this case, we should find lower rates for more exposed banks, because firm demand for credit to these banks increases. I find instead a combination of lower quantity and higher prices, consistent with a supply shock. Besides, the fact that we find an effect on interest rates within firms further reflects firms' poor ability to substitute across their lenders.

Severity of crowding out and loans' characteristics. A second question is which type of corporate loans are crowded out the most. This is theoretically ambiguous. On the one hand, textbook crowding out theory implies that crowding out is stronger for assets that are closer substitutes to government debt (Friedman (1978)). Crowding out should therefore be more severe for corporate loans that are most similar to local government loans, i.e. loans to large, highly-rated firms with risk profiles correlated to those of local governments. On the other hand, large and safe firms may be shielded from crowding out if banks systematically favor safer lending opportunities: either because lending to safe borrowers allows banks to capture all the surplus instead of leaving an informational rent[49], or because safer loans can be used to meet regulatory requirements[50]. If banks are constrained by the limited supply of safe assets, additional lending opportunities to safe local governments will induce banks to disproportionately cut the riskier part of their lending portfolio.

I investigate these competing hypothesis in Table 5 which shows how the crowding out effect varies along a number of firm characteristics. The results clearly support the second hypothesis over the first one. In response to an increase in loan demand by local governments, 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)), and the effect is the same for firms in sectors that are heavily reliant on public procurement contracts (column (6)), i.e. firms that are likely to have risk profiles highly correlated to those of local governments. These results clearly go against the predictions of the first hypothesis and are in line with the second. Within the second hypothesis, can we disentangle whether banks favor safer assets to avoid information asymmetry or for regulatory reasons? If the preference for safe assets were driven by banking 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 vs. risky. In column (5), I show that crowding out is less severe for banking relationships where the bank is likely to have invested most in information acquisition.[51] All in all, these results suggest that when banks increase their lending to local governments, they disproportionately cut credit to the segment of their corporate loan portfolio where information asymmetry is most problematic.[52]

49. 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.

50. Corporate loans rated as safe can be used as collateral with the European Central Bank and have lower regulatory risk weights.

51. To proxy this dimension, I define a banking relationship as important from the point of view of the bank, if lending to the firm is large compared to the size of the banks' portfolio in the firm's region.

52. On the bonds market, Graham, Leary, and Roberts (2015) and Akkoyun, Ersahin, and James (2020) show that government bonds have a more severe crowding out effect on AAA-rated corporate bonds, in line with the Friedman (1978) hypothesis.

[Table 5 about here.]

6 Cross-sectional crowding out: investment and employment

The previous results show that lenders that are more exposed to increased demand for local government loans reduce their supply of credit to private firms. Besides, I show that this effect is not undone by firms substituting across lenders, so that crowding out will impact firm-level borrowing. How does the reduction in corporate 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 looking at firms' exposure to the shock through their lenders. I present the empirical strategy to investigate the firm-level effects on investment, the strategy being exactly the same for employment. I estimate the following specification:

$$\Delta K_{ft} = \beta^K FirmExposure_{ft} + \Phi \cdot \mathbf{X}_{ft} + \alpha_{mt} + \alpha_{st} + \alpha_{b(f),t} + \varepsilon_{ft} \tag{6}$$

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

$$FirmExposure_{ft} = \sum_b \omega_{fb,t-1} BankExposure_{brt} \tag{7}$$

α_m are municipality×time fixed effects, α_{st} are industry×time fixed effects, and $\alpha_{b(f),t}$ are relationship bank×time fixed effects.[53] $\mathbf{X}_{ft}$ 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, this specification compares firms borrowing from banks experiencing a local government debt demand shock to firms borrowing from other banks.

To understand the logic of the identification, it is useful to go back to the firm×bank-level model (2), which we can aggregate at the firm-level using bank shares to obtain (omitting controls): $\Delta C_{ft} = d_{ft} + \beta FirmExposure_{ft} + \varepsilon_{ft}$. The challenge to obtain an unbiased estimate of β in the firm-level specification is the correlation between $FirmExposure$ and unobserved firm-level shocks d_{ft}. If $BankExposure$ was correlated to d_{ft}, $FirmExposure$ is also correlated to d_{ft}. Following Cingano, Manaresi, and Sette

53. I define a firm's relationship bank as the bank with the largest share in the firm's credit.

(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, which allows to precisely control for the correlation between $FirmExposure$ and d_{ft}. Identification in the firm-level regression then follows from identification in the within-firm specification.[54]

When looking at the effect of $FirmExposure$ on investment, the estimated coefficient β^K can be decomposed as $\beta^K = \eta^K \beta$, 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 the unobservable determinants 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 also 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 variable of interest are credit growth (defined as the mid-point growth rate), investment (defined as the growth rate of fixed assets) and wage growth. These variables are computed using corporate tax files, available for firms with annual turnover above €750,000.[55] Besides, as the frequency of corporate tax files is annual, I repeat the construction of $BankExposure$ and $FirmExposure$ at the yearly frequency. In the baseline specification, I define bank shares as mid-point shares in order to properly aggregate 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, so that my baseline effects are estimated on the sample of all firms with tax filings data.

Figure 5 shows whether firms with higher exposure to banks subject to local government debt shocks exposure are systematically different. I report both unconditional correlations and correlations conditional on the average sum of shares and the estimated firm-level demand shock. Figure 5 shows it again matters to control for the average $\omega_{br,t-1}$. Conditional on this control, firms with high $FirmExposure$ are similar to firms with low

54. 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 will bias the coefficient of interest. 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 on identification and how to deal with the opposite case.

55. Since corporate tax files are not available for all firms, I cannot study firm entry and exit and consider only the intensive margin.

FirmExposure on leverage, tangible ratio, ROA, and interest coverage. It remains that firms with high exposure are slightly larger, have a higher operating margin and a lower cash ratio. I show that my results are robust to controlling for these variables.

6.2 Results

6.2.1 Baseline results

I first repeat the within-firm estimation on the yearly data to obtain the relevant magnitudes and to recover the firm-level demand shocks used as controls. Table A.6 displays the results. I find that the bank-level crowding out parameter is equal to 0.42.

Table 6 presents the firm-level effects obtained from estimating (6). Columns (1) to (3) report the effect of firms' 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 with greater reliance on banks that lend more to local governments invest significantly less. Columns (3) shows that the effect on wage growth is also negative but smaller in magnitude.

To gauge the quantitative significance of my results, I separately estimate η^K and η^L the pass-through coefficients of credit to investment and employment. I do so by using *FirmExposure* as an instrument for firm-level credit growth. The effects are reported in columns (4) and (5). I find an elasticity of investment with respect to credit equal to 0.38, in line with previous evidence. The elasticity of credit to wage growth is equal to 0.11.[56] The weaker effect on employment is likely attributable to the fact that I focus on loans with a maturity above one year, which are typically investment loans rather than working capital loans.

[Table 6 about here.]

These estimates can be used to quantify the effect of an additional €1 in local government debt at one bank to corporate investment at firms borrowing from these banks. Starting from the effect on credit using the credit-to-investment passthrough coefficient η^K, I find that an additional €1 in local government debt at one bank leads to a €0.23 reduction in corporate investment for firms borrowing from this bank. For the total wage bill, the effect is €0.06.[57]

To obtain these estimates, I can alternatively look at the effect on investment and employment when $FirmExposure_{ft}$ is used as an instrument for its "realized quantity" version $\overline{\Delta C^{gov}_{brt}}^f = \sum_b \omega_{fb,t-1} \Delta C^{gov}_{brt}$, which is the average increase in local government

56. In my baseline specification credit growth is defined as the mid-point growth rate. I find similar credit-to-input sensitivities when using the standard growth rate of credit (Table B.10).

57. See computation details in B.2.

loans at the lenders of firm f. Appendix B.2 provides these results and shows that I obtain a €0.24 effect on investment and €0.06 on the wage bill.

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, this may be the case if 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 $\hat{d}_{ft}$.[58] This paragraph provides several additional tests that alleviate this concern.

More granular fixed effects: A key element of the identification is the inclusion of municipality×time, 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.8 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 dynamic, and I again find similar effects.

The magnitude of the coefficient is remarkably stable across the 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 with respect to municipality-level industry-specific trends and to firm invariant characteristics. This is extremely unlikely. A formal econometric treatment of this intuitive 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 including firm fixed effects, both well above the recommended value of 1.[59] Consequently, I can reasonably conclude that correlated unobservables are not driving my results.

Pre-trends: In Figure 7, I show pre-trends for the three different outcomes. The absence of pre-trends alleviates the concern that $FirmExposure$ is systematically related to firms with poor investment opportunities or declining labor demand.

58. For instance, firms facing a low product demand may invest less but at the same time demand more credit since they have lower cash flows and may need to borrow to pay back outstanding liabilities.

59. The interpretation of this parameter is that 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.2 for details.

[Figure 7 about here.]

Heterogeneity by strength of demand effects: I again exploit the fact that firms in industries highly reliant on public procurement contracts firms are likely to experience a positive demand shock when local government debt increases. If my specification was imperfectly controlling for the demand effects of local government debt shocks, I would find that exposure to local government debt shocks has a relatively more positive 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.8).

Taken together, these tests provide support to the credibility of my empirical approach.

Robustness checks. I perform a variety of robustness checks of my results, detailed in Appendix B.2. Table B.9 presents the results when dropping firm-level controls and when including additional firm-level controls. I also 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. In Table B.10, 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 look at 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 look at the effect on credit growth, investment and employment over 2008-13 and 2008-14. The results are presented in Table A.1. I find that banks' exposure to the local government debt demand shock generated by the failure of Dexia has significant real effects for firms borrowing from these banks. I find a significantly negative effect of $FirmExposure$ on investment and wage growth, at the 2013 and the 2014 horizon.I again report placebo regressions where $FirmExposure$ is used to predict 2006-2007 credit growth, and 2001-2007 investment and employment growth (leveraging the fact that the tax filings are available for previous years). I again find no effect.

6.3 Heterogeneous effects

While I have thus far estimated average coefficients, the strength of the effect of crowding out on real outcomes is likely to vary with firm characteristics. Heterogeneity may stem from two different channels. First, banks may cut credit to some types of firms, as shown with within-firm regressions in Section 5.2. Second, firms may differ in their sensitivity of investment (employment) to the availability of bank financing.

Regarding the first channel, the first panel of Table 7 shows that—even among the sub-population of relatively large firms for which we have corporate tax filings—banks appear to selectively cut credit to the smallest firms, which is in line with the evidence in Table 5. A high tangibles ratio also tends to moderate the credit supply shock, although the effect is not statistically significant.

Panel 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 sensitivity of input usage to the availability of bank financing, in line with intuition. Namely, firms in industries with higher Rajan and Zingales (1998) index (i.e. industries need more external funding to finance investment) exhibit a sensitivity of investment to credit twice larger than the baseline. Similarly, firms with high working capital over sales ratio—which are more likely to need external financing to pay workers—have a credit-to-labor sensitivity twice larger than the baseline.

I also find that small firms and firms in low tangibles ratio industries—which are typical proxies for firms capital constraints—have higher sensitivity of investment to credit availability, in line with the idea that these firms have lower ability to turn towards 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.[60] I find that the effect on credit is not different for firms with higher marginal products: the interaction terms are insignificant and if anything banks cut credit to these firms to a lesser extent. However, in line with the intuition that these firms are more constrained, I find that firms with higher Y/K have a larger credit-to-investment sensitivity and firms with higher Y/L have a larger credit-to-labor sensitivity.[61] Therefore, even though banks do not selectively cut credit to these firms, the consequence of these higher sensitivities is that crowding out generates a larger reduction in inputs for firms with higher marginal

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

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

output gains from additional inputs. This indicates that the shock reduces allocative efficiency. The next Section quantifies this effect.

[Table 7 about here.]

7 Aggregate effects

Thus far, I have documented relative crowding out effects: increases in local government loans at one bank reduce this bank's corporate credit relative to other banks, and reduce investment and employment at firms borrowing from this bank relative to other firms. However, these cross-sectional relationships do not allow to directly infer the crowding out effect of local government loans 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 have an effect on allocative efficiency. This would affect aggregate output through a change in aggregate total factor productivity (TFP). I quantify these effects in turn.

The counterfactual of interest is a situation where local government debt does not crowd out corporate credit. Importantly, it is not a world without changes in local government debt: the goal is to isolate the crowding out channel, and therefore to leave any other effects of local government debt unchanged. To take a concrete counterfactual, assume the same path for local government spending and debt, but the only difference is that local government debt is entirely financed by foreign investors on international capital markets. In this case, all other effects of local government debt are kept constant, but local government debt would not have crowding out effect on the domestic banking sector.[62] In my empirical setting, this counterfactual corresponds to the situation where $\Delta C^{gov}_{brt} = 0$ for all (b, r). I use the potential outcome notation $X(\mathbf{0})$ to denote the counterfactual value of variable X.

7.1 Crowding out and aggregate input usage

What do the cross-sectional results presented in the previous sections imply for aggregate credit, investment and employment? My empirical estimates are silent on this point: if the crowding out effect also affects credit and input usage at firms borrowing from non-exposed banks, aggregate effects will differ from relative effects. Unfortunately, the

62. For the only difference to be the financing of government debt—i.e. to avoid having also changes in the allocation of savings at home vs. abroad—one needs to assume some international capital markets fragmentation. See for instance the model in Broner, Clancy, Erce, and Martin (2021), showing that domestic crowding out effects are weaker when governments sell their debt to foreign agents.

reduced form evidence does not allow to obtain the causal effect of crowding out on non-exposed banks and firms. This is the "missing intercept" problem. I circumvent this issue by developing a simple model of crowding out in a partially fragmented banking system that predicts the behavior of non-exposed banks and firms. The model allows to interpret my cross-sectional estimates and to relate them to the aggregate crowding out parameter. I provide the key elements in the main text and leave details to Appendix C.

Model. I construct a perfectly competitive market 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 (partially) fragmented. Firms, local governments, and households are assigned to a given bank and do not arbitrage across banks. Banks can partly undo this fragmentation by trading (at a cost) on the interbank market.[63] Each bank solves the following problem:

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

subject to banks' 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 market, the deposit market and the interbank market. I assume that households have an isoelastic upward-slopping deposit supply with elasticity ϵ^s. Firms and local governments have downward-slopping isoelastic credit demand curves with elasticities ϵ^c and ϵ^g, respectively. Local governments have bank-specific credit demand shocks.

The model allows to derive the effect of a demand-driven increase in local government loans on corporate credit, at the level of each bank and at the aggregate level (Proposition 1 in Appendix C). The aggregate relationship writes:

$$\Delta C_t^{corp} = -\chi \Delta C_t^{gov} \tag{8}$$

where $\chi > 0$ is decreasing in the ratio of the elasticity of deposit supply on the elasticity of corporate credit demand $\frac{\epsilon^s}{\epsilon^c}$. ΔC_t^{corp} is aggregate corporate credit growth. ΔC_t^{gov} is the aggregate increase in local government loans normalized by banks' aggregate loan portfolio. I also obtain the relationship between bank-level changes in local government loans and bank×firm-level changes in corporate credit, which is the theoretical counterpart to my empirical specification (2):

$$\Delta C_{fbt} = \theta_{ft} - \chi(1-\nu)\Delta C_t^{gov} - \chi\nu\Delta C_{bt}^{gov} \tag{9}$$

θ_{ft} is a mean-zero firm-level demand shock. $\nu \in [0,1]$ indexes the degree of interbank

63. The model is homothetic to the case where depositors can partly arbitrage across banks. I also consider the case where firms can substitute across banks and show that the key result is the same (extension C.2.2).

frictions: ν is monotonically increasing in ϕ, $\nu = 0$ when $\phi = 0$ (perfectly integrated banks) and $\nu = 1$ when $\phi \to +\infty$ (fully fragmented banks). ΔC_{fbt} is bank×firm credit growth, and ΔC_{bt}^{gov} is the bank-level increase in local government loans normalized by the banks' total loan portfolio, as in specification (2). Bank×firm-level credit growth depends on three terms. The first term is the firm-level demand shock, that may be correlated to local government debt. 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 relative crowding out parameter–i.e. the parameter obtained from my reduced-form analysis—is $\nu\chi$. It jointly captures banking frictions and aggregate crowding out. The intuition is the following. Assume that the banking sector is perfectly integrated, i.e. $\nu = 0$. In this case, a bank subject to a local government debt demand shock will draw in capital from other banks using the interbank market, up to the point where interest rates are equalized across banks, so that the reduction in corporate credit will be uniform across banks. In this case, the relative crowding out effect is 0.[64]

This transmission across banks is precisely what explains the existence of the second term: when fragmentation 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 even banks with zero exposure reduce their corporate credit. Therefore, because of equilibrium effects across banks, each bank's corporate credit supply is negatively affected by the aggregate amount of local government loans. Hence, the relative effect is smaller than the aggregate effect: $\nu\chi \leq \chi$ (Proposition 2 in Appendix C).

Interbank market frictions determine the relative effect but do not modify the aggregate crowding out parameter depending on $\frac{\epsilon^s}{\epsilon^c}$, which captures the elasticity of the supply of credit of the aggregate banking sector. I also study the case where there is no interbank market friction but banks face balance sheet constraints (e.g. net worth constraints or regulatory constraints on lending). In this case, the aggregate crowding out parameter is increasing in the severity of this constraint, but the key insight that the relative effect is smaller than the aggregate effect remains unchanged.[65]

From the equation for firm×bank-level credit (9), we can obtain a prediction for firm-level investment (employment) by aggregating this equation at the firm-level and using the credit to investment (employment) passthrough coefficient:

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

64. This result is the counterpart to the classic result in international macroeconomics that there is no country-level crowding out effect if international capital markets are perfectly integrated.

65. These theoretical predictions are in line with the reduced form evidence presented in Section 5.1 where I documented that crowding out was larger at banks subject to more severe balance sheet constraints and with lesser access to the interbank market.

The aggregate counterpart of this equation is:

$$\Delta K_t = -\eta^K \chi \Delta C_t^{gov}$$

Quantification of the output loss due to the reduction in inputs. The quantities of interest are the aggregate shortfall in capital and labor due to crowding out. The capital shortfall is defined as:

$$\mathcal{L}(K_t) = -\frac{K_t - K_t(\mathbf{0})}{K_t(\mathbf{0})} = \eta^K \chi \Delta C_t^{gov}$$

I similarly define the labor shortfall $\mathcal{L}(L_t)$. Under the assumption that production is Cobb-Douglas, the output shortfall is then given by:

$$\mathcal{L}^{input}(Y_t) = \alpha \mathcal{L}(K_t) + (1-\alpha)\mathcal{L}(L_t)$$

where α is the capital share. To better gauge the magnitude of this effect, the shortfall can then be translated into a euro for euro relationship akin to a government spending multiplier using $m_t = \frac{Y_t - Y_t(\mathbf{0})}{D_t^{gov} - D_t^{gov}(\mathbf{0})}$.

Lower bound. The key result of the model is that the cross-sectional effect is a lower bound for the aggregate effect: $\mathcal{L}(K_t) \geq \eta^K \chi \nu \Delta C_t^{gov}$. This quantity can be obtained using the estimated coefficient from specification (6), where $FirmExposure_{ft}$ is used as an instrument for its "realized quantity" version $\overline{\Delta C_{brt}^{gov}}^f = \sum_b \omega_{fb,t-1} \Delta C_{brt}^{gov}$.[66] We obtain $\hat{\beta}_{IV}^K = -\eta^K \chi \nu$. These results are presented in Table B.7. I can compute a lower bound for $\mathcal{L}(L_t)$ in a similar manner and obtain a lower bound on the output shortfall through the aggregate input usage channel.

I find that the lower bound to the yearly output loss due to the reduction in input usage is equal to on average 0.09% of GDP over my sample period.[67] 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 decreases. The effect on aggregate output is mostly driven by the shortfall in capital due to crowding out, equal to on average 0.2% per year; the labor shortfall is only 0.04% on average.

This yields a bound for the multiplier m equal to on average -0.15. The multiplier is relatively stable across the 12 years of my sample, with a standard deviation equal to 0.03. Hence, the effect of an additional euro of local government debt on aggregate output would be at least 15 cents higher in the absence of crowding out.[68]

66. In the baseline model, I consider only banks and not regions within banks. To link this specification to the empirical specification, one needs to consider bank×region as distinct banks, which is not inappropriate since I document that frictions within banks across regions are of the same order of magnitudes as frictions across banks. Extension C.2.3 solves the model with different levels of frictions across and within banks, and show the results are unchanged.

67. All computation details are in Appendix B.3. While these equations 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 a more conservative estimate).

68. As robustness checks, Appendix B.3 quantifies the multiplier when neutralizing the effect of the joint distributions of firm size, bank size and local government debt shocks (m =-0.28) and starting from

Estimating the equilibrium effect. The lower bound relying on the cross-sectional effect misses 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 in 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 can thus estimate $1-\nu$ by regressing the change in net interbank borrowing on the increase in local government lending, instrumented by $BankExposure$. All details are in Appendix B.3. I find that the estimated coefficient is positive and statistically significant. That is, in line with the prediction of the model, banks exposed to a higher local government debt demand shock do borrow from other banks in the interbank market, and pre-trending tests show that this happens only at the time of the shock.

In terms of magnitude, I estimate $1-\nu$ to be equal to 0.17. Since all my cross-sectional effects scale with ν, the lower bound based on cross-sectional estimates is lower than the aggregate effect by 17%. This implies that the aggregate output loss due to crowding out is equal to on average 0.1% of GDP, or equivalently an additional €1 of local government loans crowds out €0.18 of corporate output. These results are summarized in Table 9.

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 are back 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 an interest-elastic demand for loans of firms and local governments and an interest-elastic supply of savings of households. I now discuss how general equilibrium effects would affect the aggregate crowding out effect. Typical general equilibrium analysis would suggest two opposing channels that may also lead unaffected firms to adjust their inputs in response to the credit supply shock generated by local government debt, and which would therefore affect the aggregate crowding out effect. 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

the back-of-the-envelope computations from the reduced-form results in Section 6 (m =-0.21).

firms will tend to increase, triggering a reallocation of demand towards least exposed firms. The magnitude of this effect depends on the substitutability of the goods produced at different firms. Second, the shock generates a reduction in aggregate expenditure, which reduces input demand at unconstrained firms. Chodorow-Reich (2014) formally quantifies these general equilibrium effects in response to a credit supply shock and finds that for plausible parameter values, the general equilibrium effects either magnify the effects from the partial equilibrium exercise or have at most a modest attenuating effect.

Alternative counterfactual: debt vs. lump-sum taxes and the Ricardian effect. I have quantified the output loss relative to a counterfactual where government debt is kept constant but is financed by foreign agents, so that there is no crowding out. 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 provide a back-of-the envelope quantification of this offsetting effect based on estimates from the literature. I find that the output loss generated by one additional euro of government debt that serves to reduce lump-sum taxes by 1€ is lower by 3.5%-12% than my baseline estimate (details in Appendix C.2.5).

7.2 Crowding out and allocative efficiency

The reduced form results presented above show that crowding out has differential effects on firm-level input usage. These distributive effects may affect aggregate output through a change in allocative efficiency. This section quantifies this effect.

Framework. 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 the deviations from marginal products 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 a firm f are $R(1+\tau_f^K)$ and $w(1+\tau_f^L)$ for capital and labor, respectively. The wedges correspond to different types of 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 leads to the modified first-order conditions for firms' marginal revenue products of inputs (henceforth MRPX):

$$\text{MRPK}_{ft} = \alpha \frac{P_{ft}Y_{ft}}{K_{ft}} = R_t(1+\tau^K_{ft})$$
$$\text{MRPL}_{ft} = (1-\alpha)\frac{P_{ft}Y_{ft}}{L_{ft}} = w_t(1+\tau^L_{ft})$$

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

$$\log(\text{TFP}_t) = \log(\text{TFP}^*_t) - \frac{\sigma-1}{2}\,\text{Var}(\tau_{ft}) - \frac{\alpha}{2}\,\text{Var}(\tau^K_{ft}) - \frac{1-\alpha}{2}\,\text{Var}(\tau^L_{ft})$$

with σ the elasticity of substitution across products of different firms (see Appendix F for details on the underlying theoretical framework). The first term corresponds to TFP under the optimal allocation of resources and the second term to misallocation. When wedges are highly dispersed, marginal products are not equalized so that there are large gains from reallocation inputs away from firms with low marginal products towards firms with high marginal products. Note that what matters is the dispersion: if wedges are high but equal across firms, there are no gains from reallocating inputs.

How do the cross-sectional crowding out effects documented above 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 wedges of inputs financed by bank loans.[69] 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 input usage drops to a larger extent for firms with the highest marginal product of inputs (i.e. the highest ex-ante wedges), the variance goes up and misallocation worsens. Conversely, if wedges fall by more for firms with higher ex-ante wedges, we get closer to marginal products equalization and TFP increases.

To quantify this effect, let us define $\text{TFP}_t(\mathbf{0})$ as TFP is a world where there is no crowding out of domestic credit because local government debt is financed by foreign investors. The quantity of interest is $\mathcal{L}(\text{TFP}_t) = \log(\text{TFP}_t) - \log(\text{TFP}_t(\mathbf{0}))$ and we have that:

$$\begin{aligned}\mathcal{L}(\text{TFP}_t) = &-\frac{\sigma-1}{2}[\text{Var}(\tau_{ft}) - \text{Var}(\tau_{ft}(\mathbf{0}))] \\ &-\frac{\alpha}{2}[\text{Var}(\tau^K_{ft}) - \text{Var}(\tau^K_{ft}(\mathbf{0}))] - \frac{1-\alpha}{2}[\text{Var}(\tau^L_{ft}) - \text{Var}(\tau^L_{ft}(\mathbf{0}))] \qquad (10)\end{aligned}$$

69. In considering a shock to financing conditions as a shock to wedges, I follow Larrain and Stumpner (2017) and Blattner, Farinha, and Rebelo (2019).

where $\tau_{ft}(\mathbf{0})$, $\tau^K_{ft}(\mathbf{0})$ and $\tau^L_{ft}(\mathbf{0})$ are the counterfactual wedges in a world without crowding out.[70]

Descriptive evidence on firm-level wedges. Before turning to the quantification of the TFP loss in (10), it is useful to look at 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' inputs acquisition decisions.[71]

Effect of crowding out on wedges. Quantifying the TFP loss in (10) requires estimates of the counterfactual wedges $\tau_{ft}(\mathbf{0})$, $\tau^K_{ft}(\mathbf{0})$ and $\tau^L_{ft}(\mathbf{0})$. That is, we need to quantify the effect of firms' exposure to crowding out on wedges. To do so, I estimate the effect of $FirmExposure$ on τ^K_{ft}, τ^L_{ft} and τ_{ft}, as defined above.[72] The specification is the same as that for firm-level inputs (equation (6)), but the dependent variable is the change in wedges.

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 weighted average (columns (2)-(4)). The fact that wedges respond to firm-level credit supply shocks further shows that wedges are indeed partly driven by credit frictions, which supports considering firms' 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. That said, the labor wedge also increases, suggesting that credit frictions also matter for firms' employment decisions.[73]

I then run 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 shows the results. Columns (3) to (8) show that the credit supply shock induced by crowding out 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 ex-ante wedges firms (if anything the effect on credit is slightly weaker

70. This expression is true under the assumption that $\log(\text{TFP}^*_t)$ is unaffected. I come back to this point below.

71. These results are reported in Table A.7. I also find that high ex-ante wedges firms have a higher profitability, in line with the idea that these firms have high marginal products of inputs as a results of constraints. They are also less leveraged, which likely reflects the constraints on borrowing. Finally, these firms have higher ratings suggesting that the higher marginal product of capital does not solely reflect the price of risk.

72. Appendix F provides more details on definitions and estimation of wedges.

73. Schoefer (2015) and Fonseca and Van Doornik (2021) provide evidence in support of this channel.

for high wedges firms as can be seen in columns (1)-(2)). Rather, a given tightening of credit supply 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.

[Table 8 about here.]

Effect on aggregate TFP. Using these results, I predict $\tau_{ft}(\mathbf{0}) = \hat{\tau}_{ft} - \hat{\beta}^{\tau} FirmExposure_{ft}$ where $\hat{\tau}_{ft}$ is obtained from the fitted value of the regression and $\hat{\beta}^{\tau}$ is allowed to differ for firms with higher ex-ante wedges. I proceed similarly for $\tau^{K}_{ft}(\mathbf{0})$ and $\tau^{L}_{ft}(\mathbf{0})$. I then compute the TFP loss for each industry using (10) and aggregate industries using industry shares in value added. I find that the misallocation effect of crowding out by local government loans reduces aggregate TFP by 0.07% per year on average. Over the sample period, this corresponds to a reduction in aggregate output of 13 cents per euro of local government loans on average.[74] These results are summarized in Table 9. Importantly, the quantification of the misallocation effect is independent of the general equilibrium effects of the shock discussed in the previous subsection (Sraer and Thesmar (2020)).

[Table 9 about here.]

Fragmentation across banks vs. heterogeneous effect of the shock. Crowding out may increase the dispersion in wedges through two channels. First, a uniform financing shock may have differentiated firm-level effects. If the financing shock has a larger effect on firms with higher ex-ante wedges, misallocation increases. Second, an effect specific to crowding out operating through banks (or any highly fragmented market) is that the distribution of local government loans across banks will generate differentiated credit supply shocks across firms. This in turn affects the distribution of firm-level wedges. The misallocation effect of this second channel depends on (i) the variance of firm-level credit shocks, (ii) the covariance between firm-level shocks and ex-ante wedges.

Both channels are at play in my setting. Table 8 shows a differentiated effect of the shock as a function of ex-ante wedges (channel 1). My empirical strategy precisely exploits firms' differential exposure to crowding out (channel 2).

To assess the quantitative importance of these channels, I decompose the TFP loss as:

$$\mathcal{L}(\text{TFP}_t) = \underbrace{\log(\text{TFP}_t) - \log(\text{TFP}_t(\overline{\mathbf{F}}_t))}_{\text{Fragmentation}} + \underbrace{\log(\text{TFP}_t(\overline{\mathbf{F}}_t)) - \log(\text{TFP}_t(\mathbf{0}))}_{\text{Heterogeneous effects}}$$

74. This effect does not linearly depend on the quantity of local government debt but rather on the distribution of exposure to crowding out across banks and firms. The multiplier computation is valid given this distribution.

where $\overline{\mathbf{F}}_t$ denotes the counterfactual where changes in local government debt are equal at all banks—or equivalently there is no fragmentation across banks—so that firm-level shocks are equal at all firms. The first term is the TFP loss due to dispersion in the credit supply shocks (channel 2). The second term is the loss due to the heterogeneous effect of a uniform shock (channel 1).

I find that all the increase in misallocation is driven by heterogeneous firm-level effects. fragmentation has a small positive effect on aggregate TFP, equal to €0.01 per €1 of local government loans, because firms which are ex-ante more constrained are slightly less exposed to the shock. Besides, the heterogeneous effects channel is large not because banks selectively cut credit to high wedges firms, but because high wedges firms are particularly 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 product of inputs, the fact that high marginal products-constrained firms tend to experience a larger reduction in inputs from a given reduction in credit can induce a large misallocation effect.[75] Second, in this specific context, the aggregate cost of the distributive effects induced by fragmentation is small.

Limitations and robustness. This computation is subject to several caveats. First, the Hsieh and Klenow (2009) framework only quantifies the losses from misallocation within industries, a limitation common to most of the misallocation literature. Since the shock under study leads to a reallocation of inputs both within and across industries, within-industry misallocation is a lower bound on the total misallocation effect. A second limitation is that 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 is not testable in the absence of data on firm-level product quantities.[76] Since there is no strong theoretical prior for expecting credit frictions to affect A_{ft}, this assumption is reasonable. Finally, an issue prevalent in the misallocation literature is measurement error in wedges. Attributing all cross-sectional dispersion in the observed marginal returns to firms' inputs to misallocation may overstate the extent of misallocation. However, looking at *within firm* changes in wedges largely alleviates this concern (Bau and Matray (2020)).

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 framework of Petrin and Levinsohn

75. This result complements the evidence in Banerjee, Breza, Townsend, and Vera-Cossio (2019) who find a large misallocation cost of having a credit expansion program targeted uniformly to all the population when the returns to credit are larger for ex-ante more constrained entrepreneurs. It contrasts with the insights of Blattner, Farinha, and Rebelo (2019) who instead quantify the effect of misallocation driven by a credit shock concentrated on firms with higher ex-ante wedges.

76. 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 the wedges.

(2012) (see details in Appendix F). Both methods yield a TFP loss equal to on average 0.03% of GDP per year, very similar to my baseline quantification.

7.3 Discussion

Crowding out and multipliers of local government spending. These results allow me to compute the effect of local government debt on aggregate output due to crowding out, combining the aggregate input usage and the misallocation channels. I find that an additional €1 of local government debt absorbed by banks leads to a reduction in aggregate output of €0.30 because of crowding out. This effect can be compared to the aggregate debt-financed multiplier of local government spending. Aggregate debt-financed multipliers are notoriously hard to estimate, but a reasonable range is 0.5-1.9.[77] Crowding out effects are therefore sizeable compared to estimated multipliers.

The existence of substantial crowding out effects of local government debt highlights the importance of considering the source of government financing when interpreting local government spending multipliers. In particular, an active strand of the literature on fiscal multipliers relies on exploiting cross-sectional geographic variation in transfer-financed government spending to estimate relative multipliers across locations. However, multipliers estimated using transfer-financed spending abstract from the distortions related to taxes or crowding out by debt.[78] The existence of substantial crowding out effects suggests that debt-financed fiscal multipliers may be substantially smaller than the transfer-financed multiplier estimated in this literature.[79] Besides, multipliers for local government debt financed by domestic banks will tend to be smaller than multipliers for spending financed by bonds traded on international capital markets.

While one must be cautious when comparing estimates relying on different sources of variation and different time periods, it is interesting to note that these insights are in line with the recent literature on this topic. Within the literature estimating local government spending multipliers, the few contributions that estimate debt-financed multipliers tend to find multipliers below those estimated by the larger literature on transfer-financed multipliers, and Dagostino (2018) who estimates a multiplier for spending financed by domestic banks finds a multiplier lower than the bonds-financed multiplier in Adelino, Cunha, and Ferreira (2017).[80]

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

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

79. This is true both for the relative multiplier across locations—since I show that relative crowding out has a local dimension—and for the aggregate fiscal multiplier.

80. There are three studies estimating cross-sectional debt-financed multipliers. Clemens and Miran (2012) finds a multiplier of 0.4 in the U.S. over 1988-2004 for debt-financed spending, Dagostino (2018) finds a multiplier below 1 in the U.S. over 2004-2010 for spending financed by domestic banks, Adelino, Cunha, and Ferreira (2017) finds a multiplier of 1.9 in Portugal over 2006-2013 for bonds-financed spending. The large literature on cross-sectional transfer-financed multipliers has typically found numbers in

External validity. The mechanism of the crowding out effect documented in this paper is fairly general—it exists as long as the supply of loanable funds is imperfectly elastic. However, my results highlight that the magnitude of the aggregate and distributive effects depend on the characteristics of the market under consideration. This section discusses the external validity of my findings.

First, 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 crowding out parameter is driven by the elasticity of the supply of funds, which is likely to be higher in the case of bonds than in the case of bank debt. The crowding out effect generated by local government loans is therefore likely to be an upper bound for the crowding out effect of government bonds, in particular if these bonds are traded on international capital markets.

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.

8 Conclusion

This paper investigates one potential adverse effect of increasing levels of local government bank debt on the real economy: crowding out effects on corporate credit and investment.

I proceed in two steps. First, I document relative crowding out effects across banks and regions. Namely, I show that a larger increase in local government debt at one bank in a given region disproportionately reduces corporate credit at this bank in this region, and the reduction in credit has real effects on investment and employment for borrowers of this bank. Importantly, my identification strategy allows me to isolate 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 theoretical framework that shows how these cross-sectional effects implied by bank fragmentation feed into the aggregate crowding out effect. Using this insight, I quantify that the crowding out effect of one additional euro of local government debt financed by domestic banks reduces output by 18 cents through a reduction in aggregate input usage. I also show that the distributive effects implied by the cross-sectional crowding out effects across banks and regions tend to reduce output through an effect on allocative efficiency, with one additional euro of local goverment debt reducing aggregate output by 12 cents. That is, debt-financed local government spending multipliers would be higher

the range of 1.5-2.6 (Chodorow-Reich (2019), Ramey (2019)). Note that this comparison cannot be made for studies on aggregate multipliers, since by definition there is no transfer-financed aggregate spending.

by 0.3 absent crowding out effects, e.g. if local government debt was entirely financed by foreign investors.

These effects are driven by banks' inability to increase their aggregate supply of credit when faced with a demand shock from local governments, due banks being capital constrained or to frictions operating within banks.

These results highlight that crowding out effects are quantitatively significant compared to typically estimated fiscal multipliers. Therefore, the mode of financing of government debt will significantly affect multiplier effects of government spending.

References

Abel, A. B. 2017. "Crowding out in Ricardian economies." *Journal of Monetary Economics* 87:52–66.

Acharya, V., I. Drechsler, and P. Schnabl. 2014. "A pyrrhic victory? Bank bailouts and sovereign credit risk." *The Journal of Finance* 69 (6): 2689–2739.

Acharya, V. V., T. Eisert, C. Eufinger, and C. Hirsch. 2018. "Real Effects of the Sovereign Debt Crisis in Europe: Evidence from Syndicated Loans." *The Review of Financial Studies* 31 (8): 2855–2896.

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

Akkoyun, C., N. Ersahin, and C. M. James. 2020. "Crowded out from the Beginning: Impact of Government Debt on Corporate Financing."

Alfaro, L., M. García-Santana, and E. Moral-Benito. 2021. "On the direct and indirect real effects of credit supply shocks." *Journal of Financial Economics* 139 (3): 895–921.

Amiti, M., and D. E. Weinstein. 2018. "How much do idiosyncratic bank shocks affect investment? Evidence from matched bank-firm loan data." *Journal of Political Economy* 126 (2): 525–587.

Banerjee, A., E. Breza, R. Townsend, and D. Vera-Cossio. 2019. "Access to Credit and Productivity." *Inter-American Development Bank Discussion Paper.*

Baqaee, D. R., and E. Farhi. 2020. "Productivity and misallocation in general equilibrium." *The Quarterly Journal of Economics* 135 (1): 105–163.

Barczyk, D. 2016. "Ricardian equivalence revisited: Deficits, gifts and bequests." *Journal of Economic Dynamics and Control* 63:1–24.

Bau, N., and A. Matray. 2020. *Misallocation and capital market integration: Evidence from India.* Technical report. National Bureau of Economic Research.

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

Bentolila, S., M. Jansen, and G. Jiménez. 2018. "When credit dries up: Job losses in the great recession." *Journal of the European Economic Association* 16 (3): 650–695.

Berger, A. N., and G. F. Udell. 1992. "Some evidence on the empirical significance of credit rationing." *Journal of Political Economy* 100 (5): 1047–1077.

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

Borusyak, K., P. Hull, and X. Jaravel. 2020. "Quasi-experimental shift-share research designs."

Bottero, M., S. Lenzu, and F. Mezzanotti. 2020. "Sovereign debt exposure and the bank lending channel: impact on credit supply and the real economy." *Journal of International Economics* 126:103328.

Broner, F., D. Clancy, A. Erce, and A. Martin. 2021. "Fiscal multipliers and foreign holdings of public debt." *Review of Economic Studies, Forthcoming.*

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

Chodorow-Reich, G. 2014. "The employment effects of credit market disruptions: Firm-level evidence from the 2008–9 financial crisis." *The Quarterly Journal of Economics* 129 (1): 1–59.

———. 2019. "Geographic cross-sectional fiscal spending multipliers: What have we learned?" *American Economic Journal: Economic Policy* 11 (2): 1–34.

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

Clemens, J., and S. Miran. 2012. "Fiscal policy multipliers on subnational government spending." *American Economic Journal: Economic Policy* 4 (2): 46–68.

Cohen, L., J. Coval, and C. Malloy. 2011. "Do powerful politicians cause corporate downsizing?" *Journal of Political Economy* 119 (6): 1015–1060.

Cremers, K. M., R. Huang, and Z. Sautner. 2011. "Internal capital markets and corporate politics in a banking group." *The review of financial studies* 24 (2): 358–401.

Dagostino, R. 2018. "The impact of bank financing on municipalities' bond issuance and the real economy."

Darmouni, O. 2020. "Informational frictions and the credit crunch." *The Journal of Finance* 75 (4): 2055–2094.

Davis, S. J., and J. Haltiwanger. 1992. "Gross job creation, gross job destruction, and employment reallocation." *The Quarterly Journal of Economics* 107 (3): 819–863.

Delatte, A.-L., A. Matray, and N. Pinardon-Touati. 2019. "Private Credit under Political Influence: Evidence from France."

Demirci, I., J. Huang, and C. Sialm. 2018. "Government Debt and Corporate Leverage: International Evidence."

Derrien, F., J.-S. Mesonnier, and G. Vuillemey. 2019. "Start-Up Costs and the Capital Structure of Young Firms."

Diamond, P. A. 1965. "National debt in a neoclassical growth model." *The American Economic Review* 55 (5): 1126–1150.

Diamond, W., Z. Jiang, and Y. Ma. 2021. "The Reserve Supply Channel of Unconventional Monetary Policy."

Drechsler, I., A. Savov, and P. Schnabl. 2017. "The deposits channel of monetary policy." *The Quarterly Journal of Economics* 132 (4): 1819–1876.

Elmendorf, D. W., and N. G. Mankiw. 1999. "Government debt." *Handbook of macroeconomics* 1:1615–1669.

Farhi, E., and I. Werning. 2016. "Fiscal multipliers: Liquidity traps and currency unions." In *Handbook of macroeconomics,* 2:2417–2492. Elsevier.

Fonseca, J., and B. Van Doornik. 2021. "Financial development and labor market outcomes: Evidence from Brazil." *Journal of Financial Economics.*

Friedman, B. M. 1978. "Crowding Out or Crowding In? Economic Consequences of Financing Government Deficits." *Brookings Papers on Economic Activity* 1978 (3): 593–641.

Gennaioli, N., A. Martin, and S. Rossi. 2014. "Sovereign default, domestic banks, and financial institutions." *The Journal of Finance* 69 (2): 819–866.

Goldsmith-Pinkham, P., I. Sorkin, and H. Swift. 2020. "Bartik instruments: What, when, why, and how." *American Economic Review* 110 (8): 2586–2624.

Gopinath, G., Ş. Kalemli-Özcan, L. Karabarbounis, and C. Villegas-Sanchez. 2017. "Capital allocation and productivity in South Europe." *The Quarterly Journal of Economics* 132 (4): 1915–1967.

Graham, J., M. T. Leary, and M. R. Roberts. 2014. *How does government borrowing affect corporate financing and investment?* Technical report. National Bureau of Economic Research.

———. 2015. "A century of capital structure: The leveraging of corporate America." *Journal of Financial Economics* 118 (3): 658–683.

Greenwald, D. L., J. Krainer, and P. Paul. 2021. *The credit line channel.* Technical report.

Greenwood, R., S. Hanson, and J. C. Stein. 2010. "A gap-filling theory of corporate debt maturity choice." *The Journal of Finance* 65 (3): 993–1028.

Hayo, B., and F. Neumeier. 2017. "The (in) validity of the Ricardian equivalence theorem–findings from a representative German population survey." *Journal of Macroeconomics* 51:162–174.

Hoffmann, M., I. Stewen, and M. Stiefel. 2021. "Growing Like Germany: Local Public Debt, Local Banks, Low Private Investment."

Hsieh, C.-T., and P. J. Klenow. 2009. "Misallocation and manufacturing TFP in China and India." *The Quarterly journal of economics* 124 (4): 1403–1448.

Huang, Y., M. Pagano, and U. Panizza. 2020. "Local Crowding-Out in China." *The Journal of Finance* 75 (6): 2855–2898.

Huang, Y., U. Panizza, and R. Varghese. 2018. "Does public debt crowd out corporate investment? international evidence."

Hubbard, G. 2012. "Consequences of government deficitsand debt." *International Journal of Central Banking.*

Ivanov, I., and T. Zimmermann. 2018. "The "Privatization" of Municipal Debt." *Hutchins Center Working Paper.*

Ivashina, V., L. Laeven, and E. Moral-Benito. 2020. *Loan types and the bank lending channel.* Technical report. National Bureau of Economic Research.

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

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

Krishnamurthy, A., and A. Vissing-Jorgensen. 2015. "The impact of treasury supply on financial sector lending and stability." *Journal of Financial Economics* 118 (3): 571–600.

Larrain, M., and S. Stumpner. 2017. "Capital account liberalization and aggregate productivity: The role of firm capital allocation." *The Journal of Finance* 72 (4): 1825–1858.

Liberti, J. M., and A. R. Mian. 2008. "Estimating the effect of hierarchies on information use." *The Review of Financial Studies* 22 (10): 4057–4090.

Liberti, J. M., A. Seru, and V. Vig. 2016. *Information, credit, and organization.* Technical report.

Liberti, J. M. 2018. "Initiative, incentives, and soft information." *Management Science* 64 (8): 3714–3734.

Manove, M., A. J. Padilla, and M. Pagano. 2001. "Collateral versus project screening: A model of lazy banks." *Rand journal of economics,* 726–744.

Martín, A., E. Moral-Benito, and T. Schmitz. 2021. "The financial transmission of housing booms: evidence from Spain." *American Economic Review* 111 (3): 1013–53.

Meissner, T., and D. Rostam-Afschar. 2017. "Learning Ricardian Equivalence." *Journal of Economic Dynamics and Control* 82:273–288.

Ongena, S., A. Popov, and N. Van Horen. 2019. “The invisible hand of the government: Moral suasion during the European sovereign debt crisis.” *American Economic Journal: Macroeconomics* 11 (4): 346–79.

Osotimehin, S. 2019. “Aggregate productivity and the allocation of resources over the business cycle.” *Review of Economic Dynamics* 32:180–205.

Oster, E. 2019. “Unobservable selection and coefficient stability: Theory and evidence.” *Journal of Business & Economic Statistics* 37 (2): 187–204.

Paravisini, D. 2008. “Local bank financial constraints and firm access to external finance.” *The Journal of Finance* 63 (5): 2161–2193.

Petrin, A., and J. Levinsohn. 2012. “Measuring aggregate productivity growth using plant-level data.” *The Rand journal of economics* 43 (4): 705–725.

Popov, A., and N. Van Horen. 2015. “Exporting Sovereign Stress: Evidence from Syndicated Bank Lending during the Euro Area Sovereign Debt Crisis.” *Review of Finance* 19 (5): 1825–1866.

Rajan, R., and L. Zingales. 1998. “Financial development and growth.” *American Economic Review* 88 (3): 559–586.

Ramey, V. A. 2019. “Ten years after the financial crisis: What have we learned from the renaissance in fiscal research?” *Journal of Economic Perspectives* 33 (2): 89–114.

Romer, D. 2006. *Advanced macroeconomics.* Mcgraw-hill.

Sauvagnat, J., and B. Vallée. 2021. “The Effects of Local Government Financial Distress: Evidence from Toxic Loans.” *Available at SSRN.*

Scharfstein, D. S., and J. C. Stein. 2000. “The dark side of internal capital markets: Divisional rent-seeking and inefficient investment.” *The Journal of Finance* 55 (6): 2537–2564.

Schivardi, F., E. Sette, and G. Tabellini. 2021. “Credit Misallocation During the European Financial Crisis.” *The Economic Journal.*

Schoefer, B. 2015. *The Financial Channel of Wage Rigidity.* Technical report.

Seater, J. J. 1993. “Ricardian equivalence.” *Journal of economic literature* 31 (1): 142–190.

Skrastins, J., and V. Vig. 2019. “How organizational hierarchy affects information production.” *The Review of Financial Studies* 32 (2): 564–604.

Sraer, D., and D. Thesmar. 2020. “A sufficient statistics approach for aggregating firm-level experiments.”

Stein, J. C. 2002. “Information production and capital allocation: Decentralized versus hierarchical firms.” *The journal of finance* 57 (5): 1891–1921.

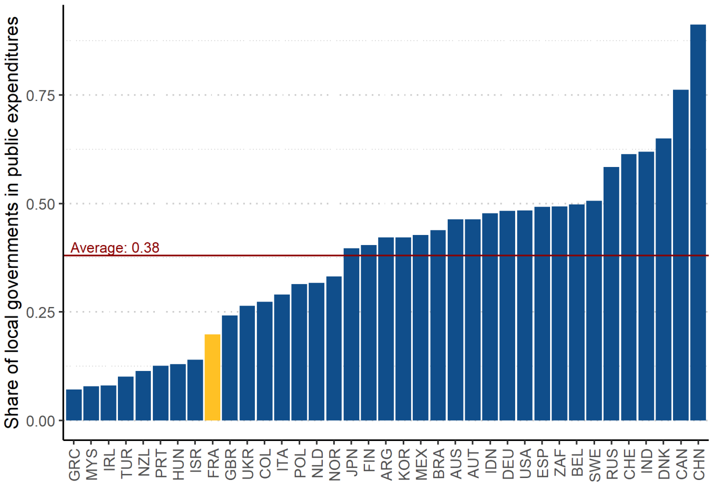
Stiglitz, J. E., and A. Weiss. 1981. "Credit rationing in markets with imperfect information." *The American Economic Review* 71 (3): 393–410.

Temin, P., and H.-J. Voth. 2005. "Credit rationing and crowding out during the industrial revolution: evidence from Hoare's Bank, 1702–1862." *Explorations in Economic History* 42 (3): 325–348.

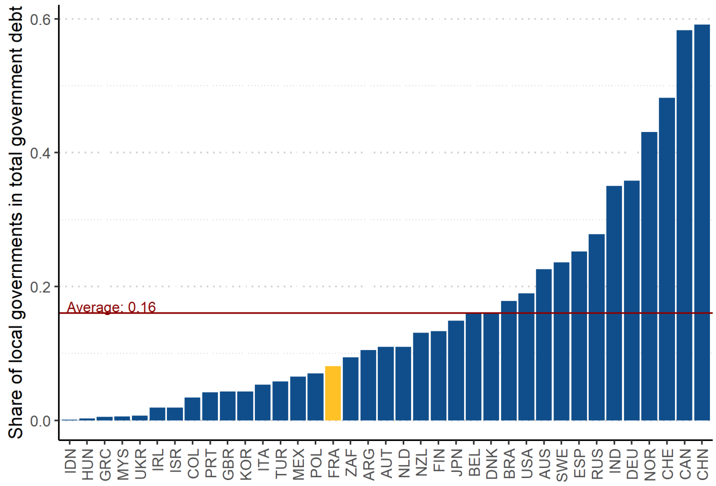
Williams, T. 2018. "Capital inflows, sovereign debt and bank lending: Micro-evidence from an emerging market." *The Review of Financial Studies* 31 (12): 4958–4994.

Figures

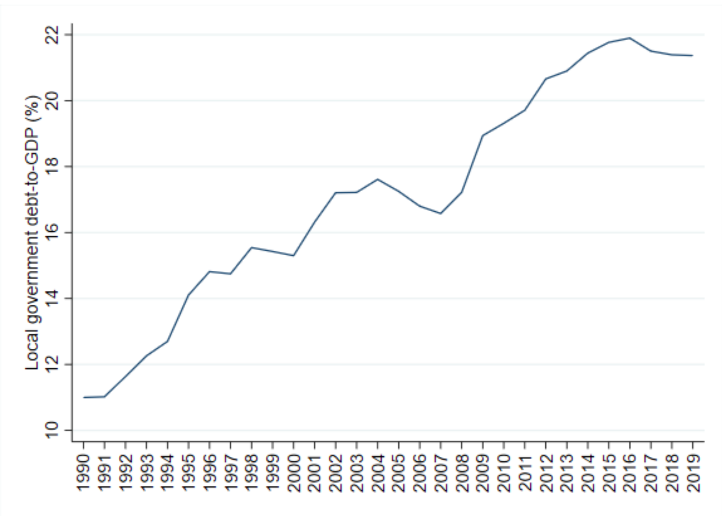
Figure 1: Local government expenditures and debt in large developed and developing economies



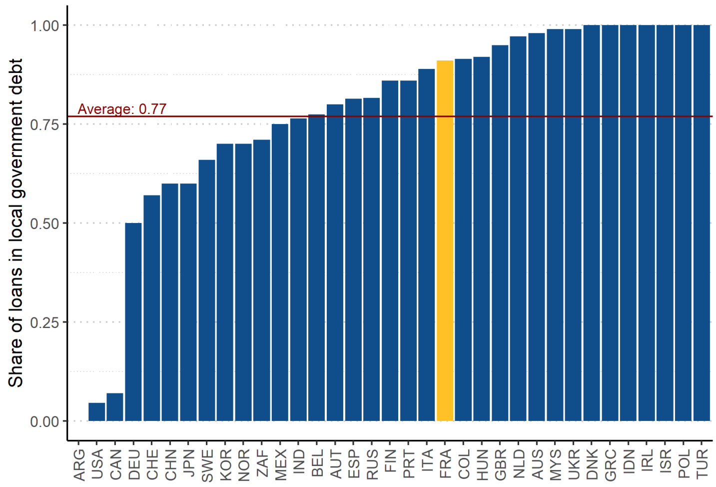
(a) Share of local governments in public expenditures



(b) Share of local governments in total government debt



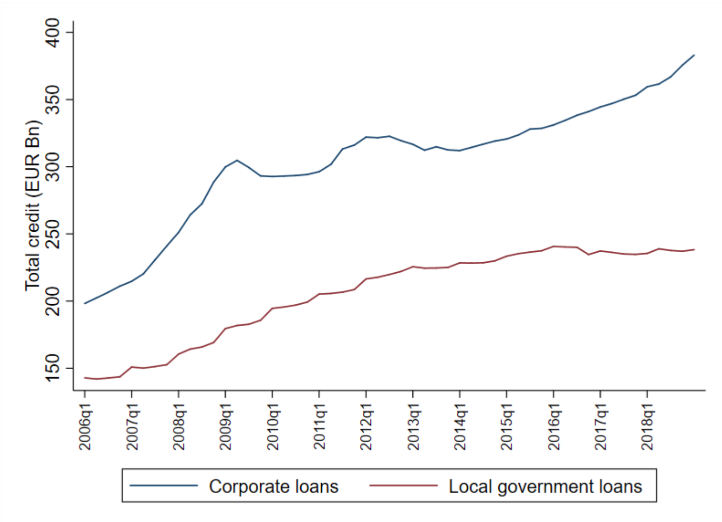
(c) Local government debt-to-GDP over time



(d) Share of loans in local government debt

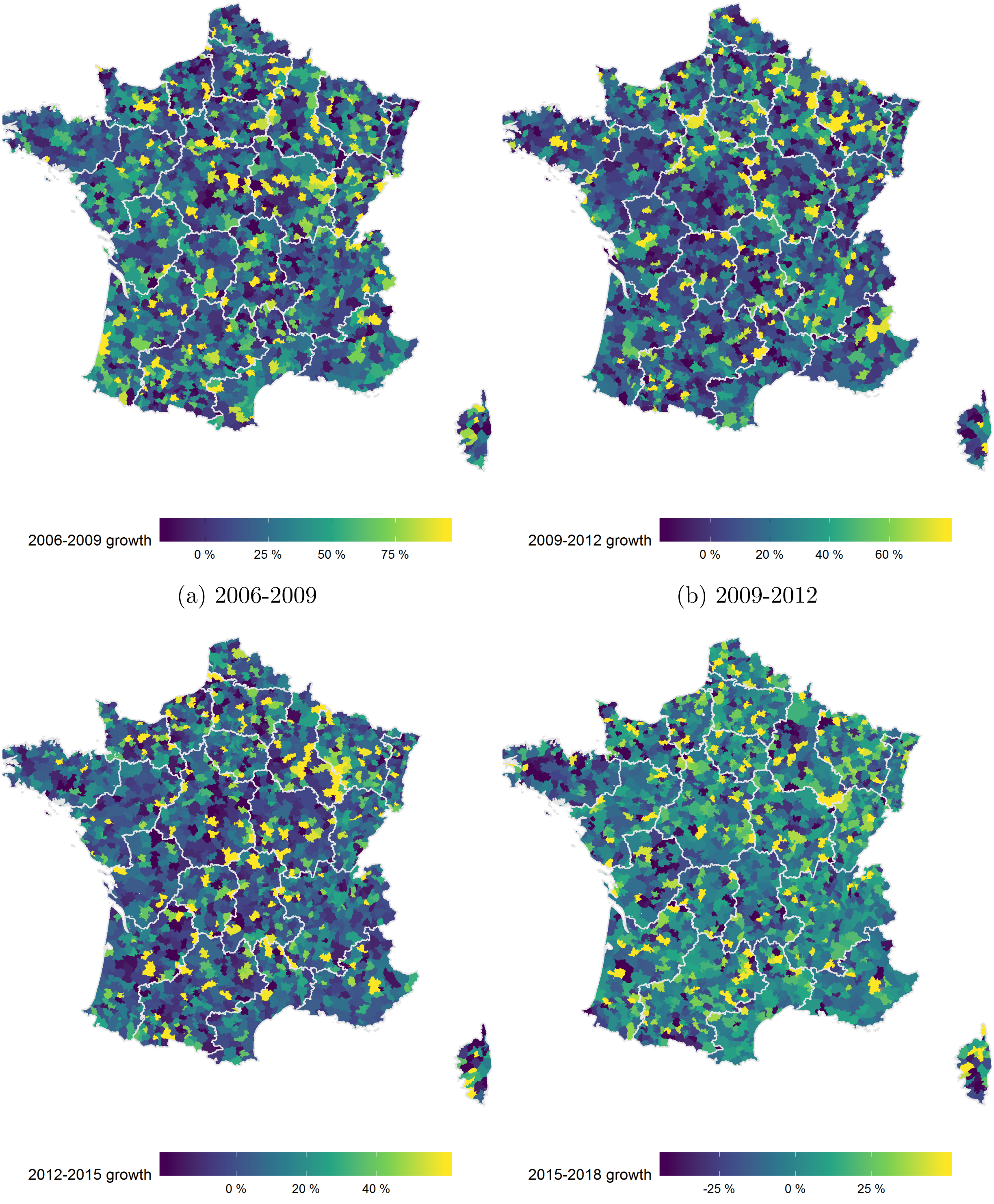
Note: Subfigure (a) shows the share of local governments in total government expenditures. Subfigure (b) shows the share of local governments in total government debt. Subfigure (c) shows the average local government debt-to-GDP ratio over time. Subfigure (d) shows the share of loans in local government debt. For panels (a), (b) and (d), the data comes from the OECD/UCLG World Observatory on Subnational Government Finance and Investment (SNG-WOFI). All the data is for 2016, for all countries with government debt higher than \$75bn (except Lebanon, New Zealand and Pakistan for which the data is not available). For panel (b), the data 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.

Figure 2: Aggregate credit to corporations and local governments



Note: This figure plots the aggregate time series obtained from the Banque de France credit register.

Figure 3: Growth rate of local government loans by municipality



(a) 2006-2009

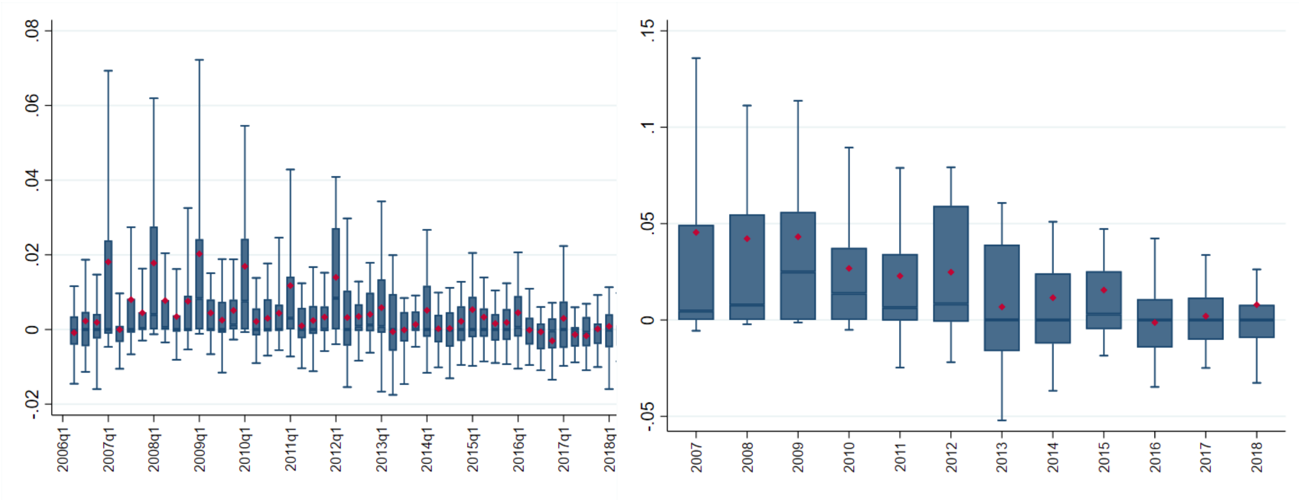
(b) 2009-2012

(c) 2012-2015

(d) 2015-2018

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

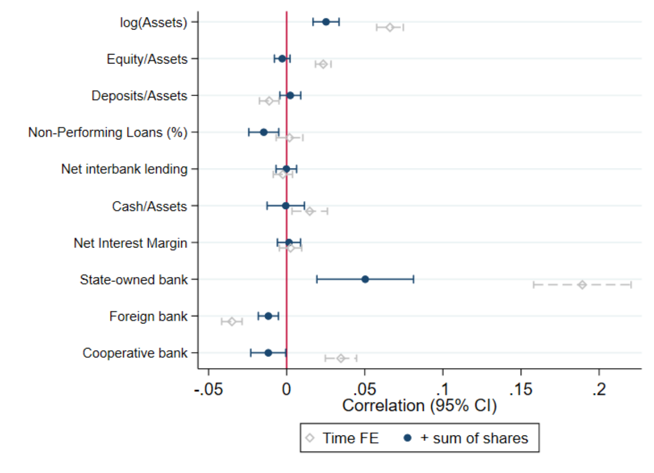
Figure 4: Variation in local government debt dynamics across banks×regions



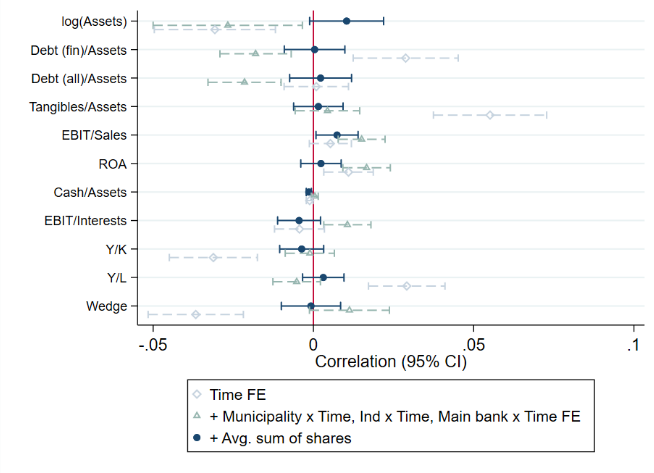
(a) Quarterly (b) Yearly

Note: This figure shows the distribution of ΔD^{gov}_{brt} the bank×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 shocks and pre-determined characteristics



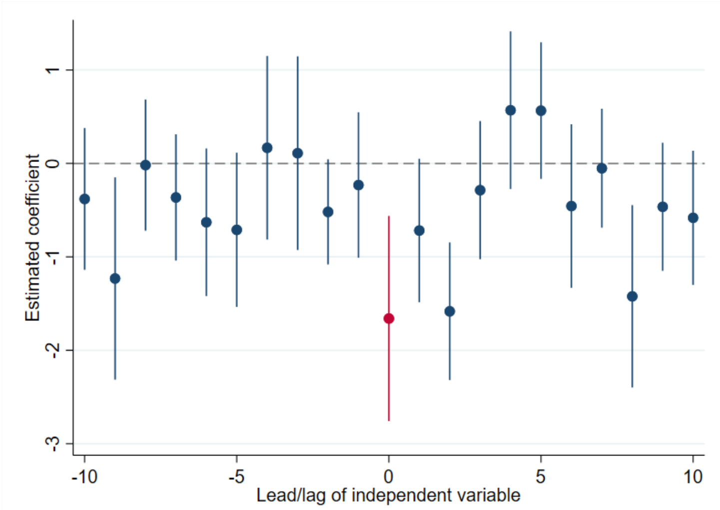
(a) Bank-level correlations



(b) Firm-level correlations

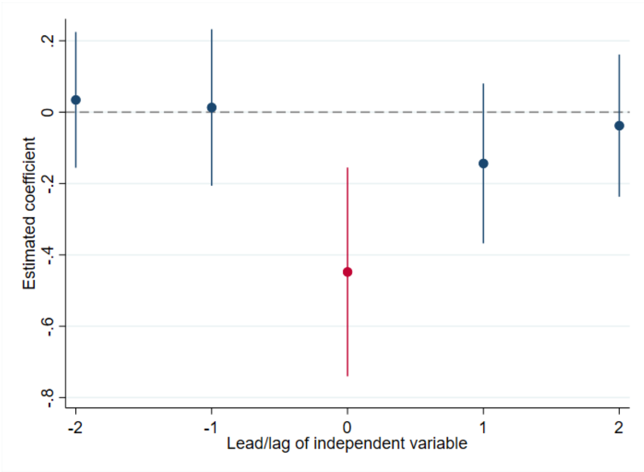
Note: Panel (a) shows the correlation between bank exposure to local government debt demand and various bank characteristics measured at $t-1$. Exposure to local government debt demand shocks is measured at the bank×region×time level, defined in (1) as the sum of municipality-level increases in local government debt weighted by municipalities' shares in the bank's loan portfolio. Regressions are weighted by lagged bank×region corporate credit volume, which is approximately equal to the weight of each bank×region in the firm×bank-level dataset. All regressions include time fixed effect. "+ sum of shares" indicates that the sum of municipality-level exposure shares $\omega^{gov}_{br,t-1}$ is included as a control, as recommended by firm exposure to Borusyak, Hull, and Jaravel (2020). Panel (b) shows the correlation between firm exposure to crowding out and firm characteristics measured at $t-1$. Firm exposure is defined in (7) 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×Time, Ind×Time, Main bank×Time FE" indicates that I include the fixed effects of my baseline specification. "+ Avg. sum of shares" indicates that the firm-level average of the sum of shares $\omega^{gov}_{br,t-1}$ is included as a control. Standard errors are clustered at the main bank×region level. The dot is the point estimate and the bar is the 95% confidence interval.

Figure 6: Pre-trends for firm×bank-level effect on credit growth

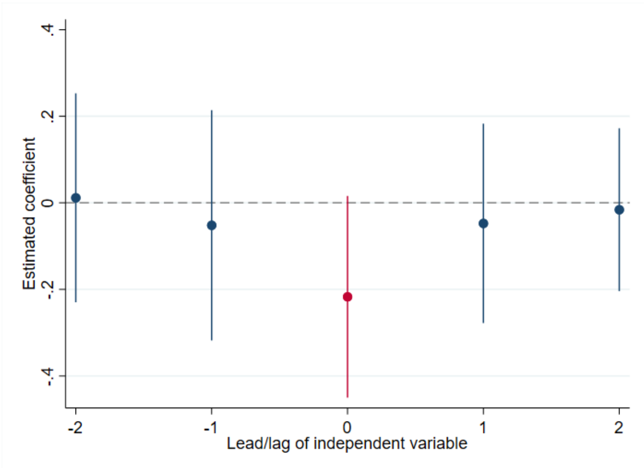


Note: This figure examines the crowding out effect of local government debt on corporate credit at the bank×region-level. It reports the results of estimating specification (2), including leads and lags of the independent variable. The outcome variable is the mid-point growth rate of credit granted to firm f by bank b. The main independent variable is exposure to local government debt demand shocks measured at the bank×region×time level, defined in (1) as the sum of municipality-level increases in local government debt weighted by municipalities' shares in the bank's loan portfolio. I include 10 leads and 10 lags of this variable. 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 also include leads and lags of the sum of shares. All regressions are estimated on the main sample of firms with multiple credit relationships. Standard errors are clustered at the region×bank level. The dot is the point estimate and the bar is the 95% confidence interval.

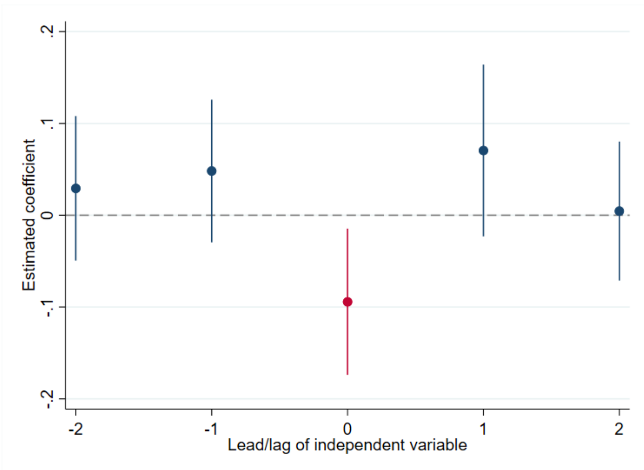
Figure 7: Pre-trends for real effects



(a) gr(credit)



(b) gr(capital)



(c) gr(wage bill)

Note: This figure examines the crowding out effect of local government debt on corporate credit (panel a), investment (panel b) and employment (panel c). It reports the results of estimating specification (6), including leads and lags of the independent variable. 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 local government debt demand shocks, defined as the sum of banks' exposure to local government debt shocks weighted by banks' shares in firms credit. I include 2 leads and 2 lags of this variable. 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. I include leads and lags of the firm-level weighted average of the sum of shares. Standard errors are clustered at the region×main bank level. The dot is the point estimate and the bar is the 95% confidence interval.

Tables

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 D_{fbt} (K€)	216.1	473.9	16	77	445	334.3	678.8	17	99	772
Credit growth ΔD_{fbt} (MPGR)	-0.011	0.69	-0.21	-0.045	0.26	-0.011	0.69	-0.24	-0.050	0.32
Credit growth ΔD_{fbt} (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 ΔD^{gov}_{brt} (%)	0.18	1.22	-0.59	-0.0035	1.26	0.16	1.16	-0.52	0	1.03
Local gvt credit D^{gov}_{brt} (K€)	433157	639095	0	120616	1256333	331009	585894	0	31586	1078466
Total credit D^{tot}_{brt} (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)

	mean	sd	p10	p50	p90
Outstanding credit D_{ft} (K€)	407.5	729.8	4	117	1109.7
Credit growth ΔD_{ft} (MPGR)	-0.067	0.97	-1.31	-0.16	1.56
Credit growth ΔD_{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 $\overline{\Delta D^{gov}_{brt}}^{f}$ (%)	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€)	829.0	2729.6	18	139	1371
Wage bill (K€)	751.1	1345.4	104	361	1474
Value added (K€)	1468.5	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	RF	RF	IV	IV	RF	IV
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Bank Exposure	-0.411	-0.882***	-1.098***			-1.061***	
	(0.345)	(0.265)	(0.266)			(0.273)	
Change in local govt loans ΔD_{brt}^{gov}				-0.822***	-0.954***		-0.916***
				(0.247)	(0.230)		(0.235)
Controls	–	–	✓	–	✓	✓	✓
Firm×Time FE	–	✓	✓	✓	✓	✓	✓
Observations	12,401,159	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×region-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 measured at the bank×region×time level, 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 columns labelled IV, $BankExposure$ is used as an instrument for the actual increase in bank×region-level local government lending 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 main 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)
BankExposure	-2.118***	-1.927***	-1.030***	-1.095***	-1.749***	-1.319*	-1.179**	-0.933**	-2.322***
	(0.389)	(0.382)	(0.313)	(0.270)	(0.600)	(0.742)	(0.573)	(0.433)	(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 of interest. 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, defined in (1) as the sum of municipality-level increases in local government debt weighted by municipalities' shares in the bank's loan portfolio. 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 liabilities held by non-residents is above median. Securitize is a dummy equal to 1 if the bank securitizes its 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 is equal to 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. 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 main 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**			0.368	0.483	
	(0.324)			(0.396)	(0.624)	
Bank×region-level exposure		-0.928***		-1.210***	0.0456	-1.041***
		(0.273)		(0.361)	(0.500)	(0.279)
Branch-level exposure			-0.978**		-1.308**	
			(0.484)		(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	6,161,886	9,729,656	12,094,234	12,496,413	12,041,821
R-squared	0.521	0.545	0.486	0.521	0.455	0.522

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. 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***	-1.826***	-1.968***	-1.968***	-1.792***	-1.090***
	(0.283)	(0.364)	(0.378)	(0.378)	(0.325)	(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 of interest. 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, 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 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 (corresponding to the size categories appearing in the credit register). In column (3), Rated is a dummy equal to 1 if the firm has a Bank of 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 (threshold: 5). In column (5), Strategic firm is a dummy equal to 1 if the share of the firm in the bank'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 (data from the public procurement watchdog). 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	RF	RF	IV	IV
	(1)	(2)	(3)	(4)	(5)
Firm Exposure	-0.577***	-0.232***	-0.063**		
	(0.124)	(0.087)	(0.030)		
gr(credit)				0.376***	0.111**
				(0.142)	(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 (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. The main independent variable is firm exposure to crowding out, defined in (7) 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 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.

Table 7: Firm-level real effects: heterogeneity

Panel A: Credit

	gr(credit)					
Interaction with D_{ft}	Large	High Tang/A	High RZ	High WC/Sales	High Y/K	High Y/L
	(1)	(2)	(3)	(4)	(5)	(6)
Firm Exposure	-0.725***	-0.619***	-0.537***	-0.530***	-0.627***	-0.662***
	(0.155)	(0.142)	(0.136)	(0.157)	(0.120)	(0.155)
Firm Exposure×D_{ft}	0.274*	0.197	-0.159	-0.063	0.127	0.157
	(0.167)	(0.150)	(0.193)	(0.161)	(0.160)	(0.159)
Controls×D_{ft}	✓	✓	✓	✓	✓	✓
FE×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

	gr(capital)									
Interaction with D_{ft}	Large		High Tang/A		High RZ		High WC/Sales		High Y/K	
	RF	IV	RF	IV	RF	IV	RF	IV	RF	IV
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Firm Exposure	-0.499***		-0.326***		-0.118		-0.202		-0.140	
	(0.143)		(0.102)		(0.099)		(0.158)		(0.094)	
Firm Exposure×D_{ft}	0.462***		0.436**		-0.464**		-0.035		-0.218	
	(0.174)		(0.181)		(0.189)		(0.178)		(0.171)	
gr(credit)		0.633***		0.485***		0.210		0.326		0.221
		(0.189)		(0.163)		(0.170)		(0.256)		(0.139)
gr(credit)×D_{ft}		-0.555*		-0.742*		0.570*		0.057		0.405
		(0.297)		(0.424)		(0.322)		(0.293)		(0.307)
Controls×D_{ft}	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
FE×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

	gr(wage bill)									
Interaction with D_{ft}	Large		High Tang/A		High RZ		High WC/Sales		High Y/L	
	RF	IV	RF	IV	RF	IV	RF	IV	RF	IV
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Firm Exposure	-0.055		-0.069*		-0.043		0.062		-0.003	
	(0.048)		(0.036)		(0.033)		(0.062)		(0.042)	
Firm Exposure×D_{ft}	-0.012		0.030		-0.112		-0.168**		-0.130**	
	(0.062)		(0.069)		(0.088)		(0.070)		(0.063)	
gr(credit)		0.071		0.111*		0.083		-0.111		0.004
		(0.061)		(0.061)		(0.064)		(0.118)		(0.065)
gr(credit)×D_{ft}		0.095		-0.012		0.135		0.292**		0.263*
		(0.128)		(0.165)		(0.143)		(0.140)		(0.137)
Controls×D_{ft}	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
FE×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 (6) 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 (7) 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 labelled 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 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. FE are municipality×time, industry×time, and main bank×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 $\Delta\tau^K_{ft}$ (2)	Labor wedge $\Delta\tau^L_{ft}$ (3)	Combined wedge $\Delta\tau_{ft}$ (4)
Firm Exposure	-0.577***	0.168*	0.070*	0.090**
	(0.124)	(0.098)	(0.037)	(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 $\Delta\tau^K_{ft}$		Labor wedge $\Delta\tau^L_{ft}$		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***	-0.488**	0.133	0.491*	0.075*	0.115	0.058	0.235**
	(0.121)	(0.227)	(0.109)	(0.273)	(0.042)	(0.076)	(0.050)	(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 (6). 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 (7) 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 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.

Table 9: Aggregate effects

	Output loss (%)	Multiplier
Aggregate input usage	0.1%	0.18
Aggregate TFP	0.07%	0.12
Aggregate output	0.17%	0.30

Note: This table reports the aggregate output loss due to crowding out. The first column reports the average yearly aggregate output loss due to crowding out. The yearly output loss is computed as the output shortfall compared to a counterfactual where local government debt has no crowding out effect. The second column reports the average multiplier, defined as the euro output loss per euro of local government loans. The first line is the output loss due to the reduction in input usage. The second line is the output loss due to a change in allocative efficiency. The third line is the sum and yields the total output loss.

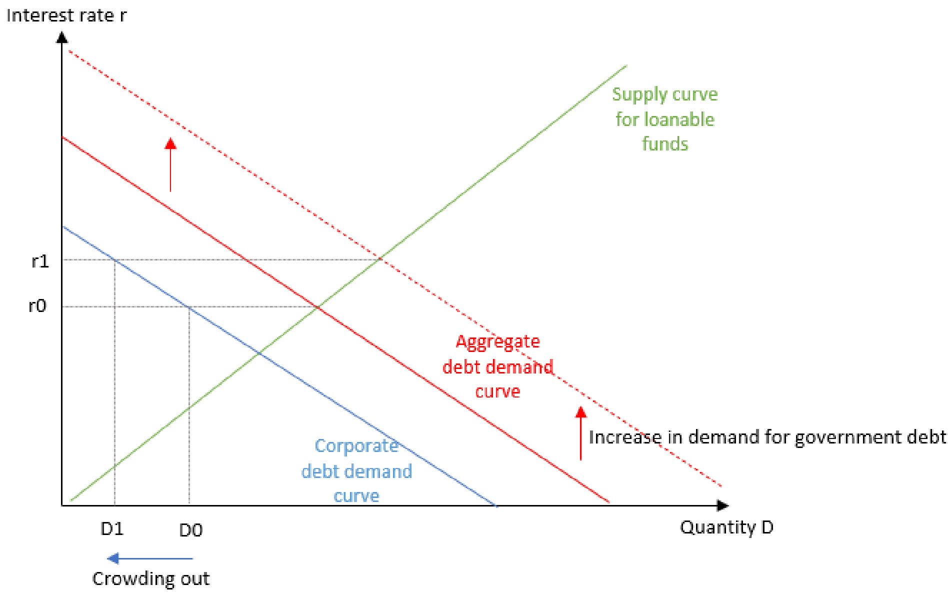
APPENDIX

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

Table of contents

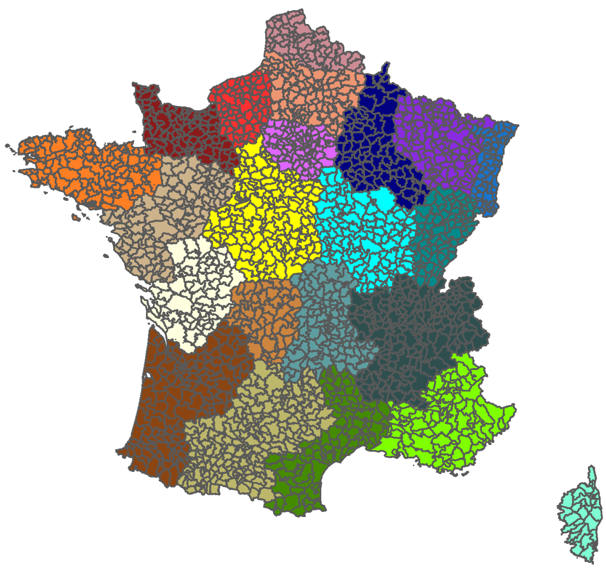
A Additional figures and tables

Figure A.1: Crowding out: simple supply and demand graph



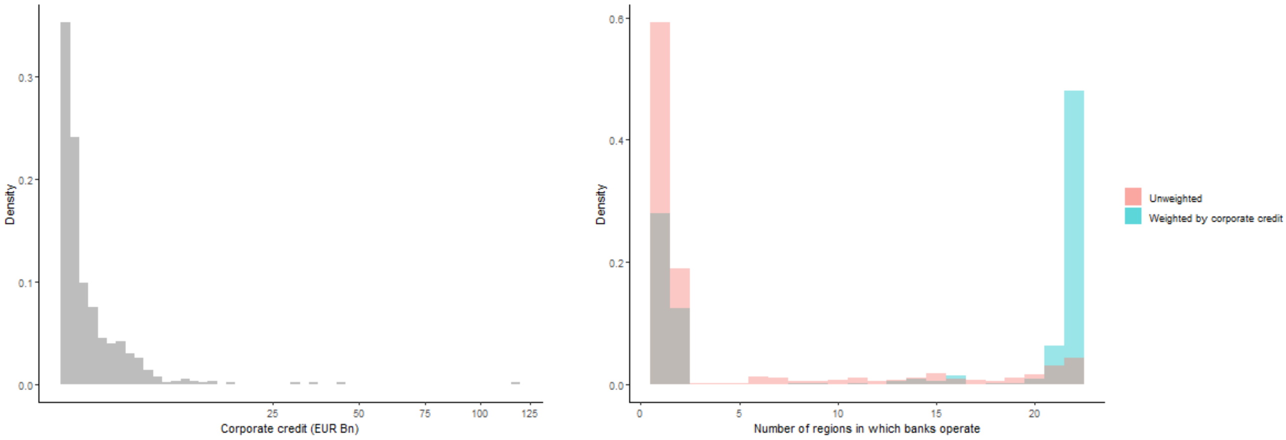
Note: This figure depicts the crowding out mechanism on a simple supply and demand graph.

Figure A.2: Geographic subdivisions



Note: This map shows the geographical subdivisions used in the paper. Grey boundaries delineate municipalities. Blocks of different colors distinguish the 22 French regions.

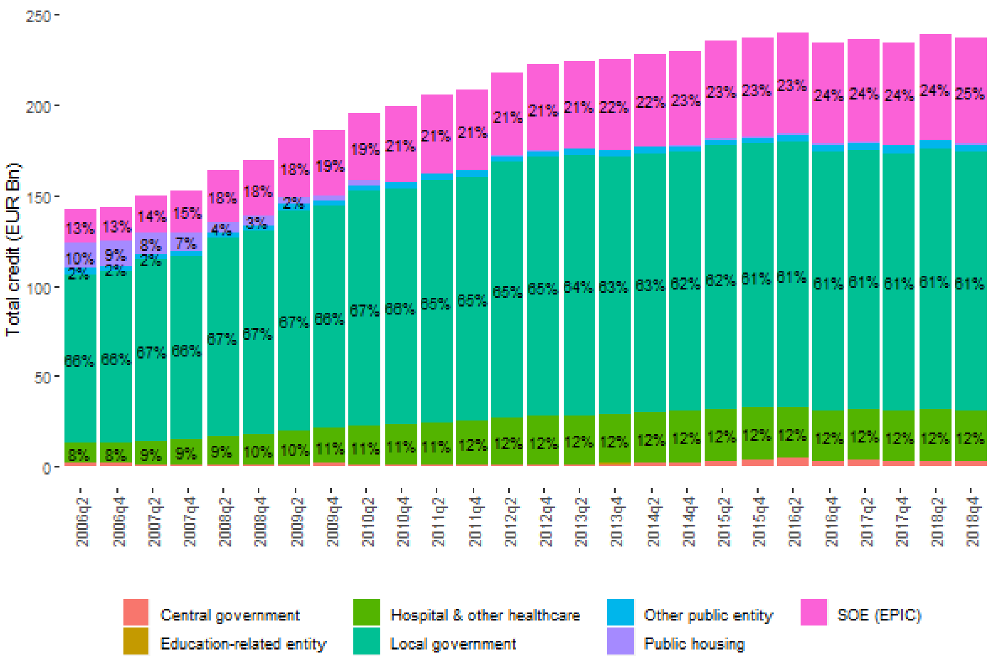
Figure A.3: Population of French banks



(a) Distribution by loan portfolio size (b) Distribution by number of regions

Note: Panel (a) shows the distribution of bank size, as defined by banks' corporate credit portfolio. 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.

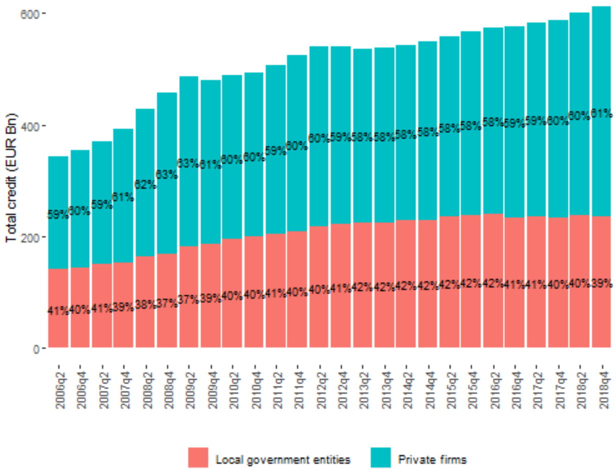
Figure A.4: Loans to local government entities by local government entity category



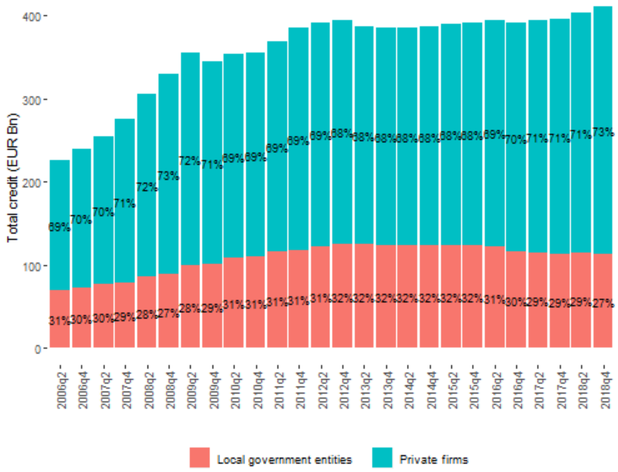
Note: This figure shows the evolution of bank lending to local government entities by type of entity. Local government refers to the four layers of local governments (regions, departements, intermunicipal cooperations, communes). SOE (EPIC) refers to state-owned public service operators. Central government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

Figure A.5: Local government loans in banks' balance sheets

Panel A: Share of local government loans in banks' loan portfolio in the time series

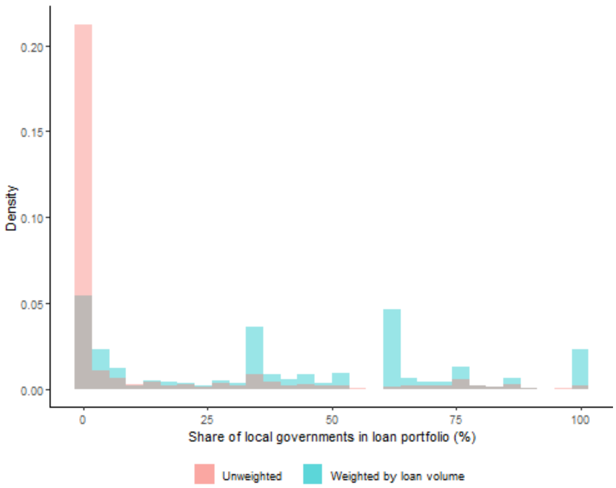


(a) All banks

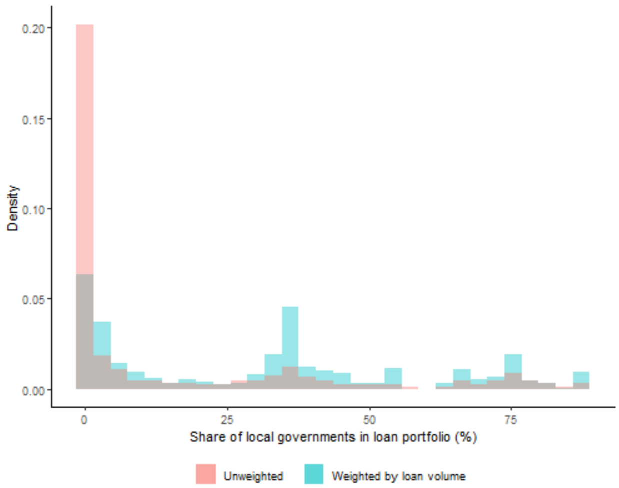


(b) Excluding state-owned banks

Panel B: Share of local government loans in banks' loan portfolio in the cross-section of banks



(c) All banks



(d) Excluding state-owned banks

Note: This figure shows the share of local government loans in banks' loan portfolio in the time series (Panel (a) and (b)) and in the cross-section of banks (Panel (c) and (d)), for all banks (Panel (a) and (c)) and excluding state-owned banks (Panel (b) and (d)). Banks' total loan portfolio is computed from the credit register, i.e. excluding loans to households.

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

Panel A: Firm×bank-level effect

	Credit growth					
	First stage		Reduced form		IV	
	2013	2014	2013	2014	2013	2014
	(1)	(2)	(3)	(4)	(5)	(6)
Dexia Exposure	1.287***	1.303***	-0.604***	-0.553***		
	(0.428)	(0.434)	(0.163)	(0.155)		
Change in local govt. loans					-0.481**	-0.440**
					(0.208)	(0.195)
Controls	✓	✓	✓	✓	✓	✓
Firm×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						IV			
	gr(credit)		gr(capital)		gr(wages)		gr(capital)		gr(wages)	
	2013	2014	2013	2014	2013	2014	2013	2014	2013	2014
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Dexia Exposure	-0.556***	-0.418***	-0.539**	-0.465**	-0.103*	-0.097				
	(0.140)	(0.147)	(0.224)	(0.226)	(0.056)	(0.065)				
gr(credit)							0.974***	1.103**	0.188**	0.240
							(0.367)	(0.504)	(0.094)	(0.150)
Controls	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Municipality×Time FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Industry×Time FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Main bank×Time FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Observations	64,860	65,075	61,578	57,417	62,485	58,555	61,578	57,417	62,484	58,554
R-squared	0.91	0.92	0.19	0.19	0.086	0.091	0.16	0.16	0.031	0.026
F stat.							15.4	7.50	14.6	6.95

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×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 municipalities' shares in the bank's loan portfolio (equation (3)). Columns (1) and (2) show the effect of Dexia exposure on the bank×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×bank-level credit growth. Columns (5) and (6) show the IV coefficient where Dexia exposure is used as an instrument for the bank×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 main sample of firms with multiple credit relationships. Standard errors are clustered at the region×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 (6). Columns (1)-(6) are the reduced form effects of firm-level Dexia exposure. Columns (7)-(10) show the credit-to-input sensitivities, obtained by instrumenting firm-level credit growth by $DexiaExposure$. 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.

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

	Within		Between		
	gr(local gvt. loans) 2006-07	gr(credit) 2006-07	gr(credit) 2001-07	gr(capital) 2001-07	gr(wages) 2001-07
	(1)	(2)	(3)	(4)	(5)
Dexia Exposure (bank)	0.012	0.027			
	(0.090)	(0.085)			
Dexia Exposure (firm)			0.099	0.159	-0.075
			(0.122)	(0.252)	(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×region-level exposure to Dexia. In column (1), the dependent variable is the 2006-07 bank×region-level increase in local government lending. In column (1), the dependent variable is the 2006-07 firm×bank-level credit growth. Controls and fixed effects are as in panel A of Table A.1. Standard errors are clustered at the region×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 (column (3)), firm-level fixed assets growth (column (4)), and firm-level wage bill growth (column (5)) over 2001-07. Controls and fixed effects are as in panel B of Table A.1. Standard errors are clustered at the region×main bank level. ***, ** and * indicate significance at the 1%, 5% and 10% levels, respectively.

Table A.3: Crowding out: asymmetric effect

	Credit growth			
Sample split by:	Region-level growth		Bank Exposure	
	Positive	Negative	Positive	Negative
	(1)	(2)	(3)	(4)
Bank Exposure	-1.272***	0.021	-1.354***	0.453
	(0.377)	(0.489)	(0.300)	(0.644)
Controls	✓	✓	✓	✓
Firm×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 measured at the bank×region×time level, 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 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 $BankExposure$. Hence, column (3) investigates within-firm credit growth across banks if at least one of the banks experiences a positive local government debt demand shock, while 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 main 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 A.4: Time-series variation in baseline coefficient

	Credit growth			
	Quarterly		Yearly	
	(1)	(2)	(3)	(4)
Bank Exposure	-1.098***		-2.548***	
	(0.266)		(0.460)	
2006-07 × Bank Exposure		-2.431***		-3.205**
		(0.701)		(1.255)
2008-09 × Bank Exposure		-2.430***		-3.577***
		(0.426)		(0.585)
2010-13 × Bank Exposure		-0.910**		-2.675***
		(0.460)		(0.524)
Post 2013 × Bank Exposure		-0.00897		-0.675
		(0.322)		(0.518)
Controls	✓	✓	✓	✓
Firm×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 measured at the bank×region×time level, 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 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×bank level. ***, ** and * indicate significance at the 1%, 5% and 10% levels, respectively.

Table A.5: Crowding out effect on interest rates

	Interest rate						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
BankExposure	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 measured at the bank×region×time level, defined as a shift-share with municipality-level local government debt shocks as shifters weighted by the share of each municipality within banks' loan portfolio in the preceding period, as defined in (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. 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 A.6: Firm×bank-level effect on credit: yearly frequency

	Credit growth			
	Full sample		Tax-filings sample	
	RF	IV	RF	IV
	(1)	(2)	(3)	(4)
BankExposure	-2.548***		-0.666**	
	(0.460)		(0.294)	
Change in local govt loans ΔD^{gov}_{brt}		-1.603***		-0.408**
		(0.346)		(0.196)
Controls	✓	✓	✓	✓
Firm×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×region-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 measured at the bank×region×time level, 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 columns labelled IV, *BankExposure* is used as an instrument for the actual increase in bank×region-level local government lending ΔD^{gov}_{brt}. 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×bank level. ***, ** and * indicate significance at the 1%, 5% and 10% levels, respectively.

Table A.7: Who are firms with high wedges?

	log(Assets)	log(Revenues)	Tang/A	RZ	WC/Sales	ROA	D/A	Rating
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
High wedge	-0.132***	-0.021	-0.183***	0.003***	0.005***	0.066***	-0.045***	0.136***
	(0.012)	(0.019)	(0.004)	(0.001)	(0.001)	(0.002)	(0.007)	(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 Banqude 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.

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 proxy 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, to punish non-complying banks or to reward complying banks. I look at two type of politicians: members of parliaments (MPs, *députés*), the most prominent local political figures, and mayors, who head the lowest level of local governments (*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.[81] I then exploit the fact that political interference is more likely when electoral incentives are strongest. Local politicians could for instance coerce banks into lending to local governments before elections to spend on 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).

Finally, 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)). These supra-competitive profits are likely to induce a supra-optimal level of lending to local government. 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 local public service operators in the regional local government loans 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 for the average spread, I find no difference in the crowding out coefficient. I also repeat the construction of *BankExposure* using only lending to local public service operators instead of total local government loans. That is, I restrict the focus to crowding out due to increases in lending to local public service operators. I find a similar crowding out effect.

These results show that the crowding out coefficient is independent of the potential political distortions that could affect the level of local government lending, in line with theory.

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

Robustness checks. Table B.3 shows the results when including additional controls (column (1)). The additional controls are: 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. 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.

81. Details on variables definitions are in the Table notes.

Table B.1: Crowding out and political interference

	Credit growth											
	MP characteristics					Mayor characteristics					Tests with *EPIC*	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Bank Exposure	-1.112***	-1.244***	-1.032***	-0.990***	-1.304***	-1.062***	-1.008***	-1.027***	-1.043***	-0.903***	-1.203***	
	(0.336)	(0.309)	(0.284)	(0.281)	(0.287)	(0.290)	(0.259)	(0.281)	(0.266)	(0.262)	(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 proxy for political interference with banks. Columns (1)-(10) test heterogeneity by local politicians' characteristics. Columns (1)-(5) relate to MPs characteristics 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-Touati (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 main 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***	-0.969***	-0.931***	-1.021***	-1.090***
	(0.267)	(0.267)	(0.224)	(0.236)	(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

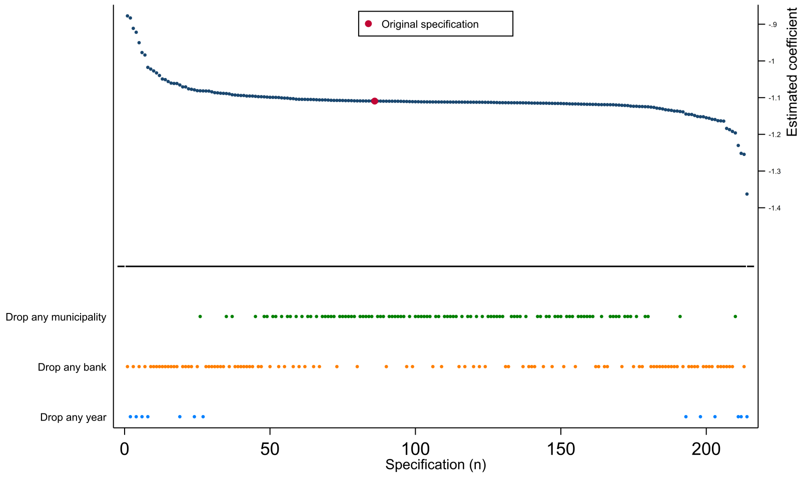
Note: This table presents tests of the assumptions that uphold a causal interpretation of the results presented in Table 2. It reports the results of estimating variations of specification (2). The outcome variable is the mid-point growth rate of credit granted to firm f by bank b. The main independent variable is exposure to local government debt demand shocks measured at the bank×region×time level, 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 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. 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×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***	-1.205***	-1.089***	-0.972***	-1.142***
	(0.261)	(0.273)	(0.262)	(0.272)	(0.305)
Controls	✓	✓	✓	✓	✓
Add. controls	✓				
Firm×Time FE	✓	✓	✓	✓	✓
Sample	Full	$\geq$ 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. Columns (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 main 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, county, 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 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 drawback being that the instrument loses predictive power for the most recent periods. In columns (3) and (4), I show the results when $\Delta D^{gov}_{br,t-1}$ is the standard growth rate and the shift-share is defined using weights normalized by $D^{gov}_{br,t-1}$ instead of $D^{tot}_{br,t-1}$. In columns (5) and (6), I show results using a different heterogeneous exposure 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 crowding out effect, with quantitative implications in line with my baseline result (except for the last specification which yields a larger estimate).

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.

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:

$$\Delta D_{fbt} = d_{ft} + \beta BankExposure_{brt} + \gamma BankExposure_{-brt} + \Phi \cdot \mathbf{X}_{brt} + \varepsilon_{fbt} \tag{11}$$

where $BankExposure_{-brt}$ 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 (11) since with firm×time fixed effects $BankExposure_{b,r,t}$ and $BankExposure_{-b,r,t}$ are collinear, and this problem is not solved by looking at firm-level effects. Propositions 5 and 6 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 substitutes of bank b (the main lender of firm f, or the second

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

	Credit growth					
	RF (1)	IV (2)	RF (3)	IV (4)	RF (5)	IV (6)
$BankExposure$ (2006 shares)	-0.770***					
	(0.288)					
Change in local govt loans ΔD^{gov}_{brt}		-0.805***				
		(0.300)				
$BankExposure$ (norm. $D^{gov}_{br,t-1}$)			-0.191***			
			(0.055)			
$\frac{D^{gov}_{brt}-D^{gov}_{br,t-1}}{D^{gov}_{br,t-1}}$				-0.206**		-0.362**
				(0.091)		(0.168)
$\frac{D^{gov}_{br,t-1}}{D^{gov}_{r,t-1}}\Delta D^{gov}_{rt}$					-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 $D^{gov}_{br,t-1}$. 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 $D^{gov}_{br,t-1}$ 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 main 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: Firm×bank-level effects: Robustness checks (3)

	Credit growth				
	MPGR (1)	MPGR (2)	MPGR (3)	Log change (4)	Norm. diff (5)
$BankExposure$	-1.109***	-1.109***	-1.109***	-0.068*	-0.153***
	(0.139)	(0.306)	(0.334)	(0.037)	(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 main 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.

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.[82] 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 thus abstract from this term so that my analysis may underestimate the true effect.

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

	Variation in n_f and ω_{bf}		Variation in n_f
	Max wgt.	Value wgt.	Equal wgt.
	(1)	(2)	(3)
β	-1.339***	-4.571***	-1.924***
	(0.306)	(0.766)	(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 when one faces a shock. 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: $\Delta D_{fbt} = d_{ft} + \beta BankExposure_{brt} + \gamma BankExposure_{-brt} + \Phi \cdot \mathbf{X}_{brt} + \varepsilon_{fbt}$ where $BankExposure_{-brt}$ captures the shock of the other banks firm firm f. Columns (1) to (3) correspond to different definitions of $BankExposure_{-brt}$. In column (1), $BankExposure_{-brt}$ is the shock of firm f's main borrower (second main borrower if bank b is the main borrower). 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 main 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 real variables

Effect of the firm-level average of bank-level changes in local government lending. Table B.7 presents the firm-level effects obtained from estimating (6), when $FirmExposure_{ft}$ is used as an instrument for its "realized quantity" version $\overline{\Delta D^{gov}_{brt}}^f = \sum_b \omega_{fb,t-1} \Delta D^{gov}_{brt}$ which is the average increase in local government loans at the lenders of firm f.

82. In this case, the coefficients are very imprecisely estimated. This is because within-firm collinearity of $BankExposure_{brt}$ and $BankExposure_{-brt}$ is more of an issue in these two cases.

Table B.7: Firm-level real effects: IV results

	gr(credit)	gr(capital)	gr(wage bill)
	IV	IV	IV
	(1)	(2)	(3)
Change in local govt loans $\overline{\Delta D_{brt}^{gov}}^{f}$	-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 (6), when $FirmExposure_{ft}$ is used as an instrument for $\overline{\Delta D_{brt}^{gov}}^{f} = \sum_b \omega_{fb,t-1} \Delta D_{brt}^{gov}$. 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 (0.41). Since I have shown that firms do not attenuate the effect of credit cuts by substituting towards other banks, the reduction in credit by a bank that lends one extra euro to local governments is equal to the total reduction in credit experienced by the borrowers of this bank. To obtain the effect on investment, I then use $\mathrm{d}K = \eta^K \frac{K}{D}\mathrm{d}D$, where I use the sample means of K and D. I proceed similarly for labor.

Another method is to use the IV coefficients in Table B.7. When bank b lends one extra euro local governments in region r, the capital shortfall at the borrowers of b in region r is given by $\beta^K \sum_{f \in \mathcal{F}_{br}} \frac{\omega_{fb,t-1}}{D_{brt}^{tot}} K_{f,t-1} = \beta^K \sum_{f \in \mathcal{F}_{br}} \frac{1}{D_{brt}^{tot}} D_{fb,t-1} \frac{K_{f,t-1}}{D_{f,t-1}} \approx \beta^K \mathrm{card}(\mathcal{F}_{br}) \frac{\bar{D}_{fb,t-1}}{\bar{D}_{brt}^{tot}} \frac{\bar{K}_{f,t-1}}{\bar{D}_{f,t-1}}$ where upper bar denotes sample mean.

Additional tests of identifying assumptions. Table B.8 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.

Robustness checks. Table B.9 presents the results when dropping firm-level controls and when including additional firm-level controls (interest coverage ratio, tangibles ratio, EBIT-to-sales ratio, and cash ratio). I also 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. In Table B.10, I show 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.8: Firm-level real effects: tests of identifying assumptions

	gr(credit)				
	(1)	(2)	(3)	(4)	(5)
Firm Exposure	-0.586***	-0.564***	-0.495***	-0.581***	-0.566***
	(0.121)	(0.135)	(0.126)	(0.126)	(0.126)
Pub. Proc. × Firm Exposure					-0.208
					(0.248)
Controls	✓	✓	✓	✓	✓
Baseline FE	✓	✓	✓	✓	✓
Region×Ind×Time FE	✓	–	–	–	–
Municipality×Ind×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***	-0.226**	-0.219**	-0.228***	-0.214**
	(0.085)	(0.103)	(0.097)	(0.088)	(0.092)
Pub. Proc. × Firm Exposure					-0.337
					(0.272)
Controls	✓	✓	✓	✓	✓
Baseline FE	✓	✓	✓	✓	✓
Region×Ind×Time FE	✓	–	–	–	–
Municipality×Ind×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*	-0.062*	-0.085**	-0.043	-0.064*
	(0.028)	(0.034)	(0.035)	(0.030)	(0.033)
Pub. Proc. × Firm Exposure					0.050
					(0.104)
Controls	✓	✓	✓	✓	✓
Baseline FE	✓	✓	✓	✓	✓
Region×Ind×Time FE	✓	–	–	–	–
Municipality×Ind×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 (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. The main independent variable is firm exposure to crowding out, defined in (7) 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. 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.

Table B.9: Firm-level real effects: Robustness checks (1)

	gr(credit)								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Firm Exposure	-0.559***	-0.589***	-0.619***	-0.590***	-0.493***	-0.391**	-0.577***	-0.577***	
	(0.122)	(0.119)	(0.093)	(0.123)	(0.144)	(0.185)	(0.076)	(0.086)	
Firm Exposure (alt.)									-0.533***
									(0.084)
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**	-0.232***	-0.210**	-0.267***	-0.206**	-0.203	-0.232***	-0.232***	
	(0.087)	(0.087)	(0.082)	(0.089)	(0.090)	(0.136)	(0.083)	(0.074)	
Firm Exposure (alt.)									-0.152*
									(0.080)
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**	-0.067**	-0.062**	-0.065**	-0.047	-0.075	-0.063**	-0.063*	
	(0.030)	(0.030)	(0.030)	(0.031)	(0.031)	(0.048)	(0.030)	(0.031)	
Firm Exposure (alt.)									-0.049
									(0.031)
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 (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. The main independent variable is firm exposure to crowding out, defined in (7) 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. "Wgt avg of bank-controls" indicates that I 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.10: Firm-level real effects: Robustness checks (2)

	Effect of exposure to local government debt shocks		Credit-to-input sensitivities	
	RF	RF	IV	IV
	(1)	(2)	(3)	(4)
Firm Exposure	-0.065**	-0.410***		
	(0.029)	(0.139)		
gr(credit) (std.)			0.407**	0.148*
			(0.185)	(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 (6). The outcome variable is the firm-level growth rate of credit (mid-point growth rate), fixed assets and total wage bill. The main independent variable is firm exposure to crowding out, defined in (7) 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. "Wgt avg of bank-controls" indicates that I 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.

B.3 Aggregate input usage

Lower bound. In the case where the distribution of firm and bank size is non-degenerate, the capital shortfall is given by:

$$\begin{aligned}\mathcal{L}(K_t) &= -\eta^K \chi(1-\nu)\Delta D_t^{gov} - \eta^K \chi\nu \sum_f \tfrac{K_{ft}(0)}{K_t(0)} \Delta D_{ft}^{gov} \\ &\leq -\eta^K \chi\nu \sum_f \tfrac{K_{ft}(0)}{K_t(0)} \overline{\Delta D_{brt}^{gov}}^f\end{aligned}$$

To compute this last quantity, I use $\hat{\beta}^K = -\eta^K \chi\nu$ and $K_{ft}(0) = \hat{K}_{ft} - \hat{\beta}^K \overline{\Delta D_{brt}^{gov}}^f$ where $\hat{K}_{ft}$ is obtained using the fitted growth rate of capital from the regression.

This quantity can starkly differ from $-\eta^K \chi\nu\Delta D_t^{gov}$—and even have an opposite sign—when the variance of $\overline{\Delta D_{brt}^{gov}}^f$ is high. To avoid this issue, I perform the baseline quantification using the coefficient on $FirmExposure_{ft}$ which has a much lower variance, and provide the quantification using the coefficient on $\overline{\Delta D_{brt}^{gov}}^f$ as a robustness check.

I proceed similarly for labor. I compute output loss at the industry level using industry specific capital shares before aggregating across industries.

Robustness checks. A potential caveat of the previous computation is that results may be highly dependent on the joint distribution of the shock and of firm size. This may be an issue to interpret our multiplier computation if this joint distribution is not 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. In this case, the formula for the output shortfall is as in the main text: $\mathcal{L}(Y_t) = (\alpha\eta^K\chi + (1-\alpha)\eta^L\chi)\Delta D_t^{gov}$. A lower bound (on average across years) is $(0.33 \times 0.189 + 0.66 \times 0.051) \times 0.0145 = 0.14\%$, slightly higher than by baseline. This translates into a multiplier equal to -0.28.

Besides, the aggregate multiplier quantification is also in line with the back-of-the-envelope computations from the reduced-form results in Section 6. The relationship is $\mathrm{d}Y = \alpha\frac{Y}{K}\mathrm{d}K + (1-\alpha)\frac{Y}{L}\mathrm{d}L$ which using sample mean values of the different variables yields 0.21.

Finally, I find a similar multiplier using the specification with $\overline{\Delta D_{brt}^{gov}}^f$ instead of $FirmExposure_{ft}$. I find an average multiplier equal to 0.17, but the standard deviation of the multiplier is much higher (0.2). This is due to the high variance of the underlying bank-level growth rates which can take highly negative (positive) values when the aggregate growth rate is positive (negative).

Equilibrium effects. I estimate $1-\nu$ by regressing the change in net interbank borrowing on the increase in local government lending, instrumented by $BankExposure$. I estimate the following specification:

$$\Delta B_{bt} = \alpha_t + \beta\Delta D_{brt}^{gov} + \Phi \cdot \mathbf{X}_{brt} + \varepsilon_{brt}$$

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×region level (the shock in my baseline specification) or at the bank level. I can additionally include region×time fixed effects to compare only banks present in the same region. I include the same bank controls as in my baseline specification. Table B.11 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.12 shows pre-trending tests.

With the model accounting for both interbank and intrabank frictions. The decomposition presented in the main text is exact when considering a bank×region unit as a bank. When looking at the effect on interbank borrowing, this amounts to considering that all regions within a bank have the same value for ΔB_{brt}. As shown in the two-layer model with regions within banks presented in Appendix C.2.3, not considering within-bank transfers and making this approximation is innocuous when within-banks and across-banks frictions have a similar order of magnitude, as shown in 5.1.[83]

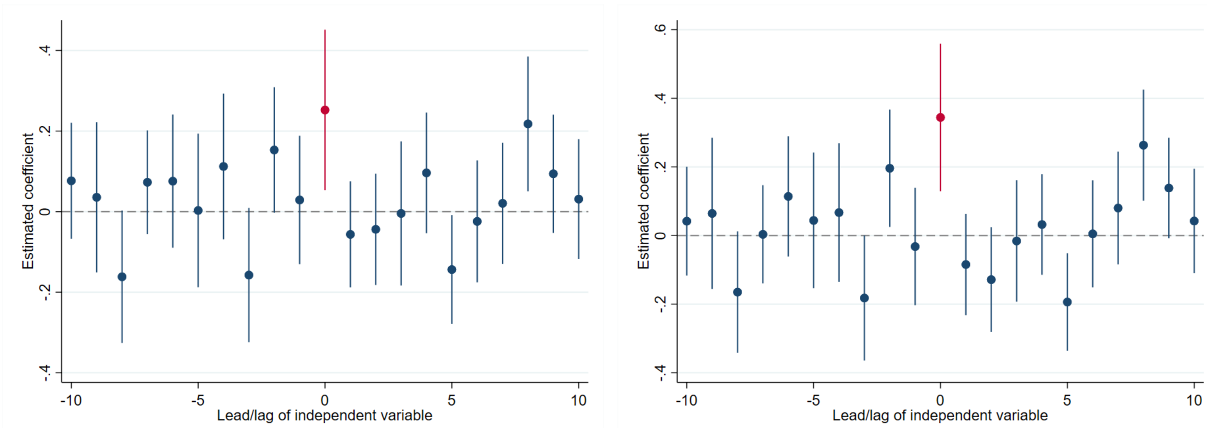
83. Otherwise, if frictions on internal capital markets are lower than interbank frictions, which is the natural assumption, omitting within-bank transfers brings us back to the lower bound argument.

Table B.11: Effect on interbank borrowing

	Change in net interbank borrowing							
	Bank×region-level						Bank-level	
	RF	RF	RF	IV	IV	IV	RF	IV
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$BankExposure_{brt}$	0.187***	0.205***	0.250***					
	(0.058)	(0.056)	(0.063)					
Change in local govt loans ΔD^{gov}_{brt}				0.167***	0.184***	0.224***		
				(0.053)	(0.052)	(0.058)		
$BankExposure_{bt}$							0.283***	
							(0.068)	
Change in local govt loans ΔD^{gov}_{bt}								0.278***
								(0.067)
Controls		✓	✓		✓	✓	✓	✓
Time FE	✓	✓	✓	✓	✓	✓	✓	✓
Region×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 measured at the bank×region×time level, 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 columns labelled IV, *BankExposure* is used as an instrument for the actual increase in bank×region-level local government lending. In columns (6) and (7), the independent variable is defined at the bank×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.

Table B.12: Effect on interbank borrowing: pre-trending tests



(a) Time FE

(b) Region×Time FE

Note: This figure 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 measured at the bank×region×time level, 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 include leads and lags on bank exposure and of 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. The dot is the point estimate and the bar is the 95% confidence interval.

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 depositors, 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. There is an identical depositor assigned to each bank, and depositors do not arbitrage across banks. All agents are price-takers.[84]

Local governments. Local governments operate on a unit square, with $b \in [0, 1]$ indicating the first dimension (banks) and $m \in [0, 1]$ indicating the second dimension (local governments borrowing from a bank). Local governments take the interest rate on loans r_b^g as given. Each local government has the following demand for loans:

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

with $\epsilon^g \geq 0$. z_m is a shock to the demand for debt by local government m. It is a choice variable of local policymakers which may be correlated to private sector activity. 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 $b \in [0, 1]$ indicating the first dimension (banks) and $f \in [0, 1]$ indicating the second dimension (local governments borrowing from a bank). Firms take the interest rate on loans r^c as given. Each firm has the following demand for loans:

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

with $\epsilon^c \geq 0$.[85] θ_f captures firm-level idiosyncratic shocks (e.g. technological shocks or shocks to the demand of a specific variety). 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 the aggregation results.

Households. Households save in the form of deposits. I assume that the supply of deposits is given by:

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

with $\epsilon^s \geq 0$.[86]

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

84. For the bank, this is as if there existed a continuum of perfectly-competitive banks with the same sets $\mathcal{F}_b$, $\mathcal{G}_b$. I solve the model with monopolistics banks in Section C.2.4, and all results remain identical.

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

86. If the savings function is derived from a consumption savings trade-off, we obtain $\epsilon^s =$ which for typical parameter values is approximately equal to 1.

borrowing. To model imperfect functionning of the interbank market, I assume that banks face a quadratic cost. The problem of the bank is:

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

subject to: $C_b^{corp} + C_b^{gov} = S_b + B_b$. The equilibrium prices consistent with the first-order condition of banks are:

$$\begin{aligned} r_b^c &= r_b^g = r_b^s = r_b \\ r_b &= i + \phi B_b \end{aligned}$$

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:

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

where λ is the share of local government loans in the bank loan portfolio in steady-state, which is 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 depends on the interbank rate and on the deviation of the bank-specific local government debt demand shock from the aggregate shock. The extent to which the bank-specific interest rate depends on the bank-specific demand shock depends on ϕ.[87] 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 bank-specific interest rate does not depend on the bank-specific demand shock but only on the aggregate shock. Conversely, when ϕ tends to $+\infty$, banks cannot use the interbank market, and the bank-specific interest rate only depends on the bank-specific demand shock, not on the aggregate shock.

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

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

that can be used to recover the relationship between a demand-driven change in local government loans and corporate credit.[88]

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

88. We are interested in the relationship between two endogeneous variables as a response to the set of shocks. I therefore solve for each variable as a function of the shock, and substitute for local government loans in the expression for corporate credit. In the specific case where $\epsilon^g = 0$, the change in local government loans is equal to the underlying shock.

Aggregate and relative crowding out. Let us derive the aggregate crowding out parameter that relates corporate credit $\hat{C}^c$ to the demand-driven change in local government loans $\hat{C}^{gov}$. The aggregate crowding out relationship is given by:

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

where $\chi = \frac{\epsilon^c}{\epsilon^s+(1-\lambda)\epsilon^c}$. χ is the aggregate crowding out parameter, which is decreasing in the relative elasticity of the supply and demand of loans ϵ^s/ϵ^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.

Let us now derive the relative crowding out parameter that relates $\hat{C}_{fb}$ to the bank-specific demand-driven increase in local government loans $\hat{C}_b^{gov}$:

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

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 $\phi \to 0$ (no interbank frictions), $\nu = 0$, and when $\phi \to +\infty$ (complete fragmentation), $\nu = 1$. We can re-write this relationship as:

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

where χ is the aggregate crowding out parameter previously defined. Therefore, reflecting the result on the bank-specific interest rate, 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 fragmented, corporate credit by bank b depends only on the bank-specific increase in local government loans, and not on the aggregate increase. Therefore, as long as $\nu < 1$ the relative crowding out parameter $\chi\nu$ captures only part of the effect because banks not directly exposed to increased demand for local government loans lend to other banks, so that corporate credit also falls at these banks.

To link these results obtained in a 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 the observed growth rates to the log-deviations from the deterministic equilibrium. Therefore, firm$\times$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. The next two propositions summarize the main results, using the notations of the main text.

Proposition 1 *(1) The aggregate crowding out relationship is given by:*

$$\Delta C^{corp} = -\chi\Delta C^{gov} \tag{12}$$

where $\chi = \frac{\epsilon^c}{\epsilon^s+(1-\lambda)\epsilon^c}$.

(2) The bank$\times$firm-level crowding out relationship writes:

$$\Delta C_{fb} = \theta_f - \chi(1-\nu)\Delta C^{gov} - \chi\nu\Delta C_b^{gov} \tag{13}$$

where $\nu \in [0,1]$ *is monotonically increasing in the interbank frictions parameter* ϕ*,* $\nu = 0$ *when* $\phi = 0$ *(perfectly integrated banks) and* $\nu = 1$ *when* $\phi \to +\infty$ *(fully fragmented banks).*

Proposition 2 *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. Net interbank borrowing of bank b is given by:

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

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.[89] I denote this variable ΔB_b.

Proposition 3 *The change in net interbank borrowing of bank b is given by:*

$$\Delta B_b = (1-\nu)(\Delta C_b^{gov} - \Delta C^{gov}) \tag{14}$$

Relationship with the empirical strategy. The reduced-form analysis presented in Section 4 aims at identifying the relative crowding out parameter $\chi\nu$. To link the model and the identification strategy, 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 classic Khwaja-Mian assumption).[90] 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:

$$\begin{aligned} \Delta C_{fbt} &= \theta_{ft} - \chi(1-\nu)\Delta C_t^{gov} - \chi\nu \Delta C_{bt}^{gov} + \iota^c \xi_{bt} \\ \Delta C_{bt}^{gov} &= \kappa Z_{bt}^{gov} + \varkappa Z_t^{gov} + \iota^g \xi_{bt} \end{aligned} \tag{15}$$

where ι^c, ι^g, κ and $\varkappa$ are parameters.[91] The coefficient we want to identify is $\beta = \chi\nu$. Section 4.1 presents the empirical strategy to estimate this coefficient. Note that 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.

C.2 Extensions

To simplify the exposition, in all extensions I assume that the interest elasticity of local government debt is 0.

C.2.1 Introducing frictions on increasing total balance sheet size

Assume now 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

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

90. This works in our setting: we have never used the fact that the sets $\mathcal{F}_b$ are disjoint.

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

to process new loans, etc. I include a fixed equity amount per bank $E_b = E$ so that the problem makes sense in the limit $\varphi \to +\infty$. Banks now maximize:

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

subject to: $C_b^{corp} + C_b^{gov} = S_b + B_b + E_b$. Solving for this model, we find that for all $f \in \mathcal{F}_b$,

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

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

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

S^* and i^* are steady-state values that now depend on φ. The aggregate crowding out parameter is now a complex function of φ. When $\varphi = 0$ and $E^* = 0$, we recover $\chi = \frac{\epsilon^c}{\epsilon^s + (1-\lambda)\epsilon^c}$. When $\varphi \to +\infty$, 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.[92]

As before, ν is in $[0,1]$, $\nu = 0$ when $\varphi = 0$ and $\nu = 1$ when $\varphi \to +\infty$. In the frictionless case, we do not have bank-specific crowding out. In the complete fragmentation 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 now 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:

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

The first-order condition writes:

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

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

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

We think of loans granted by the different banks as highly substitutable from the point of view of the firm, therefore we expect $\sigma > \epsilon^c$. Hence, the demand directed towards a given bank is decreasing in this banks' interest rate, but increasing in the interest rate charged by other banks, captured by the term $\hat{r}_f^c$.

Banks solve the same problem as before. To simplify the problem, we make the following assumptions: each bank lends to only one firm. Therefore, the sets $\mathcal{B}_f$ form a partition of the

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

sets of all banks $[0,1]$. Besides, assume that the firm demand shock is the same for all firms and equal to 0. Besides, let us denote $Z_f^{gov} = \frac{1}{M}\sum_{b\in\mathcal{B}_f} Z_b^{gov}$. Using this assumption, 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×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 estimate yields a lower bound on the aggregate crowding out parameter.

C.2.3 Regions within banks

Let us now 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 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:

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

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×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. If $\nu^b = \nu^r$, corporate credit depends on $\hat{C}_{br}^{gov}$ but not on $\hat{C}_b^{gov}$. The intuition is that in this case the bank layer is irrelevant since it is equally hard to smooth shocks across and within banks.

Aggregating across firms borrowing at all banks in all regions, one finds that the aggregate crowding out parameter is unchanged and equal to χ. This also implies that as long as $\nu^r \neq 0$, there will be region-level crowding out: we have that

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

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 Banks as monopolist

This changes the levels of interest rates set by banks but all formulas in deviation from the steady-state remain the same.

C.2.5 Introducing a Ricardian response

When there are several banks, Ricardian equivalence does not hold. Assume there is a representative household supplying deposits to each bank, indexed by b. There is a unit mass of such households. Households share taxes equally so that each household pays $T_{b1} = T_1$ and $T_{b2} = T_2$. Then, consumption is given by $\mathcal{C}_{b1} = e_1 - S_b - T_1$ and $\mathcal{C}_{b2} = (1 + r_b^s)S_b + w_b L_b - T_2$. Besides, $C^{gov} = \int_b C_b^{gov} db = G_1 - T_1$ and $T_2 = G_2 + \int_b (1 + r_b^g) C_b^{gov} db$. The interest rate on household b's deposit will be different from the average interest rate paid on government debt (in equilibrium). Therefore, households consumption does depend on C_b^{gov}, and Ricardian equivalence does not hold. The key intuition is that what matters for the Ricardian effect is households anticipation of future taxes, which depends on the total debt of the government, and not only on the debt held by the household's bank.

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.[93] Solving my model with this new 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 χ is now equal to $\chi = \frac{\epsilon^c}{(1-\kappa\lambda)\epsilon^s + (1-\lambda)\epsilon^c}$. If $\kappa = 1$, the aggregate crowding out effect, which is now given by $(\kappa - 1)\chi$ will be zero.

κ is the average 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, in practice agents are not fully Ricardian. In fact, the empirical evidence has led to widely different estimates of the savings response to a debt-financed reduction in current taxes.[94] I provide results for a savings response equal to 0.59 (the average of the most recent estimates)[95]. Second, one needs to take into account the fact that only a fraction of increased savings take the form of deposits that may be used by banks to finance loans. In my baseline quantification, I use the share of bank deposits in French households assets (6%). 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.[96] I obtain $\kappa^{baseline} = 0.59 \times 0.06 = 0.035$ and $\kappa^{upper} = 0.59 \times 0.2 = 0.12$.

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

94. In reviews of the early literature on this topic, Seater (1993) states that "Although tests of Ricardian equivalence do not quite give an unambiguous verdict on that proposition's validity, I think it reasonable to conclude that Ricardian equivalence is strongly supported by the data", while Romer (2006) writes that "There is little reason to expect Ricardian equivalence to provide a good first approximation in practice".These contrasting results likely reflect the fact that empirically testing the Ricardian equivalence hypothesis using macro data is challenging, and that many articles provide only indirect tests that give rise to various interpretations.

95. 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.

96. I implicitly assume that the marginal euro of savings is split across assets as the average euro.

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% when accounting for Ricardian effects (12% using κ^{upper}).

C.2.6 Introducing household loans

In this case, the aggregate crowding out parameter is given by:

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

C.2.7 Introducing differential cost

Assume now 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 for instance due to political connections.

Assume that banks maximize:

$$\max_{C_b^{corp}, C_b^{gov}, S_b} r_b^c C_b^{corp} + r_b^g C_b^{gov} - r_b^s S_b - iB_b - \frac{\phi^g}{2} C_b^{gov2} - \frac{\phi^c}{2} C_b^{corp2} \tag{16}$$

The crowding out parameter depends only of the cost on corporate credit, not on the cost of local government loans or on the difference between the two parameters.

From data on French households assets, financial assets account for 20% (the remaining being real estate (61%), professional assets (11%), durable goods and others (8%)). Among financial assets, bank deposits (including regulated deposit accounts) account for 30%. The other main categories are life insurance (40%) and equity (22%).

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):

$$\Delta D_{fb} = d_f + \beta \Delta D_b^{gov} + \varepsilon_{fb}$$

Note that 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 shocks (ξ_b in the equations from the main text). I instrument ΔD_b^{gov} by the shift-share instrument $BankExposure_b = \sum_c \omega_{bm,-1}^{gov} \Delta D_m^{gov}$. The standard exclusion restriction writes:

$$\mathbb{E}\left[\sum_c \omega_{bm,-1}^{gov} \Delta D_m^{gov} \varepsilon_{fb} \middle| d_f\right] = 0 \tag{17}$$

Identification based on shocks. The condition (17) is immediately satisfied if the shocks ΔD_m^{gov}, but it does not require it. The less restrictive requirement is that the instrument will be valid if the 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 (2020)). I follow the 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:

$$\mathbb{E}\left[\sum_c \Delta D_m^{gov} \sum_{f,b} \tilde{\omega}_{bm,-1}^{gov,f} \varepsilon_{fb}\right] = 0 \tag{18}$$

That is, ΔD_m^{gov} must be orthogonal to the bank-specific shocks aggregated using the (within-firm) deviations in exposure of banks to municipality c. Put differently, it must not be the case that banks experiencing negative shocks ε_{fb} have systematically higher exposure to municipalities where ΔD_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:

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

where $\bar{\omega}_{bc,-1}^{gov,f}$ is the sum of $\omega_{bm,-1}^{gov}$ for the set of banks b lending to f. Since ΔD_m^{gov} is correlated to d_f, the correlation coming for the first term in the parenthesis is unlikely to be zero.[97]

There are three main threats to identification. The first is that banks sort into locations such that banks experiencing negative credit supply shocks have high exposure to municipalities where local governments increase their borrowing. An example of problematic sorting would be if poorly capitalized banks are systematically highly exposed to municipalities where local governments increase their borrowing. Figure 5 in the main text shows that banks with high and low *BankExposure* are relatively similar, which limits this concern.[98]

97. This is all the more true since ΔD_m^{gov} and d_f are likely to be more correlated when $\bar{\omega}_{bc,-1}^{f}$, that is when the banks lending to f have large exposure weights to c, which given the local nature of lending markets indicates that f is likely to be located in or close to municipality c.

98. Borusyak, Hull, and Jaravel (2020) suggest estimating the equivalent shock-level correlations (18) to address the fact that banks' exposure to common shocks leads to incorrectly low standard errors. However, in the case of falsification tests, estimating the bank-level regression is conservative.

A second threat is if municipality-level local government debt demand shock are correlated to bank-level credit supply shock through other channels than crowding out. What would be problematic is if (i) local government debt is correlated to some variable X_c (e.g. firm characteristics in location c), (ii) X_c affects banks' balance sheets and ability to lend through the same exposure weights $\omega^{gov}_{bm,-1}$. There are two potentially problematic channels: non-performing loans and deposits. If local government debt increases in municipalities where bankruptcies are high and local government debt exposure weights are the same as corporate credit exposure weights, then $\sum_c \omega^{gov}_{bm,-1}\Delta D^{gov}_m$ is correlated to $\sum_c \omega^{gov}_{bm,-1} Bankruptcy_c$, which significantly affects $\Delta NonPerformingLoans_b$, which may in turn enter in 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 government 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. The reason is that municipalities are very narrowly-defined geographical units (i.e. the public investment projects financed by local government loans have economic effects that do not follow municipality borders) and there is high dispersion in local government debt shocks across neighbouring municipalities. Figure D.1 shows that ΔD^{gov}_m is in fact not correlated with the municipality-level GDP growth, private credit growth, change in the number of banked firms or bankruptcy rate.[99] However, again, it does not need to be the case that municipality-level changes in local government debt are uncorrelated to other municipality-level variables for condition (18) to be satisfied. What matters is that other municipality-level variables do not generate bank-level shocks correlated to $BankExposure$. I directly test 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.

Third, I address the concern that ΔD^{gov}_m aggregates the shocks of the banks lending to municipality c, that also directly enter the residual of my regression, introducing a mechanical bias. This concern is of a different nature. The two preceding caveats would apply if we observed the true municipality-level local government debt demand shock. The present point relates to the fact that I use realized quantities as a proxy for the underlying shock. As suggested by Borusyak, Hull, and Jaravel (2020), 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.[100,101] Table D.2 reports similar results using these definitions of $BankExposure$.

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 (2020) show that the conditions for consistency are that there is a sufficiently large number of shocks with sufficient shock-level variation and shocks exposure is not too concentrated.

Panel A of Table D.1 documents a large dispersion in ΔD^{gov}_m, which persists when residualizing on time, region×time or municipality fixed effects. The dispersion is also displayed on the maps of Figure 3, which show a large degree of variation even within very narrowly defined

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

100. For the LOO, let $\Delta D^{gov}_{c,-b} = \sum_{b' \neq b} \frac{D^{gov}_{b'c,-1}}{D^{gov}_{c,-1}} \Delta D^{gov}_{b'c}$ and $BankExposure_b = \sum_c \omega^{gov}_{bm,-1}\Delta D^{gov}_{c,-b}$. For the AW, obtain municipality-time fixed effects in the following regression: $\Delta D^{gov}_{bct} = \alpha_{ct} + \alpha_{bt} + \varepsilon_{bct}$. Define $BankExposure_b$ using the estimated α_{ct} instead of ΔD^{gov}_m.

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

geographic clusters. Besides, the exposure shares are not too concentrated. Define municipality-level weights as $s_{mt} = \sum_b e_{bt}\omega^{gov}_{bm,t-1}$ where e_{bt} are bank-level weights.[102] 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 numbers 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.[103]

Identification based on shares. Importantly, a correlation between ΔD^{gov}_m 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. I now show that this is unlikely to be the case, although shares exogoneity is less intuitive as a source of identification. As shown by Goldsmith-Pinkham, Sorkin, and Swift (2020) $\mathbb{E}[\varepsilon_{fb}\omega^{gov}_{bm,-1}|d_f] = 0$ for all c with $\Delta D^{gov}_m \neq 0$ is a sufficient condition for the shift-share IV to be unbiased and consistent. This assumption is credible in my setting for two reasons.

First, the variable used to define the shares, i.e. local government debt, is specific to the shock under study. This makes it less likely that my exposure shares are instead picking up banks' exposure to other types of local shocks. As a placebo test, I repeat the construction of $BankExposure$ with corporate credit exposure weights. Table D.2 shows that this alternative variable does not predict a decline in corporate credit. This further alleviates concerns that $BankExposure$ is picking up shocks correlated to ΔD^{gov}_m occurring on the corporate credit market (e.g. bankruptcies) that negatively affect banks' ability to lend.[104]

Second, there is a large number of municipalities, so that the correlation between bank-level shocks and banks' exposure to any given municipality is likely to be small. A way to formalize this intuition is to compute the municipality-level Rotemberg weights that summarize the identifying variation used by the shift-share instrument. I find that the Rotemberg weights are very dispersed, i.e. identification does not rely on variation stemming from a small number of municipalities. The 5 largest Rotemberg weights account for roughly 25% of the positive weight in the estimator.[105,106] The advantage of dispersed Rotemberg weights is that it reduces the sensitivity of the Bartik instrument to non-random exposure to a given municipality. The drawback is that 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.

102. My baseline firm-bank regression is unweighted. When running tests at the bank-level, consistency with the baseline regression requires weighting by the number of firms per bank in the sample of multibank firms. To avoid a weighting too dependent on the sample definition, I weight bank-level regressions by the lagged corporate loan portfolio of each bank.

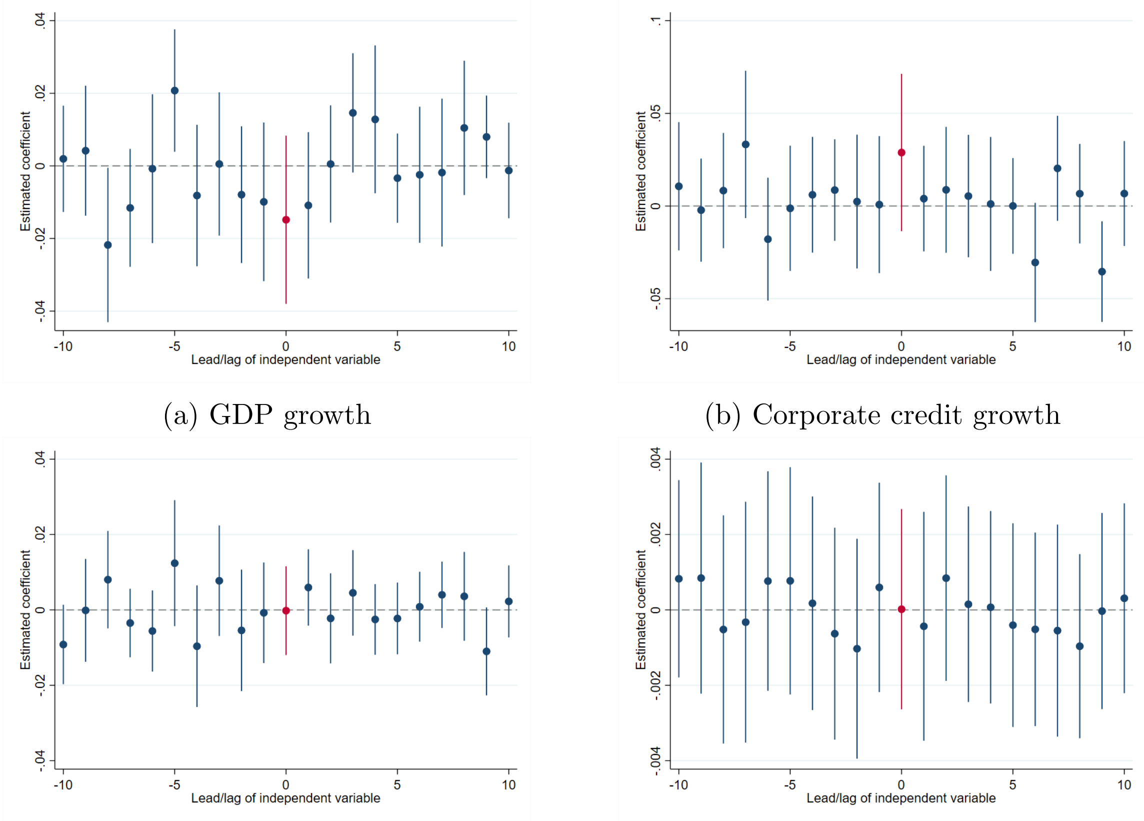
103. To provide a benchmark, Borusyak, Hull, and Jaravel (2020) show that there methodology is relevant in the Autor setting where the inverse Herfindahl is 58.4 and the largest share is 6.5%.

104. 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.

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

106. These 5 instruments are the municipalities of Strasbourg, Ajaccio, Toulouse, Dijon and Bordeaux, five mid-size French municipalities located in different regions of France. Besides, repeating this analysis at the municipality×time-level shows that these highest weight municipalities vary significantly across time periods.

Figure D.1: Municipality-level balance tests



(a) GDP growth

(b) Corporate credit growth

(c) Growth in number of banked firms

(d) Bankruptcy rate

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 all 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 register. 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 (2020), the regressions are weighted by $s_{mt} = \sum_b e_{bt}\omega^{gov}_{bm,t-1}$ where e_{bt} is the lagged corporate loan portfolio of each bank×region. The dot is the point estimate and the bar is the 95% confidence interval.

Table D.1: Shock-level summary statistics

Panel A: Summary statistics on municipality-level shocks

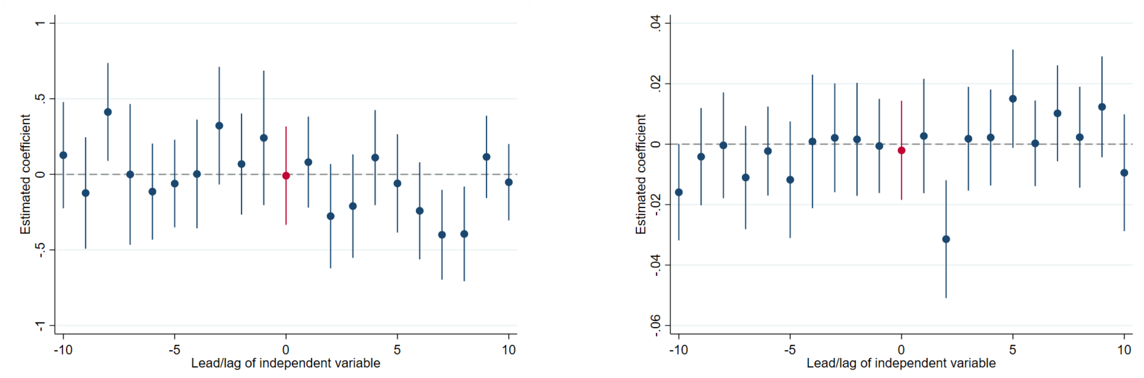
	count	mean	sd	p25	p50	p75
Municipality-level growth ΔD^{gov}_{mt}	108,062	0.010	0.092	-0.019	-0.004	0.024
Residualized on time FE	108,062	0.000	0.091	-0.029	-0.012	0.013
Residualized on region×FE	108,062	0.000	0.090	-0.029	-0.011	0.015
Residualized on municipality FE	108,062	-0.000	0.091	-0.030	-0.013	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 the municipality-level shocks, that is the municipality-level local government loans growth rates. Panel B computes the municipality-level inverse Herfindahl index $1/\sum_{m,t} s^2_{m,t}$ and the largest $\sum_{m,t}$ weight, and then the same quantities when weights are aggregated across time for a given municipality.

Figure D.2: Bank-level balance tests



(a) Change in deposits (b) Change in non-performing loans

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×region×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×region. The dot is the point estimate and the bar is the 95% confidence interval.

Table D.2: Additional robustness checks

	Credit growth			
	(1) LOO	(2) AW	(3) Corporate shares	(4) Corporate shares
Bank Exposure (LOO)	-1.388***			
	(0.383)			
Bank Exposure (AW)		-0.754***		
		(0.119)		
Bank Exposure (corporate)			0.083	0.046
			(0.126)	(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) I conduct placebo tests where I construct bank exposure using as the sum of municipality-level increases in local government debt weighted by municipalities' corporate credit shares in the bank's loan portfolio. In all tests, 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 main 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 Bank shocks vs. substitution effects in the Khwaja-Mian framework

This Appendix details the methods outlined in Section 4.2.3 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 $BankExposure_{b,r}$ variable as B_b. All proofs are stacked at the end.

E.1 The KM framework

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 $\Delta D_{b,f} = \frac{D_{b,f} - D_{b,f,-1}}{D_{b,f,-1}}$.[107] Besides, let $B_f = \sum_{b=1}^{n_f} \omega_{bf} B_b$ where ω_{bf} are the bank shares $\omega_{bf} = \frac{D_{bf,-1}}{D_{f,-1}}$. The basic credit channel equation can be written as:

$$\Delta D_{fb} = \beta B_b + d_f + \varepsilon_{fb} \tag{19}$$

The key issue is that firm- and bank-shocks may be correlated. Let $\rho_{bd} = cov(B_b, d_f)$.[108] Besides, let $Var(B_b) = \sigma_b^2$.

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

The baseline model (19) assumes that firms facing heterogeneous credit supply shocks from their banks do not substitute across lenders. In this case, as shown by KM, including firm fixed effects allows 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:

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

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

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

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

However, in the cross-sectional model (20), the firm-specific demand shock d_f cannot be absorbed so that the correlation between d_f and B_b again leads to a biased estimator and the comparison with the within-firm coefficient is not informative. Under assumption (A1), the expression for $\bar{\beta}_{OLS}$ is:

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

107. My results are unaffected if $\Delta D_{b,f}$ is defined as the mid-point growth rate.

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

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 (19) to correct for this bias. Including $\hat{d_f}$ in the estimation of (20), we get:

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

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

E.2 Introducing substitution in the KM framework

If there are spillovers across banks, equation (19) is misspecified and the true model is:

$$\Delta D_{fb} = \beta B_b + \gamma B_{-b} + d_f + \varepsilon_{fb} \tag{21}$$

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

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

One cannot run a within-firm estimation of equation (21) because B_{-b} and B_b are collinear conditional on the firm fixed effects. If we estimate equation (21) omitting the term B_{-b}, we obtain:

$$\beta_{FE} = \beta - \frac{1}{n-1}\gamma \tag{22}$$

In the case where β and γ have opposite signs, the estimated coefficient in the standard KM regression (22) overestimates the true effect (the next section generalizes this result). 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 (21) at the firm level, we obtain:

$$\Delta D_f = (\beta + \gamma) B_f + d_f + \varepsilon_f \tag{23}$$

Estimating this equation omitting d_f, we get:

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

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

$$\bar{\beta}_{OLS,\ \hat{d}} = \beta - \frac{1}{n-1}\gamma \tag{24}$$

The intuition is that since β_{FE} in (22) is biased, the estimated $\hat{d_f}$ are biased as well so that including them in the between-firm estimation leads to a biased coefficient as well. Moreover, equation (24) shows that comparing the FE and the between-firm coefficients tells us nothing: even with substitution effects, the between-firm coefficient is equal to the FE one. The reason why we may empirically find $\hat{\hat{\beta}}_{OLS,\ \hat{d}} \neq \hat{\beta}_{FE}$ 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:

$$\Delta D_{fb} = \beta B_b + \gamma B_{-b} + d_f + \varepsilon_{fb} \tag{25}$$

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_{-b}:

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

where ϕ is a parameter. Taking a generic functional form allows to make assumptions on the extent to which each banks' shock affects the firm, depending on the bank shares ω_{bf}. It nests all the intuitive forms for B_{-b}: the equal-weighted mean of other banks' shocks, their bank-share weighted mean, the shock of the bank with the highest bank share.[109] The fact that the KM estimator is biased in the presence of substitution effects is very general, as shown in the following proposition.

Proposition 4 *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. To clarify the intuitions, I first review the method using variation in n_f across firms (ω_{bf} constant within firm) and then the method using variation variation in ω_{bf} within firms (n_f constant across 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, which is equivalent to the case where B_{-b} is defined using $\phi = 0$.

Proposition 5 *Assume that n_f varies across firms. Then equation* (25) *is identified and* $(\beta_{FE}, \gamma_{FE}) = (\beta, \gamma)$.

The intuition for why the system is invertible when n_f varies while it was not with constant n relies on the interpretation of β_{FE}. As explained above, 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, allowing us to disentangle the effects of γ and β.

This method has the advantage of relying on no additional assumption. However, it requires sufficient variation in n_f across firms.

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

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_{-b} go separately identify β and γ.

Proposition 6 *If $n > 2$, ω_{bf} not constant within firms and $\phi \neq 0$, equation* (25) *is identified and $(\beta_{FE}, \gamma_{FE}) = (\beta, \gamma)$.*

When these conditions are satisfied, B_b and B_{-b} are not collinear conditional on the firm fixed effects, so that we can estimate equation (25). Intuitively, we disentangle the direct effect of B_b from the substitution term by assuming that the substitution effect from bank b' towards 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—$B_{-b} \perp \varepsilon_{fb}$ 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 (2020).[110]

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 rely on 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 allows to partial out the correlation between B_f and d_f to obtain an unbiased estimate of $\bar{\beta}$.[111]

Implementation. I test the implementations of 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 (or to B_{-b} defined with $\phi = 0$), in the case where n_f is random (column (1)) 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. In columns (3) and (4), B_{-b} is defined with $\phi = 1$ while in columns (5) and (6), B_{-b} is defined with $\phi = +\infty$. In columns (3) and (5), ω_{bf} is random while in columns (4) and (6) ω_{bf} is correlated to ε_{bf}.

The upper panel shows that the naive estimates can be very far off the true parameters. The within-firm coefficient (line 1) overestimates the true β, as predicted since β and γ have opposite signs. The second line shows the regression coefficient of $\hat{d}_f$ on the true d_f and shows that the $\hat{d}_f$ are biased. Line 3 shows the naive between firm coefficient, which suffers from the bias 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, which correspond to the true parameters. The regression coefficient of $\hat{d}_f$ on the true d_f is equal to 1. Finally, including the unbiased estimates of $\hat{d}_f$ in the between firm regressions allows

110. Namely, the full-data orthogonality condition can be rewritten as $\mathbb{E}\left[\sum_b B_b \left(\sum_f \sum_{b' \neq b} \frac{\omega_{bf}}{1-\omega_{b'f}} \varepsilon_{fb'}\right)\right]$ which holds when B_b is as-good-as random. I show in practice in simulations below that $\omega_{bf} \not\perp \varepsilon_{fb}$ does not lead to a biased coefficient.

111. 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.

to get an unbiased estimate of $\bar{\beta}$.[112] 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.

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}
Naive estimators						
$\hat{\beta}_{FE}$	-0.695	-0.695	-0.650	-0.650	-0.650	-0.650
	(0.003)	(0.004)	(0.003)	(0.003)	(0.004)	(0.003)
$\hat{\kappa}_{d,\ \hat{d}}$	1.138	1.138	1.126	1.126	1.126	1.126
	(0.003)	(0.002)	(0.003)	(0.003)	(0.003)	(0.003)
$\hat{\bar{\beta}}_{OLS}$	0.390	0.750	0.345	0.344	0.371	0.370
	(0.005)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)
$\hat{\bar{\beta}}_{OLS,\ \hat{d}}$	-0.695	-0.695	-0.653	-0.653	-0.658	-0.658
	(0.003)	(0.004)	(0.004)	(0.004)	(0.005)	(0.004)
Correct method						
$\hat{\beta}_{FE}$	-0.499	-0.499	-0.500	-0.500	-0.501	-0.499
	(0.007)	(0.007)	(0.006)	(0.006)	(0.004)	(0.004)
$\hat{\gamma}_{FE}$	0.301	0.301	0.300	0.300	0.300	0.300
	(0.010)	(0.010)	(0.011)	(0.011)	(0.004)	(0.005)
$\hat{\kappa}_{d,\ \hat{d}}$	1.000	1.000	1.000	1.000	1.001	1.000
	(0.005)	(0.005)	(0.005)	(0.005)	(0.003)	(0.003)
$\hat{\bar{\beta}}_{OLS,\ \hat{d}}$	-0.199	-0.198	-0.251	-0.251	-0.222	-0.220
	(0.017)	(0.017)	(0.015)	(0.015)	(0.008)	(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 ΔD_{bf} as in (21) with $\beta = -0.5$ and $\gamma = 0.3$.

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_{-b}, leading to potential errors due to misspecification. Using simulations, I nevertheless show that estimating (25) with a misspecified substitution term reduces the bias compared to omitting the substitution term. Table E.2 shows the estimated coefficients when introducing a misspecified substitution term i.e. if the model is estimated using B_{-b} defined in one way while the true data-generating process depends on B_{-b} defined in another way. In lines 1-2, I estimated the parameters using data generated using B_{-b} defined with $\phi = 0$. I then estimate β and γ by assuming that $\phi = 0$, $\phi = 1$ or $\phi = +\infty$ and I average the three coefficients, as an empirical researcher would do when trying different specifications. I repeat the process for a true data generating process with $\phi = 1$ or $\phi = +\infty$. I find that the estimation of the parameters is sensitive to misspecification in B_{-b} but that averaging coefficients across definitions of B_{-b} gives

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

much more reasonable estimates than when omitting B_{-b}.

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	-0.500	-0.501
		(0.004)	(0.004)	(0.004)
	Misspecified $\hat{\gamma}$	0.300	0.300	0.299
		(0.006)	(0.006)	(0.006)
$\phi = 1$	Misspecified $\hat{\beta}$	-0.503	-0.503	-0.503
		(0.004)	(0.003)	(0.004)
	Misspecified $\hat{\gamma}$	0.295	0.296	0.295
		(0.005)	(0.005)	(0.006)
$\phi = 2$	Misspecified $\hat{\beta}$	-0.578	-0.577	-0.578
		(0.003)	(0.002)	(0.003)
	Misspecified $\hat{\gamma}$	0.180	0.181	0.181
		(0.003)	(0.003)	(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_{-b} 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 ΔD_{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 ΔD_{bf}. Lines (2) and (3) follow the same logic.

Proof of Proposition 4. The KM estimator is equal to:

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

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:

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

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

Proof of Proposition 5. In this case,

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

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

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

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

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

I then use the equivalence between the least-square dummy variable and the within-firm estimation. The within-firm version of (21) writes:

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

Using the definition of B_{-b}, one obtains that $B_{-b} - \overline{B_{-b}} = -\frac{B_b - \overline{B_b}}{n_f - 1}$. 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 $\mathbf{Y} = \Delta D_{fb}$. The determinant of $\mathbb{E}[\mathbf{X}'\mathbf{X}]$ is equal to

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

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

Simple matrix algebra then yields:

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

■

Proof of Proposition 6. I detail the proof of identification in the case $B_{-b} = \frac{1}{1-\omega_{bf}} \sum_{j \neq b} \omega_{jf} B_j$, that is $\phi = 1$. Then the within firm estimation of (21) corresponds to

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

$B_{-b} - \overline{B_{-b}}$ 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 $B_{-b} - \overline{B_{-b}}$ is not collinear to $B_b - \overline{B_b}$ so that β and γ can be separately identified. By the regression anatomy formula, we obtain $\beta_{FE} = \beta$. ■

F Misallocation

This Appendix details the methodology 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 produces in each sectors, 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 $\text{TFP}_s^* = (\sum A_{fs}^{\sigma-1})^{\frac{1}{\sigma-1}}$. 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 constant does 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. I use the following variables. 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 $\text{MRPK}_{fs} = \alpha_s \frac{P_{fs}Y_{fs}}{K_{fs}}$ and $\text{MRPL}_{fs} = (1-\alpha_s)\frac{P_{fs}Y_{fs}}{L_{fs}}$. Omitting the multiplicative factor $\frac{\sigma-1}{\sigma}$ is innocuous. From MRPK and MRPL, I compute TFPR as $\text{TFPR}_{fs} = \text{MRPK}_{fs}^{\alpha_s}\,\text{MRPL}_{fs}^{1-\alpha_s}$, again omitting the sector-level constant $\frac{\sigma}{\sigma-1}\alpha_s^{-\alpha_s}(1-\alpha_s)^{\alpha_s-1}$.

I use the approximations $\tau_{fs}^K = \log(\text{MRPK}_{fs})$, $\tau_{fs}^L = \log(\text{MRPL}_{fs})$ and $\tau_{fs} = \log(\text{TFPR}_{fs})$, assuming that wedges are small. These approximations omit the sector-level constant depending on r and w, which is again innocuous.[113]

Estimation of the production function. I estimate production functions at the 2-digit level using the cost shares method, as in Osotimehin (2019) and Blattner, Farinha, and Rebelo (2019). Namely, I define the labor share as the ratio of sectoral labor compensation over value added.[114]

Estimation of the TFP loss. To quantify the change in Var(log(TFPR)) driven by the shock, we estimate the reduced form effect of the shock on log(TFPR) or equivalently τ_{fs}. I run 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 (10).

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. The goal is to directly estimate the effect of the shock on the moments of interest. This is done by comparing the evolution 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(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 then run the following regressions:

$$\Delta M_{qst} = \beta FirmExposure_{qst} + \Phi\cdot\mathbf{X}_{qst} + \varepsilon_{qst} \tag{26}$$

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 those controls. An issue is that the

113. In the regressions for the change in wedges, the changes in r or w are absorbed in the intercept so that the coefficient on $\Delta\log(\text{MRPX}_{fs})$ and $\Delta\tau_{fs}^X$ are equal. In any case the constant drops when we take the variance so that it is equivalent to compute $\text{Var}(\log(\text{TFPR}_{fs}))$, $\text{Var}(\alpha_s\log(\text{MRPK}_{fs}) + (1-\alpha_s)\log(\text{MRPL}_{fs}))$ or $\text{Var}(\alpha_s\tau_{fs}^K + (1-\alpha_s)\tau_{fs}^L)$.

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

fixed effects of the baseline regression cannot be absorbed here. To circumvent this issue, I run the firm-level specification with $\Delta\tau^K_{ft}$ as outcome, store the estimated fixed effects, take their average by date×industry×quantile and use these as controls. By construction, running this regression with $\Delta\mu(qst)$ as the outcome yields the same coefficient as when estimating equation the firm-level regression with $\Delta\tau^K_{ft}$ 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 in aggregate TFP is given by:

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

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.

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:

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

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}, which is similar in spirit to the previous decomposition.[115]

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. These estimates are reported in the Table below. To compute ε_{fx}, I estimate gross output production function using the Cobb-Douglas constant returns to scales assumption with sectoral cost shares. As for s_{fx}, 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. With all these in hand, I can compute the second term in the equation above.

115. As highlighted by Osotimehin (2019) and Baqaee and Farhi (2020), these different decompositions conceptually differ. The Hsieh and Klenow (2009) framework allows to 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. When the only force driving the change in input allocation is the shock to wedges—which is what we isolate when looking at the effect of $FirmExposure$ on inputs—the two coincide (Baqaee and Farhi (2020)).

Table F.1: Reduced form estimates to compute aggregate TFP loss using Petrin and Levinsohn (2012)

Panel A: Full sample

	gr(Intermediates) (1)	gr(Capital) (2)	gr(Labor) (3)
FirmExposure	-0.004	-0.236***	-0.048
	(0.044)	(0.087)	(0.032)
Controls	✓	✓	✓
Municipality×Time FE	✓	✓	✓
Industry×Time FE	✓	✓	✓
Main bank×Time FE	✓	✓	✓
Observations	1,117,737	1,087,588	1,075,895
R-squared	0.073	0.12	0.071

Panel B: Sample splitted by ex-ante wedge

	gr(Intermediates)		gr(Capital)		gr(Labor)	
	Low (1)	High (2)	Low (3)	High (4)	Low (5)	High (6)
FirmExposure	0.054	-0.087	-0.140	-0.358**	0.021	-0.122***
	(0.066)	(0.060)	(0.094)	(0.150)	(0.043)	(0.047)
Controls	✓	✓	✓	✓	✓	✓
Municipality×Time FE	✓	✓	✓	✓	✓	✓
Industry×Time FE	✓	✓	✓	✓	✓	✓
Main bank×Time FE	✓	✓	✓	✓	✓	✓
Observations	551,948	559,071	570,190	513,166	540,147	532,231
R-squared	0.10	0.092	0.17	0.14	0.098	0.084

Note: This table presents the results used to quantify the effect of crowding out on allocative efficiency using the methodology in Petrin and Levinsohn (2012). It reports the results of estimating specification (6). The outcome variables are the firm-level growth rates of intermediates, fixed assets and total wage bill. The main independent variable is firm exposure to crowding out, defined in (7) 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 wedge of the input of interest is above the sample median. 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×bank level. ***, ** and * indicate significance at the 1%, 5% and 10% levels, respectively.

G Data

Credit register. The main data source used in this work is the French credit register administered by Banque de France. The credit register collects data on borrowers with total exposure (debt and guarantees) above 25,000 euros towards 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 implement a number of filters based on firms' legal status (*Code Categorie Juridique*). I exclude sole proprietorships (legal status 1xxx), real estate investment trusts (legal status 6540, 6541 and size code 7), entities with legal forms implying public-private partnerships (legal status 5415, 5515, 5615, 5546, 5547, 5646, 5647) as well an non-standard legal forms (e.g. non-profits, foundations, unions, etc. corresponding to legal status 8xxx and 9xxx). I exclude borrowing by financial institutions (broad industry K) to exclude inter-bank lending.

The French banking sector experienced a significant consolidation over the sample period, which is reflected by the number of banks decreasing from xx in 2006 to xx 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 drawn credit with initial maturity above 1 year (variable *Tot MLT* in the credit register). 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 register 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 municipality-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.

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*).

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 I obtained 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 then interpolate the *BAFI* time series in case of missing values and I predict missing values of deposit ratio and interbank exposure using banks' total assets, equity ratio and time fixed effects.

All these datasets are accessible through the Banque de France virtual Open Data Room.[116].

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