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

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I study the crowding out effect of local government bank debt on corporate credit, investment, and output, using French administrative data over 2006-2018. Exploiting plausibly exogenous variation in bank-specific demand for local government debt, I show that when local governments borrow an additional €1 from a bank, this bank cuts corporate credit by €0.5, with significant effects on firm-level investment. Combining these reduced-form effects and a model, I show that crowding out reduces the output multiplier of debt-financed local government spending by 0.25. My results show that constraints on financing supply reduce the stimulus effect of debt-financed government spending.

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1. Introduction

How does debt-financed government spending affect economic activity? The answer to this question is often summarized by a “multiplier”—the output response to a unit increase in debt-financed government spending. One can decompose this debt-financed multiplier into two parts. The first term is the multiplier obtained when the additional debt is financed by an outside investor with a perfectly elastic supply of funds. It can be above or below 1, depending on whether government spending can stimulate demand. Chodorow-Reich (2019) shows the size of this term can be informed by the multipliers estimated using local variation in government spending financed by outside transfers or windfalls (e.g., Nakamura and Steinsson (2014) and others in the literature review). In practice, the supply of loanable funds is unlikely to be perfectly elastic. In this case, government debt affects the economy through an additional channel: the increase in the government’s demand for debt reduces the supply of debt to firms, hindering corporate investment and output. This second term is the financial crowding out effect. It is negative and hence reduces the debt-financed multiplier.

While the extent and determinants of crowding out are essential inputs for the level and financing of public spending, empirical evidence on crowding out is scant. This is due to severe identification challenges: government debt is rarely exogenous, and even exogenous shocks to government debt would affect firms via other channels than crowding out. The goal of this paper is to fill this gap.

I investigate this question in the context of local government bank debt: I study the crowding out effect of local government bank debt on corporate credit and quantify the implied reduction in local government spending multipliers. I focus on France over 2006-2018, exploiting rich credit registry data covering bank loans to private firms and local governments. This empirical setting is interesting for two reasons. First, local government bank debt is large and growing. In large developed and developing countries, local government debt-to-GDP increased from 11% to 22% over 1990-2019. 80% of this local government debt consists of bank loans.[1] Second, studying local government bank debt allows me to use cross-sectional variation to isolate the financial crowding out channel from other endogenous relationships between local government debt and corporate outcomes, thereby solving the long-standing empirical challenge in this literature.

I quantify crowding out in two steps. First, I consider causal relative crowding out effects in the cross-section of banks: I ask whether an increase in demand for local government

[1] See Figure A1. 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).

debt directed to a given bank causes a disproportionate reduction in that bank's corporate credit supply, and in investment for its borrowers. Second, combining the estimated relative effects and a model, I quantify the drop in aggregate output due to crowding out. That is, I quantify the aggregate output shortfall when one additional euro of local government debt is financed by banks, compared to a counterfactual where this one additional euro is financed by an outside investor with a perfectly elastic supply of funds. The counterfactual keeps constant local government spending, taxes, and debt, and thus all other effects of government spending, to only quantify the reduction in multipliers attributable to financial crowding out.

I exploit bank lending to French local governments as an empirical setting.[2] From the credit registry, I observe all outstanding loans by 543 banks to private firms (1.5 million unique firms) and local governments (aggregated into 2,081 unique municipalities). I complement the credit registry with corporate tax filings and bank balance sheet data.

I first identify a relative crowding out effect in the cross-section of banks, that is, I ask whether a larger increase in demand for local government loans directed to a given bank causes a larger reduction in that bank's corporate credit. My research design focuses on multibank firms and examines whether a given firm experiences lower credit growth from banks exposed to higher demand for local government loans.[3] To proxy for bank-specific demand for local government loans, I exploit the fact that banks' pre-determined geographic implantation across municipalities generates heterogeneous exposure to local government debt demand growth. Identification relies on the fact that other endogenous relationships between local government debt and corporate credit (e.g., demand stimulus) affect *firm*-level demand for credit. The within-firm estimator (Khwaja and Mian, 2008) thus partials out these channels. By contrast, crowding out uniquely operates as a shock to the *bank*-specific supply of corporate credit, which depends on the bank-level demand for local government loans.

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

I find that when local governments borrow an additional €1 from a given bank, that bank lends €0.54 less to private firms during the same year. The effect is statistically significant

[2] French local governments consist of four layers of elected sub-national governments, the local public entities they control (public schools, public housing, etc.), and state-owned local public service operators.

[3] 30% of firms borrow from multiple banks, they account for 70% of total corporate credit.

and economically large.[4] Local projections show that this reduction is permanent. The crowding out effect is similar when excluding state-owned banks and does not vary with proxies for political pressure on banks. Hence, crowding out is orthogonal to political interference.[5]

Why does crowding out occur? Using various proxies for banks' funding, capital, and liquidity constraints, I find that crowding out is more severe for banks that are more constrained in their ability to expand their credit supply. These results show that, in line with the theoretical prediction, crowding out reflects the elasticity of the supply of loanable funds of governments' lenders. In addition, I find that the adjustment of corporate credit implied by the constrained credit supply occurs through both a reduction in quantities and an increase in interest rates, albeit to a lesser extent.

I then study whether the reduction in corporate credit by a bank has real effects on investment for its corporate borrowers. I compare firms borrowing from banks exposed to local government debt shocks to firms borrowing from other banks. More precisely, I define firm-level exposure to crowding out as the credit-share weighted average of its banks' shocks. I compare only firms located in the same municipality$\times$industry$\times$time cell. These firms are therefore subject to a similar local-level change in local government debt, but differ in their exposure to crowding out because they borrow from different sets of banks. I also directly control for an estimate of firm-level demand shocks obtained from the within-firm specification.[6] The identifying assumption is that, conditional on controls, there are no shocks to real outcomes correlated with bank affiliation. I perform several checks and find support for this assumption.

I find that the reduction in corporate credit supply has real effects. An additional €1 in local government loans at one bank leads to a €0.32 reduction in investment for firms borrowing from that bank in the same year. Local projections show that this reduction is permanent. These effects are heterogeneous across firms, with more financially constrained firms exhibiting higher credit-to-investment 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.[7]

The *relative* effects documented so far do not add up to the *aggregate* effect because

[4]The magnitude is in line with evidence on banks' constraints as in Paravisini (2008) or Chakraborty, Goldstein, and MacKinlay (2018).

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

[6]See Cingano, Manaresi, and Sette (2016) and Jiménez et al. (2019).

[7]One concrete example of such counterfactual is if local government debt is entirely financed by foreign investors. See Diamond (1965), or Broner et al. (2021) for a recent treatment.

they ignore any effect on non-exposed banks and firms. To obtain the aggregate effect, I develop a model of crowding out in a segmented banking system. Banks lend to firms and local governments, are funded via deposits, and can access the interbank market at a cost. Firms, local governments and depositors are assigned to a given bank. Together with the cost of accessing the interbank market, this implies that banks are (partially) segmented. I study the equilibrium response of corporate credit to bank-specific local government debt demand shocks. 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 difference between the relative and the aggregate effects can be decomposed into two terms. The first term is a spillover effect due to capital mobility across banks. Unless banks are fully segmented, banks exposed to the local government debt shock draw in capital from non-exposed banks, which therefore also reduce their corporate credit supply. This effect can be quantified by estimating the effect of credit demand shocks on interbank flows. The second term captures a general equilibrium feedback due to substitution across products and the labor supply response. I calibrate this term and find that for a plausible range of parameter values it either strongly magnifies or only modestly attenuates the effect, so that it is conservative to ignore it in my baseline quantification. From this analysis, I obtain that the drop in aggregate corporate credit caused by crowding out leads to an output loss of €0.20 per euro of local government loans.

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 across firms. Using the framework of Hsieh and Klenow (2009), I find that crowding out reduces output by €0.05 per euro of local government debt via this channel. This is entirely driven by the fact that firms with higher marginal products of capital (that are likely to be financially constrained) have a higher credit-to-investment sensitivity.

Aggregating these effects, an additional €1 of local government debt reduces output by €0.25 (0.20+0.05) through crowding out. This implies that the output multiplier of debt-financed local government spending would be higher by 0.25 absent crowding out.

This paper makes two 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. This is an important finding given the surge in debt-financed local government spending. It is worth noting that this is also the first such quantification for any form of government debt, identification having proven elusive in the case of central government debt. Second, by showing that crowding out is more severe when the supply of funds is less elastic, I test and confirm the standard crowding out theory. This also applies to other forms of government debt.

There are two main policy implications from my results. First, crowding out is large, notably compared to estimates of spending multipliers.[8] This may be especially problematic during crises, when government debt tends to soar while financial intermediaries are particularly constrained. Second, in segmented financial markets, who governments borrow from has real effects on the transmission of debt-financed fiscal policy. In this respect, my results highlight an important downside of transferring debt-taking to lower levels of government, since central government debt financed by bonds issued on international capital markets is likely to generate a lower crowding out effect on the domestic economy.

Related literature. This work contributes to three strands of the literature. First, this work feeds into the literature on fiscal multipliers. Much of the recent literature on this topic has used cross-sectional variation across geographies to estimate multipliers of government spending financed by outside transfers or windfalls (Chodorow-Reich et al. (2012), Conley and Dupor (2013), Serrato and Wingender (2016), Corbi, Papaioannou, and Surico (2019)). Chodorow-Reich (2019) shows that these multipliers provide a "rough lower bound" to debt-financed multipliers in a model without capital markets, where by assumption financial crowding out does not occur. My results imply that the policy-relevant debt-financed multipliers will be lower than the transfer-financed multipliers estimated in these papers. They also complement the few estimates of debt-financed multipliers, using aggregate data (e.g., Mountford and Uhlig (2009)) and cross-sectional data (Clemens and Miran (2012), Adelino, Cunha, and Ferreira (2017), Dagostino (2018)).[9]

Second, I contribute more specifically to the literature on government debt crowding out corporate financing and investment (see Hubbard (2012) for a review). Virtually all studies focus on government bonds and rely on time-series variation in the US. No consensus has emerged, partly reflecting the challenge in establishing causality.[10] Closer to my focus, recent papers study the effect of loans to local governments on corporate credit and

[8]Estimates of transfer-financed multipliers range from 0.8 to 4, with an average around 1.5 (Chodorow-Reich (2019), Ramey (2019)). Crowding out implies that these numbers would be lower in the case of spending financed by debt.

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

[10]Several papers test the refinement of the crowding out hypothesis by Friedman (1978) according to which government debt affects the relative prices of other securities depending on their substitutability with government debt. They show that government debt affects corporate leverage (Graham, Leary, and Roberts, 2014; Demirci, Huang, and Sialm, 2018), 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.

investment: Huang, Pagano, and Panizza (2020) in China, Morais et al. (2021) in Mexico, and Koetter and Popov (2021) and Hoffmann, Stewen, and Stiefel (2021) in Germany. However, they focus on developing countries and/or state-owned banks and political interference, and only consider micro-level effects.[11] My work also relates to papers showing that banks' holdings of sovereign bonds—due to political pressure in Becker and Ivashina (2017) or to a home bias in holdings of Colombian sovereign debt in Williams (2018)—crowd out corporate credit and investment.

Third, this paper contributes to the empirical literature on banks' funding constraints, credit supply shocks, and their real effects (e.g., Khwaja and Mian (2008), Paravisini (2008), Jiménez et al. (2012), Chodorow-Reich (2014), Drechsler, Savov, and Schnabl (2017), Amiti and Weinstein (2018), Huber (2018)). In this respect, my paper is closest to Chakraborty, Goldstein, and MacKinlay (2018), Martín, Moral-Benito, and Schmitz (2021) and Greenwald, Krainer, and Paul (2021) who show how one segment of banks' loan portfolio may crowd out another one.[12] In addition, I document the consequences of banks' funding constraints for the transmission of bank-financed government spending to the real economy.[13]

2. Financial crowding out: conceptual framework

The textbook financial crowding out mechanism works as follows: an increase in local government loan demand raises the total demand for loans, which puts upwards pressure on interest rates, and leads to a reduction in corporate credit. From the point of view of firms, crowding out is akin to a shift in banks' residual credit supply curve. This mechanism is depicted on the simple supply and demand graph in Figure A3. The mechanism is very general: it occurs as long as bank credit supply is not perfectly interest-elastic. In particular, it does not depend on banks having a preference for local government loans. 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.

[11] Looking at crowding out outside of state-owned banks is critical. State-owned banks account for a small share of credit. In addition, crowding out due to political pressure may have different implications for banks' health, if they are pressured to hold risky debt (Acharya, Drechsler, and Schnabl (2014), Ongena, Popov, and Van Horen (2019)) and make losses on lending to governments ((Hoffmann, Stewen, and Stiefel, 2021)).

[12] 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.

[13] A distinct literature has shown that banks' exposure to government debt lead to a contraction in corporate lending during the European sovereign debt crisis (Acharya et al. (2018), Bottero, Lenzu, and Mezzanotti (2020)). However, their mechanism is the impairment of the value of existing sovereign holdings, not crowding out.

In this paper, I define the financial crowding out effect as a shortfall in corporate credit, capital, and output when local governments borrow one additional euro from the banking sector compared to a counterfactual where local government expenditures, tax, and debt are constant but this one additional euro is borrowed from an outside investor with a perfectly elastic supply of funds. That is, I do not estimate debt-financed multipliers, but rather the reduction in such multipliers that can be attributed to the reduced availability of corporate financing caused by government debt. In doing so, I keep constant the other effects of debt-government spending on the economy, for instance any stimulus effect on demand or any *real* crowding out effect (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).

To quantify crowding out, I first document a causal relative crowding out effect across banks, and subsequently firms. I exploit the fact that when banks are segmented, crowding out has a bank-specific dimension: a larger increase in local government debt demand directed at one bank leads to a larger drop in that bank's corporate credit supply, and in investment for firms borrowing from that bank. This occurs because frictions prevent capital from flowing across banks and firms from switching banks. Importantly, the hypothesis that banks are segmented is testable: if false, there should be no relative crowding out effect. While this relative effect is conceptually different from the aggregate effect, it is useful for two reasons. First, documenting a relative crowding out effect allows to reject the null hypothesis that crowding out does not occur. Second, the well-identified relative crowding out effect can serve as an input to quantify the aggregate crowding out effect.

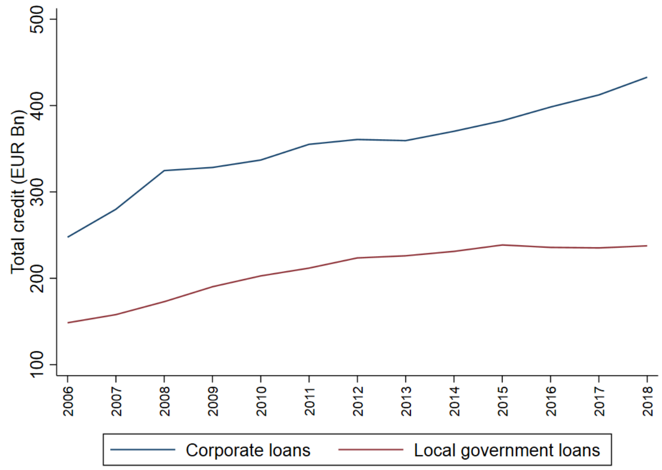
3. Data and institutional setting

3.1. Data

My main data source is the French credit registry administered by Banque de France. It records outstanding credit volumes at the bank-borrower level for all borrower-bank pairs with total exposure (debt and guarantees) above 25,000 euros. I define year t outstanding credit as the average outstanding credit over the last three months of the year. I focus on credit with initial maturity above one year to avoid measurement issues related to credit lines. Banks correspond to legal entities, not bank holding companies.[14] There are

[14] I use this level to avoid bundling the different affiliates of cooperative banking groups. These groups are networks of legally-independent banks that operate on designated geographical areas. While member 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.

FIGURE 1. Aggregate bank credit to corporations and local governments in France



Note: This figure plots the aggregate time series obtained from the Banque de France credit registry. See details on data filtering in Section 3 and Appendix F.

543 unique banks. On the corporate credit side, I focus on non-financial corporations and exclude sole proprietorships. I obtain 1,454,234 unique firms and 2,796,032 unique bank-firm relationships. As for local governments, I have 61,881 unique local governments and 196,750 unique local government-bank relationships. I complement this data with balance sheet and income statement information from the corporate tax-filings collected by Banque de France, which are the tax-filings for firms with revenues above €750,000. Finally, I obtain banks' balance sheets from regulatory filings. More details on the data can be found in Appendix F.

Figure 1 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. Throughout the text, the mid-point growth rate of x refers to $\frac{x_t - x_{t-1}}{0.5(x_t + x_{t-1})}$.

Geographic units. The credit registry provides the location of borrowers. I sort borrowers across 2,081 “municipalities". I call municipalities the geographic units defined by intermunicipal cooperations (EPCI). Throughout the text, municipalities correspond to geographical units, not to layers of subnational governments. Municipalities are a good approximation of local lending markets: the average bank branch located in a municipality has 72% of its corporate lending and 86% of its local government lending going to borrowers located in the same municipality.

TABLE 1. Summary statistics

Panel A: Firm×bank-level variables

	All					Multibank				
	mean	sd	p10	p50	p90	mean	sd	p10	p50	p90
Credit growth ΔC_{fbt} (MPGR)	-0.019	1.18	-2	-0.16	2	-0.035	1.17	-2	-0.17	2
Credit growth ΔC_{fbt} (std)	-0.14	0.75	-1	-0.19	0.44	-0.12	0.80	-1	-0.21	0.57
Outstanding loans C_{fbt} (€K)	109.6	143.7	0	53.7	300.3	130.2	162.8	0	62.7	397.3
Bank exposure $BankExposure_{bt}$ (%)	0.66	1.45	-0.23	0.089	2.59	0.52	1.30	-0.15	0.030	2.14
Local gvt loans C^{gov}_{bt} (€K)	1009691.4	1436820.2	3763.7	573756	3224721.3	909388.7	1458676.9	333.7	244738	2961459.3
Total loans C^{tot}_{bt} (€K)	6858080.9	9896208.6	716905	2745473.3	28901204	6905833.0	10139973.0	358033	2642636.3	29818668
Observations	8773498					2731110				

Panel B: Firm-level variables

	mean	sd	p10	p50	p90
Credit growth ΔC_{ft} (MPGR)	0.074	0.82	-0.66	-0.15	1.63
Credit growth ΔC_{ft} (std)	0.094	0.89	-0.51	-0.17	0.99
Outstanding credit C_{ft} (K€)	299.5	460.3	17.3	113.3	824
Firm Exposure $FirmExposure_{ft}$ (%)	0.55	1.23	-0.16	0.090	2.08
Capital growth	0.034	0.31	-0.21	-0.026	0.36
Employment growth	0.016	0.16	-0.14	0	0.19
Fixed assets (K€)	673.8	955.1	58	302	1719
Value added (K€)	1107.1	1263.5	256	657	2453
Nb. employees	20.8	22.5	5	13	45
Wage bill (K€)	590.8	659.4	133	362	1285
Assets (K€)	2987.6	3838.4	595.0	1545	7090.0
ROA	0.052	0.085	-0.019	0.047	0.14
Debt/Assets	0.26	0.22	0.038	0.20	0.56
Tangibles/Assets	0.32	0.21	0.067	0.28	0.63
EBIT/Interests	19.3	38.0	-2.60	7	60
Cash/Assets	0.095	0.11	0.0025	0.059	0.24
CFO/Assets	0.092	0.14	-0.055	0.085	0.25
Capex/Sales	0.019	0.047	-0.0068	0.0061	0.067
Observations	728733				

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) or the standard growth rate (std). 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.

3.2. Institutional details

Local government debt. French local governments obtain more than 90% of their external financing through bank loans. Therefore, bank loans to local governments are large: they amount to 14% of GDP in 2018. As can be seen from the aggregate time series on Figure 1, loans to government entities have grown at an average rate of 4% per annum on my sample period, but this average masks a dynamic growth until 2013, followed by a more subdued growth, with negative growth rates in 2016-2017.

Loans to local governments are also large from the point of view of banks. They account for 37% of total credit to local governments and corporations combined.

Throughout this paper, local government loans refers to loans to any local government entity. Looking at the split by entity types, the largest share goes to the four layers of elected local governments (communes, intermunicipal cooperations, departements, and regions, accounting for 64% of the total), followed by state-owned public service operators (20%), public hospitals (11%), and public housing (2%).[15] These local governments are scattered on the French territory and take their lending decisions in a decentralized manner.

Rules on subnational entities borrowing imply that local government debt finances investment expenditures, as opposed to operating expenditures. This is reflected in the relatively long maturity of local government loans (15 years on average). French local governments are not suject to bankruptcy proceedings. In the event of financial distress, control is transferred to the central government. This implies that local government debt benefits from an implicit guarantee of the central government, limiting the credit risk of these loans. That said, this central government "receivership" can imply long repayment delays and administrative costs for banks, so that screening and monitoring remains important in this market. This risk profile is reflected in a risk weight of 20% for regulatory capital purposes (equal to that of AAA-rated firms, higher than 0% for the French central government). Finally, loans to local governments are illiquid: they are rarely securitized and cannot easily be used as collateral.[16,17]

French banks. Figure A2 provides statistics and highlights some features of the French banking landscape. First, the size distribution of French banks is highly skewed, with a large number of mid-sized banks and a few very large banks. Second, the market is split between national and local banks: 44% of corporate credit is accounted for by "local" banks (defined as banks operating in less than 20% of municipalities), the rest being national banks. Third, most banks lends to both firms and local government, but there is a lot of heterogeneity across banks in the share of their lending going to local governments.

[15]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.

[16]Securitization is underdeveloped in France and is mostly confined to mortgages.

[17]The ECB refinancing operations authorize the use of credit claims as collateral, but the haircuts on local government loans are as high as 60%.

4. Relative crowding out: corporate credit

4.1. Empirical strategy

To investigate the relationship between bank-level demand-driven increases in local government loans and corporate credit supply, I estimate the following baseline specification:

$$\Delta C_{fbt} = d_{ft} + \beta BankExposure_{bt} + \Phi \cdot \mathbf{X}_{fbt} + \varepsilon_{fbt} \qquad (1)$$

where f indexes firms, b indexes banks, and t indexes time in years. ΔC_{fbt} is bank$\times$firm-level credit growth, defined ΔC_{fbt} as the mid-point growth rate to account for both the intensive and extensive margins (Davis and Haltiwanger, 1992). *BankExposure* is bank-level exposure to local government debt demand (defined below). d_{ft} is a firm$\times$time fixed effect. $\mathbf{X}_{fbt}$ is a vector of controls. The objective is to identify β, the relative crowding out parameter.[18]

$BankExposure_{bt}$ proxies for the local government loan demand directed to bank b. It is based on the observation that some municipalities demand more credit than others, and that bank market shares vary substantially across municipalities. It is constructed as follows. I first estimate an equation that decomposes equilibrium local government credit growth into municipality and bank components:

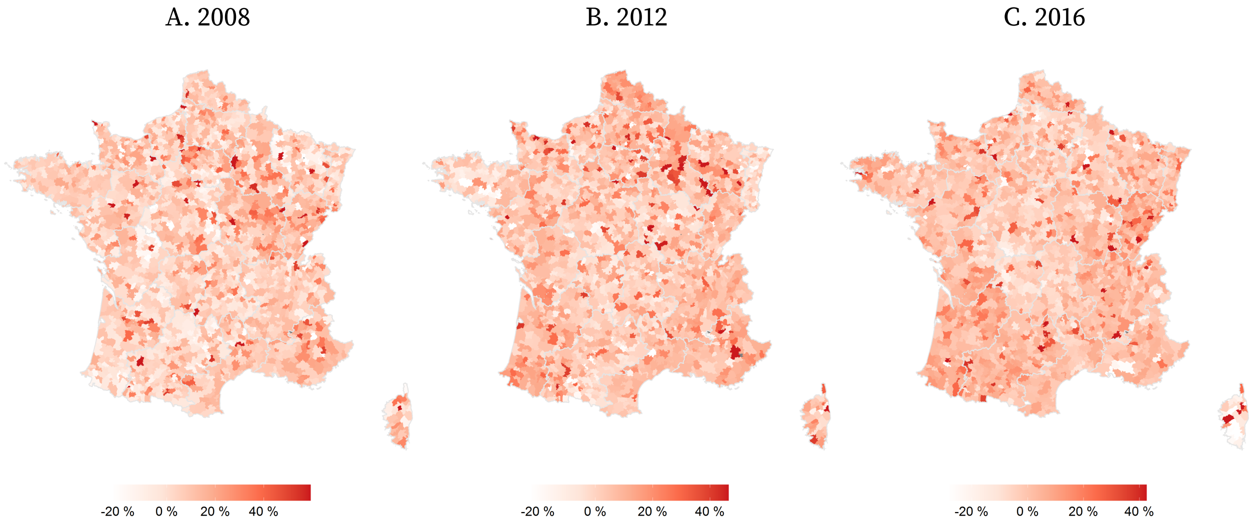
$$\Delta C^{gov}_{mbt} = \alpha^{gov}_{mt} + \alpha^{gov}_{bt} + \varepsilon_{mbt} \qquad (2)$$

where the outcome variable ΔC^{gov}_{mbt} is the mid-point growth rate in local government lending by bank b in municipality m. I estimate this equation by weighted least squares, with weights equal to the mid-point, so that estimated fixed effects allow to recover aggregate flows (Amiti and Weinstein (2018), Beaumont, Libert, and Hurlin (2019)).[19] The bank fixed effects, α^{gov}_{bt}, measure the variation in banks' lending that is common across municipalities, like bank-specific credit supply factors. Consequently, the parameters α^{gov}_{mt} are estimates of changes in municipalities m's demand for credit that are purged of municipalities' differential exposure to bank-level variation in credit supply. The maps in Figure 2 display the estimated parameters α^{gov}_{mt} for three dates in my sample. I then use the estimated municipality fixed effects α^{gov}_{mt} to construct a bank-level local government loan demand

[18] See Appendix D for a model that clarifies the parameter estimated in this regression.

[19] Namely, $\Delta C^{gov}_{bt} = \hat{\alpha}^{gov}_{bt} + \sum_m w(m)_{bt}\hat{\alpha}^{gov}_{mt}$ where $w(m)_{bt}$ is the weight of municipality m in bank b credit ; $\Delta C^{gov}_{mt} = \hat{\alpha}^{gov}_{mt} + \sum_b w(b)_{mt}\hat{\alpha}^{gov}_{bt}$ where $w(b)_{mt}$ is the weight of bank b in municipality m credit ; and $\Delta C^{gov}_{t} = \sum_m w_{mt}\hat{\alpha}^{gov}_{mt} + \sum_b w_{bt}\hat{\alpha}^{gov}_{bt}$ where w_{bt} (w_{mt}) is the weight of bank b (municipality m) in total credit.

FIGURE 2. Local government debt demand shock by municipality



Note: These maps depict the municipality-level parameters $\hat{\alpha}^{gov}_{mt}$ estimated from equation 2, for three dates in my sample. Regional boundaries appear in light gray.

shifter:

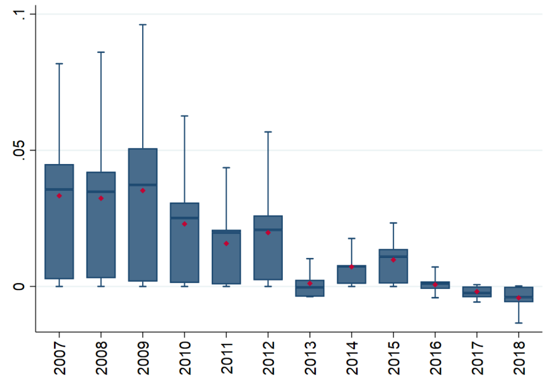
$$BankExposure_{bt} = \sum_m \omega^{gov}_{bm,t-1} \times \hat{\alpha}^{gov}_{mt} \quad \text{with} \quad \omega^{gov}_{bm,t-1} = \frac{C^{gov}_{bm,t-1}}{C^{tot}_{b,t-1}} \tag{3}$$

$\omega^{gov}_{bm,t-1}$ captures each bank's exposure to the local government loan market in municipality m relative to that bank's total credit.[20] *BankExposure* captures the bank-specific demand for local government loans attributable to the fact that banks' differential pre-determined exposure to municipalities generates heterogeneous exposure to the variation in local government debt demand shocks. The variation in exposure across banks can equivalently be understood in terms of variation in local market shares across banks.[21] The exposure weights $\omega^{gov}_{bm,t-1}$ sum to banks' local government loan share $\lambda^{gov}_{bt-1} = \frac{C^{gov}_{b,t-1}}{C^{tot}_{b,t-1}}$ which is always included as a control (as recommanded by Borusyak, Hull, and Jaravel, 2021). Figure 3 plots the distribution of *BankExposure* by year.

[20] I normalize by total credit because crowding out depends on the increase in local government's demand relative to total lending capacity, as appears in the model in Appendix D. Moreover, it is defined for banks that do not lend to local governments.

[21] To see this, define $\hat{\mathrm{d}}C^{gov}_{mt} = \hat{\alpha}^{gov}_{mt} \times C^{gov}_{m,t-1}$, akin to the predicted municipality-level euro change in demand, and $\tilde{\omega}^{gov}_{mb,t-1} = C^{gov}_{bm,t-1}/C^{tot}_{b,t-1}$, the market share of bank b in municipality m. We can rewrite $BankExposure_{bt} = \frac{1}{C^{tot}_{b,t-1}} \sum_m \tilde{\omega}^{gov}_{mb,t-1} \times \hat{\mathrm{d}}C^{gov}_{mt}$: the amount $\hat{\mathrm{d}}C^{gov}_{mt}$ is allocated to each bank in proportion to their lagged market shares in m, and the bank-level predicted amount is then normalized by bank total credit.

FIGURE 3. Bank Exposure to local government debt demand shocks



Note: This figure shows the distribution of *BankExposure* (defined in (3)) by time period. The bars indicate the median and the interquartile range. The whiskers indicate the 10th and 90th percentiles. All statistics are weighted by banks' lagged total lending.

4.2. Identifying assumptions

The goal is to identify the relative crowding out parameter β. My empirical design is meant to address two main threats to identification that arise in this setting.[22] This design will be valid if the standard orthogonality condition is satisfied: $\mathbb{E}[BankExposure_{bt}\varepsilon_{fbt}|\mathbf{X}_{fbt}, d_{ft}] = 0$ (assumption A1).

Correlated firm-level credit demand shocks. The first hurdle to estimating β is the potential correlation between local government debt and firm-level credit demand shocks. If local government debt is used as a countercyclical policy tool, changes in local government debt will be negatively correlated to firm-level shocks. Conversely, positive demand effects of local government debt would induce a positive correlation with firm-level shocks. This correlation may exist not only in the time series, but also across banks. If banks have different geographical footprints, and if the correlation between local government debt and corporate credit operates at the local level, firm-level demand shocks will differ for banks experiencing different local government loan demand.

I address this identification problem by focusing on firms with multiple lending relationships and adding firm$\times$time fixed effects, which capture the firm-level determinants of credit flows that are common to all of its lenders (Khwaja and Mian, 2008). Provided that firm-level demand shocks are symmetric across lenders, they will be absorbed by the fixed effects. This design relies on the fact that the aforementioned confounding channels predict a correlation between local government debt and *firm*-level credit *demand*, while

[22]See the model in Appendix D for a formalization of these identification concerns.

crowding out uniquely operates as a shock to the *bank*-specific *supply* of credit, which depends on the bank-level increase in local government loans.

How plausible is the assumption that firm-level demand shocks are symmetric across lenders? I focus on credit with initial maturity above one year, a relatively homogeneous loan category, which makes this assumption less demanding (Ivashina, Laeven, and Moral-Benito (2020)). Regressing firm-bank credit growth on firm$\times$time fixed effects shows that firm effects explain 28% of the variation (Table A1). Adding bank$\times$time fixed effects increases the adjusted R-squared by only 6%. As expected, firm$\times$time fixed effects explain less of the variation when I bundle all loan types instead of focusing on loans with initial maturity above one year. Section 4.3.2 presents additional tests supporting this assumption.

Correlated bank-level credit supply shocks. Estimating β presents a second endogeneity issue: bank's lending to local governments and to corporates are jointly determined in bank *b*'s optimization problem and may thus be correlated. For instance, a bank-level liquidity shock will affect its lending to both local governments and corporates. Banks may also endogenously decide to rebalance their portfolio away from corporates and into local governments. This is the rationale for constructing the demand shifter *BankExposure*, as opposed to using realized bank-level local government debt growth as an explanatory variable. $BankExposure_{bt}$ predicts local government debt growth, but it is purged from bank *b*'s supply factor that may also enter the residual ε_{fbt} (Figure A8). The validity of this approach requires that assumption A1 holds, which implies that $BankExposure_{bt}$ must not be correlated with other bank-level corporate credit supply shocks entering ε_{fbt}.

Given the shift-share structure of $BankExposure_{bt}$, the main threat to identification is if banks sort across municipalities such that unobserved bank-level shocks are correlated to both a decline in corporate credit supply and increases in local government loans in the locations where the bank operates (Borusyak, Hull, and Jaravel, 2021). Put differently, banks with negative corporate credit supply shocks must not systematically have high market shares in high local government debt demand municipalities.[23]

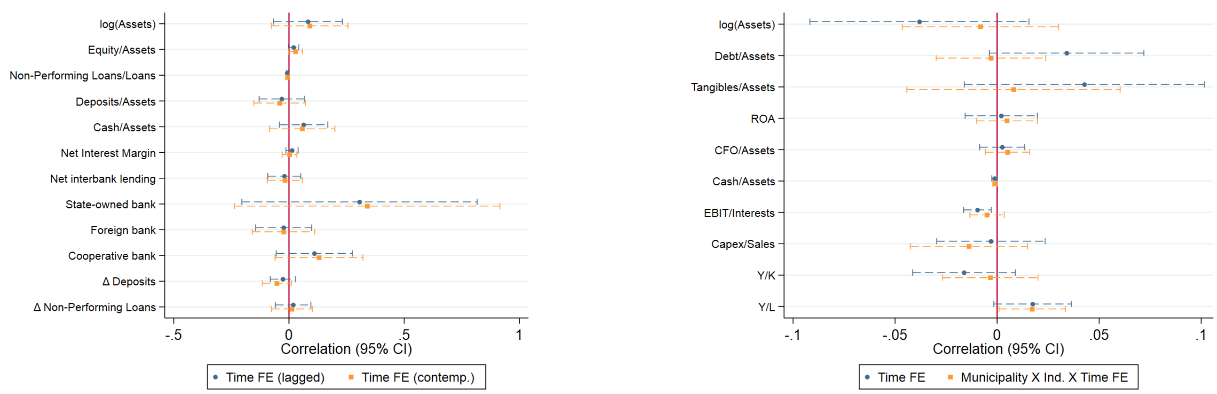
The most direct test supporting assumption A1 is bank-level balance on observables. Figure 4A shows that banks with high and low *BankExposure* are similar on variables that are known determinants of corporate credit supply, notably bank size and equity ratio. I show both lagged and contemporaneous correlation to show that banks' balance sheets do no deteriorate at the same time as the increase in local government debt.

[23]It is *not* a problem that banks sort into locations based on sectoral specialization or types of clienteles, so that banks with different exposure lend to firms with different credit *demand*. The firm$\times$time fixed effects control for these differences. What matters is that the geographic exposure to local government debt is not correlated to other bank-level credit *supply* shocks.

FIGURE 4. Correlation between exposure to local government debt demand shocks and pre-determined characteristics

A. Bank-level correlations

B. Firm-level correlations



Note: Panel (a) shows the coefficient of bank-level regressions of bank exposure to local government debt demand (defined in (3)) on bank characteristics, including time fixed effects. The blue dots correspond to correlations between *BankExposure* and lagged bank characteristics. The orange dots correspond to correlations between *BankExposure* and contemporaneous bank characteristics. Regressions are weighted by bank-level corporate credit. Standard errors are clustered at the bank level. Panel (b) shows the coefficient of firm-level regressions of firm exposure to crowding out (defined in defined in (5)) on firm characteristics, including time fixed effects (blue dots) or municipality×industry×time fixed effects (orange dots). Standard errors are clustered at the main bank and municipality level. The dot is the point estimate and the bar is the 95% confidence interval. All variables are standardized.

The next two paragraphs provide more details on the two components of the shift-share variable that support the identifying assumption, following the identification approach in Borusyak, Hull, and Jaravel (2021).

Shifters. A sufficient condition for assumption A1 to hold is if the municipality-level local government debt demand shocks α^{gov}_{mt} are "as good as random". Figure A5 shows that α^{gov}_{mt} is not correlated with municipality-level economic outcomes. This may appear surprising, as local government debt is endogenous to local outcomes. However, this relationship is unlikely to operate at the municipality level: municipalities are small and are not the relevant economic scale for stimulus spending effects, and there is high dispersion in α^{gov}_{mt} across neighboring municipalities. In addition, the α^{gov}_{mt} are not persistent, as shown by the low autocorrelations in Figure A6.

Shares. The necessary condition is that α^{gov}_{mt} is orthogonal to the average corporate credit supply shock of banks weighted by banks' exposure weights, or equivalently, bank-level corporate credit supply shock is orthogonal to the average α^{gov}_{mt} weighted by banks' exposure weights (which is just assumption A1). Three features of the weights make this assumption more likely. First, I use shares in the market for local government debt, that differ from shares in the corporate credit market. This avoids picking up bank exposure to municipality-level shocks related to corporate credit (see placebo in Table A4). Second, shares are dispersed across neighboring municipalities, ruling out that they just capture banks' exposure to a broad geographic area. Third, shares are very persistent (Figure A6). Combined with the fact that shifters are not persistent, this rules out that (i) some banks have always high or low *BankExposure*, and that (ii) banks on declining corporate credit trends strategically increase their shares in every period in high α_{mt}^{gov} municipalities.

Taken together, this makes it unlikely that BankExposure is correlated to corporate credit supply shocks. I further discuss the shifters vs. shares view of identification with shift-share instruments in this setting and provide additional tests in Appendix B.

4.3. Results

4.3.1. Baseline results

Table 2 presents the results corresponding to equation (1). This specification can only be estimated for multibank firms, which represent 30% of firms and 70% of corporate credit volumes. Because computing firm-bank credit growth and *BankExposure* requires one lag, the estimation sample is 2007-2018. In the baseline results, controls include the bank's lagged local government loan share, assets (in logs), equity ratio, a dummy indicating whether the bank is state-owned and indicating foreign banks. All regressions are weighted by the denominator of the mid-point growth rate to obtain results that are representative at the aggregate level. Because the distribution of firm size is highly skewed, I winsorize the top 0.5% of weights to avoid results being overly sensitive to a few very large firms. Standard errors are double-clustered at the bank level (the level of the shock) and at the municipality level (to account for the correlation of residuals across banks that have similar municipality exposures, an issue raised by Adão, Kolesár, and Morales (2019) and Borusyak, Hull, and Jaravel (2021)). Section 4.3.2 presents robustness checks for all of these choices.

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

TABLE 2. Crowding out effect on corporate credit

	Credit growth					
	Baseline			$\mathbb{P}$(multibank)-adjusted weight		
	(1)	(2)	(3)	(4)	(5)	(6)
BankExposure	-0.162	-0.753**	-0.853***	-0.205	-0.907***	-1.036***
	(0.191)	(0.311)	(0.311)	(0.208)	(0.351)	(0.357)
Controls	–	–	✓	–	–	✓
Firm×Time FE	–	✓	✓	–	✓	✓
Observations	2,744,597	2,744,597	2,731,110	2,744,597	2,744,597	2,731,110
R-squared	0.000039	0.53	0.54	0.000042	0.54	0.54

Note: This table reports the results of estimating equation (1). The outcome variable is the bank×firm-level mid-point growth rate of credit. The main independent variable is bank exposure to local government debt demand shocks (defined in (3)). Controls include the bank's lagged local government loan share, assets (log), equity ratio, and dummies for state-owned and foreign banks. Regressions are weighted by bank×firm-level mid-point credit (top 0.5% winsorized). In columns (3)-(6), the weight is divided by the probability that a firm belongs to the multibank sample (details in main text). Standard errors are double-clustered at the bank and municipality level. ***, ** and * indicate significance at the 1%, 5% and 10% levels, respectively.

as would occur if local government debt had positive demand effects.

The point estimate implies that a one standard deviation increase in *BankExposure* reduces bank-level corporate credit growth by 1.22pp.[24] As a back-of-the-envelope computation assuming all variables are equal to their sample means, the coefficient in column (3) implies that when local governments borrow an additional €1 from a given bank in a year, that bank lends €0.54 less to private firms in that year.[25]

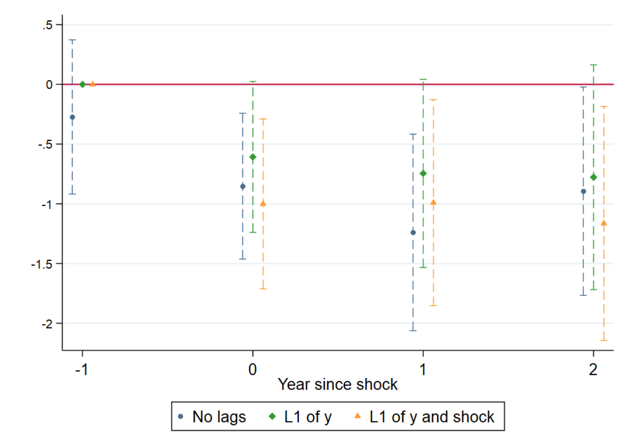
One important limitation of the within-firm estimator is that it restricts the sample to multibank firms, which may yield estimates that are not representative of the population. Figure A4 shows that the multibank sample over-represents firms that are larger in terms of outstanding credit. To alleviate this concern, I use as weight the baseline weight divided by the probability that a firm appears in the multibank sample. This probability is estimated for 20 equally-sized bins of firms based on outstanding credit quantiles. The results are in columns (4) to (6). The point estimates are in the same order of magnitudes, larger by approximately 20%, suggesting a slightly stronger crowding out for smaller firms.

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

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

[24]For the mid-point growth rate, or equivalently, reduces the standard growth rate by 1.23pp.
[25]Computation details in Section C.1.

FIGURE 5. Crowding out effect on corporate credit: dynamic effect



Note: This figure plots the estimated coefficients β_h resulting from estimating equation (1), where the outcome is the h-horizon mid-point growth rate $\frac{C_{f,b,t+h}-C_{f,b,t-1}}{0.5(C_{f,b,t+h}+C_{f,b,t-1})}$. "No lags" is the baseline specification, including controls. "L1 of y" adds one lag of the outcome variable as a control. "L1 of y and shock" adds one lag of the outcome variable and one lag of the shock as controls. The dot is the point estimate and the bar is the 95% confidence interval. All other elements of the specification are as in Table 2.

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

Figure 5 shows the effect of bank exposure to local government debt demand shocks at longer horizons by estimating local projections. The effect of *BankExposure* does not mean revert in the two years following the shock, suggesting a permanent reduction in corporate credit. The absence of a significant pre-trend and the robustness to the inclusion of lagged independent and dependent variables alleviate concerns that banks with high *BankExposure* have systematically lower corporate credit growth independently of local government debt shocks.

4.3.2. Robustness and further tests of the identifying assumption

Distortions in the market for local government lending and crowding out. I estimate the effect of a *marginal* €1 increase in local government loans on corporate credit. The market for local government loans may be subject to regulatory or political distortions that affect the *level* of local government lending. In theory, the marginal effect is independent of these level distortions and is only determined by banks' ability to expand their balance sheets.[26] I rule out one important level distortion: 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. Table A6 shows that the crowding out coefficient is independent of various proxies for political pressure on banks.

Further tests 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 the firm's banks; and (2) there are no other bank-level credit supply shocks that are systematically correlated with *BankExposure*.

More granular fixed effects: A story that would violate assumption (1) is if, when local government debt rises, corporate demand shifts toward banks that are not active in the market for local government loans. Similarly, assumption (2) would be violated if banks lending to local governments receive different credit supply shocks. If these effects are time-varying, they are not controlled for by the share of local government loans in the bank's loan portfolio. I alleviate this concern by further interacting the firm$\times$time fixed effect with a dummy equal to 1 if the bank is active in lending to local governments. Another concern regarding assumption (2) is that *BankExposure* just captures the geographic footprint of banks, which may be correlated with other bank-specific shocks. To alleviate this concern, I compute banks' lending shares in each of the 22 French regions, and I include these 22 additional controls interacted with date dummies. Finally, I further test the assumption that high exposure banks do not receive other bank-specific demand (A1) or supply (A2) shocks by including bank fixed effects that control for any time-invariant factor affecting local government and corporate credit at the bank level. These specifications produce coefficients very similar to my baseline result (Table A7).

Heterogeneity by strength of demand effects: To further test assumption (1), I exploit the fact that some firms are more likely to experience a positive demand shock when local government debt increases. Local government debt finances public investment projects, which generates an increase in public procurement contracts. I flag the top 10 industries in terms of public procurement contracts revenues as highly sensitive to local government debt

[26] 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.

shocks. If the firm$\times$time fixed effects were unable to control for firm-level credit demand, we would observe relatively higher credit growth for these firms as local government debt increases. Table A7 shows that this is not the case: the effect of exposure to local government debt shocks is not significantly different for these firms.

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

Additional robustness checks. I perform a variety of additional robustness checks of my baseline results, detailed in Appendix C.1. First, Table A8 reports results when including additional bank-specific controls, excluding banks with total loan portfolio below €50 millions, or those that never participate in the market for local government debt. Additionally, I illustrate in Figure A9 the estimated coefficients when excluding any of the 100 largest banks or municipalities, and drawing random subsets of controls in the set of all available controls. Second, Table A9 displays results for alternative definitions of dependent and independent variables. Third, Table A10 shows robustness to excluding outliers and to various assumptions on the clustering of standard errors. Fourth, Table A11 shows robustness to different weighting schemes.

5. Mechanism

5.1. What prevents banks from increasing total credit supply?

Ideally, banks should match the additional demand for credit by additional funding. However, banks only have a limited ability to attract more deposits or to raise equity, interbank markets are imperfect, and banking regulation may additionally constrain total lending. In theory, the severity of these constraints determines the extent of crowding out. To test this hypothesis, I examine whether, in the cross-section of banks, crowding out is stronger for banks that appear more constrained in their ability to increase credit supply.

Table 3 presents the results. Column (1) shows that crowding out is more severe for smaller banks, which are likely to be overall more constrained.[27] Column (2) shows that crowding out is more severe for banks with lower equity ratios, that are likely to be more capital-constrained. Liquidity constraints also appear to matter: banks with more liquid assets exhibit lower crowding out (column 3) and banks with more short-term liabilities exhibit stronger crowding out (column 4). 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

[27] The definition of all characteristics is detailed in the notes of Table 3.

TABLE 3. Severity of crowding out by banks' characteristics

	Credit growth					
	(1)	(2)	(3)	(4)	(5)	(6)
BankExposure	-1.453***	-1.178***	-0.970***	-0.502*	-1.701***	-0.950***
	(0.409)	(0.353)	(0.326)	(0.272)	(0.540)	(0.332)
Large×*BankExposure*	0.757					
	(0.466)					
High equity ratio×*BankExposure*		0.752**				
		(0.365)				
High liquid assets×*BankExposure*			0.681			
			(0.435)			
High ST debt×*BankExposure*				-0.833*		
				(0.438)		
High collateral×*BankExposure*					0.943*	
					(0.480)	
High international×*BankExposure*						1.103*
						(0.645)
Controls×Bank char.	✓	✓	✓	✓	✓	✓
Firm×Time FE	✓	✓	✓	✓	✓	✓
Observations	2,731,110	2,731,110	2,731,109	2,724,315	2,730,682	2,731,110
R-squared	0.54	0.54	0.54	0.54	0.54	0.54

Note: This table reports the results of estimating specification (1), allowing for heterogeneity by banks' characteristics. The outcome variable is the bank×firm-level mid-point growth rate of credit. The main independent variable is bank exposure to local government debt demand shocks (defined in (3)). Large is a dummy equal to 1 if bank's assets are above median. High equity ratio is a dummy equal to 1 if the bank's total equity as a fraction of its total assets exceeds the 75th percentile. High liquid assets is a dummy equal to 1 if the ratio of the bank's short-term assets to its total assets exceeds the 75th percentile. High ST debt is a dummy equal to 1 if the ratio of the bank's short-term debt to its total assets exceeds the 75th percentile. 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 international is a dummy equal to 1 if the share of bank liabilities held by non-residents is above median. Controls include the bank's lagged local government loan share, assets (log), equity ratio, and dummies for state-owned and foreign banks. Regressions are weighted by bank×firm-level mid-point credit (top 0.5% winsorized). Standard errors are double-clustered at the bank and municipality level. ***, ** and * indicate significance at the 1%, 5% and 10% levels, respectively.

as collateral with the European Central Bank, making their overall assets more liquid. Finally, crowding out is weaker for banks with better access to international financing sources (column 6), emphasizing the importance of banks' access to a large pool of funding. Together, these results imply that crowding out is related to banks' limited ability to increase their total balance sheet size, in line with the standard theory.

I explore two further implications in Table A2. 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 significantly increase corporate credit. This is in line with the mechanism proposed: constrained banks have more leeway to adjust to a reduction in credit demand (e.g., by holding liquid assets instead of increasing credit) than to an increase. Second, splitting the sample in two subperiods, I find that crowding out is more severe over 2007-2013—corresponding to the Great Financial crisis and the Euro Area sovereign debt crisis—than over 2014-2018, a period with no notable financial turmoil and characterized by an accommodative monetary policy which likely relaxed banks' balance sheet constraints.

5.2. Price vs. quantity adjustment.

The results presented so far relate to corporate credit quantities. I now investigate how increases in local government debt demand affect interest rates, using the "New contracts" dataset collected by Banque de France, which includes information on interest rates for a representative sample of loans. I estimate the effect of local government debt shocks on interest rates using the within-firm specification (1), with the interest rate charged by bank b on new loans to firm f as a dependent variable. Details are in Appendix C.2.

The results are presented in Table A12. I find that the price effect is positive, consistent with a reduction in credit supply.[28] That said, the price effect is small compared to the quantity reaction, implying a price elasticity of corporate credit demand close to 30. This is in line with the empirical evidence on loan price stickiness and on bank-level shocks inducing quantity rationing without price adjustments, as well as with structural estimations of the price elasticity of corporate credit demand.[29] This result is usually rationalized by concerns about the adverse selection effects of higher interest rates (Stiglitz and Weiss, 1981).

6. Relative crowding out: investment

The previous results show that lenders exposed to increased demand for local government loans reduce their credit supply to firms. How does the reduction in bank-level credit affect firm-level credit and investment?

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

[28]This result incidentally attenuates concerns about the baseline results being driven by bank-specific credit demand shocks: in this case, we should find lower rates for more exposed banks.

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

effect into a firm-level effect by considering firms' exposure to the shock through their lenders. I estimate the following specification:

$$\Delta K_{ft} = \beta^K FirmExposure_{ft} + \Phi \cdot \mathbf{X}_{ft} + \alpha_{mst} + \varepsilon_{ft} \tag{4}$$

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

$$FirmExposure_{ft} = \sum_b \omega_{fb,t-1} BankExposure_{bt} \tag{5}$$

α_{mst} are municipality×two-digit industry×time fixed effects. $\mathbf{X}_{ft}$ is a vector of firm-level controls. $FirmExposure_{ft}$ captures the extent to which a firm borrows from banks subject to increased demand for local government loans. Intuitively, the specification compares firms borrowing from banks subject to higher demand for local government loans to firms borrowing from other banks.

To understand the logic of the identification, it is useful to return to the firm×bank-level model (1). Aggregating this specification at the firm level using bank shares, we obtain (omitting controls): $\Delta C_{ft} = d_{ft} + \beta FirmExposure_{ft} + \varepsilon_{ft}$. That is, firm-level credit growth depends on firm-level exposure to crowding out and on firm-level unobserved credit demand shocks. This equation highlights the identification challenge. If *BankExposure* was correlated to d_{ft}, then *FirmExposure* is also correlated to d_{ft}. Besides, the firm-level specification cannot include firm×time fixed effects to absorb the firm-specific shocks. Following the logic of Cingano, Manaresi, and Sette (2016) and Jiménez et al. (2019), I overcome this issue by including as a control an estimates of the firm-level shocks d_{ft} obtained from a decomposition of corporate credit flows into firm×time and bank×time components.[30] This procedure precisely controls for the correlation between *FirmExposure* and d_{ft}. Identification of β in the firm-level credit growth regression then follows from identification in the firm×bank-level credit growth specification.

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

I further tighten my identification strategy by looking at the effect of *FirmExposure*

[30] Cingano, Manaresi, and Sette (2016) and Jiménez et al. (2019) recommend using d_{ft} estimated from the within-firm specification (1). Using the Amiti and Weinstein (2018) decomposition makes this procedure more robust to the existence of bank-specific credit supply shocks other than *BankExposure*. This choice does not affect my results, as shown in robustness checks.

within municipality×industry×time cells. Municipality×time fixed effects imply that I only compare firms experiencing a similar local-level increase in local government debt, partialling out the local-level macroeconomic relationship between local government debt and private firms' prospects. Further interacting these fixed effects with industries allows any local effect of local government debt to vary across industries. Within these cells, I exploit variation *across* firms differentially exposed to crowding out through their banking relationships.

The identifying assumption is that, conditional on fixed effects and controls, the firm-level unobserved determinants of investment are orthogonal to *FirmExposure*. Figure 4B tests whether firms with higher exposure to crowding out are systematically different on observed characteristics. I report unconditional correlations and correlations conditional on the fixed effects included in the firm-level specification. Reassuringly, *FirmExposure* is uncorrelated to the known predictors of corporate investment such as size, leverage, profitability, or availability of internal funds. Section 6.2.2 provides further tests of the identifying assumption.

In the baseline specification, the dependent variables are the mid-point growth rate of credit (obtained from the credit registry) and the growth rate of fixed assets (obtained from firms' tax-filings). The tax-filings are available only for firms with annual turnover above €750,000 and do not account for entry and exit, hence I consider only the intensive margin for investment.[31] Bank shares are defined as mid-point shares to properly aggregate the within-firm specification in mid-point growth rates. Consistency with (1) requires that $\mathbf{X}_{ft}$ contains the firm-level weighted average of $\mathbf{X}_{fbt}$. I also include additional firm-level controls most common in investment regressions: total assets (logged), leverage ratio, cash flow-to-assets, profitability (ROA), and capex intensity (capex/sales), all lagged by one period. As in Alfaro, García-Santana, and Moral-Benito (2021), I recover firm-level demand shocks for both multi-bank and single-bank firms. The firm-level effects are thus estimated on the sample of all firms with tax-filings data. Regressions are weighted by mid-point credit volumes, top-winsorized at the 0.5% level. Section 6.2.2 provides results with alternative specifications.

6.2. Results

6.2.1. Baseline results

I first repeat the within-firm estimation on the tax-filings subsample to obtain the relevant magnitudes. Table A3 lists the results. The point estimate is -0.95, slightly larger than in

[31] Figure A4 provides a visual representation of the sample selection imposed by the tax-filings.

TABLE 4. Firm-level effect on credit and investment

	Effect of exposure to local government debt shocks				Credit-to-inputs elasticities	
	gr(credit)		gr(capital)		gr(capital)	
	RF	RF	RF	RF	IV	IV
	(1)	(2)	(3)	(4)	(5)	(6)
FirmExposure	-1.100***	-1.064***	-0.428***	-0.452***		
	(0.277)	(0.259)	(0.084)	(0.081)		
gr(credit)					0.270***	0.285***
					(0.050)	(0.060)
Firm controls	–	✓	–	✓	–	✓
Municipality×Industry×Time FE	✓	✓	✓	✓	✓	✓
Observations	1042147	798769	883748	776281	814589	715695
R-squared	0.95	0.95	0.42	0.44	0.15	0.15
F stat.					21.7	22.9

Note: This table reports the results of estimating equation (4). Outcome variables are the firm-level mid-point growth rate of credit and the growth rate of fixed assets. The main independent variable is firm exposure to crowding out (defined in (5)). All regressions include the firm-level average of the bank controls included in Table 2 and the estimated firm-level credit demand shock. "Firm controls" additionally include the firm's assets (log), leverage, ROA, cash flow from operations/assets, and capex/sales ratios (all lagged). Columns (5) and (6) show the credit-to-capital elasticity, obtained by instrumenting firm-level credit growth by *FirmExposure*. Regressions are weighted by firm-level mid-point credit (top 0.5% winsorized). Standard errors are double-clustered at the main bank and municipality level. ***, ** and * indicate significance at the 1%, 5% and 10% levels, respectively.

the full sample.

Table 4 presents the firm-level effects obtained from estimating (4). Columns (1) and (2) show that firms more exposed to crowding out receive less credit. The magnitude is very much in line with the within-firm specification, suggesting that firms have very little ability to substitute toward less affected lenders when one of their lenders is shocked. This limited ability to substitute across banks has been repeatedly documented in reduced-form studies of corporate credit supply shocks. A plausible explanation is that banks interpret credit cuts at others bank as a negative signal on borrowers' quality (Darmouni, 2020).

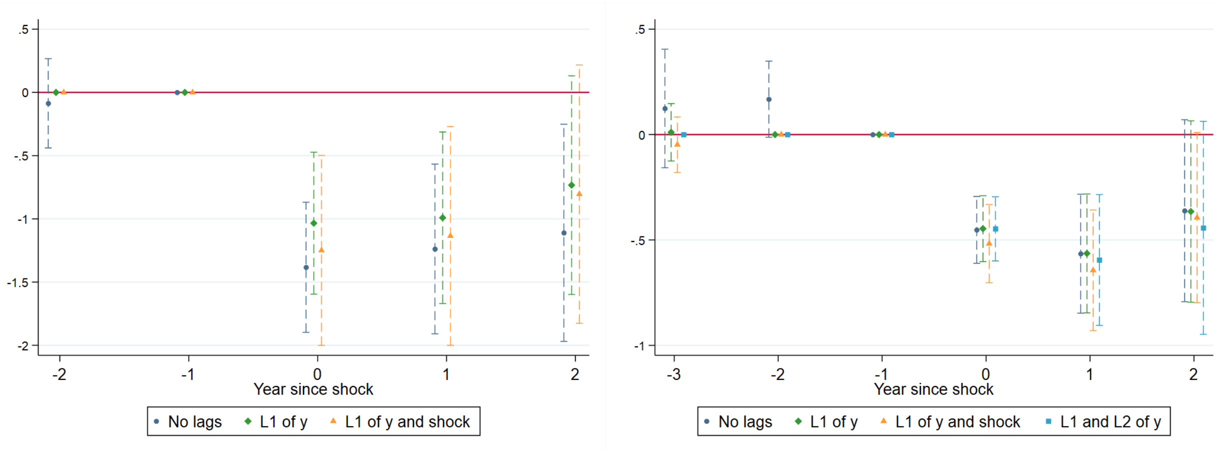
Columns (3) and (4) show that firms more exposed to crowding out invest significantly less. This indicates that the contraction in credit is not offset by other sources of financing, and forces the firm to cut back on investment. In columns (5) and (6), I separately estimate the credit-to-investment sensitivity η^K by using *FirmExposure* as an instrument for firm-level credit growth.[32] I find a credit-to-investment elasticity equal to 0.27, well within the range of existing estimates (e.g., Cingano, Manaresi, and Sette (2016), Amiti and Weinstein (2018), Acharya et al. (2018)).

These estimates can be used to quantify the effect of an additional €1 in local government debt at one bank on investment at firms borrowing from this bank. Starting from the effect on credit obtained from the within-firm estimation and using the credit-

[32] In this specification, credit growth is defined as the standard growth rate (and not mid-point growth rate) so that the obtained coefficient has the interpretation of an elasticity.

FIGURE 6. Firm-level effect of crowding out: dynamic effect

A. Credit | B. Investment



Note: This figure plots the estimated coefficients β_h resulting from estimating equation (4). For credit, the outcome is the h-horizon mid-point growth rate $\frac{C_{f,t+h}-C_{f,t-1}}{0.5(C_{f,t+h}+C_{f,t-1})}$. For investment, the outcome is the h-horizon growth rate $\frac{K_{f,t+h}-K_{f,t-1}}{K_{f,t-1}}$. "No lags" is the baseline specification, including controls. "L1 of y" adds one lag of the outcome variable as a control. "L1 of y and shock" adds one lag of the outcome variable and one lag of the shock as controls. "L1 and L2 of y" adds two lags of the outcome variable as controls. The dot is the point estimate and the bar is the 95% confidence interval. All other elements of the specification are as in Table 4.

to-investment sensitivity η^K, I find that an additional €1 in local government debt at one bank leads to a €0.32 drop in corporate investment at firms borrowing from this bank.

Figure 6 shows the effect of firm exposure to crowding out at longer horizons by estimating local projections. For investment, I use the fact that the data is available before 2006 to add an additional lag. The effect of *FirmExposure* on credit and investment does not mean revert in the two years following the shock, suggesting a permanent effect. The absence of a significant pre-trend and the robustness to the inclusion of lagged independent and dependent variables further alleviate identification concerns.

In Table A13, I present the same results for firm-level employment. I find no effect. The past literature has found mixed evidence as to the effect of credit contractions on employment, with strong effects in some studies (e.g., Chodorow-Reich (2014)) and null effects in others (e.g., Greenstone, Mas, and Nguyen (2020)). In the case at hand, the null effect may come from the fact that I focus on credit with initial maturity above one year, which typically finances investment rather than working capital.

6.2.2. Further tests and robustness checks

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

More granular fixed effects: My baseline specification includes municipality$\times$88 industries$\times$time fixed effects. Table A14 shows the results with alternative fixed effect structures. In particular, I can further tighten the identification by adding firm size bins$\times$time fixed effect, to allow for size-specific time-varying shocks. I also include firm fixed effects and lagged credit growth as a control, in order to control for firm-specific time invariant characteristics or to restrict the comparison to firms on a similar credit trend. I obtain point estimates very similar to my baseline.

The magnitude of the coefficient is remarkably stable across all specifications, despite the fact that the inclusion of the finer grid of fixed effects drastically increases the R-squared. This finding reveals that, if any unobservable is affecting both exposure to crowding out and investment, then it must be orthogonal to municipality-level industry-specific trends and to firm invariant characteristics. This is very unlikely. A formal econometric treatment of this argument is provided by Oster (2019). Applying this methodology to the investment specification, I find a value for the δ parameter equal to 10.5 when comparing the least stringent specification in Table A14 (column 1) to the most stringent specification (column 4), well above the recommended value of 1.[33] Consequently, correlated unobservables are unlikely to drive my results.

Heterogeneity by strength of demand effects: I exploit the fact that firms in industries highly reliant on public procurement contracts are likely to experience a positive demand shock when local government debt increases. If my specification imperfectly controls for the demand effects of local government debt, I would find that exposure to local government debt shocks has a less negative effect for these firms. Interacting *FirmExposure* with a dummy for industries highly reliant on public procurement contracts, I observe no differential effect for these firms (Table A14).

Robustness checks. I perform a variety of robustness checks of my results, detailed in Appendix C.3. First, Table A15 reports results when progressively adding the baseline firm-level controls, when including additional firm-level controls, when including additional controls related to banking relationships, when using an alternate version of the estimated firm demand shock, or when imposing additional sample restrictions. Second,

[33]The interpretation of this parameter is that the correlation of unobservables with the variable of interest must be $\approx$10 times larger than that of observables for a bounding set accounting for the presence of unobservables to include 0. See details in Appendix C.3.

Table A16 explores the results with alternative weighting strategies. Table A17 presents the results with an alternative definition of *FirmExposure*, different winsorization, and different assumptions on the appropriate level of clustering. The estimated coefficients are extremely similar across all specifications.

6.3. Heterogeneous effects

Heterogeneous crowding out effects across firms may arise from two channels. First, some firms may experience a larger credit cut. Second, firms may differ in their sensitivity of investment to a given credit cut.

I investigate heterogeneous effects by firm size, by dependence on external finance (using as proxies the Rajan and Zingales (1998) index or firm leverage), by bank dependence specifically (proxied by the ratio of bank debt to total debt), and by a proxy for the marginal product of capital.

The first panel of Table 5 investigates the first channel and shows that the credit cut is relatively uniform across these dimensions.

Panels B investigates the second channel. Proxies for firm dependence on external finance and on bank finance significantly affect the sensitivity of investment to the availability of bank financing, in line with intuition. For instance, columns (1) and (2) show that more leveraged firms exhibit a credit-to-investment sensitivity that is almost twice larger than the baseline. I also find that small firms, a typical proxy for capital constraints, have higher credit-to-investment sensitivities, in line with the idea that these firms have a lower ability to turn toward alternative sources of financing.

Finally, I investigate how the effect varies when sorting firms by revenues-over-capital, 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.[34] In line with this intuition, I find that firms with higher Y/K have larger credit-to-investment sensitivity. Therefore, even though banks do not selectively cut credit to high marginal product firms, these higher sensitivities imply that crowding out generates a larger reduction in inputs for firms with higher marginal output gains from those inputs. This indicates that the shock reduces allocative efficiency. The next section quantifies the aggregate cost of this effect.

[34]The advantage of looking at dispersion in marginal products is that it provides an agnostic way to study the effect of frictions on input acquisition (Hsieh and Klenow, 2009).

TABLE 5. Firm-level real effects: heterogeneity

Panel A: Credit

	gr(credit)									
	Leverage		Rajan-Zingales		Bank dep.		Size		Y/K	
	Low	High	Low	High	Low	High	Low	High	Low	High
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
FirmExposure	-0.872***	-1.065***	-1.126***	-1.040***	-1.490***	-1.020***	-1.112***	-1.025***	-0.998***	-1.129***
	(0.307)	(0.244)	(0.314)	(0.266)	(0.428)	(0.258)	(0.417)	(0.265)	(0.252)	(0.298)
Controls	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Municipality×Ind.×Time FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Observations	153,840	534,423	158,034	584,831	149,119	595,640	34,466	740,952	579,728	169,556
R-squared	0.96	0.96	0.98	0.94	0.99	0.94	0.97	0.95	0.95	0.97
High *minus* Low		-.193		.086		.471		.087		-.131
		(.187)		(.351)		(.371)		(.416)		(.26)

Panel B: Investment

	gr(capital)									
	Leverage		Rajan-Zingales		Bank dep.		Size		Y/K	
	Low	High	Low	High	Low	High	Low	High	Low	High
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
FirmExposure	-0.302***	-0.536***	0.022	-0.461***	0.425	-0.466***	-0.434	-0.341***	-0.372***	-1.072***
	(0.104)	(0.108)	(0.302)	(0.077)	(0.435)	(0.076)	(0.374)	(0.082)	(0.079)	(0.284)
Controls	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Municipality×Ind.×Time FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Observations	148,790	521,462	148,482	573,502	138,110	585,947	33,336	719,968	573,964	156,506
R-squared	0.52	0.47	0.57	0.44	0.59	0.45	0.53	0.45	0.48	0.48
Credit-to-inv. IV	.187**	.314***	.056	.308***	-.203	.314***	.1	.272***	.257***	.522**
	(.074)	(.086)	(.094)	(.065)	(.147)	(.067)	(.092)	(.059)	(.057)	(.218)
High *minus* Low (RF)		-.234		-.484		-.892**		.093		-.701***
		(.153)		(.307)		(.432)		(.371)		(.267)
High *minus* Low (IV)		.127		.253**		.517***		.172*		.265
		(.124)		(.111)		(.169)		(.092)		(.19)

Note: This table reports the results of estimating specification (4) for subsamples defined by firms' characteristics. Outcome variables are the firm-level mid-point growth rate of credit and the growth rate of fixed assets. The main independent variable is firm exposure to crowding out (defined in (5)). High leverage is defined as firms with leverage above the first quartile. High Rajan-Zingales is a dummy equal to 1 if the firm's Rajan-Zingales index is above the first quartile. The Rajan-Zingales index is defined as capex minus cash flow from operations divided by capex. High Bank Dep. is a dummy equal to 1 if the share of bank debt in total debt is above the first quartile. High Size is a dummy equal to 1 if the firm size classification is above "SME". High Y/K is a dummy equal to 1 if Y/K is in the top quartile. The line labeled Credit-to-inv. IV shows the credit-to-input sensitivity by subsamples. The lines High *minus* Low report the coefficient on the interaction term and its standard error. Controls include the firm-level average of the bank-specific controls, the estimated firm-level credit demand shock, the firms' assets (log), leverage, ROA, cash flow from operations/assets, and capex/sales ratios (all lagged). Regressions are weighted by firm-level mid-point credit (top 0.5% winsorized). Standard errors are double-clustered at the main bank and municipality level. ***, ** and * indicate significance at the 1%, 5% and 10% levels, respectively.

7. Aggregate effects

Thus far, I have documented *relative* crowding out effects: increases in local government loans at one bank reduce that bank's corporate credit relative to other banks, and adversely affect investment at firms borrowing from this bank relative to other firms. However, these cross-sectional relationships do not yield the crowding out effect on *aggregate* corporate credit, investment, and ultimately output. In particular, they miss any equilibrium effect of crowding out affecting all banks and all firms. This is the so-called "missing intercept". This section develops a framework to bridge this gap.

The counterfactual of interest is a situation where the path of local government spending and debt is unchanged, but local government debt is financed by an outside investor with a perfectly-elastic supply of funds.[35] All other effects of local government debt are kept constant, but local government debt does not crowd out corporate credit. In this counterfactual, $BankExposure_{bt}$ is zero for all banks b (but the firm-level demand shocks, which may be affected by local government debt, are kept constant). I use the potential outcomes notation $X(\mathbf{0})$ to denote the counterfactual value of variable X.

7.1. Model

I only sketch the relevant parts of the model here, a full description can be found in Appendix D. The model contains four sectors: households supply labor and save in the form of bank deposits; firms produce using capital and labor, capital being financed by bank loans; local governments borrow from banks; and banks are funded via deposits and lend to firms and local governments. There is a continuum of banks of mass 1, indexed by $b \in [0, 1]$. In the baseline version, banking relationships enter the model through the assumption that firms and local governments are assigned to a given bank. Imperfect capital mobility across banks enters the model through the assumption that depositors do not arbitrage across banks. An interbank market can be accessed at a cost. All agents are price-takers.[36]

The production side of the economy is composed of monopolistically competitive intermediate input firms indexed by $b \in [0, 1]$ (bank from which the firm borrows) and $f \in [0, 1]$ (firms borrowing from a bank). A competitive final good producer aggregates intermediate

[35] A more concrete counterfactual is government debt financed by foreign agents (Broner et al. (2021)). Note that for the only difference to be the financing of government debt—i.e. to prevent simultaneous changes in the allocation of savings at home vs. abroad—one needs to assume some form of international capital markets segmentation.

[36] The model is homothetic to having depositors partly arbitrage across banks. I also consider firms substituting across banks and the key results are unchanged (extension D.4.1).

inputs via a CES function $Y = \left(\int_0^1 \int_0^1 Y_{fb}^{\frac{\sigma-1}{\sigma}} dfdb \right)^{\frac{\sigma}{\sigma-1}}$. Each intermediate input firm produces output using a Cobb-Douglas production technology $Y_{fb} = e^{z_{fb}} K_{fb}^{\alpha} L_{fb}^{1-\alpha}$. Intermediate input firms finance their stock of capital using equity $\overline{E}$ and bank loans C_{fb}: $K_{fb} = C_{fb} + \overline{E}$. Solving the firm's problem yields a demand curve for capital for firm f borrowing from bank b.

$$\log(C_{fb} + \overline{E}) = \bar{c} + (\sigma - 1)z_{fb} + \log(Y) - (1-\alpha)(\sigma-1)\log(w) - (1+\alpha(\sigma-1))\log(r_b^c) \tag{6}$$

where $\bar{c}$ is a constant. This implicitly define a corporate credit demand curve, with an elasticity of demand ϵ^c that depends on the production function parameters and on the equity-capital ratio.

Local governments have downward-slopping isoelastic credit demand curves with elasticity ϵ^g, that can be shifted by demand shocks. This yields a bank-level local government credit demand function: $\log(C_b^{gov}) = Z_b^{gov} + \epsilon^g \log(r_b^g)$.

There is a representative household depositing their savings at each bank. To keep the model static, I assume a reduced-form deposit supply function: $\log(S_b) = \epsilon^s \log(r_b^s)$ with $\epsilon^s \geq 0$. In addition, each household supplies undifferentiated labor with a Frisch elasticity of labor supply ψ, so that $\log(L) = \psi \log(w)$.

Each bank maximizes the proceeds of lending minus the cost of funds:

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

subject to a funding constraint: $C_b^{corp} + C_b^{gov} = S_b + B_b$. B_b is net interbank borrowing. r_b^c, r_b^g, r_b^s, and i are the interest rates on the credit markets, the deposit market and the interbank market. ϕ indexes the degree of interbank frictions.

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

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

7.2. Aggregate shortfall in corporate credit and investment

7.2.1. Cross-sectional vs. aggregate crowding out effect

The model allows to derive the effect of a local government debt demand shock on corporate credit, at the aggregate level and at the bank×firm level. At the aggregate level

$$\hat{C}^{corp} = (1+\kappa^{GE})\chi\lambda Z^{gov}$$

$\chi < 0$ is the direct crowding out effect. It captures the extent of the increase in the interest rate following the demand shock, and the extent of the resulting fall in corporate credit. It only depends on the elasticities of deposit supply and credit demand, and is equal to $\frac{\epsilon^c}{\epsilon^s-\epsilon^c}$ in the simplest case where $\epsilon^c = \epsilon^g$ and $\overline{E} = 0$. Crowding out is less severe when the supply of funds is more elastic, and more severe when corporate credit demand is more elastic. κ^{GE} captures general equilibrium feedbacks. It depends on σ, ψ and α and can be positive or negative. Writing the same equation at the bank firm-level, we obtain:

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

$\nu \in [0,1]$ indexes the degree of segmentation across banks. It is monotonically increasing in ϕ, $\nu = 0$ when $\phi = 0$ (perfect integration) and $\nu = 1$ when $\phi \to +\infty$ (full segmentation). At the bank firm-level, the direct crowding out effect is now split into two terms. The second term depends on the aggregate change in local government loans, while the last term depends on the bank-specific increase in local government loans.

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

By the same logic, when segmentation is not perfect ($\nu < 1$), the pressure on rates related to an increased demand for local government debt at one bank is partly transmitted to other banks through the interbank market, so that non-exposed banks also reduce corporate credit. Because of this spillover effect across banks, each bank's corporate credit supply is negatively affected by the aggregate amount of local government loans.

Link with the empirical specification. To link the static model to the panel setting of the main text, I assimilate log-deviations from the deterministic equilibrium to growth rates and λZ_b^{gov} to $BankExposure_b$.[37] Re-writing the model equations using the notations of my empirical specifications, I obtain:

$$\Delta C_{fbt} = \kappa^{GE}\chi BankExposure_t - \chi(1-\nu)BankExposure_t - \chi\nu BankExposure_{bt}$$

This is the theoretical counterpart to my firm×bank-level empirical specification (1). The coefficient that I identify in this analysis is the relative crowding out parameter $\chi\nu$. Therefore, obtaining the aggregate corporate credit shortfall requires that I estimate $\chi(1-\nu)$ and $\kappa^{GE}\chi$. The same logic applies to investment.

7.2.2. Quantification of aggregate crowding out effect

The quantities of interest are the aggregate shortfalls in corporate credit, capital, and output due to crowding out. For corporate credit, we can define $\mathcal{L}(C_t^{corp}) = \frac{C_t^{corp} - C_t^{corp}(\mathbf{0})}{C_t^{corp}(\mathbf{0})}$: it is the difference between corporate credit given the realized local government debt demand shock and corporate credit in the counterfactual situation where *BankExposure* is equal to 0 at all banks.[38] From the model, $\mathcal{L}(C_t^{corp}) = (1+\kappa^{GE})\chi\lambda Z^{gov}$. The loss is similarly defined for other variables. To gauge the magnitude of these effects, the shortfalls can be translated into a euro for euro effect, comparable to government spending multipliers. For corporate credit, this corresponds to $m_t^C = \frac{C_t^{corp} - C_t^{corp}(\mathbf{0})}{C_t^{gov} - C_t^{gov}(\mathbf{0})}$.

Direct effect - Lower bound from cross-sectional estimates. A first exercise consists in using my cross-sectional estimates to estimate the corporate credit shortfall relative to a counterfactual where all firms borrow from banks with $BankExposure_{bt} = 0$.[39] I find a yearly corporate credit shortfall attributable to crowding out equal to 0.76% on average. This implies a multiplier m^C equal to –0.52. This is the empirical counterpart to the model quantity $\chi\nu\lambda Z_t^{gov}$. Because the cross-sectional effect captures only part of the direct effect, this quantity underestimates the total direct effect: $\chi\lambda Z_t^{gov} \leq \chi\nu\lambda Z_t^{gov}$.

[37] λZ_b^{gov} corresponds to the increase in local government loan demand divided by the bank total lending in the deterministic equilibrium, which corresponds to the normalization by total lending used to define $BankExposure_b$. Aggregate variables are defined accordingly.

[38] In the panel setting, *BankExposure* is equal to 0 at all banks implies that local government debt remains at its previous period value.

[39] It is equal to the cross-sectional coefficient multiplied by average exposure to crowding out. Computation details are in Appendix D.3.

I perform the same exercise for capital and find a shortfall equal to 0.21%. This translates into an output loss due reduced inputs equal to 0.06%, or equivalently, a multiplier m^Y equal to −0.16.

Direct effect - Estimating the spillover across banks. The aggregation based on cross-sectional estimates misses the spillover effect due to capital flows across banks. This spillover is negative. The size of this spillover 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 higher than average demand shocks should borrow from other banks on the interbank market, with an elasticity equal to $1 - \nu$. I perform this estimation using bank-level data on interbank borrowing and lending. Appendix D.3 details the identification strategy and the results. In line with the prediction of the model, banks exposed to a higher demand shock borrow from other banks on the interbank market. I estimate $1 - \nu$ to be equal to 0.17. Since all the cross-sectional effects scale with ν, the lower bounds underestimate the direct effect by 17%.

General equilibrium feedback. Finally, the general equilibrium feedback κ^{GE} introduces a wedge between the direct effect χ and the total effect. General equilibrium analysis suggests opposing channels that may also lead firms borrowing from non-exposed banks to adjust their inputs. First, the credit shock generates an increase in the cost of capital for firms borrowing from exposed banks, so that the relative price of goods produced by these firms increases, triggering a reallocation of demand toward firms borrowing from non-exposed banks. This dampens the direct effect. The magnitude of this effect depends on the substitutability of goods produced by different firms σ. Second, the shock generates a reduction in the wage, which reduces labor supply for all firms, in proportion to the labor supply elasticity ψ. Table A19 in Appendix D.3 calibrates the general equilibrium feedback for credit, investment, and output as a function of parameter values. For plausible parameter values, the general equilibrium effects either magnify the direct effect or have at most a modest attenuating effect. To remain as close as possible to the spirit of the empirical exercise and to avoid introducing additional uncertainty related to calibrated parameters' values, I thus use the conservative approximation $\kappa^{GE} \approx 0$ and use my estimates of the direct effect χ as the total effect.

This analysis implies that the aggregate corporate credit loss due to crowding out is equal to on average 0.91%, or equivalently, €1 of local government loans crowds out €0.63 of corporate credit. The capital shortfall is equal to 0.26%. The aggregate output loss from the reduction in input usage is equal to 0.08%, or equivalently, €1 of local government loans crowds out €0.20 of corporate output. The multipliers are summarized in Table 6.

Figure 7 plots the time series of the output loss. The output loss is highest at the beginning of the sample when local government debt growth was the highest, and turns negative in 2016 and 2017 when local government debt recedes.

Appendix D.3 reports robustness checks for the aggregate effects computations. Appendix D.4 discusses extensions of the baseline model and shows they do not affect the key aggregation results.

TABLE 6. Aggregate effects

	Multiplier
Aggregate credit & capital	
Corporate credit	-0.62
Capital	-0.36
Aggregate output	
Input usage channel (A)	-0.20
TFP channel (B)	-0.05
Output (A+B)	-0.25

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

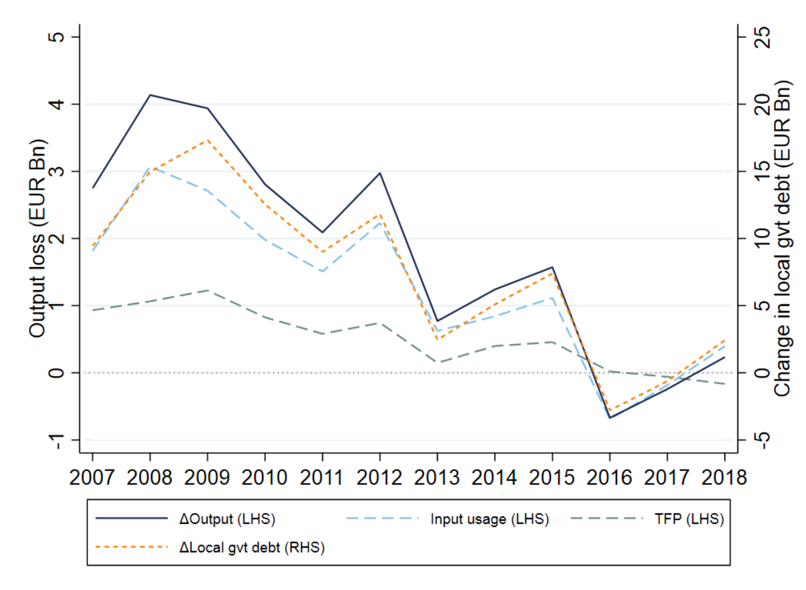
7.3. Crowding out and input misallocation?

The reduced-form results presented above show that crowding out affects the distribution of investment across firms. This implies that—with segmented financial intermediaries and heterogeneous firms—crowding out may also affect aggregate output through a change in allocative efficiency. I now quantify this effect. Details are in Appendix E.

Crowding out and allocative efficiency I follow the standard practice in the literature and model misallocation as wedges on the prices of inputs. The allocative price of capital paid by firm f is denoted $r(1+\tau_f^K)$. In the model, the dispersion in τ_f^K comes from dispersion in interest rates across firms borrowing from different banks. More generally, the wedges correspond to frictions, such as distortionary regulation or taxation, financial constraints, or imperfect competition, that distort actual or shadow input prices. With this notation, we can write first-order conditions for firms' marginal revenue products of capital: $\text{MRPK}_{ft} = \alpha \frac{P_f Y_f}{K_f} = R_t(1+\tau_f^K)$. Hsieh and Klenow (2009) show that aggregate productivity is a function of the dispersion in wedges:

$$\log(\text{TFP}) = \log(\text{TFP}^*) - \frac{\alpha}{2}(1+\alpha(\sigma-1))\,\text{Var}(\tau_f^K) \tag{7}$$

FIGURE 7. Aggregate effects



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

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; consequently, there are large gains from reallocating inputs away from firms with low marginal products toward firms with high marginal products.

Firm exposure to the credit supply shock generated by crowding out constitutes a shock to the wedges.[40] Heterogeneous cross-sectional exposure to crowding out may thus imply a change in allocative efficiency. Let us define the TFP loss due to crowding out as $\mathcal{L}(\mathrm{TFP}_t) = \log(\mathrm{TFP}_t) - \log(\mathrm{TFP}_t(\mathbf{0}))$.

Reduced-form effect of crowding out on wedges Quantifying the TFP loss requires estimates of the counterfactual wedges $\tau^K_{ft}(\mathbf{0})$. To do so, I follow Bau and Matray (2023) and estimate the effect of *FirmExposure* on wedges using the specification for firm-level inputs (equation (4)) with the change in wedges $\Delta\tau^K_{ft}$ as dependent variable, allowing for heterogeneity by ex-ante wedge. The results are reported in Table 7. Columns (1) and (2) show that firms' exposure to the credit supply shock generated by crowding out generates a significant increase in the capital wedge, in line with the idea that wedges are partly

[40] In considering a shock to financing conditions as a shock to wedges, I follow Larrain and Stumpner (2017) and Blattner, Farinha, and Rebelo (2020). The observed reduction in firms' input usage (Table 4) is to be understood as the reaction to this shock to wedges.

driven by credit frictions. Columns (3) to (6) investigate heterogeneous effect as a function of the ex-ante wedge. I define "low wedge"-unconstrained firms as firms with a capital wedge below the 25th percentile of the within industry distribution. The results show that the credit supply shock corresponds to a larger increase in wedges for firms with higher ex-ante wedges. This differential effect is not driven by the fact that banks cut credit to a larger extent to high-wedge firms. Rather, a given tightening of credit represents an increase in the cost of acquiring capital that is larger for firms that are more constrained. Therefore, investment drops by a larger amount for firms with higher ex-ante marginal products of capital. This corroborates the findings of Table 5, which showed that more constrained firms have higher credit-to-investment sensitivities.

TABLE 7. Effect on firm-level wedges

	gr(credit)	Wedge $\Delta\tau^K_{ft}$	gr(credit)		Wedge $\Delta\tau^K_{ft}$	
	Full	Full	Low	High	Low	High
	(1)	(2)	(3)	(4)	(5)	(6)
FirmExposure	-1.064***	0.525***	-0.964***	-1.075***	0.356**	0.715***
	(0.259)	(0.118)	(0.233)	(0.294)	(0.166)	(0.112)
Controls	✓	✓	✓	✓	✓	✓
Municipality×Industry×Time FE	✓	✓	✓	✓	✓	✓
Observations	798,769	783,649	152,070	582,456	147,173	574,780
R-squared	0.95	0.43	0.94	0.96	0.47	0.45
Credit-to-MRPK IV		-.325***			-.264**	-.432***
		(.093)			(.107)	(.154)
High *minus* Low (RF)				-.11		.36*
				(.222)		(.189)
High *minus* Low (IV)						-.168
						(.151)

Note: This table examines the crowding out effect of local government debt on corporate credit and the capital wedge. It reports the results of estimating specification (4). The outcome variables are the firm-level mid-point growth rate of credit and the changes in the capital wedge, as defined in the main text. Details on the construction of wedges can be found in Appendix E. The main independent variable is firm exposure to crowding out (defined in (5)). In columns (3) to (6), the sample is splitted along a dummy equal to 1 if the ex-ante capital wedge is above the first quartile. The line labeled IV shows the credit-to-input sensitivities by subsamples, obtained by instrumenting firm-level credit growth by *FirmExposure*. The lines High-Low report the coefficient on the interaction term and its standard error. Controls include the firm-level average of the bank-specific controls, the firms' assets, leverage, cash flow-to-assets, capex-to-sales ratios, ROA, and estimated firm-level credit demand shock. Regressions are weighted by firm-level mid-point credit (top 0.5% winsorized). Standard errors are clustered at the main bank and municipality level. ***, ** and * indicate significance at the 1%, 5% and 10% levels, respectively.

Aggregate TFP loss due to crowding out Using the results of Table 7, I obtain the predicted wedge given actual exposure to crowding out and the counterfactual wedge in absence of crowding out. I then use equation (7) to obtain the TFP loss due to crowding out. Computation details are in Appendix E.

I find that the misallocation effect of crowding out reduces aggregate TFP, and thus output, by 0.03% per year on average. The time series of the output loss is depicted on

Figure 7. This effect is not linear in the change in local government debt but depends on the distribution of exposure to crowding out across banks and firms. Over the sample period, the output loss corresponds to a multiplier m^Y equal to –0.05. These results are summarized in Table 6.

In Appendix E.3, I decompose the TFP loss into two terms: the misallocation effect due to heterogeneous credit supply shocks across firms, and the misallocation effect due to the heterogeneous effects of a uniform credit shock. I find that the increase in misallocation is entirely driven by the latter. Because high-MRPK firms have investment more sensitive to a given credit cut, a uniform credit shock can have a large misallocation effect. In contrast, the fact that banks are segmented, and so firms are exposed to heterogeneous credit supply shocks, is negligible.

7.4. Discussion

Crowding out and multipliers of local government spending. Aggregating these results, I find that an additional €1 in local government loans reduces aggregate output by €0.25 via crowding out. Debt-financed multipliers are notoriously hard to estimate, but a reasonable range is 0.5-1.9 (Ramey (2019)). My results imply that these multipliers would be higher by 0.25 in the absence of crowding out, a quantitatively significant effect.

The existence of substantial crowding out effects shows that the source of financing matters when interpreting local government spending multipliers. In particular, an active strand of the fiscal multipliers literature exploits geographic variation in transfer-financed government spending to estimate relative multipliers across locations. My results suggest that debt-financed multipliers may be substantially smaller than the transfer-financed multipliers. While one must be cautious when comparing estimates relying on different sources of variation, estimates of debt-financed multipliers (ranging from 0.5 to 1.9) tend to be lower than estimates of transfer-financed multipliers (ranging from 0.8 to 4), in line with this reasoning (Ramey (2019) and Chodorow-Reich (2019)).

External validity. My results have the greatest external validity for other countries where local governments heavily rely on bank debt. As shown on Figure A1, 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, in line with theory, the output loss due to crowding out reflects the elasticity of the supply of loanable funds. Confirming this prediction allows to extrapolate for the plausible magnitude of crowding out in other markets. For instance, we may think that the elasticity of the supply of loanable funds is higher in the case of government bonds: these bonds are traded on international capital markets with a deeper supply and held by agents not subject to bank regulation. Then, crowding out would be less severe.

A specific case is when local or central government bonds are acquired by banks. This is notably frequent in the U.S. municipal bonds markets, as documented in Dagostino (2018). In this case, similar crowding out effects can be expected.

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

8. Conclusion

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

I first document relative crowding out effects across banks, and then firms. I show that a larger increase in demand for local government debt at one bank disproportionately reduces that bank's corporate credit supply, with real effects on investment for its borrowers. My identification strategy isolates the crowding out channel operating through a reduction in credit supply, holding constant any other effect that local government debt may have on the real economy. In a second step, I build a simple model that shows how these relative effects implied by bank segmentation feed into aggregate effects. I quantify that an additional €1 in local government loans reduces aggregate output by €0.20 via the crowding out-induced reduction in investment. I also show that crowding out reduces allocative efficiency which leads to a €0.05 output loss per euro of local government loans.

Aggregating these results, crowding out reduces the output multiplier of debt-financed local government spending by 0.25. This is a large effect. The severity of crowding out reflects banks' limited ability to increase credit supply when faced with a demand shock.

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

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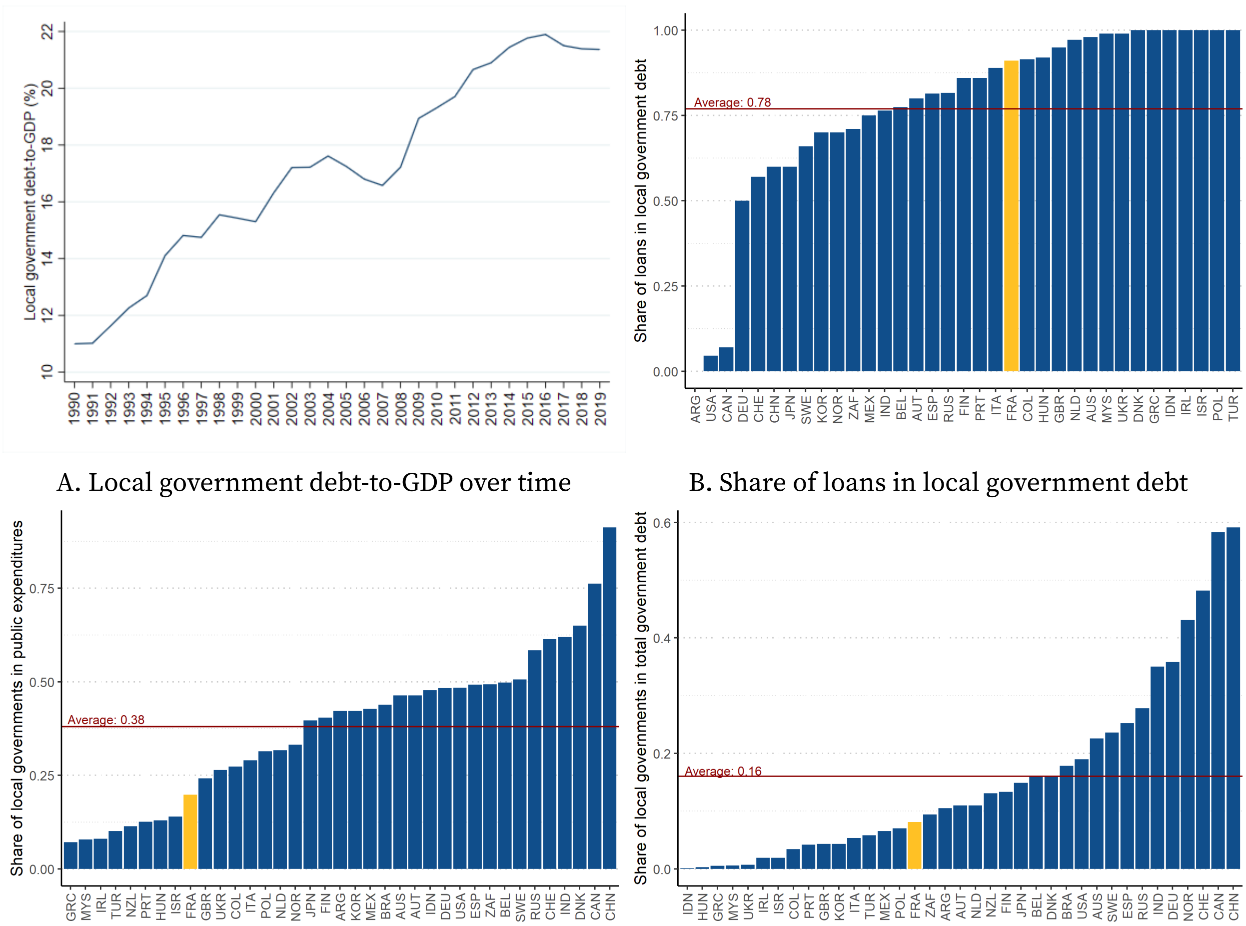
Appendix A. Additional tables and figures



A. Local government debt-to-GDP over time

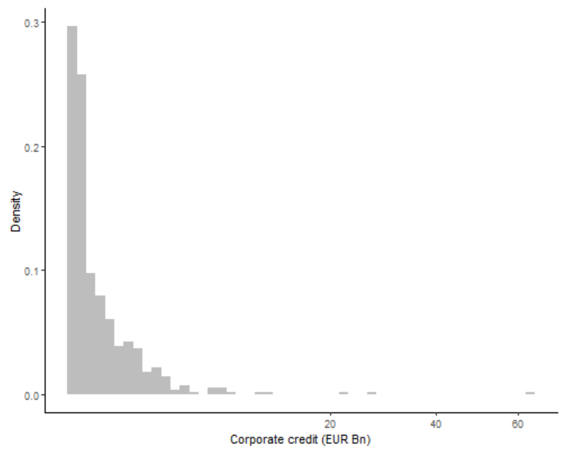
B. Share of loans in local government debt

C. Share of local governments in public expenditures

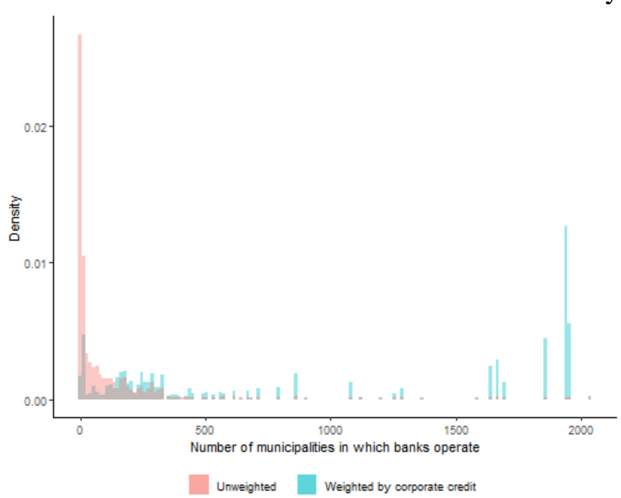
D. Share of local in total government debt

FIGURE A1. Local government debt in large developed and developing economies

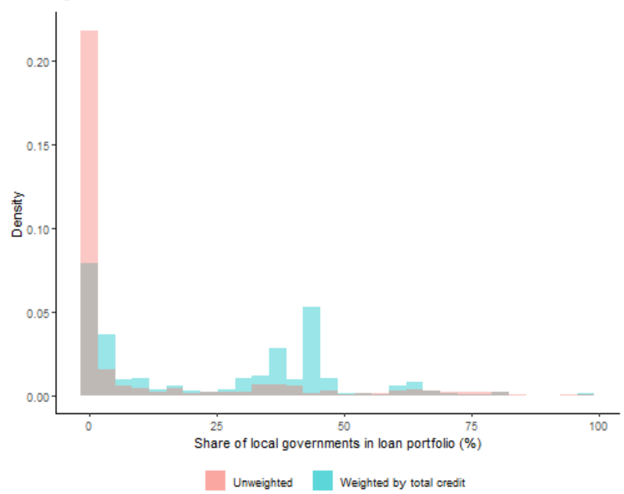
Note: Subfigure (a) shows the average local government debt-to-GDP ratio over time. Subfigure (b) shows the share of loans in local government debt in 2016. Subfigure (c) shows the share of local governments in total government expenditures. Subfigure (d) shows the share of local governments in total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



A. Distribution by loan portfolio size



B. Distribution by number of municipalities

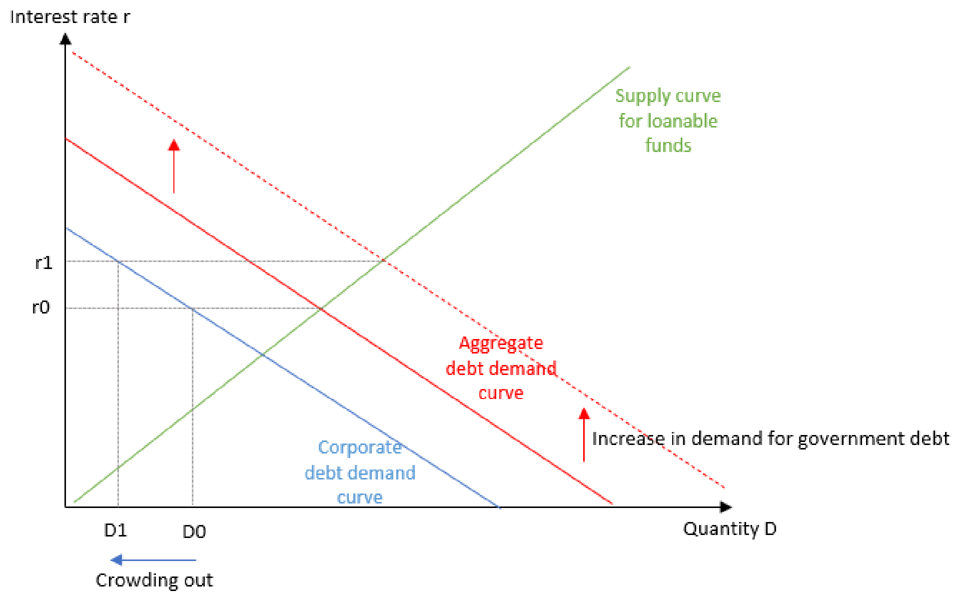


C. Distribution by local government loan share

FIGURE A2. Population of French banks

Note: Panel (a) shows the distribution of bank size, as defined by banks' corporate credit portfolios. Panel (b) shows the distribution across banks of the number of municipalities in which a bank operates. Panel (c) shows the distribution across banks of the share of local government loans in their total portfolio (local governments and corporates combined). Panels (b) and (c) show distributions unweighted and weighted by corporate credit volume.

FIGURE A3. Crowding out: simple supply and demand graph



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

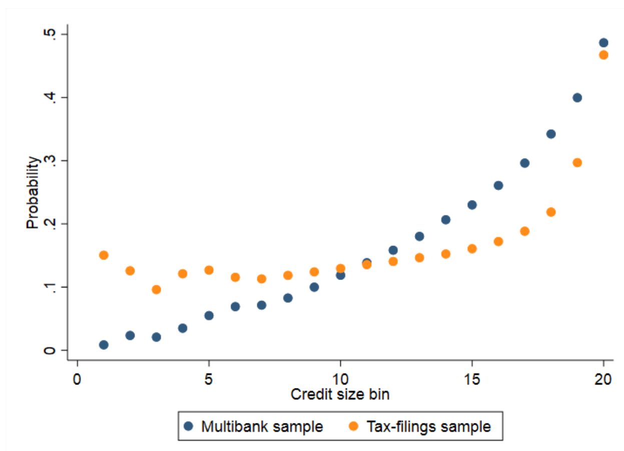


FIGURE A4. Sample description

Note: This figure describes the selection effect of considering the multibank sample or the tax-filings sample. Starting from the universe of firms in the credit registry, I define 20 equally-sized bins based on firms' total outstanding credit. For each bin, then estimate the probability that the firm is in the multibank sample (blue dots) or the tax-filing sample (orange dot).

TABLE A1. Regression of credit flows on firm and bank fixed effects

	Credit growth (baseline)			Credit growth (all credit types)		
	(1)	(2)	(3)	(4)	(5)	(6)
Constant	0.035***	0.042***	0.035***	0.029***	0.034***	0.029***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Firm×Time FE	✓		✓	✓		✓
Bank×Time FE		✓	✓		✓	✓
Observations	3,576,948	10,989,900	3,576,458	8,327,897	16,260,942	8,327,515
R-squared	0.58	0.039	0.62	0.47	0.040	0.51
Adj. R-squared	0.28	0.039	0.34	0.19	0.039	0.24

Note: This table reports the results of the regression of the firm×bank mid-point growth rate of credit on firm×time and bank×time fixed effects. In columns (1)-(3), credit is term loans with initial maturity above 1 year (as used in my baseline sample). In columns (4)-(6), credit is all credit (drawn and undrawn, and including leasing contracts). All regressions are weighted by the denominator of the mid-point growth rate.

TABLE A2. Crowding out effect: asymmetry and time series variation

	Credit growth			
	(1)	(2)	(3)	(4)
BankExposure	-0.850**	-0.770	-1.080**	-0.849*
	(0.346)	(0.931)	(0.546)	(0.464)
Controls	✓	✓	✓	✓
Firm×Time FE	✓	✓	✓	✓
Sample	Positive	Negative	Pre-2013	Post-2013
Observations	2,445,052	299,545	1,460,456	1,284,141
R-squared	0.53	0.57	0.55	0.51

Note: This table reports the results of estimating equation (1) for various subsamples. In columns (1) and (2), I split the sample based on the sign of *BankExposure*. To avoid breaking-up multibank firms, I compute the maximum value of *BankExposure* for each firm×time, and define Positive/Negative based on this value. In columns (3) and (4), I split the sample between 2007-2013 and 2014-2018. The outcome variable is the bank×firm-level mid-point growth rate of credit. The main independent variable is bank exposure to local government debt demand shocks (defined in (3)). Controls include the bank's lagged local government loan share, assets (log), equity ratio, and dummies for state-owned and foreign banks. Regressions are weighted by bank×firm-level mid-point credit (top 0.5% winsorized). Standard errors are double-clustered at the bank and municipality level. ***, ** and * indicate significance at the 1%, 5% and 10% levels, respectively.

TABLE A3. Firm×bank-level effect on credit: tax-filings subsample

	Credit growth					
	(1)	(2)	(3)	(4)	(5)	(6)
BankExposure	-0.294	-0.850***	-0.929***	-0.401*	-0.956***	-1.040***
	(0.213)	(0.309)	(0.299)	(0.216)	(0.312)	(0.302)
Controls	–	–	✓	–	–	✓
Firm×Time FE	–	✓	✓	–	✓	✓
Observations	1,011,904	1,011,904	1,008,710	1,011,904	1,011,904	1,008,710
R-squared	0.000046	0.51	0.51	0.000056	0.52	0.53

This table reports the results of estimating equation (1) on the tax-filings subsample.The outcome variable is the bank×firm-level mid-point growth rate of credit. The main independent variable is bank exposure to local government debt demand shocks (defined in (3)). Controls include the bank's lagged local government loan share, assets (log), equity ratio, and dummies for state-owned and foreign banks. Regressions are weighted by bank×firm-level mid-point credit (top 0.5% winsorized). In columns (3)-(6), the weight is adjusted for the probability that a firm belongs to the multibank sample (details in main text). Standard errors are double-clustered at the bank and municipality level. ***, ** and * indicate significance at the 1%, 5% and 10% levels, respectively.

Appendix B. Identification with the shift-share instrument

To guide the discussion on identification, it is useful to repeat the structural equation obtained from the model (equation (A28)):

$$\Delta C_{fbt} = z_{ft} + \chi(1+\kappa^{GE} - \nu)BankExposure_t + \chi\nu BankExposure_{bt} + \iota\xi_{bt} \tag{A1}$$

Firm-bank equilibrium credit growth depends on firm-specific shocks z_{ft}, the bank-specific local government debt demand shock $BankExposure_{bt}$, other bank-specific credit supply shocks ξ_{bt}, and a time-varying term depending on $BankExposure_t$ capturing the equilibrium effects of crowding out.

The empirical specification that I estimate (equation (1)) is:

$$\Delta C_{fbt} = d_{ft} + \beta BankExposure_{bt} + \varepsilon_{fbt} \tag{A2}$$

Equation (A1) immediately highlights the two identification challenges: correlated firm-level credit demand shocks and correlated bank-level credit supply shocks. I circumvent the former by including firm×time fixed effects d_{ft} in the specification. ε_{fbt} is by construction orthogonal to the firm-level fixed effects, hence it captures the firm×*bank*-specific unobservable determinants of credit flows, in particular due to bank-specific supply shocks (ξ_{bt} in equation (A1)). The key concern therefore remains a correlation between Bank Exposure and bank-specific corporate credit supply shocks.

In what follows, I omit time subscripts to simplify notations. The standard exclusion restriction writes:

$$\mathbb{E}\left[\sum_m \omega_{bm}^{gov}\hat{\alpha}_m^{gov}\varepsilon_{fb}\middle| d_f\right] = 0 \tag{A3}$$

B.1. Identification based on shifters.

Condition (A3) is immediately satisfied if the shocks $\hat{\alpha}_m^{gov}$ are as good as random, but does not require it. The less restrictive requirement is that municipality-level shocks are uncorrelated with the average bank-level determinants of corporate credit for the banks most exposed to each municipality (Borusyak, Hull, and Jaravel, 2021). To see this, I follow these authors and write the full-data orthogonality condition. Since my specification includes firm×time fixed effects, I write the orthogonality condition in terms of deviations from the within-firm average, denoted with a tilde:

$$\mathbb{E}\left[\sum_m \hat{\alpha}_m^{gov}\left(\sum_{f,b} \tilde{\omega}_{bm}^{gov,f}\,\varepsilon_{fb}\right)\right] = 0 \tag{A4}$$

$\hat{\alpha}_m^{gov}$ must be orthogonal to the bank-specific shocks ε_{fb} aggregated using the (within-firm deviations in) exposures of banks to municipality m. Put differently, it must not be the case that banks experiencing negative bank-specific shocks ε_{fb} have systematically higher exposure to municipalities where $\hat{\alpha}_m^{gov}$ is high. One problematic example would be if (i) $\hat{\alpha}_m^{gov}$ were correlated to some variable X_m (e.g., deposits in m), (ii) X_m affects banks' ability to lend through the same exposure weights ω_{bm}^{gov} (e.g., local government debt weights are similar to deposit weights). In this case, *BankExposure* would be correlated with another bank-specific supply shock (e.g., bank-level deposits flows). A second problematic example is if corporate credit supply shocks hitting bank b systematically lead to higher local government debt demand α_m^{gov} in municipalities where bank b is located.

Sufficient condition for identification. A sufficient condition for identification is if municipality-level changes in local government debt are not correlated to other municipality-level variables. Figure A5 shows that $\hat{\alpha}_m^{gov}$ is not correlated with the lagged or contemporaneous municipality-level GDP growth, private credit growth, change in the number of banked firms or bankruptcy rate. This may appear surprising, as local government debt is endogenous to local outcomes. However, this relationship is unlikely to operate at the municipality level: municipalities are small and are not the relevant economic scale for stimulus spending effects, and there is high dispersion in α_{mt}^{gov} across neighboring municipalities (Figure 2). In addition, Figure A6 show that the $\hat{\alpha}_m^{gov}$ are not persistent, which reduces the risk of a correlation with persistent economic outcomes. The lumpiness is due to the fact that local government credit finances capital expenditures.

Necessary condition for identification While reassuring, these municipality-level orthogonality conditions are not necessary. What matters is that other municipality-level shocks do not generate bank-level shocks correlated to *BankExposure*. Several features of the shares support this assumption. First, I use shares specifically in the local government credit market. Any municipality-level shock emanating from corporates would affect banks via their exposure to the corporate credit market. Conversely, corporate credit supply shocks would affect municipality-level outcomes of municipalities with large corporate credit presence of affected banks. As a placebo test, I repeat the construction of

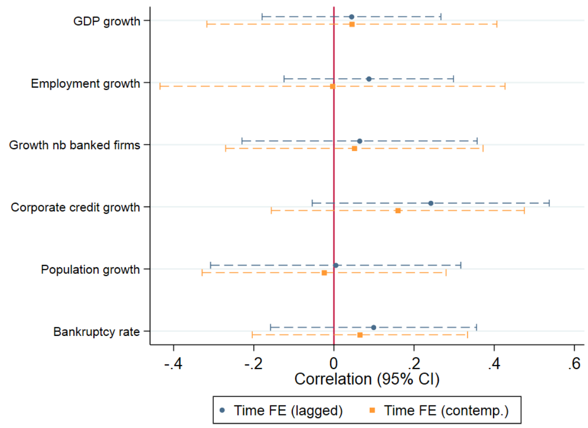


FIGURE A5. Municipality-level balance tests

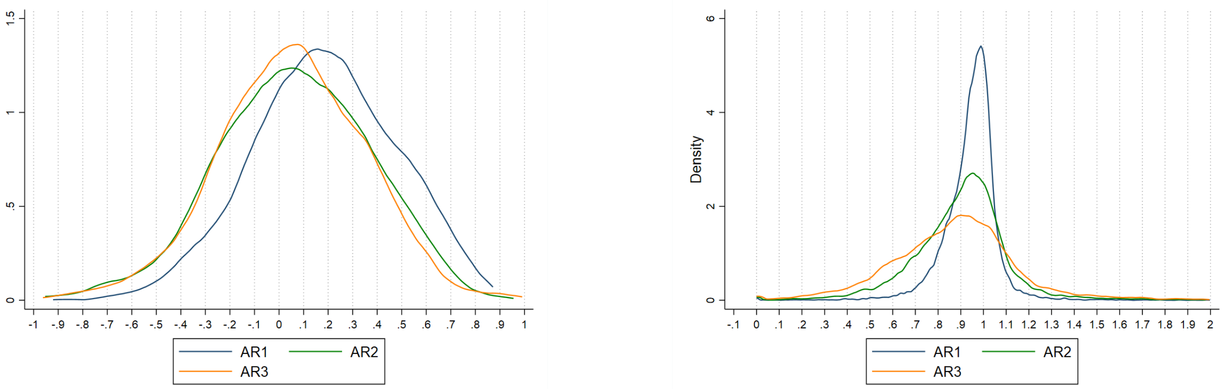
Note: This figure shows the coefficient of municipality-level regressions of local government debt demand shocks $\hat{\alpha}_m^{gov}$ on municipality-level variables. All regressions include time fixed effects. The blue dots correspond to correlations between $\hat{\alpha}_m^{gov}$ and lagged municipality characteristics. The orange dots correspond to correlations between $\hat{\alpha}_m^{gov}$ and contemporaneous municipality characteristics. As recommended by Borusyak, Hull, and Jaravel (2021), the regressions are weighted by $s_{mt} = \sum_b e_{bt} \omega_{bm,t-1}^{gov}$ where e_{bt} is the lagged corporate loan portfolio of each bank. Standard errors are clustered at the municipality level. The dot is the point estimate and the bar is the 95% confidence interval. All variables are standardized.

BankExposure with corporate credit exposure weights. Table A4 shows that this alternative variable does not predict a decline in corporate credit. This further alleviates concerns that *BankExposure* is picking up local shocks occurring on the corporate credit market and correlated to $\hat{\alpha}_m^{gov}$ that reduce banks' credit supply.[41] Second, the maps in Figure A7 show that for a given banks, shares are highly dispersed across municipalities. This high dispersion implies that the shares do not just capture banks' exposure to broad geographic areas, which could be correlated with other bank-level shocks. These maps make clear that some banks have higher market shares on average, which is controlled for by the sum of weights. Third, the autocorrelations in Figure A6 shows that shares are highly persistent. This rules out banks on declining corporate credit supply trends strategically increasing their shares in high local government debt demand municipalities in every period. As a further check, Table A4 shows that my results are virtually identical when I fix shares in 2006.

These additional tests allow to better understand the bank-level balance tests shown in Figure 4 and provide strong support to assumption (A3).

Bias due to measurement of demand shocks Finally, I address a measurement concern: I do not observe the underlying local government debt demand shock but instead use a

[41] 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.



A. Local government demand shocks

B. Bank×municipality's market shares

FIGURE A6. Autocorrelation of shifters and shares

Note: Panel (a) plots the kernel density of municipality-specific AR(1), AR(2), and AR(3) coefficients for municipality's local government debt demand shocks. Panel (b) plots the kernel density of bank×municipality-specific AR(1), AR(2), and AR(3) coefficients for bank×municipality's market shares.

proxy $\hat{\alpha}^{gov}_m$ estimated from realized local government-bank credit growth. In small samples, it may be contaminated by the supply shocks of the large banks in m, which also enter the residual of my bank-level regression.[42]

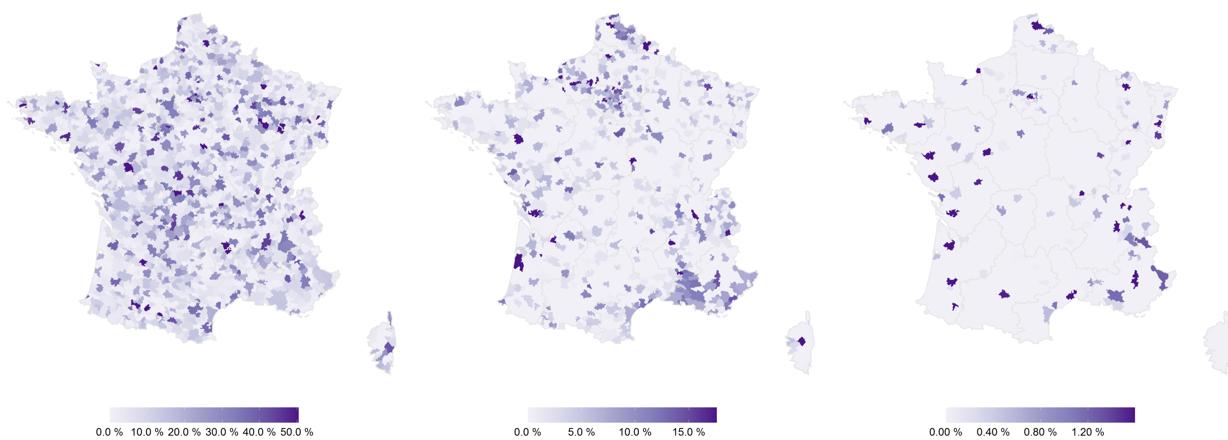
First, this concern is alleviated if the market shares of banks in municipalities are not too concentrated. In the case at hand, the average Herfindahl index is 0.17.

Second, I repeat the construction of $\hat{\alpha}^{gov}_m$ excluding banks with market shares higher than 40%. Table A4 shows that I obtain very similar results.

Third, the coefficient on the shift-share variable *BankExposure* would be biased towards the coefficient with its "realized" quantity equivalent as an explanatory variable. Define $\mathbb{d}C^{gov}_{bt} = \sum_m \omega^{gov}_{bm} \Delta C^{gov}_{bm} \approx \frac{C^{gov}_{bt} - C^{gov}_{bt-1}}{C^{tot}_{bt-1}}$ the "realized" quantity equivalent of my shift-share variable (ignoring the distinction between mid-point and standard growth rates). By construction, $\mathbb{d}C^{gov}_{bt} = \lambda^{gov}_{bt-1}\hat{\alpha}^{gov}_{bt} + BankExposure_{bt}$ (see footnote 19). If *BankExposure* is contaminated by supply factors $\hat{\alpha}^{gov}_{bt}$, this biases the coefficient on *BankExposure* in the direction of that on $\mathbb{d}C^{gov}_{bt}$.

Figure A8 depicts the relationships between $BankExposure_{bt}$, $\mathbb{d}C^{gov}_{bt}$ and ΔC_{fbt}. Panel (a) is the binned scatterplot equivalent of my baseline specification, and shows a negative relationship between $BankExposure_{bt}$ and ΔC_{fbt}. On the other hand, while $BankExposure_{bt}$ strongly predicts $\mathbb{d}C^{gov}_{bt}$ (panel (b)), the regression of ΔC_{fbt} on $\mathbb{d}C^{gov}_{bt}$ yields an *opposite* sign

[42]The fact that the dependent variable is not the same as the variable used to construct the shifters makes this issue less problematic than in the standard shift-share setting: α^{gov}_m may be contaminated by local government credit supply shocks while what mechanically enters the residual of my regression is corporate credit supply shocks.



A. Bank A B. Bank B C. Bank C

FIGURE A7. Municipality-level market shares by bank

Note: These maps depict municipality-level market shares in the market for local government loans for the three largest French banks (bank A, bank B, and bank C) in 2008.

(panel (c)). These considerations are robust to including estimated supply shocks $\hat{\alpha}_{bt}^{gov}$ and $\lambda_{bt-1}^{gov}\hat{\alpha}_{bt}^{gov}$ as controls. The positive bank-level correlation between local government and corporate credit displayed in panel C corresponds to the expected sign of the bias if banks are hit by shocks affecting their ability to lend to both segments, as clarified by equation A1.

Note that including firm$\times$time fixed effect is critical for the assumption (A3) to plausibly hold. Otherwise, this condition would write:

$$\mathbb{E}\left[\sum_m \hat{\alpha}_m^{gov}\left(\sum_f \bar{\omega}_{fm}^{gov} d_f + \sum_{f,b} \omega_{bm}^{gov}\varepsilon_{fb}\right)\right] = 0$$

where $\bar{\omega}_{fm}^{gov}$ is the sum of ω_{bm}^{gov} for the set of banks b lending to f. $\sum_f \bar{\omega}_{fm}^{gov} d_f$ is the weighted average corporate credit demand shock, where each firm f's shock is weighted by the average exposure to municipality m of banks lending to f. If the geographic footprints of banks in the local government and corporate credit markets are correlated, $\sum_f \bar{\omega}_{fm}^{gov} d_f$ will put a large weight on the corporate credit demand shocks of firms located in m. $\sum_f \bar{\omega}_{fm}^{gov} d_f$ is then likely to be correlated with $\hat{\alpha}_m^{gov}$. Hence, this condition is unlikely to hold.

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

TABLE A4. Additional robustness checks

	Credit growth					
	(1)	(2)	(3)	(4)	(5)	(6)
BankExposure	-0.836***	-0.728***	-0.872***	-0.197	-0.288*	-0.261
	(0.306)	(0.277)	(0.310)	(0.167)	(0.167)	(0.175)
Controls	✓	✓	✓	–	✓	✓
Firm×Time FE	✓	✓	✓	✓	✓	✓
Indep. var. def.	2006 shares	Excl. largest banks	Leave-one-out	Corporate shares placebo	Corporate shares placebo	Corporate shares placebo
Observations	2,709,023	2,731,110	2,710,202	2,744,597	2,731,110	2,582,698
R-squared	0.54	0.54	0.54	0.53	0.54	0.54

Note: This table explores robustness of the results in Table 2 to concerns related to the shift-share structure of the shock. The outcome variable is the bank×firm-level mid-point growth rate of credit. In column (1), I define *BankExposure* fixing shares in 2006. In column (2), the α_m^{gov} are estimated excluding bank observations corresponding to market shares larger than 40%. In column (3), I regress ΔC_{fb} on a leave-one-out version of $BankExposure_{bm(f)}$ which does not consider the shock of the municipality where the firm is located. In column (4)-(6), *BankExposure* is computed with weights $\omega_{bmt-1}^{corp} = C_{bt-1}^{corp}/C_{bt-1}^{tot}$. Column (6) restricts the sample to banks active in lending to local governments. Controls include the bank's lagged local government loan share, assets (log), equity ratio, and dummies for state-owned and foreign banks. Regressions are weighted by bank×firm-level mid-point credit (top 0.5% winsorized). Standard errors are double-clustered at the bank and municipality level. ***, ** and * indicate significance at the 1%, 5% and 10% levels, respectively.

number of shocks with sufficient shock-level variation, and (ii) that shocks exposure is not too concentrated.

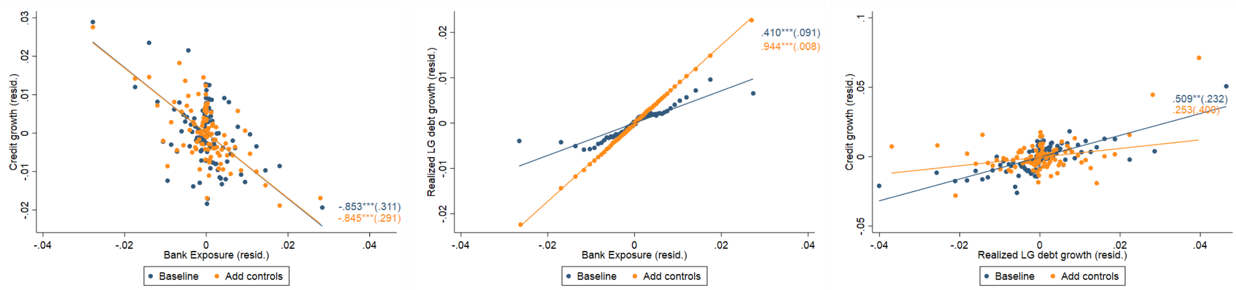
Panel A of Table A5 documents a large dispersion in $\hat{\alpha}_m^{gov}$, which persists when residualizing on time, region×time or municipality fixed effects. Besides, exposure shares are not too concentrated. Define municipality-level weights as $s_{mt} = \sum_b e_{bt}\omega_{bm,t-1}^{gov}$ where e_{bt} are bank-level corporate credit weights. Panel B shows that the largest weight is very small (0.6%) and the inverse Herfindahl index is large: 1,265. I report the same statistics when exposure weights are aggregated at the municipality-level, and there is sufficient municipality-level dispersion even when shocks are allowed to be serially correlated.[43]

B.2. Identification based on shares.

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

First, the variable used to define the shares, local government loans, is specific to the mechanism under study. This makes it less likely that shares are correlated to generic bank-level credit supply shocks.

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



A. ΔC_{fbt} on $BankExposure_{bt}$ B. $\text{đ}C^{gov}_{bt}$ on $BankExposure_{bt}$ C. $\text{đ}C^{gov}_{bt}$ on ΔC_{fbt}

FIGURE A8. Binned scatter plots of reduced form, first-stage and OLS regressions

Note: These figures present binned scatter plots corresponding to the regression of ΔC_{fbt} on $BankExposure_{bt}$ (panel A), $\text{đ}C^{gov}_{bt}$ on $BankExposure_{bt}$ (panel B) and $\text{đ}C^{gov}_{bt}$ on ΔC_{fbt} (panel C). I plot the binned scatterplots of the variables residualized on firm$\times$time fixed effects and controls. In the "baseline" specification, included controls are the baseline bank-level controls. In the "add controls" specification, additional controls are $\hat{\alpha}^{gov}_{bt}$ and $\lambda^{gov}_{bt-1}\hat{\alpha}^{gov}_{bt}$. Corresponding regression coefficients and standard errors are printed.

Second, there are many municipalities, so that the correlation between bank-level shocks and banks' exposure to any given municipality is likely small. I find that the municipality-level Rotemberg weights—which summarize the identifying variation used by the shift-share instrument—are very dispersed. The 5 largest Rotemberg weights account for roughly 27% of the positive weight in the estimator.[44,45] Dispersed Rotemberg weights reduce the sensitivity of the Bartik instrument to non-random exposure to a given municipality. On the other hand, it makes it harder to interpret the identifying variation. The fact that the intuition of the identification does not rely on comparing local government debt dynamics in a handful of "extreme" municipalities but instead relies on banks being exposed to a large number of municipalities justifies the favored interpretation of identification as coming for shocks.

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

[45]These 5 instruments are the municipalities of Rennes, Strasbourg, Angers, Rodez and Saint-Denis, five mid-size French municipalities located in different regions of France. Repeating the analysis at the municipality$\times$time-level shows that these highest weight municipalities vary across time.

Table A5. Shock-level summary statistics

Panel A: Summary statistics on municipality-level shocks

	count	mean	sd	p25	p50	p75
Municipality-level shock $\hat{\alpha}^{gov}_{mt}$	24,887	0.033	0.157	-0.040	0.023	0.098
Residualized on time FE	24,887	0.000	0.153	-0.072	-0.007	0.063
Residualized on region×time FE	24,887	0.000	0.145	-0.069	-0.010	0.058
Residualized on municipality FE	24,886	0.000	0.150	-0.071	-0.009	0.063

Panel B: Summary statistics on exposure shares

	Across municipalities and dates	Across municipalities
Inverse HHI	1,265	111
Largest weight	0.006	0.041

Note: This table presents descriptive statistics relevant for the shift-share design. Panel A presents summary statistics of the municipality-level shocks $\hat{\alpha}^{gov}_{m}$. Panel B presents summary statistics of municipality-level weights $s_{mt} = \sum_b e_{bt}\omega^{gov}_{bm,t-1}$. Weights are normalized to sum to 1 for the whole sample. I compute the municipality-level inverse Herfindahl index $1/\sum_{m,t} s^2_{mt}$ and the largest s_{mt} weight, and then the same quantities when weights are aggregated across time for a given municipality.

Appendix C. Additional details and robustness checks

C.1. Cross-sectional effects on credit

Euro-for-euro crowding out computation. I provide a back-of-the-envelope quantification of the euro-for-euro crowding out effect using the estimates in Table 2. I estimate the corporate credit shortfall due to crowding out. To do so, I use the estimated regression model (1) to obtain the difference between predicted corporate credit $\hat{C}^{corp}$ when the local government debt demand shocks α^{gov}_{mt} are equal to their true value and counterfactual corporate credit $C^{corp}(\mathbf{0})$ when the local government debt demand shocks α^{gov}_{mt} are equal to 0 (holding constant demand effects). I assume all variables are equal to their sample means, denoted with an upper bar. Computations taking into account the joint distribution of firm and bank size and exposure to crowding out are left to Section 7. Ignoring the distinction between mid-point and standard growth rates (which is innocuous for small growth rates), the corporate credit shortfall is equal to:

$$\hat{C}^{corp}_t - C^{corp}_t(\mathbf{0}) = \beta \times \overline{BankExposure}_{bt} \times \overline{C}^{corp}_{t-1} = \beta \times \overline{\hat{\alpha}}^{gov}_{mt} \times \frac{\overline{C}^{gov}_{t-1}}{\overline{C}^{tot}_{t-1}} \times \overline{C}^{corp}_{t-1} \tag{A5}$$

The corresponding increase in local government debt is

$$\hat{C}^{gov}_t - C^{gov}_t(\mathbf{0}) = \overline{\hat{\alpha}}^{gov}_{mt} \times \overline{C}^{gov}_{t-1} \tag{A6}$$

The euro-for-euro crowding out coefficient is given by $\frac{\hat{C}_t^{corp} - C_t^{corp}(\mathbf{0})}{\hat{C}_t^{gov} - C_t^{gov}(\mathbf{0})} = \beta \times \frac{\overline{C}_{t-1}^{corp}}{\overline{C}_{t-1}^{tot}} = 0.54$.

Distortions in the market for local government lending and crowding out. Table A6 shows that the crowding out coefficient does not vary along a number of proxies for political interference with banks. I first use the fact that state-owned banks are more exposed to political interference. Column (1) presents the results of estimating equation (1) excluding state-owned banks from the sample. I find point estimates that are highly similar to my main results. I then perform a test based on the premise that political interference is more likely (i) if local politicians are sufficiently powerful to exert coercion on banks, and/or (ii) when electoral incentives are strongest (e.g., politicians could coerce banks into lending to local governments before contested elections to fund public investment projects). I define *Powerful* and *Contested* dummies for two types of politicians: members of parliaments (MPs, *députés*), the most prominent local political figures, and mayors, who head *communes*, the largest borrower category within local governments. Details on variables definitions are in the table notes. I then compute bank exposure to political interference by taking a weighted mean of politicians' characteristics across municipalities (for mayors) or legislative constituencies (for MPs), with weights corresponding to the share of each location in the banks' local government loans. I then split banks based on the sample mean of this exposure variables. The results in columns (2)-(7) of Table A6 show that the crowding out coefficient is not driven by instances where political interference is likely potent.

Additional tests of identifying assumptions. Table A7 presents further tests that support the identifying assumptions of my main results, described in the main text.

Robustness checks. Table A8 shows the results when including additional controls and adding sample restrictions. Columns (1) and (2) report again the results of estimating equation (1), without and with baseline controls, respectively. Column (3) adds a set of bank-specific controls, including the bank's deposit ratio, share of non-performing loans, net interbank lending position, and a dummy equal to 1 if the bank is a cooperative bank. Column (4) controls for the bank×time fixed effects estimated in the Amiti-Weinstein decomposition (α_{bt}^{gov} as in equation (2)). This provides an estimate for any unobservable bank-specific credit supply shock. The point estimate remains precisely equal to my baseline coefficient. Column (5) restricts the sample to banks with total loan portfolio (corporates and local governments combined) above €50 millions. Column (6) drops banks

TABLE A6. Crowding out and political distortions in the market for local government loans

	Credit growth						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
BankExposure	-0.953***	-0.728**	-1.108**	-1.015***	-1.119***	-1.114***	-0.732**
	(0.327)	(0.313)	(0.458)	(0.389)	(0.291)	(0.395)	(0.317)
× High Powerful Exp		-0.662					
		(0.585)					
× High Contested Exp			0.398				
			(0.593)				
× High (Contested×Powerful) Exp				-0.136			
				(0.522)			
× High Powerful Exp					0.082		
					(0.663)		
× High Contested Exp						0.248	
						(0.582)	
× High (Contested×Powerful) Exp							-0.609
							(0.500)
Sample	Excl. state-owned	All	All	All	All	All	All
Controls×Dummy	✓	✓	✓	✓	✓	✓	✓
Firm×Time FE	✓	✓	✓	✓	✓	✓	✓
Dummy×Time FE		✓	✓	✓	✓	✓	✓
Observations	2,598,349	2,726,877	2,726,877	2,726,877	2,729,246	2,729,246	2,729,246
R-squared	0.53	0.54	0.54	0.54	0.54	0.54	0.54

Note: This table shows that the crowding out coefficient does not vary along a number of proxies for political pressure on banks. Column (1) repeats the main specification exluding state-owned banks. Columns (2)-(7) look at heterogeneity of the main coefficient by bank exposure to political interference, based on characteristics of local politicians. For MPs (mayors), *Powerful* is defined as a dummy equal to 1 if the politician has ever been a minister of the 5th Republic, a mayor (an MP), or has been in office at least three times. For both mayors and MPs, *Contested* is a dummy equal to 1 if the office was held by the other party prior to the politician'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. For mayors, I define these variables at the municipality (*EPCI*) level, taking the mayor of the largest *commune* in each *EPCI*: French communes are extremely fragmented—there are more than 36,000 communes, 95% of them with a population below 6000 inhabitants, with mayors that are often not professional politicians—so that the mayor of the largest nearby *commune* best corresponds to the notion of relevant local politicians. I aggregate *Powerful* and *Contested* at the bank level taking their weighed means across locations (municipalities for mayors or legislative constituencies for MPs) with weights corresponding to the lagged share of each location in the bank's local government loans. I then split banks along the median of this variable. "High X Exp" refers to high bank exposure to variable X. Standard errors are double-clustered at the bank and municipality level. ***, ** and * indicate significance at the 1%, 5% and 10% levels, respectively.

that are never active in local government lending. All these specifications provide very similar results.

In Figure A9, I further test the sensitivity of my results by showing a estimated coefficients for small changes compared to my baseline specification. Panel A displays estimated coefficients when I drop any of the 100 largest banks or any of the 100 largest municipalities from my estimating sample. Panel B shows coefficients estimated in regressions with each of the 8 available controls individually and 30 random draws of two to four controls within the set of all available controls, for three different fixed effects structure.

Table A9 shows results for alternative definitions of dependent and independent vari-

TABLE A7. Firm×bank-level effects: Tests of identifying assumptions

	Credit growth				
	(1)	(2)	(3)	(4)	(5)
BankExposure	-0.983***	-1.301***	-1.074***	-0.808***	-0.910***
	(0.315)	(0.294)	(0.314)	(0.298)	(0.318)
BankExposure× Pub. Proc.					0.248
					(0.486)
Controls	✓	✓	✓	✓	✓
Firm×Time FE	✓	✓	✓	✓	✓
Firm×Active bank×Time FE	✓	–	–	–	–
Bank FE	–	✓	–	–	–
Regional shares (pub)×Time FE	–	–	✓	–	–
Regional shares (all)×Time FE	–	–	–	✓	–
Observations	2,595,432	2,731,067	2,598,842	2,731,110	2,731,110
R-squared	0.54	0.54	0.54	0.54	0.54

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 (1). The outcome variable is the bank×firm-level mid-point growth rate of credit. The main independent variable is bank exposure to local government debt demand shocks (defined in (3)). Controls include the bank's lagged local government loan share, assets (log), equity ratio, and dummies for state-owned and foreign banks. In columns (3) and (4), I further interact time fixed effects with 22 variables corresponding to the share of each of the 22 French regions in the bank's loan portfolio (local governments only in column 3 or total portfolio in column 4). For column (5), Pub. Proc. is a dummy equal to 1 for the top 10 industries by public procurement contract revenues (data from *Données essentielles de la commande publique* available here). Regressions are weighted by the denominator of the bank×firm-level mid-point growth rate (top 0.5% winsorized). Standard errors are double-clustered at the bank and municipality level. ***, ** and * indicate significance at the 1%, 5% and 10% levels, respectively.

TABLE A8. Firm×bank-level effects: Robustness to additional controls and sample restrictions

	Credit growth					
	(1)	(2)	(3)	(4)	(5)	(6)
BankExposure	-0.753**	-0.853***	-0.793**	-0.845***	-0.902***	-0.983***
	(0.311)	(0.311)	(0.315)	(0.291)	(0.313)	(0.316)
Baseline controls	–	✓	✓	✓	✓	✓
Add. bank controls (1)	–	–	✓	–	–	–
Add. bank controls (2)	–	–	–	✓	–	–
Firm×Time FE	✓	✓	✓	✓	✓	✓
Sample	Full	Full	Full	Full	$\geq 50€M$	Active
Observations	2744597	2731110	2731110	2611795	2631988	2582698
R-squared	0.53	0.54	0.54	0.54	0.54	0.54

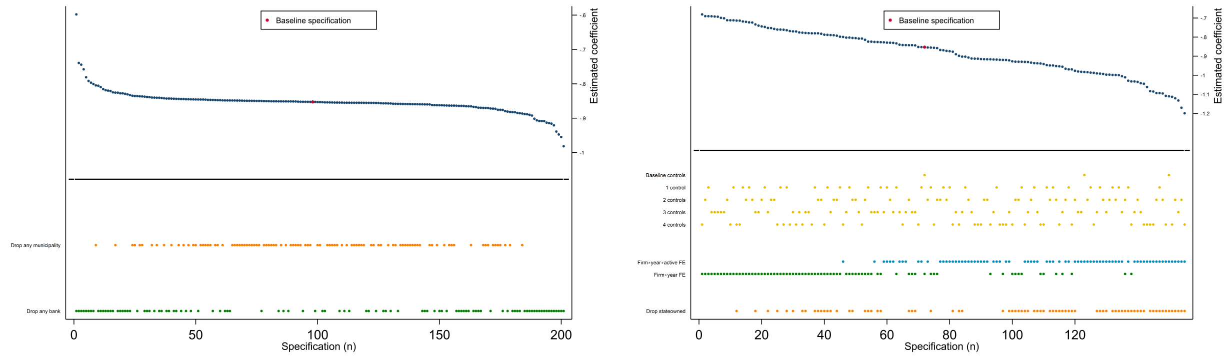
Note: This table presents robustness checks of the main results presented in Table 2. Columns (1) and (2) repeat baseline results for comparison. Column (3) adds a set of bank-specific controls, including the bank's deposit ratio, share of non-performing loans, net interbank lending position, and a dummy equal to 1 if the bank is a cooperative bank. Column (4) controls for the extracted bank×year fixed effects from the Amiti-Weinstein decomposition (α_{bt}^{gov} as in equation (2)). Column (5) restricts the sample to banks with total loan portfolio (corporates and local governments combined) above €50 millions. Column (6) drops banks that are never active in local government lending. Baseline controls include the bank's lagged local government loan share, assets (log), equity ratio, and dummies for state-owned and foreign banks. Regressions are weighted by bank×firm-level mid-point credit (top 0.5% winsorized). Standard errors are double-clustered at the bank and municipality level. ***, ** and * indicate significance at the 1%, 5% and 10% levels, respectively.

ables. Columns (1) to (3) report results when replacing the mid-point growth rate (MPGR) of credit granted to firm f by bank b with its positive truncation, the standard growth rate, and

FIGURE A9. Firm×bank-level effects: Specification curves

A. Dropping large banks or municipalities

B. Controls, fixed effects and sample



Note: This figure shows the coefficient obtained from estimating specification (1). The red dot is the baseline estimate, corresponding to column (3) in Table 2. In panel A, the blue dots correspond to the estimated coefficients when dropping any of the 100 largest banks or any of the 100 largest municipalities. In panel B, the blue dots correspond to the estimated coefficients in regressions with each of the 8 available controls individually and 30 random draws of two to four controls within the set of all available controls, for two different fixed effects structure, and with the baseline sample or the sample excluding state-owned banks. All coefficients are significant at the 5% level.

the normalized growth rate. All three specifications yield a negative and significant effect. The coefficient on the positive truncation of the MPGR shows that most of the effect comes from variation in credit growth, conditional on credit growth being positive. Positive credit growth can be considered as a proxy for firms taking on a new loan (while negative credit growth mostly corresponds to firms gradually repaying the principal of previous loans). This makes sense, as this is when banks have most leeway to adjust their credit supply. The coefficient on the standard growth rate shows that it matters to consider the creation of new relationships. In columns (4) to (6), I alter the definition of *BankExposure*. For column (4), the Amiti-Weinstein decomposition (2) is estimated without filtering out the bank×time cells that I identify as likely bank mergers (as detailed in Appendix F).[46] In columns (5) and (6), I fit the Amiti-Weinstein decomposition (2) aggregating local government loans at the *communes* (smaller) or *bassin de vie* (larger) levels instead of municipalities. Results are robust to these alternative definitions of shift-share IV.

Table A10 presents results when excluding outliers in *BankExposure* and when changing clustering levels. *BankExposure* is bounded, since it is the average of estimated fixed effects comprised between -2 and 2. That said, the results may be influenced by extreme values of *BankExposure*. To alleviate this concern, in column (1) I winsorize the extreme values of *BankExposure*, defined as exceeding p50 ± 2.5(p90-p10). The coefficient remains

[46]The advantage of including these bank×time cells is that I recover estimated municipality×time and bank×time fixed effects that allow to perfectly recover the aggregate time series. However, acquiring or acquired banks are characterized by extremely high or low credit growth, which may introduce some noise in the estimation of the fixed effects, which is the reason why they are excluded from my baseline sample.

TABLE A9. Firm×bank-level effects: Robustness to alternative variable definitions

	Credit growth					
	(1) MPGR (pos.)	(2) Std growth	(3) Norm. diff.	(4) MPGR	(5) MPGR	(6) MPGR
BankExposure	-0.605**	-0.188*	-0.201**	-1.051***	-1.113***	-0.617**
	(0.264)	(0.108)	(0.081)	(0.319)	(0.320)	(0.310)
Controls	✓	✓	✓	✓	✓	✓
Firm×Time FE	✓	✓	✓	✓	✓	✓
Indep. var. def.	Baseline	Baseline	Baseline	Incl. bank merger	*Communes* level	*Bassin de vie* level
Observations	2,731,110	1,982,477	2,579,749	2,731,110	2,731,110	2,731,110
R-squared	0.60	0.53	0.42	0.54	0.54	0.54

Note: This table presents robustness checks of the main results presented in Table 2. It reports the results of estimating specification (1). In column (1), the outcome variable is the bank×firm-level mid-point growth rate of credit, where negative values are replaced by zeros. In column (2), the outcome is the bank×firm-level growth rate of credit. In column (3), the outcome is the bank×firm-level change in credit, normalized by the firm total credit in the previous period. Column (4) estimates the Amiti-Weinstein decomposition (2) without excluding merging banks (see main text). Columns (5) and (6) estimates the Amiti-Weinstein decomposition (2) at the *communes* or *bassin de vie* levels, respectively, instead of municipalities. Controls include the bank's lagged local government loan share, assets (log), equity ratio, and dummies for state-owned and foreign banks. Regressions are weighted by bank×firm-level mid-point credit (top 0.5% winsorized). Standard errors are double-clustered at the bank and municipality level. ***, ** and * indicate significance at the 1%, 5% and 10% levels, respectively.

very similar. Columns (2) to (4) report results when changing the clustering level to firm, municipality, and bank-level, respectively. The estimated coefficient remains significant at the 5% level.

TABLE A10. Firm×bank-level effects: Robustness to outliers and clustering

	Credit growth			
	(1)	(2)	(3)	(4)
BankExposure	-0.772**	-0.853***	-0.853***	-0.853**
	(0.305)	(0.122)	(0.142)	(0.402)
Controls	✓	✓	✓	✓
Firm×Time FE	✓	✓	✓	✓
Indep. var. def.	Winsor.	Baseline	Baseline	Baseline
Cluster	Baseline	Firm-level	Municipality-level	Bank-level
Observations	2,731,110	2,731,110	2,731,110	2,731,110
R-squared	0.54	0.54	0.54	0.54

Note: This table presents robustness checks of the main results presented in Table 2. It reports the results of estimating specification (1). In column (1), *BankExposure* is winsorized for values exceeding p50 $\pm$ 2.5(p90-p10). Columns (2) to (4) report estimations on the baseline specification, except that standard errors are clustered at the firm, municipality, and bank level, respectively. Controls include the bank's lagged local government loan share, assets (log), equity ratio, and dummies for state-owned and foreign banks. Regressions are weighted by bank×firm-level mid-point credit (top 0.5% winsorized). Baseline standard errors are double-clustered at the bank and municipality level. ***, ** and * indicate significance at the 1%, 5% and 10% levels, respectively.

In the baseline results, the regressions are weighted by the denominator of the mid-point growth rate, top winsorized at the 0.5% level. Table A11 presents results for alternative weighting schemes. In columns (1) to (3), I show results when the weight is not winsorized, winsorized at 1 %, and winsorized at 10%. In columns (4) to (6), I perform the same exercise when the weights are divided by the probability that the firm belongs to the multibank sample, computed by regressing a multibank dummy on 20 firm-level credit bins.. Results are highly similar to my baseline results.

TABLE A11. Firm×bank-level effects: Robustness to alternative weighting scheme

	Baseline weighting			Adjusted weighting		
	(1)	(2)	(3)	(4)	(5)	(6)
BankExposure	-0.890***	-0.882***	-0.998***	-1.051***	-1.067***	-1.219***
	(0.326)	(0.318)	(0.352)	(0.356)	(0.365)	(0.415)
Controls	✓	✓	✓	✓	✓	✓
Firm×Time FE	✓	✓	✓	✓	✓	✓
Weighting	unwins	1%	10%	unwins	1%	10%
Observations	2,731,110	2,731,110	2,731,110	2,731,110	2,731,110	2,731,110
R-squared	0.55	0.53	0.51	0.55	0.54	0.53

Note: This table presents robustness checks of the main results presented in Table 2. It reports the results of estimating specification (1). Regressions are weighted by firm-level mid-point credit. In Columns (1) to (3), I vary the winsorizing strategies of the baseline weights. In columns (3)-(6), I repeat the same exercise but use weights adjusted for the probability that a firm belongs to the multibank sample (details in main text). Controls include the bank's lagged local government loan share, assets (log), equity ratio, and dummies for state-owned and foreign banks. Standard errors are double-clustered at the bank and municipality level. ***, ** and * indicate significance at the 1%, 5% and 10% levels, respectively.

C.2. Cross-sectional effects on interest rates

The “New contracts" dataset collected by Banque de France is a representative sample of new loans granted by French banks to corporations. It accounts for approximately 75% of the total new lending amount in each quarter. It contains information on the interest rate, as well as other contractual features (notably maturity, fixed or variable rate, benchmark index in the case of variable rate). The empirical specification is:

$$i_{lfbt} = d_{ft} + \beta BankExposure_{bt} + \Phi \cdot \mathbf{X}_{fbt} + \Lambda \cdot \mathbf{W}_{l} + \varepsilon_{lfbt}$$

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

This specification tests whether the same firm borrowing from different banks borrows at a higher interest rates from the relatively more exposed ones. The estimation requires that the firm takes on new loans of the same type from two different banks in the same period. This is demanding and mechanically less likely than having a same firm with ongoing relationships with two banks at the same time.

In my baseline results, I exclude credit lines and loans benefiting of any form of subsidy. I also present results corresponding to different sample restrictions.

The results are presented in Table A12. Columns (1) to (3) present the results with different control variables. Columns (4) to (6) explore alternative definitions of the sample. The effect is positive and statistically significant in most specifications. The point estimate is consistently around 0.03.

TABLE A12. Crowding out effect on interest rates

	Interest rate					
	(1)	(2)	(3)	(4)	(5)	(6)
BankExposure	0.029	0.033**	0.031**	0.024*	0.029**	0.026**
	(0.019)	(0.014)	(0.013)	(0.012)	(0.013)	(0.013)
Controls	–	–	✓	✓	✓	✓
Firm×Time FE	✓	✓	✓	✓	✓	✓
Loan char FE	–	✓	✓	✓	✓	✓
Sample	Baseline	Baseline	Baseline	$\leq$ 25 loans	Add leasing	Add subsidized
Observations	472,214	472,183	472,172	310,691	593,234	658,433
R-squared	0.93	0.94	0.94	0.95	0.94	0.93

Note: This table examines the crowding out effect of local government debt on interest rates. It reports the results of estimating equation C.2. The outcome variable is the interest rate on loan l granted to firm f by bank b. The main independent variable is bank exposure to local government debt demand shocks (defined in (3)). The bank's lagged local government loan share is always included as a control. Controls refers to the banks' lagged assets (log), equity ratio, dummies for state-owned and foreign banks, and the amount of the loan. Loan char FE refer to maturity bucket×index×time and type of loan×time fixed effects. Regressions are weighted by bank×firm-level mid-point credit (top 0.5% winsorized). Standard errors are double-clustered at the bank and municipality level. ***, ** and * indicate significance at the 1%, 5% and 10% levels, respectively.

C.3. Cross-sectional effects on real variables

Euro-for-euro crowding out computation. The quantification provided in the main text starts from the bank-level crowding out parameter (0.54). Since firms do not substitute across banks, the reduction in credit by a bank is equal to the reduction in credit for the borrowers of this bank. To obtain the effect on investment, I then use $\mathrm{d}\bar{K}_{ft} = \eta^K \frac{\bar{K}_{ft}}{\bar{C}_{ft}} \mathrm{d}\bar{C}_{ft}$, where upper bar denotes sample mean as found in Table 1. η^K is estimated in Table 4 and is equal to 0.27.

Effect on employment. Table A13 provides the results of estimating equation (4) when the outcome is firm-level employment. The reduced form results of *FirmExposure* on employment show that the effect is very close to 0.

Additional tests of identifying assumptions. Table A14 presents further tests that support the identifying assumptions of my main results. Columns (1) to (4) display results for various fixed effects structure. Column (1) has the coarsest fixed effects structure: 12 industries $\times$ 22 regions $\times$ year. Column (4) has the finest fixed effects structure: 88 industries $\times$ 2081 municipalities $\times$ year as well as firm fixed effects. Column (5) controls for lagged

TABLE A13. Firm-level effect on credit and employment

	Effect of exposure to local government debt shocks				Credit-to-inputs elasticities	
	gr(credit)		gr(emp)		gr(emp)	
	RF	RF	RF	RF	IV	IV
	(1)	(2)	(3)	(4)	(5)	(6)
FirmExposure	-1.100***	-1.064***	-0.000	0.004		
	(0.277)	(0.259)	(0.042)	(0.049)		
gr(credit)					0.007	0.003
					(0.027)	(0.030)
Firm controls	–	✓	–	✓	–	✓
Municipality×Industry×Time FE	✓	✓	✓	✓	✓	✓
Observations	1042147	798769	845001	757484	770655	691126
R-squared	0.95	0.95	0.29	0.31	0.0093	0.025
F stat.					17.9	19.9

Note: This table reports the results of estimating equation (4). Outcome variables are the firm-level mid-point growth rate of credit and the growth rate of employment. The main independent variable is firm exposure to crowding out (defined in (5)). All regressions include the firm-level average of the bank controls included in Table 2 and the estimated firm-level credit demand shock. "Firm controls" additionally include the firm's assets (log), leverage, ROA, cash flow from operations/assets, and capex/sales ratios (all lagged). Columns (5) and (6) show the credit-to-capital elasticity, obtained by instrumenting firm-level credit growth by *FirmExposure*. Regressions are weighted by firm-level mid-point credit (top 0.5% winsorized). Standard errors are double-clustered at the main bank and municipality level. ***, ** and * indicate significance at the 1%, 5% and 10% levels, respectively.

credit growth, which restricts the comparison to firms on a similar credit trajectory. Column (6) looks at the differential effect of exposure to crowding out for firms in industries highly reliant on public procurement.

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

Robustness checks. Table A15 presents results of incorporating additional controls and of imposing additional sample restrictions. In column (1), I estimate equation (4) with only the average bank-level controls and the fixed effects (but omitting the estimated firm-level demand shock $\hat{d}_{ft}$). In columns (2) and (3), I sequentially add the estimated demand shock and the baseline set of firm-level controls. Column (4) expands the set of controls to include EBIT ratio, cash ratio, interest coverage ratio, and tangible assets ratio. Column (5) further includes controls related to the firm's banking relationships: the HHI of bank shares, number of banks from whom the firm borrows, and dummies indicating the

TABLE A14. Firm-level real effects: Tests of identifying assumptions

Panel A: Credit

	gr(credit)					
	(1)	(2)	(3)	(4)	(5)	(6)
FirmExposure	-1.198***	-1.125***	-1.065***	-1.391***	-0.745***	-1.094***
	(0.315)	(0.276)	(0.259)	(0.323)	(0.246)	(0.272)
FirmExposure× Pub. Proc.						0.168
						(0.206)
Controls	✓	✓	✓	✓	✓	✓
Baseline FE	–	–	✓	✓	✓	✓
Industry(12)×Region×Time FE	✓	–	–	–	–	–
Industry(38)×Municipality×Time FE	–	✓	–	–	–	–
Size×Time FE	–	–	✓	–	–	–
Firm FE	–	–	–	✓	–	–
Lagged credit growth	–	–	–	–	✓	–
Observations	936,822	845,293	798,764	771,109	704,280	798,769
R-squared	0.93	0.95	0.95	0.97	0.95	0.95

Panel B: Investment

	gr(capital)					
	(1)	(2)	(3)	(4)	(5)	(6)
FirmExposure	-0.437***	-0.505***	-0.449***	-0.393***	-0.408***	-0.473***
	(0.083)	(0.085)	(0.081)	(0.096)	(0.078)	(0.098)
FirmExposure× Pub. Proc.						0.059
						(0.261)
Controls	✓	✓	✓	✓	✓	✓
Baseline FE	–	–	✓	✓	✓	✓
Industry(12)×Region×Time FE	✓	–	–	–	–	–
Industry(38)×Municipality×Time FE	–	✓	–	–	–	–
Size×Time FE	–	–	✓	–	–	–
Firm FE	–	–	–	✓	–	–
Lagged credit growth	–	–	–	–	✓	–
Observations	913,372	822,281	776,278	748,178	690,964	776,281
R-squared	0.20	0.39	0.44	0.59	0.45	0.44

Note: This table presents robustness checks of the main results presented in Table 4. It reports the results of estimating specification (4). Controls include the firm-level average of the bank-specific controls, the estimated firm-level credit demand shock, the firms' assets (log), leverage, ROA, cash flow from operations/assets, and capex/sales ratios (all lagged). Baseline FE are municipality×88 industries×time fixed effects. For column (6), Pub. Proc. is a dummy equal to 1 for the top 10 industries by public procurement contract revenues (data from *Données essentielles de la commande publique* available here). Regressions are weighted by firm-level mid-point credit (top 0.5% winsorized). Standard errors are double-clustered at the main bank and municipality level. ***, ** and * indicate significance at the 1%, 5% and 10% levels, respectively.

start and the end of a relationship. Column (6) uses the firm-level demand shock estimated from specification (1) as opposed as from the Amiti-Weinstein decomposition. Column (7) restricts the sample to firms borrowing from at least two banks. Column (8) restrains the analysis to firms filing their tax statements in the last quarter of the financial year, so that the timing of *FirmExposure*, credit growth, and investment growth perfectly coincide. Panel A reports these results when the outcome is firm-level credit growth. Panel B repeats the same exercises for the firm-level investment. The results are very similar to the baseline across all these specifications.

TABLE A15. Firm-level real effects: Robustness to additional controls

Panel A: Credit

	gr(credit)							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
FirmExposure	-0.537***	-1.100***	-1.064***	-1.030***	-0.993***	-1.164***	-0.782***	-0.960***
	(0.184)	(0.277)	(0.259)	(0.249)	(0.250)	(0.185)	(0.247)	(0.269)
Wgt bank controls	✓	✓	✓	✓	✓	✓	✓	✓
$\hat{d}_{ft}$	–	✓	✓	✓	✓	–	✓	✓
$\hat{d}_{ft}$ (alt)	–	–	–	–	–	✓	–	–
Firm controls (base)	–	–	✓	✓	✓	✓	✓	✓
Firm controls (add)	–	–	–	✓	✓	✓	–	–
Rel. controls	–	–	–	–	✓	✓	–	–
FE	✓	✓	✓	✓	✓	✓	✓	✓
Sample	–	–	–	–	–	–	Multibank	Q4
Observations	1,049,031	1,042,147	798,769	750,309	750,309	798,756	240,928	558,845
R-squared	0.25	0.95	0.95	0.95	0.95	0.97	0.97	0.96

Panel B: Investment

	gr(capital)							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
FirmExposure	-0.303***	-0.428***	-0.452***	-0.446***	-0.410***	-0.488***	-0.433***	-0.442***
	(0.091)	(0.084)	(0.081)	(0.082)	(0.079)	(0.077)	(0.155)	(0.086)
Wgt bank controls	✓	✓	✓	✓	✓	✓	✓	✓
$\hat{d}_{ft}$	–	✓	✓	✓	✓	–	✓	✓
$\hat{d}_{ft}$ (alt)	–	–	–	–	–	✓	–	–
Firm controls (base)	–	–	✓	✓	✓	✓	✓	✓
Firm controls (add)	–	–	–	✓	✓	✓	–	–
Rel. controls	–	–	–	–	✓	✓	–	–
FE	✓	✓	✓	✓	✓	✓	✓	✓
Sample	–	–	–	–	–	–	Multibank	Q4
Observations	889,398	883,748	776,281	733,918	733,918	776,271	234,767	541,536
R-squared	0.32	0.42	0.44	0.44	0.45	0.44	0.51	0.47

Note: This table presents robustness checks of the main results presented in Table 4. It reports the results of estimating specification (4). Wgt bank controls refers to the firm-level average of the bank-specific controls. $\hat{d}_{ft}$ refers to the estimated firm-level credit demand shock (baseline). $\hat{d}_{ft}$ (alt) refers to the estimated firm-level credit demand shock (extracted from the wihtin-firm specification). Firm controls (base) includes the firms' assets (log), leverage, ROA, cash flow from operations/assets, and capex/sales ratios (all lagged). Firm controls (add) includes includes EBIT ratio, cash ratio, interest coverage ratio, and tangible asset ratio as controls. Rel. controls includes the HHI of bank shares, number of banks from whom the firm borrows, and dummies indicating the start and the end of a firm-bank relationship. FE are municipality×88 industries×time fixed effects. Regressions are weighted by firm-level mid-point credit (top 0.5% winsorized). Standard errors are double-clustered at the main bank and municipality level. ***, ** and * indicate significance at the 1%, 5% and 10% levels, respectively.

In the baseline results, I consistently weight regressions by the denominator of the firm-level mid-point growth rate of credit, top-winsorized at the 0.5% level. Consistent weighting ensures that the coefficients are directly comparable across specifications, in particular when I estimate the credit-to-input IV regressions. Table A16 presents results for alternative weighting schemes. In Columns (1) to (3), weights are the denominator of the firm-level mid-point growth rate of credit with different levels of top-winsorization.

In Columns (4) to (7), weights are the firm's lagged capital stock, with different levels of top-winsorization. Panel A reports these results when the outcome is firm-level credit growth. Panel B repeats the same exercises for the firm-level investment. The results are similar to the baseline across all these specifications. It is worth noting that the effect on investment is lower when weighting by the capital stock, suggesting larger effects for firms with higher credit-to-capital ratios.

TABLE A16. Firm-level real effects: Robustness to weighting

Panel A: Credit

	gr(credit)						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
FirmExposure	-1.103***	-1.049***	-1.037***	-0.889***	-1.122***	-1.116***	-1.021***
	(0.249)	(0.264)	(0.258)	(0.282)	(0.227)	(0.225)	(0.249)
Controls	✓	✓	✓	✓	✓	✓	✓
FE	✓	✓	✓	✓	✓	✓	✓
Weighting	C	C (1%)	C (10%)	K	K (0.5%)	K (1%)	K (10%)
Observations	798,769	798,769	798,769	797,135	797,135	797,135	797,135
R-squared	0.96	0.95	0.95	0.99	0.97	0.97	0.97

Panel B: Investment

	gr(capital)						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
FirmExposure	-0.477***	-0.433***	-0.307***	-0.284**	-0.268***	-0.276***	-0.238***
	(0.133)	(0.075)	(0.067)	(0.119)	(0.093)	(0.091)	(0.045)
Controls	✓	✓	✓	✓	✓	✓	✓
FE	✓	✓	✓	✓	✓	✓	✓
Weighting	C	C (1%)	C (10%)	K	K (0.5%)	K (1%)	K (10%)
Observations	776,281	776,281	776,281	776,281	776,281	776,281	776,281
R-squared	0.47	0.43	0.37	0.43	0.41	0.39	0.33

Note: This table presents robustness checks of the main results presented in Table 4. It reports the results of estimating specification (4). The line Weighting refers to the weighting scheme. C indicates weighting by firm-level mid-point credit. K indicates weighted by firm-level lagged fixed assets. The number in parenthesis indicates the top-winsorization of weights. Controls refers to the firm-level average of the bank-specific controls, the estimated firm-level credit demand shock, the firms' assets (log), leverage, ROA, cash flow from operations/assets, and capex/sales ratios (all lagged). FE are municipality$\times$88 industries$\times$time fixed effects. Standard errors are double-clustered at the main bank and municipality level. ***, ** and * indicate significance at the 1%, 5% and 10% levels, respectively.

Table A17 reports findings when altering the definition of *FirmExposure* or when changing the level of clustering for standard errors. In columns (1) and (2), I construct *FirmExposure* using the lagged shares of bank b in firm f's total credit, as opposed to the mid-point shares that properly aggregate mid-point growth rates. In columns (3) and (4), I winsorize the extreme values of *FirmExposure*, defined as exceeding p50 $\pm$ 2.5(p90-p10). Columns (5), (6) and (7) cluster standard errors at the firm, municipality, and lead bank levels, respectively. Lead bank is defined as the bank from which the firm borrows the most in a specific year. Estimated coefficients are again highly similar to the baseline and remain statistically significant at the 1% level.

TABLE A17. Firm-level real effects: Robustness to variable definition and clustering

Panel A: Credit

	gr(credit)						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
FirmExposure	-1.477***	-1.403***	-1.144***	-1.104***	-1.064***	-1.064***	-1.064***
	(0.329)	(0.293)	(0.285)	(0.263)	(0.085)	(0.135)	(0.268)
Firm controls	–	✓	–	✓	✓	✓	✓
FE	✓	✓	✓	✓	✓	✓	✓
Indep. var. def.	Alt. shares	Alt. shares	Winsor.	Winsor.	Baseline	Baseline	Baseline
Cluster	Baseline	Baseline	Baseline	Baseline	Firm	Municipality	Lead bank
Observations	942345	728514	1042147	798769	798769	798769	798769
R-squared	0.94	0.94	0.95	0.95	0.95	0.95	0.95

Panel B: Investment

	gr(capital)						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
FirmExposure	-0.418***	-0.421***	-0.440***	-0.461***	-0.452***	-0.452***	-0.452***
	(0.087)	(0.079)	(0.087)	(0.084)	(0.071)	(0.070)	(0.084)
Firm controls	–	✓	–	✓	✓	✓	✓
FE	✓	✓	✓	✓	✓	✓	✓
Indep. var. def.	Alt. shares	Alt. shares	Winsor.	Winsor.	Baseline	Baseline	Baseline
Cluster	Baseline	Baseline	Baseline	Baseline	Firm	Municipality	Lead bank
Observations	814589	715695	883748	776281	776281	776281	776281
R-squared	0.42	0.45	0.42	0.44	0.44	0.44	0.44

Note: This table presents robustness checks of the main results presented in Table 4. It reports the results of estimating specification (4). Columns (1) and (2) use standard bank shares to construct *FirmExposure*. Columns (3) and (4) winsorize *FirmExposure* at the p50 $\pm$ 2.5(p90-p10) level. Columns (5), (6) and (7) cluster standard errors alternatively at the firm, municipality and lead bank levels. All regressions include the firm-level average of the bank controls included in Table 2 and the estimated firm-level credit demand shock. "Firm controls" additionally include the firm's assets (log), leverage, ROA, cash flow from operations/assets, and capex/sales ratios (all lagged). Regressions are weighted by firm-level mid-point credit (top 0.5% winsorized). Baseline standard errors are double-clustered at the main bank and municipality level. ***, ** and * indicate significance at the 1%, 5% and 10% levels, respectively.

Appendix D. Model

The model contains four sectors: households that supply labor and save in the form of bank deposits; firms that produce using capital and labor, capital being financed by bank loans; local governments that borrow from banks; and banks that are funded via deposits and lend to firms and local governments. There is a continuum of banks of mass 1, indexed by $b \in [0, 1]$. Banking relationships enter the model through the assumption that firms and local governments are assigned to a given bank. Imperfect capital mobility across banks enters the model through the assumption that there is an identical depositor assigned to each bank that does not arbitrage across banks. An interbank market can be accessed at a cost. All agents are price-takers.[47]

D.1. Baseline model

Firms The production side of the economy . There is a continuum of intermediate input firms indexed by $b \in [0, 1]$ (bank to which the firm is attached) and $f \in [0, 1]$ (firms borrowing from a bank). A competitive final good producer aggregates differentiated inputs via a CES function with elasticity of substitution σ. Variety or the firm f borrowing from bank b is assumed to be differentiated from all the varieties produced by the firms borrowing from bank b'.

$$Y = \left(\int_0^1 \int_0^1 Y_{fb}^{\frac{\sigma-1}{\sigma}} dfdb \right)^{\frac{\sigma}{\sigma-1}} \tag{A7}$$

The demand for intermediate input f is given by:

$$Y_{fb} = P_{fb}^{-\sigma} Y \tag{A8}$$

where I normalize the aggregate price index $P = \left(\int_0^1 \int_0^1 P_{fb}^{1-\sigma} dfdb \right)^{\frac{1}{1-\sigma}}$ to be the numeraire.

Each intermediate input firm produces output using a Cobb-Douglas production technology:

$$Y_{fb} = e^{z_{fb}} K_{fb}^{\alpha} L_{fb}^{1-\alpha} \tag{A9}$$

Intermediate input firms finance their stock of capital using equity and bank loans: $K_{fb} = C_{fb} + E$. E is the same for all firms. A firm borrowing from bank b borrows at rate r_b^c. Profits

[47] Introducing monopolistic banks leaves all key results unchanged.

are distributed to households. Firms maximize profits, given by:

$$\Pi_f = P_f Y_f - wL_{fb} - r_b^c C_{fb} \tag{A10}$$

taking the demand curve as given. The first-order conditions are:

$$\alpha \frac{\sigma - 1}{\sigma} P_f Y_f = R_b^c K_f \tag{A11}$$

$$(1 - \alpha) \frac{\sigma - 1}{\sigma} P_f Y_f = w_t L_f \tag{A12}$$

The profit maximizing price equals a mark-up over marginal cost:

$$P_f = \frac{\sigma}{\sigma - 1} \frac{1}{e^{z_f}} \left(\frac{w}{1 - \alpha}\right)^{1-\alpha} \left(\frac{R_b^c}{\alpha}\right)^{\alpha} \tag{A13}$$

Using this equation, the FOCs and the production function, we obtain the firms' input demand functions:

$$K_f = e^{(\sigma-1)z_f} \left(\frac{\sigma - 1}{\sigma}\right)^{\sigma} Y \left(\frac{1 - \alpha}{w}\right)^{(1-\alpha)(\sigma-1)} \left(\frac{\alpha}{R_b^c}\right)^{1+\alpha(\sigma-1)} \tag{A14}$$

$$L_f = e^{(\sigma-1)z_f} \left(\frac{\sigma - 1}{\sigma}\right)^{\sigma} Y \left(\frac{1 - \alpha}{w}\right)^{\alpha+(1-\alpha)\sigma} \left(\frac{\alpha}{R_b^c}\right)^{\alpha(\sigma-1)} \tag{A15}$$

Using $K_{fb} = C_{fb} + E$ defines a credit demand function C_{fb} for each firm. Aggregating across the firms f, we obtain corporate credit demand at bank b:

$$C_b^{corp} = \int_0^1 C_{fb} df \tag{A16}$$

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

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

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

$$C_b^{gov} = \int_0^1 C_{mb}^{gov} dm$$

I define $Z_b^{gov} = \int_0^1 z_m^{gov} dm$ and $Z^{gov} = \int_0^1 \int_0^1 z_m^{gov} dmdb$.

Households. There is a representative household depositing their savings at each bank. To keep the model static, I assume a reduced-form deposit supply function:

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

with $\epsilon^s \geq 0$. In addition, each household supplies undifferentiated labor with a Frisch elasticity of labor supply ψ:

$$L^s = lw^{\psi}$$

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

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

subject to: $C_b^{corp} + C_b^{gov} = S_b + B_b$. The equilibrium prices consistent with the first-order condition of banks are $r_b^c = r_b^g = r_b^s = r_b$ and $r_b = i(1 + \phi B_b)$.

Equilibrium. An equilibrium consists of quantities $(\{Y_{fb}\}, \{K_{fb}\}, \{C_{fb}\}, \{L_{fb}\}, \{S_b\}, \{C_b^{gov}\}, \{B_b\})$ and prices $(\{P_{fb}\}, \{R_b\}, i, w)$ such that:

(a) Firms' optimization: Taking $(\{P_{fb}\}, \{r_b^c\}, w)$, firms maximize profits

(b) Bank's optimization: Taking $(\{r_b^c\}, \{r_b^g\}, \{r_b^s\}, i)$, banks maximize profits

(c) Local governments': Taking $(\{r_b^g\})$ as given, local governments demand loans as given by their demand function

(d) Households: Taking $(\{r_b^s\}, w)$ as given, households supply deposits and labor as given by their supply functions

(e) Market clearing: For each bank b, fund demand equals fund supply $C_b^{corp} + C_b^{gov} = S_b + B_b$; the labor market clears $L^s = \int_0^1 \int_0^1 L_{fb} df db$; the interbank market clears $\int_0^1 B_b db = 0$.

All prices and quantities as a function of the exogenous shocks $(\{z_{fb}\}, \{Z_b^{gov}\})$

Solution. I solve the model by log-linearisation around the deterministic equilibrium (DE), characterized by $z_{fb} = 0$ for all f, b 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, there is no interbank market borrowing in the DE.

In log-linear form, the solution of the banks problem writes:

$$\hat{r}_b = \hat{i} + \phi B_b \tag{A17}$$

$$\lambda \hat{C}_b^{gov} + (1-\lambda)\hat{C}_b^{corp} = \hat{S}_b + \frac{1}{S^*} B_b \tag{A18}$$

where λ is the share of local government loans in the bank loan portfolio in the DE, equal for all banks.

To obtain the log-linearized version of the bank-specific corporate credit demand, note that:

$$\hat{K}_f = \ell \hat{C}_{fb} \tag{A19}$$

where $\ell = \frac{C^{corp*}}{K^*}$ is the share of capital financed by bank loans in the DE, equal for all firms. Therefore,

$$\hat{C}_{fb} = \frac{1}{\ell}[(\sigma-1)z_{fb} + \hat{Y} - (1-\alpha)(\sigma-1)\hat{w} - (1+\alpha(\sigma-1))\hat{r}_b^c] \tag{A20}$$

Let $\epsilon^c = -\frac{1}{\ell}(1+\alpha(\sigma-1))$ denote the elasticity of corporate credit demand.

Substituting the corporate credit demand (aggregated across firms f borrowing from bank b), local government credit demand, the deposit supply function, aggregating across banks, and using the interbank market clearing condition yields:

$$\hat{i} = \frac{\lambda Z^g + (1-\lambda)\frac{1}{\ell}[\hat{Y} - (1-\alpha)(\sigma-1)\hat{w}]}{\epsilon^s - \lambda\epsilon^g - (1-\lambda)\epsilon^c} \tag{A21}$$

Combining this equation with the aggregate versions of the firm FOC (A11) and (A12) and the production function (A10) allows to get the solution for all aggregate variables $\hat{Y}$, $\hat{w}$, $\hat{i}$,

$\hat{K}$, $\hat{L}$, $\hat{C}^{corp}$. The solution for $\hat{i}$ writes:

$$\hat{i} = \frac{\lambda Z^g}{\epsilon^s - \lambda\epsilon^g + (1-\lambda)\frac{1}{\ell}\frac{1+\psi\alpha}{1-\alpha}} \tag{A22}$$

In addition, by differencing the aggregate and the bank-specific bank balance sheet constraint condition (A18), we obtain:

$$B_b = \frac{1}{\phi}\frac{\lambda(Z^g_b - Z^g)}{\epsilon^s - \lambda\epsilon^g - (1-\lambda)\epsilon^c + \frac{1}{\phi S^*}} \tag{A23}$$

$$\hat{r}_b = \frac{\lambda Z^g}{\epsilon^s - \lambda\epsilon^g + (1-\lambda)\frac{1}{\ell}\frac{1+\psi\alpha}{1-\alpha}} + \frac{\lambda(Z^g_b - Z^g)}{\epsilon^s - \lambda\epsilon^g - (1-\lambda)\epsilon^c + \frac{1}{\phi S^*}} \tag{A24}$$

Banks that receive larger demand shocks borrow from the other banks on the interbank market. This amount is decreasing in the intensity of the friction ϕ.

D.2. Relation to empirical work

Model prediction for aggregate and cross sectional crowding out. Using the corporate credit demand function (A20) and the solution for aggregate variables $\hat{Y}$, $\hat{w}$ and for $\hat{r}_b$, we can re-write firm$\times$bank equilibrium credit as follows:

$$\hat{C}_{fb} = z_{fb} + \kappa^{GE}\chi\lambda Z^{gov} + \chi(1-\nu)\lambda Z^{gov} + \chi\nu\lambda Z^{gov}_b \tag{A25}$$

while the aggregate corporate credit change writes:

$$\hat{C}^{corp} = (1+\kappa^{GE})\chi\lambda Z^{gov} \tag{A26}$$

where $\chi = \frac{\varepsilon^c}{\epsilon^s - \lambda\epsilon^g - (1-\lambda)\epsilon^c}$, $\nu = \frac{\epsilon^s - \lambda\epsilon^g - (1-\lambda)\epsilon^c}{\epsilon^s - \lambda\epsilon^g - (1-\lambda)\epsilon^c + \frac{1}{\phi S^*}}$ and $\kappa^{GE} = \frac{\frac{1}{\ell}\frac{1+\alpha\psi}{1-\alpha}}{\frac{1}{\ell}(1+\alpha(\sigma-1))}\frac{\epsilon^s - \lambda\epsilon^g + (1-\lambda)\frac{1}{\ell}(1+\alpha(\sigma-1))}{\epsilon^s - \lambda\epsilon^g + (1-\lambda)\frac{1}{\ell}\frac{1+\psi\alpha}{1-\alpha}} - 1$.

χ corresponds to the direct crowding out effect. It captures the extent of the interest rate increase in response to a demand shock (the denominator), and the decline in corporate credit for a given interest rate changed (governed by the elasticity of credit demand at the numerator). It does not depend on interbank market frictions.

κ^{GE} captures the general equilibrium feedback. It can be positive or negative, depending on the difference between $\frac{1+\alpha\psi}{1-\alpha}$ and $1+\alpha(\sigma-1)$, and is equal to 0 when these two terms are equal. I elaborate on the intuition for these comparative statics below.

The direct effect χ combined with the general equilibrium feedback sum to the aggregate effect in equation (A26).

The bank$\times$firm-level equation (A25) shows another parameter: ν. $\nu \in [0, 1]$ is monotonically increasing in ϕ. It captures the degree of interbank market frictions. When $\phi \to 0$ (no interbank frictions), $\nu = 0$, and when $\phi \to +\infty$ (complete segmentation), $\nu = 1$. Comparing equations (A25) and (A26) shows that at the firm$\times$bank-level, the direct effect of crowding out is split into two terms. $\chi\nu$ is the relative crowding out parameter. When banks are perfectly integrated, corporate credit by bank b does not depend on the bank-specific increase in local government loans, but only on the aggregate increase. Conversely, when banks are fully segmented, corporate credit by bank b only depends on the bank-specific increase in local government loans, and not on the aggregate increase. As long as $\nu < 1$, banks not directly exposed to increased demand for local government loans lend to other banks and corporate credit also falls at these banks.

Link with the empirical specification. Equation (A25) yields an estimation equation corresponding to the regression specification in the main text. To link the static model with the panel setting of the main text, I consider that in each period the economy starts from the deterministic equilibrium, so that I can assimilate observed growth rates to log-deviations from the deterministic equilibrium. Therefore, firm$\times$bank credit growth ΔC_{fb} is approximately equal to $\hat{C}_{fb}$. λZ_b^{gov} corresponds to the demand amount normalized by the total bank lending as *BankExposure*. Aggregate variables are defined accordingly. Equation (A25) then writes:

$$\Delta C_{fbt} = \chi(\kappa^{GE} + 1 - \nu) BankExposure_t + \chi\nu BankExposure_{bt} \tag{A27}$$

The β coefficient that I estimate in the regression specification (1) corresponds to $\chi\nu$.

Identification. To link the model and the identification strategy, I present here a generalization of equation (A27) when firms borrow from multiple banks and banks are subject to idiosyncratic liquidity shocks ξ_b (see equation A37 in extension D.4.1).

$$\Delta C_{fbt} = \upsilon z_{ft} + \Upsilon Z_t^{corp} + \chi(\kappa^{GE} + 1 - \nu) BankExposure_t + \chi\nu BankExposure_{bt} + \iota\nu\xi_{bt} \tag{A28}$$

This equation clarifies the two identification concerns highlighted in Section 4.1: a correlation between bank-level local government debt demand shocks Z_b^{gov} and firm-level corporate credit demand shocks z_f and a correlation between bank-level local government debt demand shocks Z_b^{gov} and other bank-level corporate credit supply shocks ξ_b.

Analytical formulas for the missing intercept. Equation (A25) clarifies that the cross-sectional regression only accounts for part of the aggregate effect of crowding out, because it misses equilibrium effects affecting all firms and banks uniformly. This is the usual "missing intercept" problem.

The model yields a closed form prediction for the missing intercept problem: it is equal to $\kappa^{GE}\chi + \chi(1-\nu)$ multiplied by the aggregate shock $BankExposure_t$. It can be decomposed into two channels: (i) a spillover effect due to capital mobility across banks $\chi(1-\nu)$, (ii) a general equilibrium feedback $\kappa^{GE}\chi$.

To further clarify the difference between the reduced form estimates and the aggregate effect, consider the exercise consisting in cumulating corporate credit shortfalls relative to a situation in which all *Bankexposure* is 0 as predicted by my reduced-form regression:

$$\mathcal{L}^{Xsec}(C^{corp}) = \int_0^1 \int_0^1 \chi\nu\lambda Z_b^{gov} dfdb = \chi\nu\lambda Z^{gov} \tag{A29}$$

Next, consider the general equilibrium exercise of cumulating the corporate credit shortfalls relative to a situation in which all *Bankexposure* is 0. Taking into account the spillover effect due to capital mobility across banks leads to:

$$\mathcal{L}^{direct}(C^{corp}) = \chi\nu\lambda Z^{gov} = \frac{1}{\nu}\mathcal{L}^{Xsec}(C^{corp}) \tag{A30}$$

Taking into account both the spillover effect due to capital mobility across banks and the general equilibrium feedback leads to:

$$\mathcal{L}^{total}(C^{corp}) = (1+\kappa^{GE})\chi\nu\lambda Z^{gov} = \frac{1+\kappa^{GE}}{\nu}\mathcal{L}^{Xsec}(C^{corp}) \tag{A31}$$

Unless $\kappa^{GE} = 0$ and $\nu = 1$, the aggregation $\mathcal{L}^{Xsec}(C^{corp})$ will not allow to recover $\mathcal{L}^{total}(C^{corp})$. The same reasoning applies for investment. The firm-level equation for capital writes:

$$\hat{K}_{fb} = \ell\kappa^{GE}\chi\lambda Z^{gov} + \ell\chi(1-\nu)\lambda Z^{gov} + \ell\chi\nu\lambda Z_b^{gov} \tag{A32}$$

The cross-sectional coefficient I estimate in specification (4) corresponds to $\ell\chi\nu$. The difference between the reduced-form and the aggregate effect is again given by: $\mathcal{L}^{total}(K) = (1+\kappa^{GE})\mathcal{L}^{direct}(K) = \frac{1+\kappa^{GE}}{\nu}\mathcal{L}^{Xsec}(K)$.

D.3. Estimation of aggregate effect.

The preceding discussion shows that I can obtain the aggregate effect of crowding out by combining three elements: (i) the aggregate shortfall computed using my cross-section estimates $\mathcal{L}^{Xsec}(C^{corp})$; (ii) an estimate of ν ; (iii) an estimate of κ^{GE}. I now show how to estimate these three quantities.

Aggregation using cross-sectional estimates. When the distribution of firm and bank size is non-degenerate,

$$\mathcal{L}^{Xsec}(C_t^{corp}) = \chi\nu \sum_f \sum_b \frac{C_{fbt}(0)}{C_t^{corp}(0)} \lambda Z_b^{gov} = \chi\nu \sum_f \frac{C_{ft}(0)}{C_t^{corp}(0)} \sum_b \frac{C_{fbt}(0)}{C_{ft}^{corp}(0)} \lambda Z_b^{gov} \tag{A33}$$

I estimate this quantity using the coefficient of the firm-level regression as:

$$\mathcal{L}^{Xsec}(C_t^{corp}) = \beta^C \sum_f \frac{C_{ft}(0)}{C_t^{corp}(0)} FirmExposure_{ft} \tag{A34}$$

β^C is the coefficient in Table 4, column 1. In the baseline model, the coefficient of the bank-firm level and the firm level regressions are equal. Extension D.4.1 clarifies that if there is some substitution across banks, the relevant coefficient for the aggregation exercise is the coefficient of the firm-level regression. $C_{ft}(0)$ is obtained using the predicted value of the regression. I proceed similarly for capital:

$$\mathcal{L}^{Xsec}(K_t) = \beta^K \sum_f \frac{K_{ft}(0)}{K_t(0)} FirmExposure_{ft} \tag{A35}$$

I obtain the output loss as $\mathcal{L}^{Xsec}(Y_t) = \alpha\mathcal{L}^{Xsec}(K_t)$. To account for industry-specific capital shares, I compute the industry-level output loss using industry-specific capital shares before aggregating across industries. This yields the estimates presented in the main text.

Robustness checks. This computation depends on the joint distribution of the shock and of firm size, which may not be the result of an invariant economic mechanism but rather of luck. I also provide the quantification of the output shortfall based on the assumption that all firms are symmetric, which neutralizes this effect. I obtain lower bounds equal to 0.75%, 0.26% for corporate credit, capital, respectively. The output loss is then equal to 0.07%. The equivalent output multiplier is –0.20.

The lower bound for the output multiplier can also be recovered from the back-of-the-envelope computations from the reduced-form results in Section 6. The relationship is

$\mathrm{d}Y = \alpha\frac{Y}{K}\mathrm{d}K$. Using sample mean values of the different variables, I obtain a multiplier equal to -0.18.

Estimation of the interbank market spillover. To estimate ν, I use an additional prediction of the model. Namely, equation (A23) can be rewritten as:

$$\frac{B_b}{S^*} = (1-\nu)\lambda(Z_b^{gov} - Z^{gov})$$

Banks with larger than average exposure to demand for local government loans borrow from other banks on the interbank market. The extent of this reaction is informative of the degree of bank segmentation ν.

Challenges to identification. In the baseline version of the model, I make the simplifying assumptions that (i) the firm productivity shocks are mean zero and identically distributed at each banks, (ii) there is no bank-specific funding shock. In the more general version of the model presented in equation (A39) of Extension D.4.1, this equation writes:

$$\frac{B_b}{S^*} = (1-\nu)\left[\lambda(Z_b^{gov} - Z^{gov}) + (1-\lambda)\frac{\sigma-1}{\ell}(\tilde{Z}_b^{corp} - \tilde{Z}^{corp}) - \frac{1}{S^*}\xi_b\right]$$

where $\tilde{Z}$ rescales firm productivity shocks into corporate credit demand shocks. This equation highlights two identification concerns here: a correlation between bank-level local government debt demand shocks and corporate credit demand shocks and a correlation between bank-level local government debt demand shocks and other corporate credit supply shocks. In addition, I cannot resort to the within-firm identification strategy. This implies that I cannot control for firm-specific credit demand shocks. In addition, the orthogonality condition regarding bank-level corporate credit supply shocks has to hold without conditioning on the firm fixed effects.

Empirical strategy. To circumvent these concerns, I construct a bank-specific credit demand shock that aggregates demand from local governments and from firms. I decompose credit flows into bank and borrower fixed effects by estimating $\Delta C_{ibt} = \alpha_{it}^D + \alpha_{bt}^S + \varepsilon_{ibt}$ where i can be either a firm or a municipality. Again following the Amiti and Weinstein (2018) logic, α_{it}^D captures borrower-specific (demand) factors, while α_{bt}^S captures bank-specific (supply) factors. I then aggregate the borrower fixed effects at the bank level using the share of each borrower in the bank total lending: $\alpha_{bt}^D = \sum_i \frac{C_{ibt-1}}{C_{bt-1}}\hat{\alpha}_{it}^D$. α_{bt}^D constitutes a proxy for $\left[\lambda Z_b^{gov} + (1-\lambda)Z_b^{corp}\right]$. I also recover $\hat{\alpha}_{bt}^S$ which proxies for ξ_{bt}.

To estimate (D.3), I assimilate $\frac{B_b}{S^*}$ to the change in interbank borrowing normalized by

TABLE A18. Estimation of the interbank market effect

	Change in net interbank borrowing				
	(1)	(2)	(3)	(4)	(5)
Credit demand shock	0.058**	0.209***	0.170***	0.155***	0.166***
	(0.023)	(0.043)	(0.033)	(0.034)	(0.039)
Time FE	✓	✓	✓	✓	✓
Bank FE					✓
Est. supply shock		✓			
Est. supply shock (pub/private)			✓	✓	✓
Add. controls				✓	✓
Observations	3896	3434	3423	3401	3363
R-squared	0.064	0.11	0.11	0.13	0.21

Note: This table reports the results of estimating equation (A36). The outcome variable is the bank-level change in net interbank lending normalized by lagged total assets. The main independent variable is the bank-level credit demand shock α^D_{bt} (defined above). Est. supply shock indicates that the estimated α^S_{bt} is included as a control. Est. supply shock (pub/private) indicates that the estimated α^S_{bt} separately estimated for firms and local governments is included as a control. Add. controls include the bank's lagged local government loan share, assets (log), equity ratio, and dummies for state-owned and foreign banks. Regressions are weighted by bank-level lagged corporate credit (top 0.5% winsorized). Standard errors are clustered at the bank level. ***, ** and * indicate significance at the 1%, 5% and 10% levels, respectively.

the banks' total assets, denoted ΔB_b.[48] I estimate

$$\Delta B_{bt} = \delta_t + \beta \alpha^D_{bt} + \varepsilon_{bt} \tag{A36}$$

I can include as controls the estimated α^S_{bt}, other bank-specific controls, and bank fixed-effects.

Results. The results are presented in Table A18. My preferred specification is column (5) which includes as controls the estimated bank-specific supply factors, estimated separately for bank lending to private firms and to local governments, the bank-level controls of my baseline specification (**??**), as well as bank fixed effects. The estimated coefficient is 0.17.

To recover $\mathcal{L}^{direct}(C^{corp})$, $\mathcal{L}^{direct}(K)$, and $\mathcal{L}^{direct}(Y)$, I can then simply divide their $\mathcal{L}^{Xsec}$ counterpart by ν.

Calibration of the general equilibrium feedback. κ^{GE} captures the general equilibrium feedback that uniformly affect all firms.

$$\kappa^{GE} = \frac{\frac{1}{\ell}\frac{1+\alpha\psi}{1-\alpha}}{\frac{1}{\ell}(1+\alpha(\sigma-1))} \frac{\epsilon^s - \lambda\epsilon^g + (1-\lambda)\frac{1}{\ell}(1+\alpha(\sigma-1))}{\epsilon^s - \lambda\epsilon^g + (1-\lambda)\frac{1}{\ell}\frac{1+\psi\alpha}{1-\alpha}} - 1$$

[48]In the model, net interbank borrowing is zero for all banks in the deterministic equilibrium, so B_b corresponds to the change with respect to the deterministic equilibrium. If we add bank equity to the model, the denominator is total assets and not total deposits.

κ^{GE} is increasing in labor supply elasticity ψ. Because the credit supply shock generates a fall in the wage, when labor supply is elastic, the direct effect of the credit shock is further amplified by a reduction in labor supply. κ^{GE} is decreasing in σ the elasticity of substitution across goods. The credit shock generates an increase in the cost of capital for exposed firms, so that the relative price of goods produced by exposed firms will tend to increase, triggering a reallocation of demand toward the least exposed firms. This general equilibrium effect dampens the direct effect. When $\frac{1+\alpha\psi}{1-\alpha} = 1 + \alpha(\sigma - 1)$, these two forces exactly cancel out and $\kappa^{GE} = 0$.

Calibrating κ^{GE} only requires to calibrate ψ, α, σ. χ and ℓ have previously been estimated. λ can be directly observed in the data. ϵ^s and ϵ^g do not need to be calibrated: only $\epsilon^s - \lambda\epsilon^g$ matters and can be backed out from the other parameters. This is a desirable feature: ψ, α and σ are common parameters for which the literature provides estimates.

Table A19 shows the value of κ^{GE} for various choices of ψ, α, and σ. I set the capital share α to 1/3. For the elasticity of substitution across goods, I report results for σ equal to 3, 5, and 6.5. For the elasticity of labor supply, Hall (2009) argues that a Frisch elasticity of 2 best captures the slope of the aggregate labor supply curve in a model without explicit treatment of the extensive margin. I also report results for ψ equal to 0.58 (following Chetty (2012)) and ψ equal to 0 (to completely mute the labor supply response).

For these parameter values, κ^{GE} varies from -14.8% to +7.0%. That is, $\mathcal{L}^{total}(C^{corp}) \in [0.85\mathcal{L}^{direct}(C^{corp}), 1.07\mathcal{L}^{direct}(C^{corp})]$. This suggests that the general equilibrium feedback on corporate credit is modest in magnitude. The relationship between direct and total effect is the same for capital.

For output, the aggregate output loss due to the reduction in aggregate inputs is equal to $\mathcal{L}^{total}(Y) = \alpha\mathcal{L}^{total}(K) + (1-\alpha)\mathcal{L}^{total}(L)$. The cross-sectional evidence does not reveal any effect on labor: $\mathcal{L}^{Xsec}(L) \approx 0$.[49] This raises the question of whether we want to account for the predicted fall in aggregate labor when estimating the output loss. To assess the sensitivity to this choice, I make two polar assumptions. In the case where $\psi = 0$, this channel is muted and the output loss does not allow for general equilibrium effects from labor supply in the estimation of the capital and output losses. In the case where $\psi > 0$, I assume that the aggregate labor shortfall is as predicted by the model, that is $\mathcal{L}^{total}(L) = \frac{\alpha\psi}{1+\alpha\psi}\mathcal{L}^{total}(K)$ (even if the measured cross-sectional effect is 0). This leads to a modified general equilibrium feedback parameter $\tilde{\kappa}^{GE}$ defined as $1 + \tilde{\kappa}^{GE} = (1 + \kappa^{GE})\frac{1+\psi}{1+\alpha\psi}$

Table A19 shows that the general equilibrium feedback allowing to go from $\mathcal{L}^{direct}(Y)$

[49] Note that this contradicts the prediction of the model in equation (A15), which suggests a negative effect cross-sectional on labor, due to capital-labor complementarities. The absence of cross-sectional effect may reveal that the drop in the marginal product of labor takes some time to materialize, even as investment falls, or may reflect the high degree of labor market frictions in France.

TABLE A19. Calibration of general equilibrium feedback

Parameter values									
σ	6.5	6.5	6.5	5	5	5	3	3	3
ψ	2	0.58	0	2	0.58	0	2	0.58	0
κ^{GE}	-2.5%	-10.2%	-14.8%	1.3%	-5.6%	-9.8%	7.0%	1.4%	-2.1%
$\tilde{\kappa}^{GE}$	75.4%	18.9%	-14.8%	82.4%	25.0%	-9.8%	92.5%	34.2%	-2.1%

Note: This table reports value of the general equilibrium feedback for values of the elasticity of substitution across goods σ and the labor supply elasticity ψ reported in the first two lines. κ^{GE} is the general equilibrium feedback for corporate credit and capital. $\tilde{\kappa}^{GE}$ is the general equilibrium feedback for output. These parameters are defined in the main text. A negative value of the general equilibrium feedback indicates the general equilibrium dampens the direct effect. In all cells, the capital share α is set to 1/3.

to $\mathcal{L}^{total}(Y)$ is the same as for capital is we assume labor supply is inelastic. If we allow labor to respond, we instead observe a large amplification, up to +92.5%.

While the general equilibrium feedback does vary substantially depending on the parameter choices, considering only the direct effect χ does not appear to substantially overstate the importance of crowding out in general equilibrium. To remain as close as possible to estimated moments, I thus consider the aggregate effect of crowding out to be captured by χ, and provide estimates that are likely to be on the conservative side.

D.4. Extensions

D.4.1. Adding multibank firms and bank-specific liquidity shocks

This section presents a version of the model with three additional features: (i) banks receive mean-zero bank-specific liquidity shocks; (ii) firms borrow from multiple banks; (iii) I allow for more general firm productivity shocks: firm productivity shocks may not aggregate to zero and there may be a correlation between firm-specific and bank-specific shocks. This extended model is useful to clarify the identification concerns that motivate my identification strategy.

I assume that banks receive bank-specific liquidity shocks ξ_b. The balance sheet constraint of banks becomes $C_b^{corp} + C_b^{gov} = S_b + B_b + \xi_b$.

I introduce multibank firms. I assume that each firm borrows from a set of banks denoted $\mathcal{B}_f$. The problem is analytically intractable for a generic firm-bank network. To obtain closed-form solutions, I assume that each bank lends to only one firm (as in Khwaja and Mian (2008)). That is, f borrowing from b in a singleton (instead of the [0, 1] continuum) and the sets $\mathcal{B}_f$ form a partition of the continuum of banks [0, 1].

Independent demand. I first solve the model when firms independently demand credit from their banks using an identical demand function. That is, firms do not substitute across banks. This is the assumption in Khwaja and Mian (2008). A firm is to be understood as a collection of f sharing the same productivity shock z_f. The demand for credit of firm f directed to bank $b \in \mathcal{B}_f$ remains given by (A20). Solving the model with these modified assumptions:

$$\hat{C}_{fb} = \upsilon z_f + \Upsilon Z^{corp} + \chi(\kappa^{GE} + 1 - \nu)\lambda Z^{gov} + \chi\nu\lambda Z_b^{gov} + \iota\nu\xi_b \tag{A37}$$

with $\upsilon = \frac{\sigma-1}{\ell}(1 + (1-\lambda)\chi\nu)$, $\Upsilon = \frac{\frac{1}{\ell}\frac{1+\psi}{1-\alpha}(\varepsilon^s - \lambda\varepsilon^g)}{\varepsilon^s - \lambda\varepsilon^g + \frac{1-\lambda}{\ell}\frac{1+\psi\alpha}{1-\alpha}} - \upsilon$, and $\iota = -\frac{\chi}{S^*}$.

The aggregate corporate credit change writes:

$$\hat{C}^{corp} = (\upsilon + \Upsilon)Z^{corp} + (1 + \kappa^{GE})\chi\lambda Z^{gov} \tag{A38}$$

We can also write the equation for net interbank borrowing:

$$\frac{B_b}{S^*} = (1-\nu)\left[\lambda(Z_b^{gov} - Z^{gov}) + (1-\lambda)\frac{\sigma-1}{\ell}(\tilde{Z}_b^{corp} - \tilde{Z}^{corp}) - \frac{1}{S^*}\xi_b\right] \tag{A39}$$

where $Z_b^{corp} = z_f$.[50] The notation $\tilde{Z} = \frac{\sigma-1}{\ell}Z$ rescales productivity shocks in corporate credit demand shocks.

Firms substitute across banks. I now assume that firms optimize on the allocation of their credit across banks. Loans from different banks are differentiated inputs with constant elasticity of substitution θ. In addition to the problem described above, firms solve:

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

The first-order condition writes:

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

Equation (A20) now corresponds to the demand for firm-level credit C_f. Bank-firm level credit demand is given by the log-linearized version of the equation above:

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

[50] In a more general model where each bank lends to several firms, we would have $Z_b^{corp} = \int_{f\in\mathcal{F}_b} z_f df$ with $\mathcal{F}_b$ the set of firms borrowing from b.

Let use define $Z_f^{gov} = \int_{b \in \mathcal{B}_f} Z_b^{gov} db$, $Z\xi_f = \int_{b \in \mathcal{B}_f} xi_b db$. Solving the model with these modified assumptions, we obtain:

$$\hat{C}_{fb} = \upsilon z_f + \Upsilon Z^{corp} + \chi(\kappa^{GE} + 1 - \nu)\lambda Z^{gov} + (\chi\nu - \tilde{\chi}\tilde{\nu})\lambda Z_f^{gov} + \tilde{\chi}\tilde{\nu}\lambda Z_b^{gov} + (\iota\nu - \tilde{\iota}\tilde{\nu})\xi_f + \tilde{\iota}\tilde{\nu}\xi_b \tag{A40}$$

$\tilde{\chi}$ and $\tilde{\nu}$ are defined analogously to χ and ν but with the elasticity of substitution across banks in place of the firm-level elasticity of credit demand: $\tilde{\chi} = \frac{-\theta}{\epsilon^s - \lambda\epsilon^g + (1-\lambda)\theta}$, $\tilde{\nu} = \frac{\epsilon^s - \lambda\epsilon^g + (1-\lambda)\theta}{\epsilon^s - \lambda\epsilon^g + (1-\lambda)\theta + \frac{1}{\phi S^*}}$. $\tilde{\iota}$ is given by $\tilde{\iota} = -\frac{\tilde{\chi}}{S^*}$.

Firm-level corporate credit is given by:

$$\hat{C}_f = \upsilon z_f + \Upsilon Z^{corp} + \chi(\kappa^{GE} + 1 - \nu)\lambda Z^{gov} + \chi\nu\lambda Z_f^{gov} + \iota\nu\xi_f \tag{A41}$$

The aggregate corporate credit change writes as before:

$$\hat{C}^{corp} = (\upsilon + \Upsilon)Z^{corp} + (1 + \kappa^{GE})\chi\lambda Z^{gov} \tag{A42}$$

These derivations yield several insights. When firms can substitute across banks, the within-firm specification provides an estimate of $\tilde{\chi}\tilde{\nu}$ (as opposed to $\chi\nu$). If $\theta > -\epsilon^c$ (loans from different banks are highly substitutable), then $\tilde{\chi}\tilde{\nu} < \chi\nu \leq 0$. In this case, the estimate in the within-firm specification overestimates the firm-level effect. The coefficient of the firm-level relationship is $\chi\nu$ and is the same as that of the firm$\times$bank-level relationship when firms do not substitute across banks. It is the relevant coefficient to perform the aggregation exercise, since the aggregate effect depends on χ (as opposed to $\tilde{\chi}$). The coefficient of the firm-level relationship remains a lower bound on the direct effect when we allow for substitution across banks.

Appendix E. Details on the TFP loss derivation

E.1. Framework

I consider a multi-sector version of the model presented in Appendix D. Consumers consume an aggregate output of S sectors $Y = \prod_s Y_s^{\theta_s}$. Production in each sector corresponds to the model in Appendix D, where we allow for industry-specific capital shares α_s. In this model, the marginal cost of capital for firm f in industry s borrowing from bank b is $r_{fsb} = r_b^c$. To use the framework most common in the misallocation literature, I decompose the firm-specific interest rate into a common component and a mean-zero wedge. Omitting the b subscript, I denote $r_{fs} = r(1+\tau_{fs}^K)$.[51] The firm's first-order conditions write

$$\mathrm{MRPK}_{fs} = \frac{\sigma - 1}{\sigma}\alpha_s \frac{P_{fs}Y_{fs}}{K_{fs}} = r(1+\tau_{fs}^K)$$
$$\mathrm{MRPL}_{fs} = \frac{\sigma - 1}{\sigma}(1-\alpha_s)\frac{P_{fs}Y_{fs}}{L_{fs}} = w$$

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

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

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

$$\log \mathrm{TFP}_s = \log \mathrm{TFP}_s^* - \frac{\alpha}{2}(1+\alpha_s(\sigma-1))\,\mathrm{Var}(\tau_{fs}^K)$$

where the variance is taken over all firms within each sector and $\mathrm{TFP}_s^* = (\sum A_{fs}^{\sigma-1})^{\frac{1}{\sigma-1}}$. I used the approximation $\mathrm{Var}(\log(\mathrm{MRPK}_{fs})) = \mathrm{Var}(\log(1+\tau_{fs}^K)) = \mathrm{Var}(\tau_{fs}^K)$.

[51] In the model, $\tau_{fs}^K = \phi B_b$ and all the wedge is explained by bank's heterogeneous exposure to credit demand shocks. The TFP loss derivation is very general and holds for any distortion in firm-level actual or allocative price.

E.2. Baseline quantification

Measurement of wedges. Nominal output $P_{fs}Y_{fs}$ is defined as value added (gross sales minus intermediate input costs). The capital stock is defined as the value of fixed assets, net of depreciation. MRPK is then defined as $\text{MRPK}_{fs} = \alpha_s \frac{P_{fs}Y_{fs}}{K_{fs}}$. To obtain α_s, I estimate industry-specific Cobb-Douglas production functions at the 2-digit level using the cost shares method, as in Osotimehin (2019) and Blattner, Farinha, and Rebelo (2020). Namely, I define the labor share as the ratio of sectoral labor compensation over value added.

Descriptive evidence on firm-level wedges Before turning to the quantification of the TFP loss, I present descriptive statistics on firm-level wedges. A key assumption in the TFP loss computation is that wedges capture firm-level distortions or frictions that prevent firms from using the optimal amount of inputs. In practice, Table A20 shows that firms with higher wedges tend to be smaller, to have a lower tangibles ratio, and to be less leveraged, suggesting that wedges partly reflect financing frictions that constrain firms' input acquisition decisions. I also find that high-wedge firms are more profitable, in line with the idea that they have higher marginal products of inputs due to constraints.

TABLE A20. Who are the high-wedge firms?

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

Baseline estimation of the TFP loss. To obtain counterfactual wedges, I estimate the following regression:

$$\Delta\tau^K_{ft} = \beta_0 \mathit{FirmExposure}_{ft} + \beta_1 \mathit{FirmExposure}_{ft} \times \mathbb{1}[\mathit{High}\ \tau^K_{f,t-1}] + \Phi \cdot \mathbf{X}_{ft} \otimes \mathbb{1}[\mathit{High}\ \tau^K_{f,t-1}] + \varepsilon_{ft}$$

The outer product denotes that I include all interacted and non-interacted terms. I define $\hat{\tau}^K_{ft} = \tau^K_{f,t-1} + \hat{\Delta\tau}^K_{ft}$ where $\hat{\Delta\tau}^K_{ft}$ is the fitted value from the regression. $\hat{\tau}^K_{ft} - \tau^K_{ft}(0) = \hat{\beta}_0 \mathit{FirmExposure}_{ft} + \hat{\beta}_1 \mathit{FirmExposure}_{ft} \mathbb{1}[\mathit{High}\ \tau^K_{f,t-1}]$ yields $\tau^K_{ft}(0)$.

The TFP loss is then given by:[52]

$$\mathcal{L}(\text{TFP}_t) = -\frac{\alpha}{2}(1 + \alpha_s(\sigma - 1))[\text{Var}(\hat{\tau}^K_{ft}) - \text{Var}(\tau^K_{ft}(\mathbf{0}))] \tag{A43}$$

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

I compute the TFP loss for each industry and aggregate across industries using industry shares in value added. In the baseline computation, I use $\sigma = 3$ to provide a conservative quantification of the TFP loss. This leads to the TFP loss presented in the main text.

Using an elasticity of substitution across goods $\sigma = 5$ closer to usual estimates, the TFP loss is instead equal to €0.08 per €1 of local government loans.

Discussion. This computation is subject to several caveats. First, Hsieh and Klenow (2009)'s framework only quantifies the losses from misallocation within industries, a limitation common to most of the misallocation literature. Since the shock under study causes a reallocation of inputs both within and across industries, within-industry misallocation is likely a lower bound on the total misallocation effect. Second, the previous computation is correct under the assumption that $\log(\text{TFP}_t^*)$ is unaffected by the shock. This assumption would be violated if the shock affects firm-level productivity A_{ft}. Unfortunately, this cannot be tested in the absence of data on firm-level product quantities.[53] Since there is no strong theoretical prior for expecting credit frictions to affect A_{ft}, this assumption is reasonable. Third, measurement error in wedges is a prevalent issue in the misallocation literature. Attributing all cross-sectional dispersion in the observed marginal returns to misallocation may overstate the extent of misallocation. However, focusing on *within firm* changes in wedges largely alleviates this concern (Bau and Matray, 2023).

Section E.4 presents robustness exercises on this quantification. They provide estimates consistent with my baseline quantification.

E.3. Segmentation across banks vs. heterogeneous effect of the shock

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

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

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

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

I find that the increase in misallocation is entirely driven by heterogeneous firm-level effects. Segmentation has an economically negligible effect (€0.002 per €1 of local government loans). In addition, the heterogeneous effects channel is potent not because banks selectively cut credit to high-wedge firms, but because high-wedge firms are more sensitive to a given credit cut. This decomposition is important for two reasons. First, even if the credit cut is not larger for firms with high marginal products of capital, the fact that high marginal product-constrained firms tend to experience a larger reduction in capital from a given reduction in credit can induce a large misallocation effect.[54] Second, the aggregate cost of the distributive effects induced by bank segmentation is negligible.

E.4. Robustness

Estimation of the TFP loss accounting for labor wedges My baseline quantification does not account for the potential presence of labor wedges. Let us denote τ^L_{fs} the labor wedge and τ_{fs} the average of the capital and labor wedges $\tau_{fs} = \alpha\tau^K_{fs} + (1-\alpha)\tau^L_{fs}$. In the presence of labor wedges, equation 7 becomes:

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

To quantify the effect of crowding out on allocative efficiency, I additionally estimate the effect of firm exposure to crowding out on the labor wedge, and then use the methodology described above. I find that considering the labor wedge leads to a very small increase in the TFP loss due to crowding out: the multiplier goes from -0.05 to -0.07. This is consistent with the absence of strong effects of crowding out on firm-level employment.

Alternative quantification based on Sraer and Thesmar (2020). My TFP loss computation is exact only under the functional form restriction on the effect of *FirmExposure* on wedges implied by my empirical specification. I provide an alternative quantification of

[54] This complements Banerjee et al. (2019) who find that a credit expansion program that uniformly targets the population induces misallocation when the returns to credit are larger for more constrained entrepreneurs. Similarly, Bau and Matray (2023) find that foreign capital liberalization reduces misallocation because it generates a larger reduction in wedges for high-wedge firms. In contrast, Blattner, Farinha, and Rebelo (2020) quantify misallocation induced by a credit shock concentrated on high-wedge firms.

the TFP loss relying on the same framework but using the alternative estimation strategy proposed in Sraer and Thesmar (2020). This method directly estimates the effect of the shock on the moments of interest, by comparing changes in the mean wedge, the variance of the wedge and the covariance between the wedge and sales, across exposed (treated) and non-exposed (control) firms. To compute these moments, I discretize the treatment by defining 20 quantiles of *FirmExposure*, indexed by q. For each date$\times$industry$\times$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$\times$industry$\times$quantile level, taking the average of $FirmExposure_{ft}$ and firm-level controls $\mathbf{X}_{ft}$. I estimate the following regression:

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

It is important to include the average of the firm-level controls since the orthogonality condition that supports the causal interpretation of β is conditional on these controls. The fixed effects of the baseline regression cannot be absorbed here. To circumvent this issue, I run the firm-level specification with $\Delta\tau^K_{ft}$ as outcome, store the estimated fixed effects, take their average by date$\times$industry$\times$quantile and use these as controls. By construction, estimating this regression with $\Delta\mu(qst)$ as 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. I find that crowding out reduces allocative efficiency, and through this channel, aggregate output by €0.07 per euro of local government loans. Hence, the two quantification strategies yield similar magnitudes.

Appendix F. Data

This paper uses data collected from Banque de France. The data was accessed through the Banque de France virtual Open Data Room.[55]

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

Credit registry. For corporate credit, I focus on borrowers located in mainland France. I exclude borrowing by the finance, insurance, and real estate sector. This is to exclude inter-bank lending and lending to real estate investment trusts. I exclude lending to holding companies. I exclude legal forms implying public-private partnerships as well an non-standard legal forms (e.g. non-profits, foundations, unions, etc.). Finally, I exclude sole proprietorships due to a change in the reporting of these loans in the credit registry in 2012.

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

I define credit as total credit with initial maturity above 1 year (variable *Tot MLT* in the credit registry).

I classify entities as local government entities or private corporations based on their legal status (4xxx and 7xxx). All other entities (after applying the filters described above) are considered private corporations. Unless stated otherwise, all locations correspond to

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

the geographical identifier of the borrower. The credit registry provides the location at the commune level. Based on this information, I assign each borrower to a given municipality and region, using time-invariant commune-to-municipality and commune-to-region mappings. I use regions before the 2015 redistricting.

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 obtained through the data room has only broad balance sheet aggregates. To build consistent time series, I predict the 2018 *BAFI* variables using the corresponding item in *SURFI*. To avoid having missing values for my control variables, I interpolate the *BAFI* time series in case of missing values.

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