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

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I study the financial 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 a €1-increase in local government borrowing from a bank reduces that bank's corporate credit by €0.5, and lowers investment for its borrowers. Combining these reduced-form effects and a model, crowding out causes an aggregate output shortfall equal to €0.2 per €1-increase in local government bank debt. My results show that constraints on financing supply reduce the stimulus effect of debt-financed government spending.

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1. Introduction

Increasing levels of government debt may adversely affect the private sector via a financial crowding out effect. As per the standard theory (Diamond 1965; Friedman 1972), if the supply of loanable funds is imperfectly elastic, an increase in governments' demand for debt will reduce the supply of debt to firms, hindering corporate investment and output. While the extent and determinants of financial crowding out are essential inputs for fiscal policy, empirical evidence of crowding out remains scarce (see Hubbard 2012 for a review). This is due to severe identification challenges. First, government debt reacts endogenously to economic conditions. Second, even exogenous shocks to government debt may affect firms via other channels than crowding out, for instance via any stimulus effect of debt-financed government spending on aggregate demand.

In this article, I quantify the crowding out effect of local government bank debt on corporate credit, investment, and output. I focus on France over 2006-2018, exploiting rich credit registry data covering bank loans to firms and local governments. This empirical setting is interesting for two reasons. First, local government bank debt is large and growing: in developed and emerging countries, local government debt-to-GDP increased from 11% to 22% over 1990-2019, and 80% of this debt consists of bank loans.[1] Second, I can exploit plausibly exogenous variation in local government lending across banks to isolate financial crowding out, solving the key identification challenge in this literature.

I first document a crowding out effect in the cross-section of banks: a €1-increase in demand for local government debt directed to a bank reduces that bank's corporate credit supply by €0.5, and lowers investment for its corporate borrowers. I then show that crowding out is more severe for banks with tighter credit supplies. Finally, combining the estimated cross-sectional effects and a model, I find that a €1-increase in local government loans reduces aggregate output by €0.2 via crowding out. This is the output shortfall when €1 of local government debt is financed by banks, compared to a counterfactual where this €1 is financed by an outside investor with a perfectly elastic supply of funds. The counterfactual keeps constant government spending and debt, and thus all their other effects, to only quantify the negative effect attributable to financial crowding out.

This article makes two contributions. First, I quantify financial crowding out in the

[1] See Figure A.1. Note that the United States' large reliance on local government bonds is an exception.

case of local government bank debt. This is an important finding given the surge in local government debt. This is also the first such quantification for any type of government debt, identification having proven elusive for central government debt. Second, by showing that crowding out is more severe when lending banks' credit supply is less elastic, I test and confirm the standard crowding out theory. A general implication is that, in segmented financial markets, who governments borrow from affects the transmission of fiscal policy and the size of debt-financed fiscal multipliers.[2]

I exploit bank lending to French local governments as an empirical setting.[3] 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 bank causes a larger reduction in that bank's corporate credit. My research design focuses on multi-bank firms (30% of firms accounting for 70% of corporate credit) and examines whether a given firm experiences lower credit growth from banks exposed to higher demand for local government loans. 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-specific demand for local government loans.

This design yields the relative crowding out parameter under two identifying assumptions. First, any residual firm$\times$bank demand effect not absorbed by the firm fixed effects must be orthogonal to the bank-level local government debt demand shocks I construct. Second, these bank-level shocks must be orthogonal to other bank-level determinants of credit supply. I run a large number of tests and find support for these assumptions.

[2]An additional implication of my results is that *debt*-financed fiscal multipliers will be lower than the *transfer*-financed multipliers estimated in most of the recent literature on this topic (see literature review).

[3]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.

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 and economically large.[4] Local projections suggest 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, suggesting that the extent of crowding out is orthogonal to political interference.

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 occurs through both a reduction in quantities and a (small) increase in interest rates.

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 only compare firms located in the same municipality×industry×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 control for firm fixed effects and for an estimate of firm-level demand shocks obtained from the within-firm specification.[5] 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 €0.30 reduction in investment for firms borrowing from that bank in the same year. Local projections suggest that this reduction in the capital stock is permanent. These effects are heterogeneous across firms, with more financially constrained firms exhibiting higher credit-to-investment sensitivities.

How does crowding out affect aggregate corporate credit, investment, and output? That is, what is the aggregate output shortfall relative to a counterfactual in which the increase

[4]The magnitude is in line with existing evidence on banks' constraints, e.g., Paravisini (2008) or Drechsler, Savov, and Schnabl (2017).

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

in local government debt has no crowding out effect, for instance because it is financed by an outside investor with a perfectly elastic supply of funds?

The *relative* effects documented so far do not add up to the *aggregate* effect because they ignore any equilibrium effect on non-exposed banks and firms: this is the so-called "missing intercept" problem. 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 segmented. I study the equilibrium response of corporate credit, investment, and output 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 is a spillover effect due to capital mobility across banks. Unless banks are fully segmented, banks exposed to the local government debt demand shock draw in capital from non-exposed banks, which also reduce their corporate credit supply. This effect can be quantified by estimating the effect of credit demand shocks on interbank capital flows. The second term captures a general equilibrium feedback due to substitution across products and a labor supply response. I calibrate this term and find that for plausible parameter values it either 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 a €1-increase in local government loans reduces aggregate output by €0.2 via financial crowding out.[6] This reveals a substantial cost of the long-run increase in local government debt. It also implies that crowding out impedes the stimulus effect of debt-financed local government spending. Namely, the output multiplier of such spending would be higher by 0.2 absent crowding out. This is a large effect, typical debt-financed multiplier estimates ranging from 0.5 to 1.9 (Ramey 2019).

There are two policy implications of my findings. First, financial crowding out should be taken into account by policymakers making debt decisions. It may be especially prob-

[6]This quantification accounts for the effect of crowding out on output via the change in aggregate corporate credit and investment. In an Online Appendix, I additionally investigate how the distributive effects of crowding out affect allocative efficiency, and find a small negative effect on aggregate TFP.

lematic during crises, when government debt tends to soar while financial intermediaries are constrained. Second, in segmented financial markets, the sources of government borrowing will affect the transmission of fiscal policy and the size of debt-financed multipliers. To minimize crowding out, government should issue debt in "deep" and elastic markets.

Related literature. This work contributes to three strands of the literature. First, I contribute to the literature on government debt crowding out corporate financing and investment (see Hubbard 2012; Murphy and Walsh 2022 for reviews). 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. A recent contribution by Broner et al. (2022) shows that, across countries, fiscal multipliers are increasing in the share of government debt held by foreigners, which is suggestive of financial crowding out. Relative to this literature, the main contribution of this article is to identify a causal financial crowding out effect and to provide a quantification of the aggregate output shortfall that can be attributed to the financial crowding out channel.[7] Closer to my empirical setting, recent papers study the effect of bank loans to local governments on corporate credit and investment: Huang, Pagano, and Panizza (2020) in China, Morais et al. (2021) in Mexico, and Hoffmann, Stewen, and Stiefel (2022) in Germany. However, they focus on developing countries or state-owned banks and political interference.[8] In addition, they only consider micro-level effects.

Second, 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 (e.g., Cohen, Coval, and Malloy 2011; Chodorow-Reich et al. 2012; Nakamura and Steinsson 2014; Corbi, Papaioannou, and Surico 2019; see Chodorow-Reich 2019; Ramey 2019 for reviews). Transfer-financed multipliers are approximately equal to debt-financed multipliers in

[7] Some articles test the refinement of the crowding out hypothesis by Friedman (1978) which posits that government debt affects the relative prices of 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 2019), short-term debt in the financial sector (Krishnamurthy and Vissing-Jorgensen 2015), maturity (Greenwood, Hanson, and Stein 2010), but have no direct implication for investment and output.

[8] It is difficult to extrapolate from studies of state-owned banks. State-owned banks typically account for a small share of credit. They have a different objective function. In addition, bank lending to local governments due to political pressure has different implications for banks' health if they are pressured to hold risky debt (Acharya, Drechsler, and Schnabl 2014; Ongena, Popov, and Van Horen 2019) or make losses on lending to governments (Hoffmann, Stewen, and Stiefel 2022).

the case where government debt does not cause financial crowding out, for instance if financed by an outside investor with a perfectly elastic supply of funds.[9] My results imply that, when the supply of debt is imperfectly elastic, debt-financed multipliers will be lower than transfer-financed multipliers. They also complement the few estimates of debt-financed multipliers, from aggregate (e.g., Mountford and Uhlig 2009) and cross-sectional data (Clemens and Miran 2012; Adelino, Cunha, and Ferreira 2017; Dagostino 2018).

Third, this article 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). This work is closest to articles showing how one segment of banks' loan portfolio may crowd out another one: Chakraborty, Goldstein, and MacKinlay (2018) and Martín, Moral-Benito, and Schmitz (2021) (mortgages crowding out commercial loans), and Greenwald, Krainer, and Paul (2023) (credit line drawdowns crowding out term loans). I contribute to this literature by documenting how banks' funding constraints affect the transmission of bank-financed fiscal policy. In addition, methodologically, I develop a simple framework to map cross-sectional effects on credit into aggregate effects using one additional moment related to capital flows across banks, complementing the approaches in Chodorow-Reich (2014), Herreño (2021), and Mian, Sarto, and Sufi (2022).

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 contraction in corporate credit. For firms, crowding out is akin to a shift in banks' residual credit supply curve. This mechanism is depicted on the supply and demand graph in Figure A.2. 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 textbook 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.

[9] Chodorow-Reich (2019) shows that—in a model without capital markets where financial crowding out does not occur—the transfer-financed multiplier is equal to the debt-financed multiplier plus the effect of the wealth transfer, and that the latter is quantitatively negligible.

In this article, I quantify financial crowding out as the output shortfall due to a 1€-increase in local government bank debt, compared to a counterfactual where government spending, taxes, and debt are the same, but banks do not absorb this 1€-increase in debt because it is financed by an outside investor with a perfectly elastic supply of funds. To fix ideas, let us write output $Y = Y(G, T, D^g, C^g)$ as a function of government spending G, taxes T, government debt D^g, and government bank credit C^g. Government debt can be financed by banks or by an outside investor: $D^g = C^g + O^g$. Totally differentiating Y, the effect of a change in government bank credit $\mathrm{d}C^g$ is given by:

$$\frac{\mathrm{d}Y}{\mathrm{d}C^g} = \frac{\partial Y}{\partial G}\frac{\mathrm{d}G}{\mathrm{d}C^g} + \frac{\partial Y}{\partial T}\frac{\mathrm{d}T}{\mathrm{d}C^g} + \frac{\partial Y}{\partial D^g}\frac{\mathrm{d}D^g}{\mathrm{d}C^g} + \frac{\partial Y}{\partial C^g} \tag{1}$$

The first three terms correspond to the output response to the changes in government spending, taxes, and debt induced by $\mathrm{d}C^g$. This response captures the "real" effects of fiscal policy.[10] This response would be unchanged if the same changes in spending $\mathrm{d}G$, taxes $\mathrm{d}T$, and debt $\mathrm{d}D^g$ were financed by outside debt $\mathrm{d}O^g$. The last term is the additional effect that occurs when governments borrow from imperfectly interest-elastic banks and compete funds away from firms. This last term is the financial crowding out effect. It constitutes the object of interest in this article.[11]

To quantify financial 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—i.e., frictions prevent capital from flowing across banks and firms from switching banks—crowding out has a bank-specific dimension: a larger increase in demand for local government debt directed to one bank leads to a larger drop in that bank's corporate credit supply, and in investment for firms borrowing from that bank. The hypothesis that banks are segmented is testable: if false, there will be no relative effect. While this relative effect is conceptually different from the aggregate effect, it is useful for two reasons. First, it uniquely allows to isolate financial crowding out from the other endogenous relationships between local government debt and corporate outcomes. A non-zero relative effect suffices

[10] The first term is the effect of the change in spending G. Theoretical predictions for this term vary across models. In neoclassical models, G has a *real* crowding out effect: independently of financing, if production factors are fully employed, government consumption can only be at the expense of private consumption. Hence, $\frac{\partial Y}{\partial G} < 1$. In New Keynesian models, G can stimulate aggregate demand. Under some conditions, $\frac{\partial Y}{\partial G} > 1$. The second and third terms are the partial effects of a change in taxes T and debt D^g, and can be non-zero if Ricardian equivalence does not hold. Balanced government budget implies $\mathrm{d}G = \mathrm{d}T + \mathrm{d}D^g$.

[11] This definition of financial crowding out is in the spirit of Diamond (1965).

to reject the null hypothesis that crowding out does not occur. Second, the well-identified relative effect is a highly informative statistics to quantify the aggregate 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.[12] There are 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. These firms account for 63% of total value added by non-financial corporations in the national accounts. 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", 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

[12] 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 solvency, all matters related to business operations, risk management, or supervision operate at the level of individual banks.

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_{bt}^{gov} (€K)	1,009,691	1,436,820	3,764	573,756	3,224,721	909,389	1,458,677	334	244,738	2,961,459
Total loans C_{bt}^{tot} (€K)	6,858,081	9,896,209	716,905	2,745,473	28,901,204	6,905,833	10,139,973	358,033	2,642,636	29,818,668
Observations	8,773,498					2,731,110				

Panel B: Firm-level variables

	mean	sd	p10	p50	p90
Credit growth ΔC_{ft} (MPGR)	0.070	0.81	-0.65	-0.15	1.55
Credit growth ΔC_{ft} (std)	0.11	0.96	-0.51	-0.16	0.98
Outstanding credit C_{ft} (K€)	282.5	385.9	17.7	116	842.7
Firm Exposure $FirmExposure_{ft}$ (%)	0.57	1.25	-0.15	0.095	2.15
Capital growth	0.035	0.31	-0.21	-0.026	0.36
Employment growth	0.018	0.16	-0.14	0	0.20
Fixed assets (K€)	667.3	933.0	57	301	1,716
Value added (K€)	1,090.7	1,352.1	242	628	2,364
Nb. employees	20.9	23.8	5	13	45
Wage bill (K€)	591.1	717.0	127	350	1,260
Assets (K€)	2,298.5	3,177.0	437	1,160	5,235
EBIT/Sales	0.044	0.073	-0.013	0.033	0.12
Debt/Assets	0.65	0.23	0.36	0.65	0.91
EBIT/Interests	19.7	41.3	-2.57	6.83	58.3
Observations	815,425				

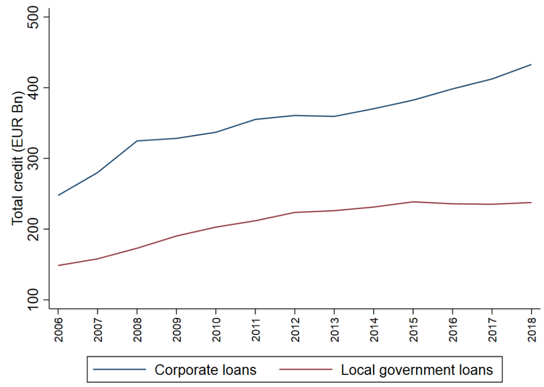
Note: This table reports the summary statistics of the relationship-specific (panel A), and firm-specific (panel B) variables used in the analysis. Credit growth is defined either as the mid-point growth rate (MPGR) or the standard growth rate (std). Multibank firms refers to firms with at least two 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.

lending markets: the average bank branch has 72% of its corporate lending and 86% of its local government lending going to borrowers located in the same municipality.

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. Loans to government entities have grown at an average rate of 4% per annum in 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

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. Details on data source and filtering are in Section 3 and Appendix F.

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 article, 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%).[13] These local governments are scattered on the French territory and take their lending decisions in a decentralized manner. I aggregate local government loans at the municipality level by summing credit amounts for all local governments located in a municipality.

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 loans benefit from an implicit guarantee of the central government, limiting their credit risk. 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

[13]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.

(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.

French banks. The size distribution of French banks is highly skewed, with a large number of mid-sized banks and a few very large banks. The market is split between national and local banks (defined as banks operating in less than 20% of municipalities), with the latter accounting for 44% of corporate credit. Most banks lend 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. Figure A.3 displays these facts.

4. Bank-level effect on corporate credit

4.1. Empirical strategy

The goal is to identify the "across banks" relative crowding out effect, defined as the causal effect of a bank-specific change in demand for local government loans on bank-level corporate credit supply. To do so, I estimate the following baseline specification:

$$\Delta C_{fbt} = d_{ft} + \beta BankExposure_{bt} + \Phi \cdot \mathbf{X}_{bt} + \varepsilon_{fbt} \tag{2}$$

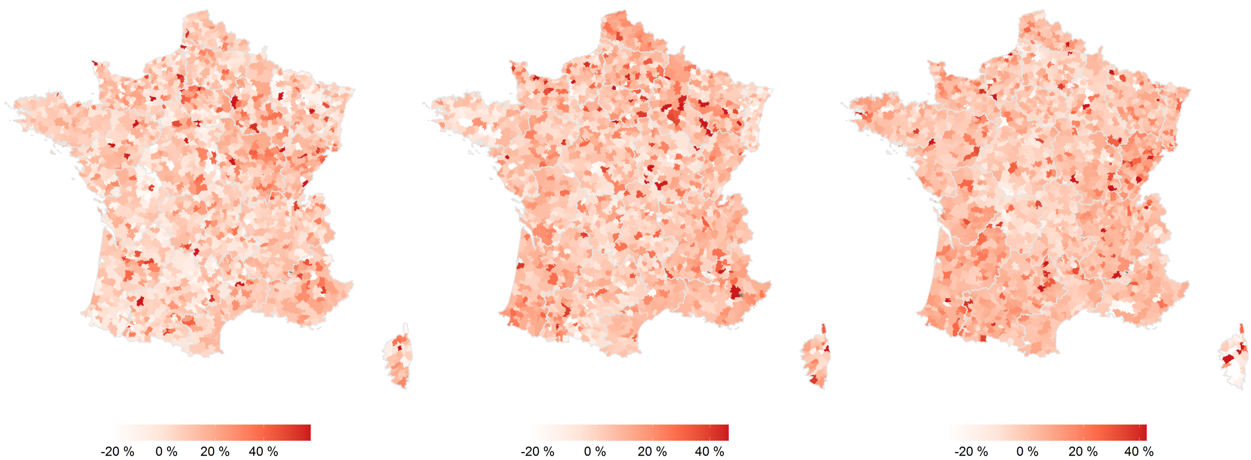
where f indexes firms, b indexes banks, and t indexes time in years. ΔC_{fbt} is bank$\times$firm-level credit growth. I define ΔC_{fbt} as the mid-point growth rate $\frac{C_{fbt}-C_{fbt-1}}{0.5(C_{fbt}+C_{fbt-1})}$ to account for both the intensive and extensive margins (Davis and Haltiwanger 1992). d_{ft} is a firm$\times$time fixed effect. $\mathbf{X}_{bt}$ is a vector of controls.

BankExposure$_{bt}$ proxies for the demand for local government loans 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. Following the approach in Amiti and Weinstein (2018) and Greenstone, Mas, and Nguyen (2020), I first estimate an equation that decomposes equilibrium local government credit growth into municipality and bank components:

$$\Delta C^{gov}_{mbt} = \alpha^{gov}_{mt} + \alpha^{gov}_{bt} + \varepsilon_{mbt} \tag{3}$$

FIGURE 2. Local government debt demand shocks by municipality

(a) 2008 (b) 2012 (c) 2016



Note: These maps depict the municipality-level parameters $\hat{\alpha}^{gov}_{mt}$ estimated from equation (3), for three dates in my sample. Regional boundaries appear in white.

ΔC^{gov}_{mbt} is the mid-point growth rate of credit extended by bank b to local governments 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 (Beaumont, Libert, and Hurlin 2019).[14] The bank fixed effects α^{gov}_{bt} measure the variation in banks' lending that is common across municipalities, like bank-specific credit supply factors. Similarly, the municipality fixed effects α^{gov}_{mt} measure the change in credit explained by municipality factors, like municipalities' credit demand. Amiti and Weinstein (2018) show that this procedure yields estimates of structural bank supply and borrower demand shocks for a wide class of models of lending behavior. For my purpose, the key advantage of this procedure is that the parameters α^{gov}_{mt} are estimates of municipality-level credit growth that are purged of municipalities' differential exposure to bank-level shocks. The maps in Figure 2 show the estimated $\hat{\alpha}^{gov}_{mt}$ for three dates and display a lot of variation across municipalities and within municipality across time, reflecting the lumpy nature of local government capital expenditure.

I then use the estimated municipality fixed effects $\hat{\alpha}^{gov}_{mt}$ to construct a bank-level local

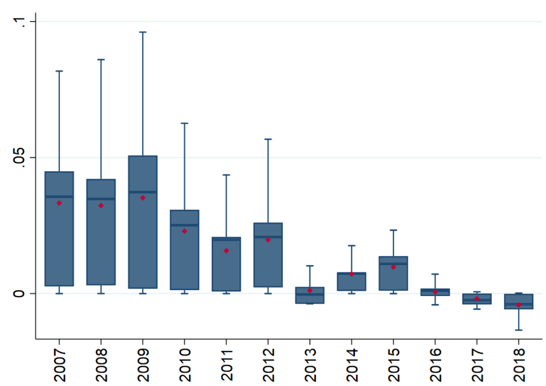
[14] Namely, $\Delta C^{gov}_{bt} = \hat{\alpha}^{gov}_{bt} + \sum_m w(m)_{bt}\hat{\alpha}^{gov}_{mt}$ where $w(m)_{bt}$ is the weight of municipality m in bank b credit ; $\Delta C^{gov}_{mt} = \hat{\alpha}^{gov}_{mt} + \sum_b w(b)_{mt}\hat{\alpha}^{gov}_{bt}$ where $w(b)_{mt}$ is the weight of bank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

government loan demand shifter:

$$(4) \qquad BankExposure_{bt} = \sum_m \omega^{gov}_{bm,t-1} \times \hat{\alpha}^{gov}_{mt} \quad \text{with} \quad \omega^{gov}_{bm,t-1} = \frac{C^{gov}_{bm,t-1}}{C^{tot}_{b,t-1}}$$

$\omega^{gov}_{bm,t-1}$ is bank b's exposure to local government credit in municipality m relative to its total credit.[15] *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 *BankExposure* across banks can equivalently be understood in terms of variation in municipality-level market shares across banks.[16] The exposure weights $\omega^{gov}_{bm,t-1}$ sum to banks' local government loan share $\lambda^{gov}_{b,t-1} = \frac{C^{gov}_{b,t-1}}{C^{tot}_{b,t-1}}$ which is always included as a control (as recommanded by Borusyak, Hull, and Jaravel 2022). Figure 3 plots the distribution of *BankExposure* by year.

FIGURE 3. Bank exposure to local government debt demand shocks



Note: This figure shows the distribution of $BankExposure_{bt}$ (defined in (4)) by year. The bars indicate the median and the interquartile range. The whiskers indicate the 10th and 90th percentiles. The red dot indicates the mean. Statistics are weighted by credit.

[15] 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.

[16] To see this, define $\hat{d}C^{gov}_{mt} = \hat{\alpha}^{gov}_{mt} \times C^{gov}_{m,t-1}$, akin to the predicted municipality-level euro change in demand, and $\tilde{\omega}^{gov}_{mb,t-1} = C^{gov}_{bm,t-1}/C^{gov}_{m,t-1}$, the market share of bank b in municipality m. We can rewrite $BankExposure_{bt} = \frac{1}{C^{tot}_{b,t-1}} \sum_m \tilde{\omega}^{gov}_{mb,t-1} \times \hat{d}C^{gov}_{mt}$: the amount $\hat{d}C^{gov}_{mt}$ 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.

4.2. Identifying assumptions

The goal is to identify the relative crowding out effect β. My empirical design is meant to address two main threats to identification that arise in this setting.[17] This design will be valid if the standard orthogonality condition is satisfied:

$$\mathbb{E}[BankExposure_{bt}\varepsilon_{fbt}|\mathbf{X}_{fbt}, d_{ft}] = 0 \tag{A1}$$

Correlated firm-level credit demand shocks. The first hurdle 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×time fixed effects. Any firm-level demand shock that is symmetric across lenders will be absorbed by the fixed effects (Khwaja and Mian 2008). This design relies on the fact that the aforementioned confounding channels predict a correlation between local government debt and *firm*-level credit *demand*, while crowding out uniquely operates as a shock to the *bank*-specific *supply* of credit, which depends on the bank-level demand for local government loans. Hence, the within-firm design allows to estimate the effect of bank exposure to local government loan demand, holding other effects of government debt constant. This corresponds to the (bank-level) crowding out effect, as defined in equation (1).

This design assumes that firm demand shocks are symmetric across lenders (sufficient condition), or that any residual firm×bank demand shock not absorbed by the firm fixed effects is orthogonal to *BankExposure* (necessary condition). How plausible is this assumption? I focus on credit with initial maturity above one year, a relatively homogeneous loan category, which makes this assumption less demanding (Ivashina, Laeven, and

[17] Model equation (D.24) in Appendix D.4.1 formalizes these identification concerns.

Moral-Benito 2022). Regressing firm-bank credit growth on firm×time fixed effects yields an adjusted R-squared of 28%, showing that firm effects explain a sizable share of the variation in credit flows (Table A.1). Adding bank×time fixed effects increases the adjusted R-squared by only 6%. Section 4.3.2 presents additional tests supporting this assumption.

Correlated bank-level credit supply shocks. Estimating β presents a second endogeneity issue: lending to local governments and corporates are jointly determined in banks' optimization problem and may be correlated. For instance, a bank-level liquidity shock will affect its lending to both local governments and firms. Banks may also decide to rebalance their portfolio away from firms and into local governments. This is the rationale for using the demand shifter *BankExposure*, as opposed to realized bank-level local government loan growth, as an explanatory variable. $\mathit{BankExposure}_{bt}$ shifts the realized quantity, but the shift-share structure combined with the Amiti-Weinstein shocks is designed to purge $\mathit{BankExposure}_{bt}$ from bank b's supply factors that may also enter the residual ε_{fbt}.[18]

Can this design ensure that assumption (A1) holds? The main threat to identification is if banks sort across municipalities such that banks with negative corporate credit supply shocks systematically have high market shares in high local government debt demand municipalities (Borusyak, Hull, and Jaravel 2022).[19] The most direct test supporting assumption (A1) is bank-level balance on observables. Figure 4(a) shows that banks with high and low *BankExposure* are similar on variables that are known determinants of corporate credit supply, e.g., bank size and equity ratio. I report both lagged and contemporaneous correlations to show that banks' balance sheets do not deteriorate at the time of the change in local government debt. Balanced bank-level characteristics make it less likely that high *BankExposure* banks are systematically subject to different corporate credit supply shocks.

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 (2022). I further discuss the shifters vs. shares view of identification and provide additional tests in Appendix B.

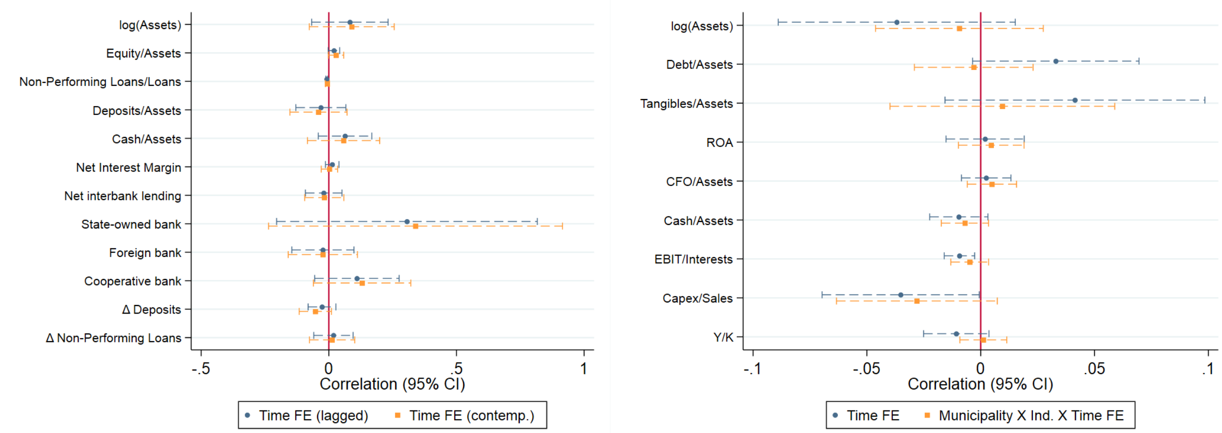
Shifters. A sufficient condition for assumption (A1) to hold is if the municipality-level "shifters" $\hat{\alpha}^{gov}_{mt}$ are "as good as random". Figure B.1 shows that the local government debt

[18]Figure B.4 plots the relationships between *BankExposure* and realized local government growth.

[19]Given the firm×time fixed effects, it is *not* a problem that banks sort into locations based on sectoral specialization or types of clienteles, and lend to firms with different firm-level credit *demand*.

FIGURE 4. Balance tests

(a) Bank-level correlations (b) Firm-level correlations



Note: Panel (a) shows the coefficients of bank-level regressions of bank exposure to local government debt demand (defined in (4)) on bank characteristics. The regressions include time fixed effects. The blue (orange) dots correspond to correlations between *BankExposure* and lagged (contemporaneous) bank characteristics. Regressions are weighted by bank-level corporate credit. Standard errors are clustered at the bank level. Panel (b) shows the coefficients of firm-level regressions of firm exposure to crowding out (defined in defined in (6)) on firm characteristics. The regressions include time fixed effects (blue dots) or municipality×industry×time fixed effects (orange dots). Regressions are weighted by firm-level corporate credit. 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.

demand shocks $\hat{\alpha}^{gov}_{mt}$ are not correlated with other 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 too small to be the relevant economic scale for stimulus spending effects, and there is high dispersion in $\hat{\alpha}^{gov}_{mt}$ across neighboring municipalities. In addition, the $\hat{\alpha}^{gov}_{mt}$ are not persistent (Fig. B.2), reflecting the lumpy nature of local government capital expenditure.

Shares. The necessary condition is that the average $\hat{\alpha}^{gov}_{mt}$ weighted by banks' exposure "shares" is orthogonal to bank-level corporate credit supply shocks. Three features of the shares support this assumption. First, I use shares specific to the local government credit market, that differ from shares in the corporate credit market. This avoids picking up bank exposure to potential municipality-level shocks correlated with $\hat{\alpha}^{gov}_{mt}$ but related to corporate credit. Second, shares are dispersed across neighboring municipalities (Fig. B.3), ruling out that they only capture exposure to broad areas. Third, shares are very persistent (Fig. B.2). Combined with the fact that the $\hat{\alpha}^{gov}_{mt}$ are not persistent, this rules out that some banks have always high (low) *BankExposure* or that banks on declining corporate credit trends strategically increase their shares in every period in high $\hat{\alpha}^{gov}_{mt}$ municipalities.

TABLE 2. Crowding out effect on corporate credit

	Credit growth					
	Baseline			$\mathbb{P}$(multibank)-adjusted weight		
	(1)	(2)	(3)	(4)	(5)	(6)
BankExposure	-0.164	-0.723**	-0.853***	-0.208	-0.876**	-1.036***
	(0.191)	(0.310)	(0.311)	(0.207)	(0.350)	(0.357)
Controls	–	–	✓	–	–	✓
Firm×Time FE	–	✓	✓	–	✓	✓
Observations	2,731,110	2,731,110	2,731,110	2,731,110	2,731,110	2,731,110
R-squared	0.000039	0.54	0.54	0.000035	0.54	0.54

Note: This table reports the results of estimating equation (2). The outcome variable is the firm×bank-level mid-point growth rate of credit. The main independent variable is bank exposure to local government debt demand shocks (defined in (4)). 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 firm×bank-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.

4.3. Results

4.3.1. Baseline results

Table 2 presents the results corresponding to equation (2). 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. Regressions are weighted by the denominator of the mid-point growth rate to obtain results 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 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 with similar municipality exposures, an issue raised by Adão, Kolesár, and Morales 2019 and Borusyak, Hull, and Jaravel 2022). 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×time fixed effects to only exploit within-firm variation (column 2). 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.

The point estimate implies that a one standard deviation increase in *BankExposure* reduces corporate mid-point credit growth by 1.22pp (or equivalently, the standard growth rate by 1.23pp). 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 (computation details are in Section C.1).

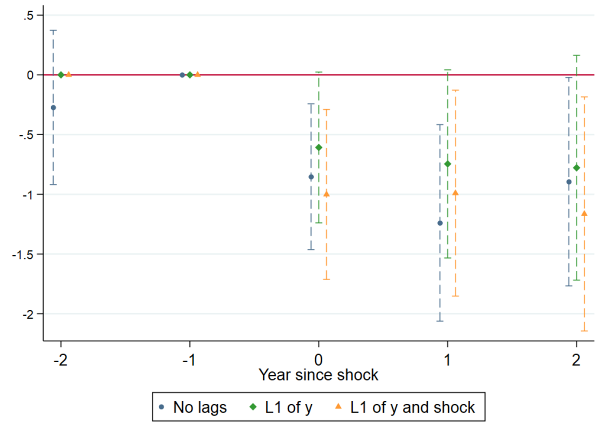
One limitation of the within-firm estimator is that it restricts the sample to multibank firms, which may yield estimates that are not representative of the population. Figure A.4 shows that the multibank sample over-represents firms that are larger in terms of outstanding credit. To alleviate this concern, I weight observations by the baseline weight divided by the probability that the observation appears in the multibank sample. This probability is estimated for 20 equally-sized bins of firms based on 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.

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. In addition, the absence of a significant pre-trend and the robustness to the inclusion of lagged independent and dependent variables further alleviate identification concerns.

These estimates isolate the crowding out effect of local government debt operating through a reduction in corporate credit supply. They hold constant other effects of government debt as well as government debt endogenously responding to private sector financing conditions. Interestingly, the comparison between column (1) and columns (2) and (3) suggests that the endogenous bias plays in a direction opposite to crowding out, as would occur if local government debt had positive demand effects.

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

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



Note: This figure plots the estimated coefficients β_h resulting from estimating equation (2), 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. All other elements of the specification are as in Table 2. The dot is the point estimate and the bar is the 95% confidence interval.

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.

4.3.2. Robustness and further tests of the identifying assumption

Distortions in the market for local government lending and crowding out. 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 that I estimate is independent of these level distortions and is only determined by banks' ability to expand their balance sheets.[20] 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., driven by banks making losses on coerced government lending as in Hoffmann, Stewen, and Stiefel 2022) or the distortion in banks' objective

[20]Taking a simple example, assume total lending is fixed to 100. Distortions on the relative desirability of local government vs. corporate debt affect the split between x local government and $100 - x$ corporate debt. However, the euro for euro crowding out effect is always equal to -1, irrespective of x.

function could make credit supply artificially inelastic. Table C.1 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 assumption (A1): (A1-a) firm$\times$time fixed effects absorb firm-level demand shocks; and (A1-b) there are no other bank-level credit supply shocks that are systematically correlated with *BankExposure*.

More granular fixed effects: High exposure banks must not be systematically subject to other bank-specific demand (A1-a) or supply (A1-b) shocks. More specifically, (A1-a) will be violated if, when local government debt rises, firms redirect their demand toward banks that are not lending to local governments. Similarly, assumption (A1-b) 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 local government loan share λ^{gov}_{bt-1}. I alleviate these concerns 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 (A1-b) is that *BankExposure* captures the geographic footprint of banks, which may be correlated with other bank-specific shocks. To alleviate this concern, I control for bank exposure to the 22 French regions, interacted with time dummies. Finally, I include bank fixed effects that control for any time-invariant factor affecting local government and corporate credit at the bank level. These specifications produce coefficients similar to my baseline results (Table C.2).

Heterogeneity by strength of demand effects: To further test assumption (A1-a), I exploit the fact that some firms are more likely to experience a positive demand shock when local government debt rises. 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 contract revenues as highly sensitive to local government debt 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 C.2 shows that this is not the case.

Additional tests related to the shift-share structure: First, to alleviate concerns that banks with negative corporate credit supply shocks strategically relocate in high α^{gov}_{mt} municipalities, I fix exposure weights in 2006. Second, to avoid concerns that the estimated α^{gov}_{mt} may be contaminated by the supply shocks of large banks present in m, I re-estimate equation (3) excluding the largest banks in each municipality, and I include the estimated supply shock α_{bt}^{gov} as a control. Third, I define $BankExposure_{bt,-m(f)}$ leaving out the shock α_{mt}^{gov} of the municipality where f is located, i.e., the α_{mt}^{gov} shock that most likely correlates with firm demand. Table C.3 shows that I find very similar results. Finally, Table C.3 presents a placebo test where *BankExposure* is defined with corporate credit exposure weights.

Additional robustness checks. I perform a variety of additional robustness checks of my baseline results, detailed in Appendix C.1. Table C.4 reports results when including additional bank-specific controls or imposing additional sample filters. Figure C.1 shows specification curves with 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. Table C.5 displays results for alternative definitions of the dependent and independent variables. Table C.6 shows robustness to winsorization and to various assumptions on the clustering of standard errors. Table C.7 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. 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

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 (2), allowing for heterogeneity by banks' characteristics. The outcome variable is the firm × bank-level mid-point growth rate of credit. The main independent variable is bank exposure to local government debt demand shocks (defined in (4)). 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 bank is non-French or if the share of bank liabilities held by non-residents is above 50%. Controls include the bank's lagged local government loan share, assets (log), equity ratio, and dummies for state-owned and foreign banks, interacted with the relevant characteristic dummy. Regressions are weighted by firm × bank-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 A.2. 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.[21] 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 (although a countervailing force may have been the implementation of tighter banking regulation).

5.2. Price vs. quantity adjustment

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

Table C.8 shows that the price effect is positive, consistent with a reduction in credit supply.[22] 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. This result is usually rationalized by concerns about the adverse selection effects of higher interest rates (Stiglitz and Weiss 1981).[23]

6. Firm-level effect on 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

[21] This result could be one channel for the result in Barnichon, Debortoli, and Matthes (2022) that multipliers on expansionary fiscal shocks are smaller than multipliers on contractionary fiscal shocks.

[22] 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.

[23] 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). The structural estimation in Diamond, Jiang, and Ma (2023) yields an extensive margin elasticity of -109. Finally, the interest elasticity of investment demand is typically also very high in macro models; e.g., the calibration in Boehm (2020) yields an elasticity of -20.

affect firm-level credit and investment?

6.1. Empirical strategy

To investigate real effects on investment, I follow the literature and translate the bank-level effect into a firm-level effect by considering firms' exposure to the shock through their lenders. I estimate the following specification:

$$\Delta K_{ft} = \beta^K FirmExposure_{ft} + \Phi \cdot \mathbf{X}_{ft} + \alpha_{mst} + \alpha_f + \varepsilon_{ft} \tag{5}$$

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

$$FirmExposure_{ft} = \sum_b \omega_{fb,t-1} BankExposure_{bt} \tag{6}$$

α_{mst} are municipality×two-digit industry×time fixed effects. α_f are firm 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 (2). Aggregating this specification at the firm level using bank shares, we obtain (omitting controls): $\Delta C_{ft} = d_{ft} + \beta FirmExposure_{ft} + \varepsilon_{ft}$. That is, firm-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.[24] This procedure precisely controls for the correlation between *FirmExposure*

[24] Cingano, Manaresi, and Sette (2016) and Jiménez et al. (2019) recommend using d_{ft} estimated from the within-firm specification (2). 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

and d_{ft}. Identification of β in the firm-level credit growth regression then follows from identification in the firm$\times$bank-level credit growth specification.

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

I further tighten my identification strategy by looking at the effect of *FirmExposure* *within* municipality$\times$industry$\times$time cells. Municipality$\times$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 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. In addition, I can exploit the panel structure of the data to include firm fixed effects that control for any firm-specific time-invariant determinants of investment.

The identifying assumption is that, conditional on fixed effects and controls, the firm-level unobserved determinants of investment are orthogonal to *FirmExposure*. Figure 4(b) tests whether firms with higher *FirmExposure* are systematically different on observed characteristics. I report unconditional correlations and correlations conditional on municipality$\times$industry$\times$time fixed effects. 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 this 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.[25] Bank shares are defined as mid-point shares to properly aggregate the within-firm specification in mid-point growth rates. Consistency with (2) requires that $\mathbf{X}_{ft}$ contains the firm-level weighted average of $\mathbf{X}_{bt}$. I also include additional firm-level controls most common in investment regressions: size (log revenues), leverage, profitability (EBIT margin), and capex intensity (capex/sales), all lagged by one period. As in Alfaro, García-

does not affect my results, as shown in robustness checks.

[25]Figure A.4 provides a visual representation of the sample selection imposed by the tax-filings.

Santana, and Moral-Benito (2021), I recover firm-level demand shocks for both multibank 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 A.3 lists the results. The point estimate is -1.03 (-1.13 with weights adjusted for the probability that a firm is multibank), slightly larger than in the full sample.

Table 4 presents the firm-level effects obtained from estimating (5). Columns (1) to (3) show that firms more exposed to crowding out receive less credit. The magnitude is in line with the within-firm specification, suggesting that firms have 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 (see, e.g., Khwaja and Mian 2008; Chodorow-Reich 2014; Huber 2018). A plausible explanation is that banks interpret credit cuts at others bank as a negative signal on borrowers' quality (Darmouni 2020).

Columns (3) to (6) 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 firms cut investment. In columns (7) and (8), I separately estimate the credit-to-investment elasticity η^K by using *FirmExposure* as an instrument for firm credit growth. I find a credit-to-investment elasticity equal to 0.23-0.28, close to existing estimates (e.g., 0.26 in Cingano, Manaresi, and Sette 2016; 0.36 in Amiti and Weinstein 2018).

These estimates can be used to quantify the effect of an additional €1 in local government debt on investment. Starting from the effect on credit obtained from the within-firm estimation and using the credit-to-investment sensitivity η^K equal to 0.23, I find that an additional €1 in local government debt at one bank leads to a €0.30 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

TABLE 4. Firm-level effect on credit and investment

	Effect of exposure to local government debt shocks						Credit-to-inv. elasticity	
	gr(credit)			gr(capital)			gr(capital)	
	RF (1)	RF (2)	RF (3)	RF (4)	RF (5)	RF (6)	IV (7)	IV (8)
FirmExposure	-1.056***	-1.050***	-1.403***	-0.476***	-0.465***	-0.455***		
	(0.260)	(0.261)	(0.324)	(0.085)	(0.079)	(0.110)		
gr(credit)							0.288***	0.232***
							(0.057)	(0.047)
Firm controls	–	✓	✓	–	✓	✓	✓	✓
Municipality×Industry×Time FE	✓	✓	✓	✓	✓	✓	✓	✓
Firm FE	–	–	✓	–	–	✓	–	✓
Observations	807,979	807,979	780,138	785,314	785,314	757,023	724,028	693,378
R-squared	0.95	0.95	0.97	0.43	0.43	0.57	0.15	0.17
F stat.							23.6	24.5

Note: This table reports the results of estimating equation (5). 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 (6)). 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 revenues (log), debt/assets, EBIT/sales and capex/sales ratios (all lagged). Columns (7) and (8) show the credit-to-capital elasticity, obtained by instrumenting firm-level credit growth by *FirmExposure* (where credit growth is the standard growth rate to obtain an elasticity). 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.

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.

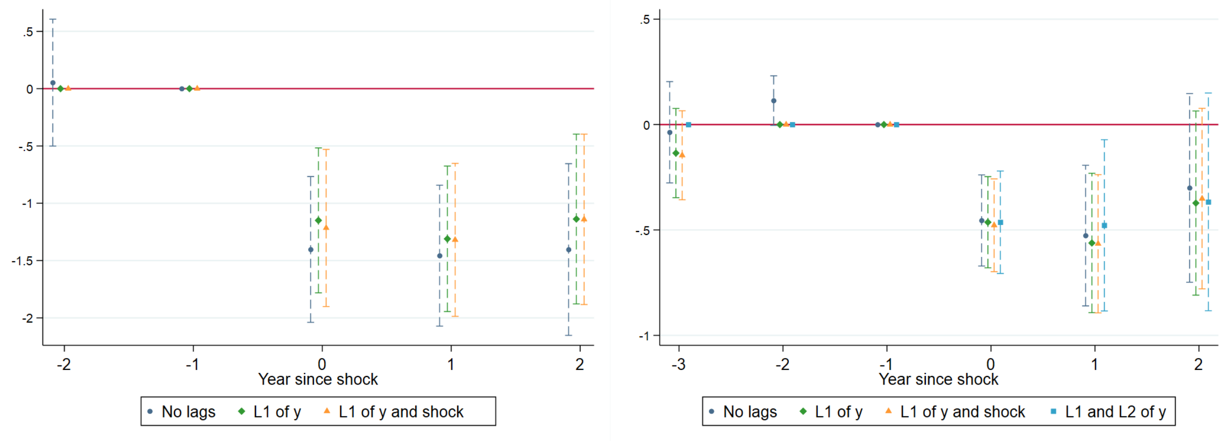
In Table C.9, I present the same results for firm-level employment. I find no effect. I focus on credit with initial maturity above one year, which typically finances investment rather than working capital, so that the credit cut is unlikely to have a direct effect on labor. The indirect effect of the contraction in investment on labor demand due to capital-labor complementarities may be too small to detect or may take time to materialize.

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.

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

(a) Credit (b) Investment



Note: This figure plots the estimated coefficients β_h resulting from estimating equation (5). 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 and firm fixed effects. "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. All other elements of the specifications are as in Table 4. The dot is the point estimate and the bar is the 95% confidence interval.

More granular fixed effects: Table C.10 shows the results for seven different fixed effect structures, using different levels of granularity for geographic units and industries. I can also further tighten the identification by adding firm size$\times$time fixed effect. Additionally, I can include lagged credit growth as a control to restrict the comparison to firms on a similar credit trend. The magnitude of the investment 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.[26]

Heterogeneity by strength of demand effects: 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 demand effects, 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 C.10).

[26]The point estimate of the credit specification increases with the inclusion of firm fixed effects, but the difference across coefficients is not statistically significant.

Robustness checks. I perform a variety of robustness checks of my results, detailed in Appendix C.3. First, Table C.11 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, Table C.12 explores the results with alternative weighting strategies. Table C.13 presents the results with an alternative definition of *FirmExposure*, different winsorization, and different assumptions on the appropriate level of clustering. The estimated coefficients are similar across all specifications.

6.3. Heterogeneous effects

Table 5 investigates heterogeneous effects by dependence on external finance (proxied by firm leverage), by bank dependence specifically (proxied by the ratio of bank debt to total debt), by availability of liquidity (proxied by the ratio of cash to assets), and by a proxy for the marginal product of capital.

Heterogeneous 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. Panel A investigates the first channel and shows that the credit cut is relatively uniform across firms. Panel B investigates the second channel. Firms' dependence on external finance and on bank finance significantly affects the sensitivity of investment to the availability of bank financing, in line with intuition. For instance, columns (3) and (4) show that highly bank dependent firms exhibit a credit-to-investment sensitivity that is more than twice larger than that of other firms. In addition, firms with a high cash ratio have a credit-to-investment sensitivity close to 0, in line with the idea that these firms can use their internal resources to finance investment. Finally, I investigate how the effect varies when sorting firms by revenues-over-capital, which provide within-industry measures of firms' marginal product of capital when the production function is Cobb-Douglas. Sorting firms by marginal products provides an agnostic way to study the effect of frictions on input acquisition (Hsieh and Klenow 2009). In line with intuition, firms with higher Y/K have a larger credit-to-investment sensitivity.

TABLE 5. Firm-level effects of crowding out: heterogeneity

Panel A: Credit

	gr(credit)							
	Leverage		Bank dep.		Cash		Y/K	
	Low	High	Low	High	Low	High	Low	High
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
FirmExposure	-1.425***	-1.413***	-1.517***	-1.264***	-1.372***	-1.733***	-1.252***	-1.525***
	(0.485)	(0.327)	(0.317)	(0.364)	(0.336)	(0.441)	(0.379)	(0.329)
Controls	✓	✓	✓	✓	✓	✓	✓	✓
Municipality×Industry×Time FE	✓	✓	✓	✓	✓	✓	✓	✓
Firm FE	✓	✓	✓	✓	✓	✓	✓	✓
Observations	108,954	626,464	556,914	167,274	555,537	136,006	146,253	572,367
R-squared	0.98	0.96	0.97	0.95	0.96	0.98	0.96	0.97
High *minus* Low (RF)		.012		.252		-.112		-.273
		(.443)		(.265)		(.255)		(.317)

Panel B: Investment

	gr(capital)							
	Leverage		Bank dep.		Cash		Y/K	
	Low	High	Low	High	Low	High	Low	High
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
FirmExposure	0.200	-0.512***	-0.226	-0.586***	-0.520***	0.017	-0.167	-0.616***
	(0.373)	(0.110)	(0.148)	(0.137)	(0.136)	(0.369)	(0.177)	(0.129)
Controls	✓	✓	✓	✓	✓	✓	✓	✓
Municipality×Industry×Time FE	✓	✓	✓	✓	✓	✓	✓	✓
Firm FE	✓	✓	✓	✓	✓	✓	✓	✓
Observations	104,459	608,681	538,456	163,460	542,294	128,039	144,616	552,012
R-squared	0.66	0.58	0.59	0.61	0.59	0.72	0.65	0.58
Credit-to-inv. IV	-.08	.249***	.133**	.304***	.275***	.001	.106	.269***
	(.13)	(.05)	(.057)	(.075)	(.072)	(.197)	(.08)	(.061)
High *minus* Low (RF)		-.715*		-.36**		.575		-.448**
		(.377)		(.161)		(.351)		(.223)
High *minus* Low (IV)		.332**		.171**		-.256		.163*
		(.134)		(.076)		(.244)		(.093)

Note: This table reports the results of estimating specification (5) 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 (6)). High leverage is defined as firms with leverage above the 25th percentile. High Bank Dep. is a dummy equal to 1 if the share of bank debt in total debt is above the 75th percentile. High Cash is a dummy equal to 1 if the firm's cash/assets ratio is above the 25th percentile. High Y/K is a dummy equal to 1 if the firm's value added/capital is above the 25th percentile (within-industry). The line labeled Credit-to-inv. IV shows the credit-to-input elasticity 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 firm's revenues (log), debt/assets, EBIT/sales 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

The goal of this article is to quantify the crowding out effect of local government bank debt on corporate credit, investment, and output, holding constant other effects of government debt. That is, the quantity of interest is the shortfall in corporate credit implied by banks' actual exposure to demand for local government debt, compared to a counterfactual where the only change is that all banks have zero exposure. An example of such counterfactual is if the change in local government debt is instead financed by an outside investor with a perfectly elastic supply of funds.

Thus far, I have shown that banks exposed to local government loan demand reduce corporate credit relative to non-exposed banks, and this reduces investment at exposed firms relative to non-exposed firms. These *relative* effects do not immediately add up to the *aggregate* effect because they difference out any equilibrium effect of crowding out affecting all banks and firms. In this section, I combine the estimated relative effects with a model that predicts these equilibrium effects to obtain aggregate effects.

Let $Y_t(\mathbf{0})$ denote counterfactual output when local government loan demand shocks α_{mt}^{gov} are zero for all municipalities, and hence $BankExposure_{bt}$ is zero for all banks. I denote the log-change shortfall attributable to crowding out as $\mathcal{L}(Y_t) = \log(Y_t) - \log(Y_t(\mathbf{0}))$. I can also express the shortfall in "euro for euro" terms, comparable to government spending multipliers: $m_t^Y = \frac{Y_t - Y_t(\mathbf{0})}{C_t^g - C_t^g(\mathbf{0})}$. This corresponds to the object of interest defined in equation (1). These quantities can be similarly defined for other variables.

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. I consider extensions of this baseline model in Appendix D.4.

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 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.

$$(7) \qquad \log(C_{fb} + \overline{E}) = \bar{c} + (\sigma - 1)z_{fb} + \log(Y) - (1-\alpha)(\sigma-1)\log(w) - (1+\alpha(\sigma-1))\log(r_b^c)$$

where $\bar{c}$ is a constant. This implicitly defines a corporate credit demand curve with an elasticity denoted $\epsilon^c \leq 0$, as well as a credit-to-investment elasticity denoted ℓ. One can think of the real stimulus effects of government spending as one determinant of z_{fb}.

Local governments have downward-slopping isoelastic demand curves for bank credit with elasticity $\epsilon^g \leq 0$. Local governments are assigned to banks. This yields a bank-level local government credit demand function: $\log(C_b^g) = \tilde{Z}_b^g + \epsilon^g \log(r_b^g)$, where $\tilde{Z}_b^g$ aggregates the demand shocks of municipalities m borrowing from b.

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, households supply undifferentiated labor with a Frisch elasticity of labor supply ψ, so that $\log(L) = \psi \log(w)$.

Banks are price-takers and maximize the proceeds of lending minus the cost of funds:

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

subject to a funding constraint: $C_b^c + C_b^g = S_b + B_b$. B_b is net interbank borrowing. r_b^c, r_b^g, r_b^s, and i are the interest rates for corporate, local government loans, deposits, and interbank loans, respectively. 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 $\tilde{Z}_b^g$ and z_{fb}. I solve for these quantities 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.

Let λ be the share of local governments in banks' loan portfolio in the deterministic equilibrium. Let $Z_b^g = \lambda \tilde{Z}_b^g$ be the change in local government demand normalized by banks' total loan portfolio. Define $Z^c = \int_0^1 \int_0^1 z_{fb} df db$ and $Z^g = \int_0^1 Z_b^g db$.

7.2. Aggregate and relative crowding out effect

Aggregate crowding out effect. With both firm and local government credit demand shocks, the equilibrium change in aggregate corporate credit is given by:

$$\hat{C}^c = \Upsilon Z^c + (1 + \kappa^{GE}) \chi Z^g \tag{8}$$

where Υ, χ, and κ^{GE} depend on model parameters (see Appendix equation (D.13)). The corporate credit shortfall due to crowding out is equal to $\mathcal{L}(C^c) = (1 + \kappa^{GE})\chi Z^g$. It is the change in aggregate corporate credit due a change in aggregate demand for local government debt directed to banks, holding everything else constant—notably the corporate credit demand shock Z^c that may be affected by other effects of fiscal policy.

What determines the severity of crowding out? $\chi < 0$ 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. In the limit $\epsilon^s \to +\infty$, χ tends to 0 and there is no crowding out. κ^{GE} captures general equilibrium feedbacks through the product and labor markets. It depends on σ, ψ, and α and can be positive or negative.

Relative vs. aggregate effect. Writing the same equation at the bank firm-level yields:

$$\hat{C}_{fb} = \upsilon z_{fb} + (\Upsilon - \upsilon) Z^c + \kappa^{GE} \chi Z^g + \chi(1 - \nu) Z^g + \chi \nu Z_b^g \tag{9}$$

Crowding out now corresponds to the last three terms. The term $\kappa^{GE}\chi Z^g$ is as in equation (8). The direct crowding out effect χZ^g is split into two terms: $\chi(1-\nu)Z^g$ depends on the aggregate shock, while $\chi\nu Z_b^g$ depends on the bank-specific shock. $\nu \in [0,1]$ is a function of model parameters and indexes the degree of segmentation across banks. It is monotonically increasing in the interbank market friction ϕ. $\nu = 0$ when $\phi = 0$ (no friction) and $\nu = 1$ when $\phi \to +\infty$ (full segmentation).

The effect of the bank-level local government loan demand shock Z_b^g on bank-level corporate credit depends on ν. 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 jointly captures the size of the direct effect χ and the degree of banking frictions ν. By the same logic, when segmentation is not perfect ($\nu < 1$), a demand shock at one bank is partly transmitted to other banks through the interbank market. This spillover term $\chi(1-\nu)Z^g$ implies that each bank's corporate credit supply is negatively affected by the aggregate local government loan demand shock.

The same logic applies to investment. Firm-level investment is given by equation (9) where the right-hand side is multiplied by the credit-to-investment elasticity ℓ. When $\nu \neq 0$, a local government loan demand shock at one bank reduces investment for firms borrowing from that bank. The cross-sectional effect is $\ell\chi\nu$. As long as $\nu < 1$, the shock is transmitted across banks, and firms borrowing from non-exposed banks are affected.

Link with the empirical specification. To link the static model to the panel setting of the empirical sections, I assimilate log-deviations from the deterministic equilibrium $\hat{C}_{fb}$ to growth rates ΔC_{fbt} and the local government loan demand shock Z_b^g to my demand shifter $BankExposure_{bt}$.[27] Equation (9) is the theoretical counterpart to my firm×bank-level empirical specification (2). The coefficient that I identify in this analysis is the relative crowding out parameter that relates a bank-specific local government loan demand shock to bank-level corporate credit. It corresponds to $\chi\nu$. The same logic applies to the investment specification. The coefficient identified in specification (5) corresponds to $\ell\chi\nu$.

[27] Z_b^g is the increase in local government loan demand divided by total lending in the deterministic equilibrium, which corresponds to the normalization used to define $BankExposure_{bt}$.

What do we learn from the relative crowding out parameter? This analysis highlights that the relative crowding out parameter estimated in the previous sections differs from the aggregate effect of crowding out. Nevertheless, this parameter already allows to draw some conclusions. First, the fact that I estimate $\chi\nu \neq 0$ implies that $\chi \neq 0$. That is, we can reject the null that crowding out has no direct effect on corporate credit. Second, since $\nu \in [0, 1]$, χ is more negative than $\chi\nu$. The relative effect captures only the part of the direct effect that has cross-sectional implications due to banking frictions, and therefore underestimates the direct effect. The same reasoning applies to investment: we can reject the null that crowding out has no direct effect on corporate investment, and the relative effect underestimates the direct effect.

7.3. Quantification of the aggregate crowding out effect

I quantify the aggregate crowding out effect $(1 + \kappa^{GE})\chi$ by combining: (i) the relative effect identified in my empirical analysis $\chi\nu$; (ii) an estimate of ν; (iii) an estimate of κ^{GE}.

Direct effect - Lower bound from cross-sectional estimates. First, consider the aggregate corporate credit shortfall relative to a counterfactual where all local government loan demand shocks α^{gov}_{mt} are zero, as implied by my cross-sectional estimate. This is given by:

$$\mathcal{L}^{Xsec}(C^c_t) = \sum_f \frac{C_{ft}(\mathbf{0})}{C^c_t(\mathbf{0})} \hat{\beta} FirmExposure_{ft} \tag{10}$$

where $\hat{\beta}$ is the credit coefficient estimated from the firm-level specification (5). Computation details are in Appendix D.3. This is the empirical counterpart to the model object $\chi\nu Z^g_t$ (which assumes a degenerate distribution of baseline firm and bank size). I find a yearly corporate credit shortfall attributable to crowding out equal to 0.86% on average. The output shortfall can be compared to the change in local government credit $C^g_t - C^g_t(\mathbf{0}) = C^g_t - C^g_{t-1}$. This implies a multiplier m^C equal to –0.55 on average across years. I similarly estimate the investment shortfall using the coefficient of the investment regression. I find a shortfall equal to 0.24%. This translates into an output loss due reduced inputs equal to 0.07%, or equivalently, a multiplier m^Y equal to –0.18.

Because the cross-sectional effects captures only part of the direct effect ($\nu \leq 1$), these quantities underestimate the direct effect of crowding out.

Direct effect - Spillover across banks. The aggregation based on cross-sectional estimates misses the spillover effect due to capital flows across banks. 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. 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 0.15. Since all the cross-sectional effects scale with ν, the lower bounds underestimate the direct effect χ by 15%.

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 lead firms borrowing from non-exposed banks to adjust their inputs. First, the relative price of goods produced by exposed firms increases (reflecting their higher cost of capital). This triggers a reallocation of demand toward non-exposed firms, the extent of which depends on the substitutability across goods σ. Second, the wage falls, which reduces labor supply for all firms, in proportion to the labor supply elasticity ψ. Table D.2 in Appendix D.3 calibrates the general equilibrium feedback. For plausible parameter values, general equilibrium effects either magnify the direct effect or have at most a modest attenuating effect. To avoid introducing additional uncertainty related to calibrated parameter values, I thus use the conservative approximation $\kappa^{GE} \approx 0$ and use my estimates of the direct effect χ as the total effect.

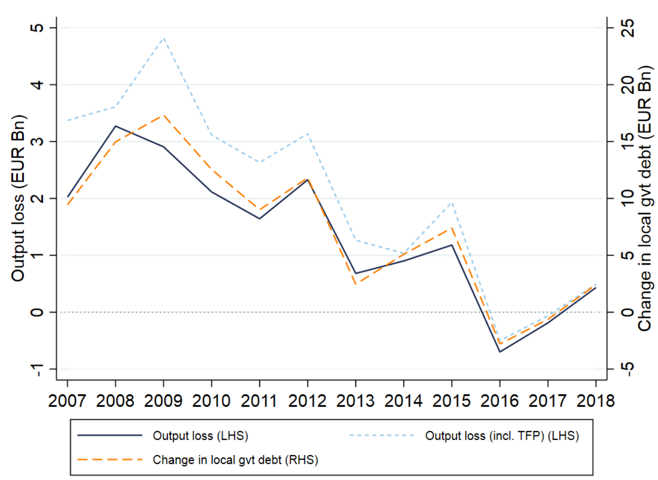
This analysis implies that the aggregate corporate credit loss due to crowding out is equal to 1.02% on average across years. Equivalently, €1 of local government loans crowds out €0.65 of corporate credit. The capital shortfall is equal to 0.28%, corresponding to a multiplier equal to -0.39. The aggregate output loss is equal to 0.08% on average. Equivalently, a €1 increase in local government loans reduces output by €0.21 via financial crowding out. The multipliers are summarized in Table 6. Figure 7 plots the time series of the output loss. It closely follows the time series of the change in local government loans, scaled by 0.2, showing that the multiplier is stable across years. The output loss is highest at the beginning of the sample when local government debt growth is the highest, and turns negative in 2016 and 2017 when local government debt recedes. In Figure D.1, I present specification curves to assess the sensitivity of the multiplier estimates to choices of empirical specifications. Considering 96 different specification choices for $\hat{\beta}$ and $\hat{\nu}$, the output multiplier consistently falls between -0.10 and -0.35.

TABLE 6. Aggregate effects of crowding out

	Multiplier	
	Implied by cross-sectional estimates	Aggregate effect
Corporate credit	-0.55	-0.65
Capital	-0.33	-0.39
Aggregate output	-0.18	-0.21

Note: This table reports the effects of crowding out on aggregate variables. The reported quantities are multipliers, defined as the euro change in the quantity of interest with respect to the no-crowding out counterfactual, per euro change in local government loans. The first column is the aggregation implied by the cross-sectional coefficients. The second column is the estimate of aggregate effects accounting for equilibrium effects of crowding out. Reported multipliers are averages of yearly multipliers.

FIGURE 7. Aggregate output loss due to crowding out



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%. "Output loss" refers to the baseline output loss from the main text. "Output loss (incl. TFP)" refers to the output loss including the change in aggregate TFP computed in Appendix E.

Appendix D.4 discusses extensions of the baseline model and shows they do not affect the key aggregation results. A key advantage of starting from the reduced form coefficient (as opposed to a structural estimation of the model) is indeed that it makes the quantification more robust to model misspecification. First, χ is a sufficient statistic for the direct crowding out effect, so that I do not need to estimate all the parameters underlying the credit supply and demand functions. Second, the decomposition of the direct effect χ into the effect identified in the cross-section $\chi\nu$ due to segmentation and a spillover term due to capital flows across banks $\chi(1-\nu)$ is very general. Hence, my quantification of χ is robust to different modeling choices regarding the functioning of credit markets.

In using a model to inform the "missing intercept" of the cross-sectional regression, I follow Chodorow-Reich (2014). My exercise also resembles Herreño (2021) who targets reduced-form estimates of credit supply shocks in a structural estimation to obtain aggregate effects of lending cuts. On top of developing a model suited to my setting, I clarify that the cross-sectional effect jointly captures the aggregate effect of the credit supply shock and the degree of segmentation across banks, and provide a simple method for separately estimating the two. I obtain a credit-to-output elasticity equal to 0.08 (in the conservative quantification with $\kappa^{GE} \approx 0$), which can be compared to 0.2 in Herreño (2021).

7.4. Crowding out and capital misallocation?

The preceding quantification corresponds to the output loss due to the crowding out-induced reduction in the stock of capital. Crowding out of aggregate investment is the main channel through which crowding out affects output and has been the key object of interest in the literature on this topic. My reduced-form results 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. In Appendix E, I quantify this effect using the framework of Hsieh and Klenow (2009) and I find that crowding out reduces aggregate TFP by 0.04% per year on average. This effect is entirely driven by the fact that firms with higher marginal products of capital have a higher credit-to-investment sensitivity. Figure 7 displays the time series of the output loss due to crowding out when including the TFP loss. The additional loss is large at the beginning of the sample and negligible afterwards. On average over the sample period, it is equivalent to an output loss of €0.05 per €1-increase in local government loans. This effect has no reason to be proportional to the change in local government loans and hence is not included in my baseline multiplier quantifications.

7.5. Discussion

Crowding out and multipliers of local government spending. My results show that an additional €1 in local government loans reduces aggregate output by €0.2 via financial crowding out. This implies that the debt-financed multiplier of local government spending would be higher by 0.2 in the absence of crowding out. Debt-financed multipliers are notoriously hard to estimate, but a reasonable range is 0.5-1.9 (Ramey 2019). This suggests that crowding out significantly dampens any stimulus effects of debt-financed spending. In line with this result, Broner et al. (2022) use cross-country data to document that debt-financed multipliers are increasing in the share of public debt held by foreigners.

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. These multipliers do not account for crowding out.[28] More precisely, one can show that transfer-financed multipliers are approximately equal to debt-financed multipliers when crowding out does not occur, e.g., if the debt is financed by an outside investor with a perfectly elastic supply of funds.[29] My results imply that, because crowding out is quantitatively significant, debt-financed multipliers may be substantially smaller than transfer-financed multipliers.

External validity. I provide a quantification of crowding out in the case of local government bank debt. My results thus have the greatest external validity for other countries where local governments heavily rely on bank debt. As shown on Figure A.1, this represents a large sample of countries.

Do my results teach us something about crowding out generated by central or local government bonds? On top of quantifying crowding out in one market, I show that, in line with theory, the output loss due to crowding out reflects the elasticity of the supply of loanable funds. Testing and confirming this prediction allows to extrapolate about the plausible magnitude of crowding out in other markets. For instance, the elasticity of the supply of loanable funds is likely to be higher in the case of government bonds than for bank loans: these bonds are traded on international capital markets with a deeper supply and held by agents not subject to bank regulation. Then, my quantification provides an upper bound for the crowding out effect of government bonds. A specific case is when

[28] Even if the spending is financed by debt at the federal level, crowding out will be differenced out in the missing intercept of the cross-regional regressions.

[29] Chodorow-Reich (2019) shows that—in a model without capital markets where financial crowding out does not occur—the transfer-financed multiplier is equal to the debt-financed multiplier plus the effect of the wealth transfer, which is quantitatively negligible.

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

8. Conclusion

This article investigates one potential adverse effect of increasing levels of local government bank debt: financial 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 other endogenous relationships between local government debt and corporate outcomes. 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 €0.2 in the long run via financial crowding out. This highlights a significant cost of the long-run increasing trend in local government indebtedness. In addition, my results imply that crowding out reduces the potency of debt-financed local government spending as a stimulus tool: namely, crowding out reduces the output multiplier of such spending by 0.2.

What determines the extent of crowding out? I find that, in line with the theoretical prediction, the severity of crowding out reflects banks' limited ability to increase credit supply when faced with a demand shock. A key implication is that, in segmented financial markets, the sources of government borrowing will affect the transmission of fiscal policy and the size of debt-financed multipliers. To minimize crowding out, government should issue debt in "deep" and elastic markets. This result notably highlights 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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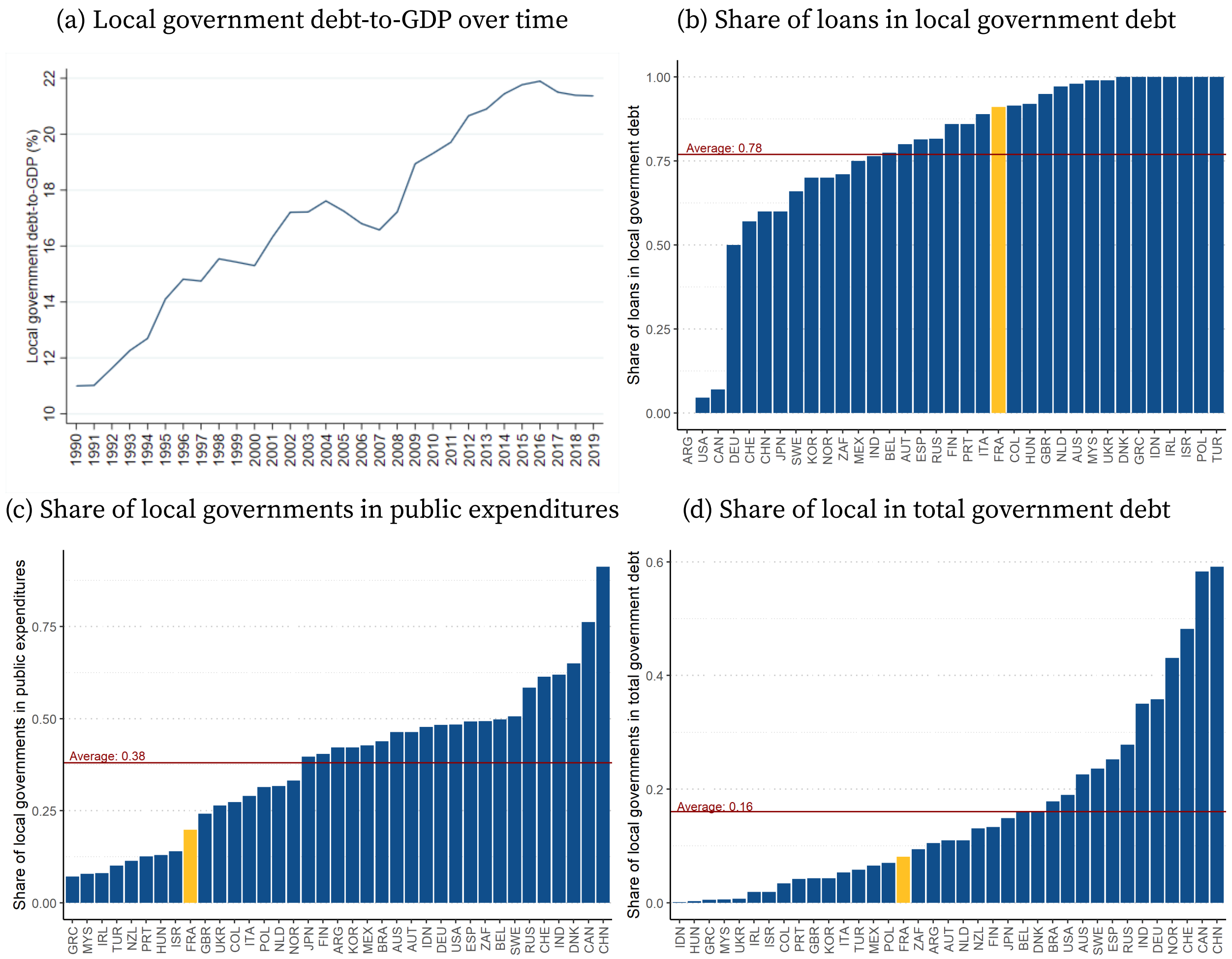
Appendix for online publication

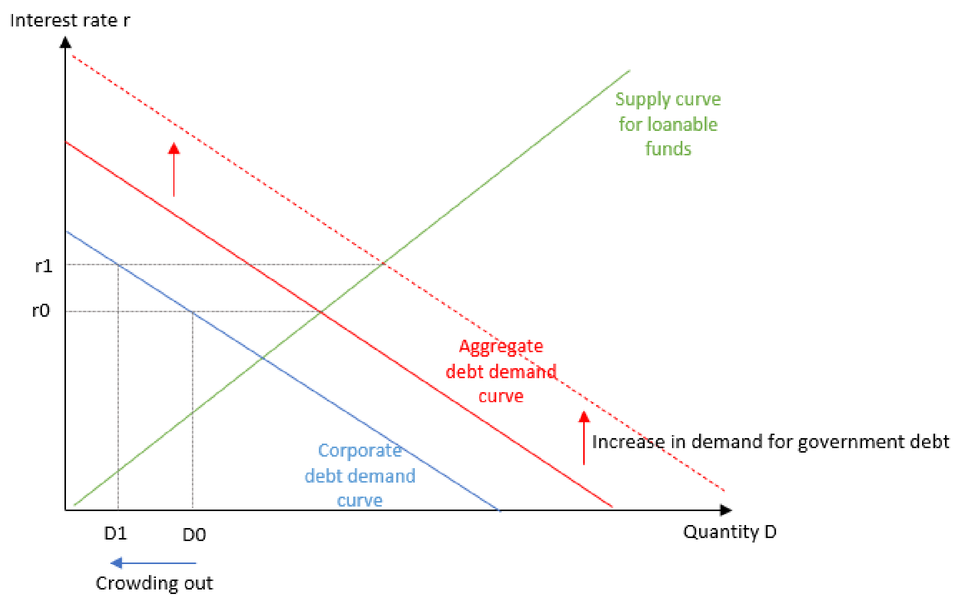
Appendix A. Additional tables and figures

FIGURE A.1. Local government debt in large developed and developing economies



Note: Subfigure (a) shows the average local government debt-to-GDP ratio over time. Subfigure (b) shows the share of loans in local government debt in 2016. 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.

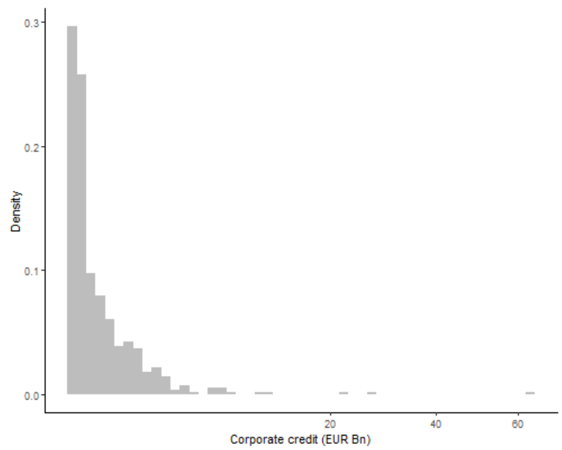
FIGURE A.2. Crowding out: simple supply and demand graph



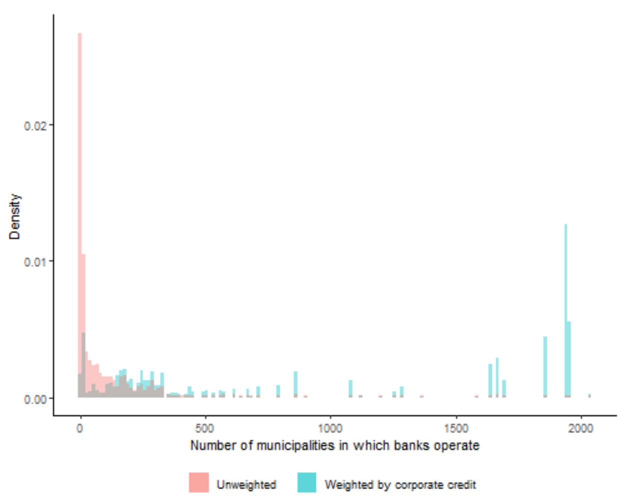
Note: This figure depicts the crowding out mechanism on a simple supply and demand graph.

FIGURE A.3. Population of French banks

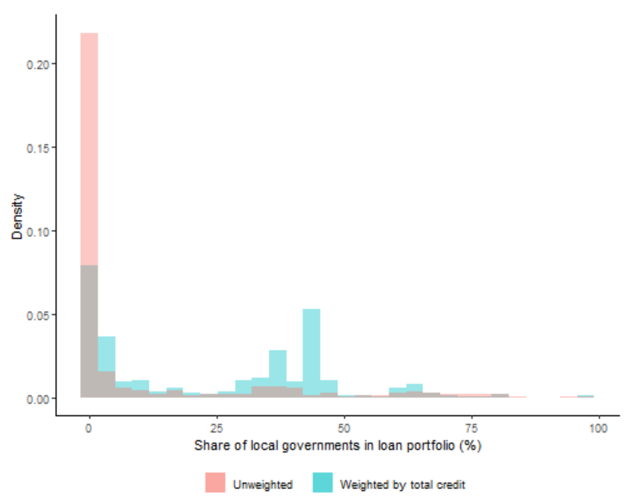
(a) Distribution by loan portfolio size



(b) Distribution by number of municipalities

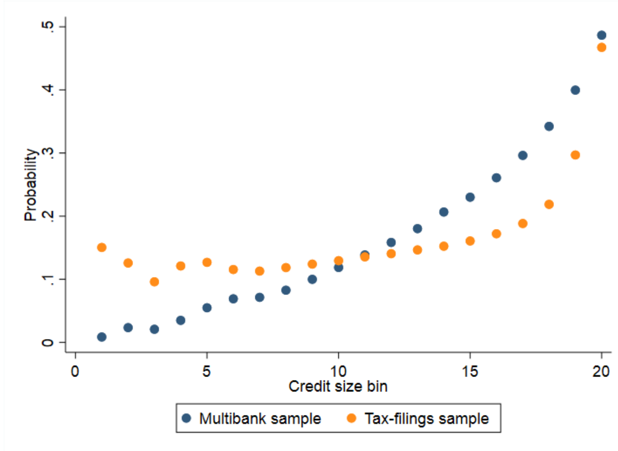


(c) Distribution by local government loan share



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 credit volume.

FIGURE A.4. 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 A.1. 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). As expected, firm×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. All regressions are weighted by the denominator of the mid-point growth rate. ***, ** and * indicate significance at the 1%, 5% and 10% levels, respectively.

Table A.2. Crowding out effect: asymmetry and time series variation

	Credit growth			
	(1)	(2)	(3)	(4)
BankExposure	-0.803**	0.105	-1.080**	-0.849*
	(0.339)	(0.916)	(0.546)	(0.464)
Controls	✓	✓	✓	✓
Firm×Time FE	✓	✓	✓	✓
Sample	Positive	Negative	Pre-2013	Post-2013
Observations	2,528,347	216,250	1,460,456	1,284,141
R-squared	0.53	0.55	0.55	0.51

Note: This table reports the results of estimating equation (2) 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 firm×bank-level mid-point growth rate of credit. The main independent variable is bank exposure to local government debt demand shocks (defined in (4)). 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 firm×bank-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 A.3. Firm×bank-level effect on credit: tax-filings subsample

	Credit growth					
	Baseline			$\mathbb{P}$(multibank)-adjusted weight		
	(1)	(2)	(3)	(4)	(5)	(6)
BankExposure	-0.398*	-0.908***	-1.028***	-0.528**	-0.994***	-1.125***
	(0.211)	(0.297)	(0.289)	(0.212)	(0.301)	(0.293)
Controls	–	–	✓	–	–	✓
Firm×Time FE	–	✓	✓	–	✓	✓
Observations	927,459	927,459	927,459	927,459	927,459	927,459
R-squared	0.000086	0.50	0.50	0.000095	0.51	0.51

Note: This table reports the results of estimating equation (2) on the tax-filings subsample. The outcome variable is the firm×bank-level mid-point growth rate of credit. The main independent variable is bank exposure to local government debt demand shocks (defined in (4)). 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 firm×bank-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 design

To guide the discussion on identification, it is useful to repeat the structural equation obtained from the model (equation (D.24)):

$$\Delta C_{fbt} = \upsilon z_{ft} + \delta_t + \chi\nu BankExposure_{bt} + \iota\nu\xi_{bt} \tag{B.1}$$

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 that I denote δ_t.

The empirical specification that I estimate (equation (2)) is:

$$\Delta C_{fbt} = d_{ft} + \beta BankExposure_{bt} + \varepsilon_{fbt} \tag{B.2}$$

Equation (B.1) 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 (B.1)). The key threat to the orthogonality condition (A1) is therefore a correlation between *BankExposure* and bank-specific corporate credit supply shocks. In what follows, I omit time subscripts to simplify notations.

B.1. Identification based on shifters

Condition (A1) 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 2022). 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:

$$\mathbb{E}\left[\sum_{m} \hat{\alpha}_{m}^{gov}\left(\sum_{f,b} \tilde{\omega}_{bm}^{gov,f}\,\varepsilon_{fb}\right)\right] = 0 \tag{B.3}$$

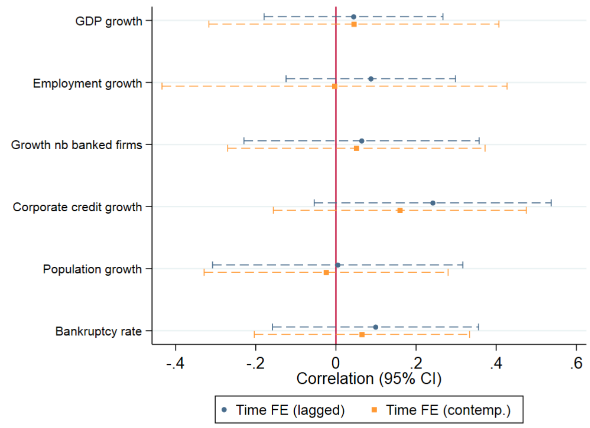
$\hat{\alpha}_{m}^{gov}$ must be orthogonal to the bank-specific shocks ε_{fb} aggregated using the (within-firm deviations in) exposures of banks to municipality m. Put differently, it must not be the case that banks experiencing negative bank-specific shocks ε_{fb} have systematically higher exposure to municipalities where $\hat{\alpha}_{m}^{gov}$ is high.

What are the main identification concerns in this setting? One class of issues is if (i) $\hat{\alpha}_{m}^{gov}$ is correlated to some variable municipality-level variable X_m (e.g., deposits in m), and (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 class of issues is if 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 the municipality-level shocks $\hat{\alpha}_{m}^{gov}$ are not correlated to other municipality-level variables. Figure B.1 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 (Fig. 2). In addition, Figure B.2 show that the $\hat{\alpha}_{m}^{gov}$ are not persistent, which reduces the risk of a correlation with persistent economic outcomes. This lumpiness across time and space 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 gov-

FIGURE B.1. Municipality-level balance tests



Note: This figure shows the coefficient of municipality-level regressions of local government debt demand shocks $\hat{\alpha}^{gov}_{mt}$ on municipality-level variables. All regressions include time fixed effects. The blue (orange) dots correspond to correlations between $\hat{\alpha}^{gov}_{mt}$ and lagged (contemporaneous) municipality characteristics. As recommended by Borusyak, Hull, and Jaravel (2022), the regressions are weighted by $s_{mt} = \sum_b C^{corp}_{bt-1}\omega^{gov}_{bm,t-1}$ where C^{corp}_{bt-1} 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.

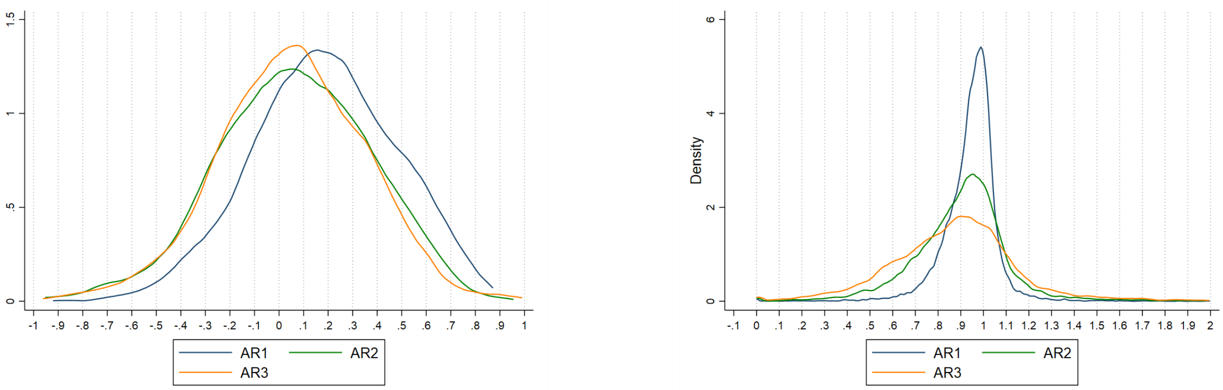
ernment credit market. Any municipality-level shock emanating from corporates would affect banks via their exposure to the corporate credit market. Conversely, bank-specific corporate credit shocks would affect municipality-level outcomes (like local government debt demand) of municipalities with large corporate credit presence of affected banks. As a placebo test, Table C.3 shows that *BankExposure* constructed with corporate credit exposure weights does not predict a decline in corporate credit. Second, the maps in Figure B.3 show the municipality-level market shares of the three largest banks. The shares are highly dispersed across municipalities. This 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 B.2 shows that shares are highly persistent. This rules out banks on declining corporate credit supply trends strategically increasing their shares in high $\hat{\alpha}^{gov}_{m}$ municipalities in every period. As a further check, Table C.3 shows that my results are virtually identical when I fix shares in 2006.

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 proxy $\hat{\alpha}^{gov}_{m}$ estimated from realized local government-bank credit growth. In small samples,

FIGURE B.2. Autocorrelation of shifters and shares

(a) Local government demand shocks

(b) Bank×municipality market 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.

it may be contaminated by the supply shocks of the large banks in m, which also enter the residual of my bank-level regression.[30]

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, Table C.3 shows that repeating the construction of $\hat{\alpha}_m^{gov}$ excluding banks with municipality-level market shares higher than 40% leads to very similar results.

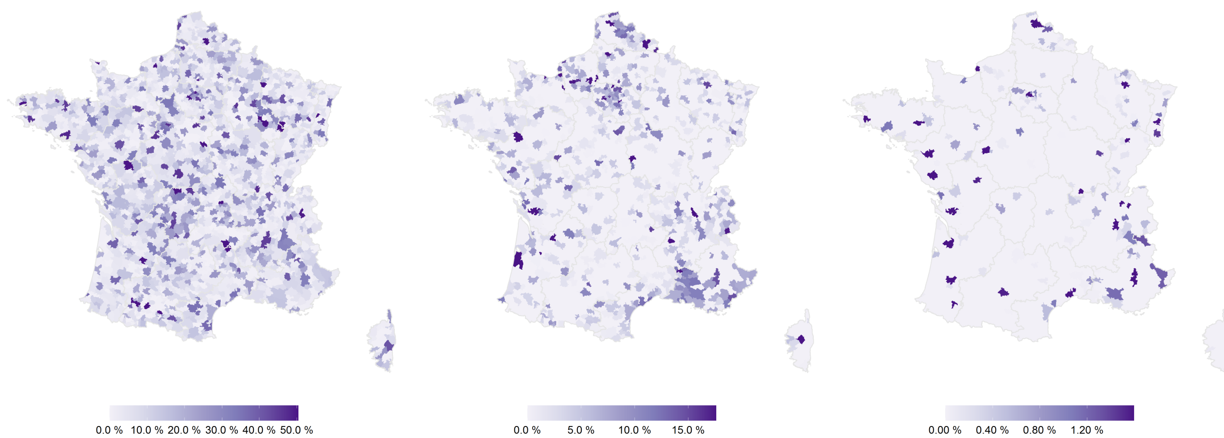
Third, the coefficient on the shift-share variable *BankExposure* would then be biased towards the coefficient with its "realized" quantity equivalent as an explanatory variable. Define $\mathbb{d}C_{bt}^{gov} = \sum_m \omega_{bmt-1}^{gov} \Delta C_{bmt}^{gov} = \frac{C_{bt}^{gov} - C_{bt-1}^{gov}}{C_{bt-1}^{tot}}$ the "realized" quantity equivalent of my shift-share variable (ignoring the distinction between mid-point and standard growth rates). By construction, $\mathbb{d}C_{bt}^{gov} = \lambda_{bt-1}^{gov} \hat{\alpha}_{bt}^{gov} + BankExposure_{bt}$ (see footnote 14). If *BankExposure* is contaminated by supply factors $\hat{\alpha}_{bt}^{gov}$, this biases the coefficient on *BankExposure* in the direction of that on $\mathbb{d}C_{bt}^{gov}$.

Figure B.4 depicts the relationships between $BankExposure_{bt}$, $\mathbb{d}C_{bt}^{gov}$ 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 $\mathbb{d}C_{bt}^{gov}$ (panel b), the regression of ΔC_{fbt} on $\mathbb{d}C_{bt}^{gov}$ yields an *opposite* sign

[30]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: $\hat{\alpha}_m^{gov}$ may be contaminated by local government credit supply shocks. while what mechanically enters the residual of my regression is corporate credit supply shocks.

FIGURE B.3. Municipality-level market shares by bank

(a) Bank A (b) Bank B (c) Bank C



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 2012.

(panel c). These considerations are robust to including estimated supply shocks $\hat{\alpha}^{gov}_{bt}$ and $\lambda^{gov}_{bt-1}\hat{\alpha}^{gov}_{bt}$ 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.

Note that including firm×time fixed effect is critical for the assumption (B.3) to plausibly hold. Otherwise, this condition would write:

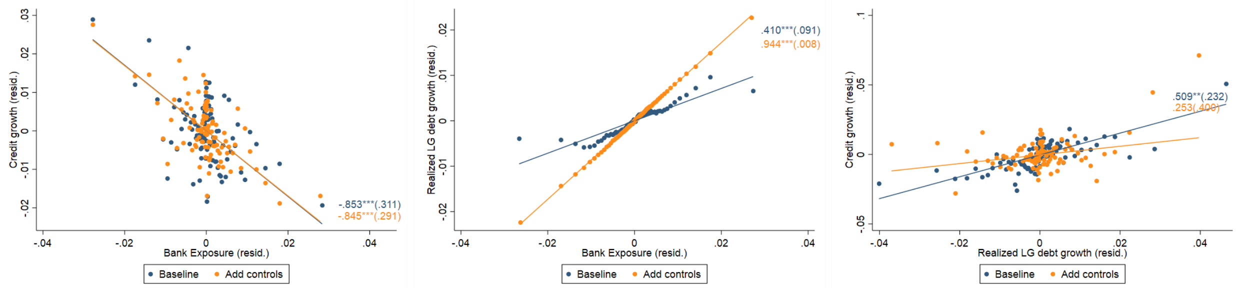
$$\mathbb{E}\left[\sum_m \hat{\alpha}^{gov}_m \left(\sum_f \bar{\omega}^{gov}_{fm} d_f + \sum_{f,b} \omega^{gov}_{bm} \varepsilon_{fb}\right)\right] = 0$$

where $\bar{\omega}^{gov}_{fm}$ is the sum of ω^{gov}_{bm} for the set of banks b lending to f. $\sum_f \bar{\omega}^{gov}_{fm} d_f$ is a weighted average of corporate credit demand shocks, 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, $\sum_f \bar{\omega}^{gov}_{fm} d_f$ will put a large weight on the corporate credit demand shocks of firms located in m. $\sum_f \bar{\omega}^{gov}_{fm} d_f$ is then likely to be correlated with $\hat{\alpha}^{gov}_m$. 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 Jar-

FIGURE B.4. Binned scatterplots of $BankExposure_{bt}$, $\mathbb{d}C^{gov}_{bt}$, and ΔC_{fbt}

(a) ΔC_{fbt} on $BankExposure_{bt}$ (b) $\mathbb{d}C^{gov}_{bt}$ on $BankExposure_{bt}$ (c) ΔC_{fbt} on $\mathbb{d}C^{gov}_{bt}$



Note: These figures present binned scatterplots corresponding to the regression of ΔC_{fbt} on $BankExposure_{bt}$ (panel a), $\mathbb{d}C^{gov}_{bt}$ on $BankExposure_{bt}$ (panel b) and ΔC_{fbt} on $\mathbb{d}C^{gov}_{bt}$ (panel c). I plot the binned scatterplots of the variables residualized on firm×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.

avel (2022) show that the conditions for consistency are that (i) there is a sufficiently large number of shocks with sufficient shock-level variation, and (ii) that shocks exposure is not too concentrated. Panel A of Table B.1 documents a large dispersion in $\hat{\alpha}^{gov}_{m}$, which persists when residualizing on fixed effects. Besides, exposure shares are not too concentrated. Define municipality-level weights as $s_{mt} = \sum_b C^{corp}_{bt-1}\omega^{gov}_{bm,t-1}$ where C^{corp}_{bt-1} are bank-level corporate credit weights. Panel B shows that the largest weight is 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.[31]

B.2. Identification based on shares

A correlation between $\hat{\alpha}^{gov}_{m}$ and any other municipality-level variable is problematic only to the extent that this other variable affects banks through the same exposure shares, i.e. that shares are correlated to bank-level credit supply shocks. As shown by Goldsmith-Pinkham, Sorkin, and Swift (2020), $\mathbb{E}[\varepsilon_{fb}\omega^{gov}_{bm}|d_f] = 0$ for all m with $\hat{\alpha}^{gov}_{m} \neq 0$ 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

[31] A a benchmark, Borusyak, Hull, and Jaravel (2022) show that their methodology is relevant in the canonical "China shock" setting where the inverse Herfindahl is 58.4 and the largest share is 6.5%.

TABLE B.1. 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 $\hat{\alpha}^{gov}_{mt}$. Panel B presents summary statistics of municipality-level weights $s_{mt} = \sum_b C^{corp}_{bt-1} \omega^{gov}_{bm,t-1}$ where C^{corp}_{bt-1} are bank-level corporate credit weights. Weights are normalized to sum to 1 for the whole sample. I compute the inverse Herfindahl index and the largest weight, and then the same quantities when weights are aggregated across time for a given municipality.

bank-level credit supply shocks. 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 Rotemberg weights—which summarize the identifying variation used by the shift-share variable—are very dispersed. The 5 largest Rotemberg weights account for 27% of the positive weight in the estimator.[32,33] Dispersed Rotemberg weights reduce the sensitivity of the shift-share variable 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 from shocks.

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

[33] 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×time-level shows that these highest weight municipalities vary across time.

Appendix C. Additional details and robustness checks

C.1. Cross-sectional effects on firm×bank credit

Euro-for-euro crowding out computation. Using the results in Table 2, I estimate the corporate credit shortfall compared to a counterfactual where the local government debt demand shocks α^{gov}_{mt} are all equal to 0. I assume all variables are equal to their sample means, denoted with an upper bar and ignore the distinction between mid-point and standard growth rates (which is innocuous for small growth rates). Then,

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

The corresponding increase in local government debt is $\hat{C}^{gov}_t - C^{gov}_t(\mathbf{0}) = \overline{\alpha}^{gov}_{mt} \times \overline{C}^{gov}_{t-1}$. The euro-for-euro crowding out coefficient is given by $\frac{\hat{C}^c_t - C^c_t(\mathbf{0})}{\hat{C}^{gov}_t - C^{gov}_t(\mathbf{0})} = \beta \times \frac{\overline{C}^c_{t-1}}{\overline{C}^{tot}_{t-1}} = 0.54$.

Distortions in the market for local government lending and crowding out. Table C.1 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 (2) 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. The results in columns (2)-(7) of Table C.1 show that the crowding out coefficient is not driven by instances where political interference is likely potent.

TABLE C.1. 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 estimated in Table 2 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 election results the share of votes for the incumbent differs by less than 6% from her closest rival. For mayors, I define these variables at the municipality (*EPCI*) level, taking the mayor of the largest *commune* in each *EPCI*. 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 credit. I then split banks along the median of this variable. "High X Exp" refers to high bank exposure to variable X. 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 firm×bank-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.

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

Table C.3 presents further tests related to the shift-share structure of *BankExposure*. The rationale for these tests and other tests related to the shift-share structure are further discussed in Appendix B. In column (1), I fix exposure weights in 2006. In column (2), I repeat the construction of $\hat{\alpha}^{gov}_{mt}$ excluding municipality×bank observations corresponding to market shares higher than 40%. In column (3), I regress ΔC_{fbt} on a leave-one-out version of $BankExposure_{bt,-m(f)}$ which does not consider the shock of the municipality m where the firm f is located. Column (4) controls for the bank×time fixed effects $\hat{\alpha}^{gov}_{bt}$ estimated in the Amiti-Weinstein decomposition (equation (3)) and their interaction with the local

Table C.2. 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 robustness checks of the main results presented in Table 2. “Active bank” is a dummy equal to 1 if the bank has a non-zero share of local government loans in its portfolio. “Regional shares(pub)” (“Regional shares(all)”) are 22 variables for the shares of each of the 22 French regions in the bank’s local government loan portfolio (total loan portfolio). “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*). 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 firm×bank-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.

government loan share λ^{gov}_{bt-1}. $\hat{\alpha}^{gov}_{bt}$ provides an estimate for any unobservable bank-specific credit supply shock. The point estimates remain highly similar to my baseline coefficient. In column (5)-(7), I conduct a placebo test where *BankExposure* is computed with exposure weights based on banks’ exposure to corporates $\omega^{corp}_{bmt-1} = C^{corp}_{bmt-1}/C^{tot}_{bt-1}$ instead of exposure to local governments. This further alleviates concerns that *BankExposure* is picking up municipality-level shocks occurring on the corporate credit market and potentially correlated to $\hat{\alpha}^{gov}_{mt}$.[34]

Robustness checks. Table C.4 shows the results when including additional controls and adding sample restrictions. Columns (1) and (2) report the results of estimating equation (2), without and with baseline controls, respectively. Column (3) adds more bank controls: 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) restricts the sample to banks with total loan portfolio (corporates and local governments combined) above €50 million. Column (5) restricts the sample to banks active in lending to local governments. All these specifications provide very similar results. In Figure C.1, I further test the sensitivity of

[34]This test is demanding since corporate and local government exposure weights—which are both largely determined by the banks’ branch network—are significantly correlated.

TABLE C.3. Firm×bank-level effects: Further tests of shift-share design

	Credit growth						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
BankExposure	-0.836***	-0.728***	-0.872***	-0.845***	-0.197	-0.288*	-0.261
	(0.306)	(0.277)	(0.310)	(0.291)	(0.167)	(0.167)	(0.175)
Controls	✓	✓	✓	✓	–	✓	✓
Add $\hat{\alpha}_{bt}^{gov}$	–	–	–	✓	–	–	–
Firm×Time FE	✓	✓	✓	✓	✓	✓	✓
Indep. var. def.	2006 shares	Excl. largest banks	Leave-one-out	Baseline	Corporate placebo	Corporate placebo	Corporate placebo
Sample	Full	Full	Full	Full	Full	Full	Active
Observations	2,709,023	2,731,110	2,710,202	2,611,795	2,744,597	2,731,110	2,582,698
R-squared	0.54	0.54	0.54	0.54	0.53	0.54	0.54

Note: This table presents robustness checks of the main results presented in Table 2. "Controls" include the bank's lagged local government loan share, assets (log), equity ratio, and dummies for state-owned and foreign banks. "Add $\hat{\alpha}_{bt}^{gov}$" indicates that $\hat{\alpha}_{bt}^{gov}$ estimated from (3) and its interaction with λ_{bt-1}^{gov} are included as controls. "Indep. var. def." refers to the definition of *BankExposure*. "Excl. largest banks" indicates that the α_{mt}^{gov} are estimated excluding bank observations corresponding to market shares larger than 40%. "Leave-one-out" indicates that $BankExposure_{bt,-m(f)}$ does not consider the shock of the municipality where the firm is located. "Corporate placebo" indicates that *BankExposure* is constructed with weights $\omega_{bmt-1}^{corp} = C_{mbt-1}^{corp}/C_{bt-1}^{tot}$. "Active" refers to banks with a non-zero share of local government loans in their portfolio. Regressions are weighted by firm×bank-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.

my results by showing the estimated coefficients for various perturbations of 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 control individually and 30 random draws of two to four controls within the set of available controls, for two different fixed effects structure, and with the baseline sample or the sample excluding state-owned banks.

TABLE C.4. Firm×bank-level effects: Additional controls and sample restrictions

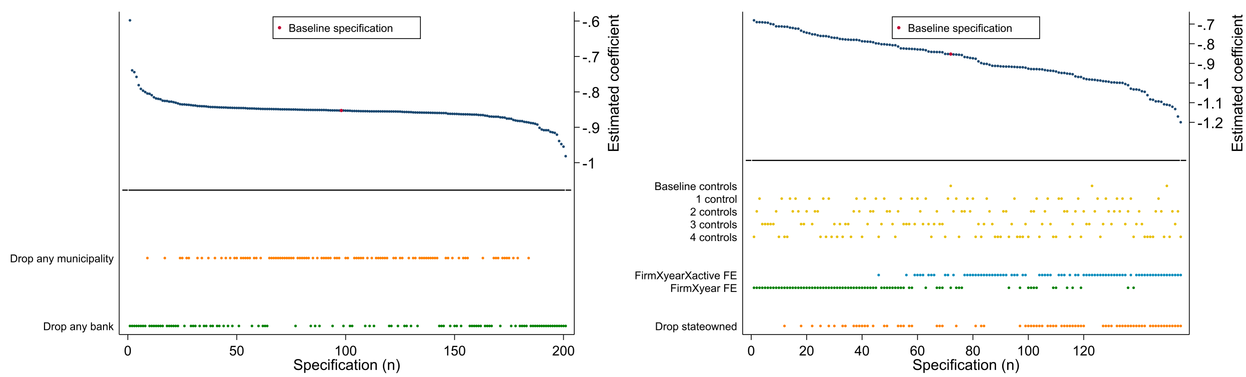
	Credit growth				
	(1)	(2)	(3)	(4)	(5)
BankExposure	-0.723**	-0.853***	-0.855***	-0.902***	-0.983***
	(0.310)	(0.311)	(0.306)	(0.313)	(0.316)
Baseline controls	–	✓	✓	✓	✓
Add. bank controls	–	–	✓	–	–
Firm×Time FE	✓	✓	✓	✓	✓
Sample	Full	Full	Full	$\geq$ 50€*M*	Active
Observations	2,731,110	2,731,110	2,731,110	2,631,988	2,582,698
R-squared	0.54	0.54	0.54	0.54	0.54

Note: This table presents robustness checks of the main results presented in Table 2. "Baseline controls" are the bank's lagged local government loan share, assets (log), equity ratio, and dummies for state-owned and foreign banks. "Add. bank controls" are 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. "Active" refers to banks with a non-zero share of local government loans in their portfolio. Regressions are weighted by firm×bank-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.

FIGURE C.1. 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 (2). 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 available controls individually and 30 random draws of two to four controls within the set of 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.

Table C.5 shows results for alternative definitions of dependent and independent variables. 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 the normalized first difference (bank×firm-level change in credit, normalized by firm total credit in the previous period). All three specifications yield a negative and significant effect. The coefficient on the positive truncation of the MPGR (column 1) 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 is intuitive: this is when banks have most leeway to adjust their credit supply. The coefficient on the standard growth rate (column 2) shows that it matters to consider the creation of new relationships. If the assumption that firm demand shocks are symmetric across the firm's banks holds for unit-changes as opposed to %-changes, then the correct specification is the one using the normalized first difference as an outcome variable (column 3). Accounting for the different normalization, the coefficient in column (3) is consistent with my baseline coefficient. In columns (4) to (6), I alter the definition of *BankExposure*. For column (4), the Amiti-Weinstein decomposition (3) is estimated without filtering out the bank×time cells that I identify as likely bank mergers (as detailed in Appendix F).[35] In columns (5) and (6), I fit the Amiti-Weinstein decomposition (3) aggregating local government loans at the *communes* (smaller) or *bassin de vie* (larger) levels instead of municipalities. Results are robust to these alternative definitions.

TABLE C.5. Firm×bank-level effects: 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. "MPGR (pos.)" is the bank×firm-level mid-point growth rate of credit, where negative values are replaced by zeros. "Std growth" is the bank×firm-level growth rate of credit. "Norm. diff." is the bank×firm-level change in credit, normalized by firm total credit in the previous period. "Indep. var. def." refers to the definition of *BankExposure*. The alternative definitions are detailed in the text. 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 firm×bank-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 C.6 presents results when excluding outliers in *BankExposure* and when changing clustering levels. *BankExposure* is bounded, since it is the average of estimated fixed effects $\hat{\alpha}_{mt}^{gov}$ 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 $\pm$ 2.5(p90-p10). The coefficient remains 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.

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 C.7 presents results for alternative weighting schemes. Results are highly similar to my baseline results.

[35] 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 C.6. 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	Municipality	Bank
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. In column (1), *BankExposure* is winsorized for values exceeding p50 ± 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 firm×bank-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.

TABLE C.7. Firm×bank-level effects: Alternative weighting scheme

	Baseline weighting			$\mathbb{P}$(multibank)-adjusted weight		
	(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	✓	✓	✓	✓	✓	✓
Weight winsorization	0%	1%	10%	0%	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. Regressions are weighted by firm-level mid-point credit. In columns (1) to (3), I vary the top-winsorization of the weights from 0 to 10%. In columns (4)-(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. The empirical specification is:

$$i_{lfbt} = d_{ft} + \beta BankExposure_{bt} + \Phi \cdot \mathbf{X}_{bt} + \Lambda \cdot \mathbf{W}_l + \varepsilon_{lfbt} \tag{C.1}$$

where the additional subscript l indexes loans. The interest rate is expressed in decimals (as opposed to percentage points). Loan-level controls $\mathbf{W}_l$ are the size of the loan and a granular set of fixed effects. I include maturity×index×time fixed effects. Maturity×time effects absorb changes in the yield curve. Further interacting with index estimates the yield curve separately for fixed rate loans, and by index for variable rate loans. I also include 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 banks. 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 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 C.8. 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 C.8. 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.1). 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 (4)). 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" refers to maturity×index×time and type of loan×time fixed effects. In column (4), I exclude firm×year observations with more than 25 new loans. In column (5), I include leasing contracts. In column (6), I include loans marked as benefiting from a subsidy. Regressions are weighted by the loan amount (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 firm-level 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 $d\bar{K}_{ft} = \eta^K \frac{\bar{K}_{ft}}{\bar{C}_{ft}} d\bar{C}_{ft}$, where upper bar denotes sample mean as found in Table 1. η^K is estimated in Table 4 and is equal to 0.23.

Effect on employment. Table C.9 provides the results of estimating equation (5) when the outcome is firm-level employment. The effect is very close to 0.

TABLE C.9. Firm-level effect on credit and employment

	Effect of exposure to local government debt shocks						Credit-to-emp. elasticity	
	gr(credit)			gr(emp)			gr(emp)	
	RF (1)	RF (2)	RF (3)	RF (4)	RF (5)	RF (6)	IV (7)	IV (8)
FirmExposure	-1.056***	-1.050***	-1.403***	-0.024	0.002	-0.040		
	(0.260)	(0.261)	(0.324)	(0.048)	(0.046)	(0.077)		
gr(credit)							0.004	0.022
							(0.028)	(0.041)
Firm controls	–	✓	✓	–	✓	✓	✓	✓
Municipality×Industry×Time FE	✓	✓	✓	✓	✓	✓	✓	✓
Firm FE	–	–	✓	–	–	✓	–	✓
Observations	807,979	807,979	780,138	766,288	766,288	738,302	699,170	668,566
R-squared	0.95	0.95	0.97	0.30	0.30	0.47	0.020	0.012
F stat.							21.1	18.6

Note: This table reports the results of estimating equation (5). 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 (6)). 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 revenues (log), debt/assets, EBIT/sales and capex/sales ratios (all lagged). Columns (7) and (8) show the credit-to-employment elasticities, 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.

Additional tests of identifying assumptions. Table C.10 presents further tests that support the identifying assumptions of my main results. Columns (1) to (6) display results for various fixed effects structure. Column (1) has the coarsest fixed effects structure: 12 industries × 22 regions × year. Column (6) has the finest fixed effects structure: ISIC 2-digit industries × 2081 municipalities × year, size × year, as well as firm fixed effects. Column (7) controls for lagged credit growth, which restricts the comparison to firms on a similar credit trajectory. Column (8) looks at the differential effect of exposure to crowding out for firms in industries highly reliant on public procurement.

TABLE C.10. Firm-level effects: Tests of identifying assumptions

Panel A: Credit

	gr(credit)							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
FirmExposure	-1.191***	-1.116***	-1.050***	-1.050***	-1.403***	-1.402***	-1.149***	-1.380***
	(0.316)	(0.277)	(0.261)	(0.260)	(0.324)	(0.323)	(0.322)	(0.345)
FirmExposure × Pub. Proc.								-0.082
								(0.267)
Controls	✓	✓	✓	✓	✓	✓	✓	✓
Industry(base)×Municipality×Time FE	–	–	✓	✓	✓	✓	✓	✓
Firm FE	–	–	–	–	✓	✓	–	✓
Industry(12)×Region×Time FE	✓	–	–	–	–	–	–	–
Industry(38)×Municipality×Time FE	–	✓	–	–	–	–	–	–
Size×Time FE	–	–	–	✓	–	✓	–	–
Lagged credit growth	–	–	–	–	–	–	✓	–
Observations	936,822	845,293	807,979	807,974	780,138	780,135	683,665	770,739
R-squared	0.93	0.95	0.95	0.95	0.97	0.97	0.96	0.97

Panel B: Investment

	gr(capital)							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
FirmExposure	-0.449***	-0.511***	-0.465***	-0.463***	-0.455***	-0.455***	-0.479***	-0.492***
	(0.081)	(0.081)	(0.079)	(0.080)	(0.110)	(0.111)	(0.124)	(0.130)
FirmExposure × Pub. Proc.								0.150
								(0.262)
Controls	✓	✓	✓	✓	✓	✓	✓	✓
Industry(base)×Municipality×Time FE	–	–	✓	✓	✓	✓	✓	✓
Firm FE	–	–	–	–	✓	✓	–	✓
Industry(12)×Region×Time FE	✓	–	–	–	–	–	–	–
Industry(38)×Municipality×Time FE	–	✓	–	–	–	–	–	–
Size×Time FE	–	–	–	✓	–	✓	–	–
Lagged credit growth	–	–	–	–	–	–	✓	–
Observations	913,372	822,281	785,314	785,311	757,023	757,021	670,136	747,811
R-squared	0.19	0.39	0.43	0.43	0.57	0.57	0.58	0.57

Note: This table presents robustness checks of the main results presented in Table 4. Controls include the firm-level average of the bank-specific controls, the estimated firm-level credit demand shock, the firm's revenues (log), debt/assets, EBIT/sales and capex/sales ratios (all lagged). "Industry(base)" are ISIC 2-digit industries. "Industry(12)" and "Industry(38)" are coarser classifications provided by the French Statistical Institute. "Size" is a dummy equal to 0 if the firm is classified as SME by the French Statistical Institute and 1 otherwise. "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*). 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.

Robustness checks. Table C.11 presents results of incorporating additional controls and of imposing additional sample restrictions. In column (1), I estimate equation (5) with only the average bank-level controls and the fixed effects (but omitting the estimated firm-level demand shock $\hat{d}_{ft}$ and other baseline firm-level controls). Column (2) is my baseline specification. Column (3) expands the set of controls to include the ROA, cash flow from operations to assets ratio, interest coverage ratio, and tangible asset ratio. Column (4) 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 start and the end of a relationship. Column (5) uses the firm-level demand shock estimated from specification (2) as opposed as from the Amiti-Weinstein decomposition. Column (6) restricts the sample to firms borrowing from at least two banks. Column (7) 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. The results are similar to the baseline across all these specifications.

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 C.12 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. The results are consistent with my baseline across all these specifications.

Table C.13 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 main bank levels, respectively. Main bank is defined as the bank from which the firm borrows the most in a specific year. Estimated coefficients are again similar to the baseline.

TABLE C.11. Firm-level effects: Additional controls and sample restrictions

Panel A: Credit

	gr(credit)						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
FirmExposure	-0.756***	-1.403***	-1.366***	-1.345***	-1.224***	-0.977***	-1.233***
	(0.259)	(0.324)	(0.319)	(0.316)	(0.206)	(0.332)	(0.335)
Wgt bank controls	✓	✓	✓	✓	✓	✓	✓
$\hat{d}_{ft}$	–	✓	✓	✓	–	✓	✓
$\hat{d}_{ft}$ (alt)	–	–	–	–	✓	–	–
Firm controls (base)	–	✓	✓	✓	✓	✓	✓
Firm controls (add)	–	–	✓	✓	–	–	–
Rel. controls	–	–	–	✓	–	–	–
FE	✓	✓	✓	✓	✓	✓	✓
Observations	1,023,539	780,138	730,820	730,820	780,119	228,292	545,175
R-squared	0.39	0.97	0.96	0.96	0.98	0.98	0.97

Panel B: Investment

	gr(capital)						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
FirmExposure	-0.334***	-0.455***	-0.431***	-0.430***	-0.424***	-0.553**	-0.369***
	(0.112)	(0.110)	(0.111)	(0.112)	(0.103)	(0.226)	(0.130)
Wgt bank controls	✓	✓	✓	✓	✓	✓	✓
$\hat{d}_{ft}$	–	✓	✓	✓	–	✓	✓
$\hat{d}_{ft}$ (alt)	–	–	–	–	✓	–	–
Firm controls (base)	–	✓	✓	✓	✓	✓	✓
Firm controls (add)	–	–	✓	✓	–	–	–
Rel. controls	–	–	–	✓	–	–	–
FE	✓	✓	✓	✓	✓	✓	✓
Observations	866,142	757,023	713,794	713,794	757,006	221,909	527,417
R-squared	0.48	0.57	0.58	0.58	0.57	0.64	0.60

Note: This table presents robustness checks of the main results presented in Table 4. "Wgt bank controls" refers to the firm-level average of the bank-specific controls included in Table 2. $\hat{d}_{ft}$ 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 within-firm specification). "Firm controls (base)" includes the firm's revenues (log), debt/assets, EBIT/sales and capex/sales ratios (all lagged). "Firm controls (add)" includes the ROA, cash flow from operations to assets ratio, interest coverage ratio, and tangible asset ratio (all lagged). "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" corresponds to baseline municipality×industry×time and firm 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.

TABLE C.12. Firm-level effects: Alternative weighting schemes

Panel A: Credit

	gr(credit)						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
FirmExposure	-1.360***	-1.395***	-1.408***	-1.160***	-1.345***	-1.359***	-1.322***
	(0.309)	(0.332)	(0.330)	(0.321)	(0.273)	(0.277)	(0.309)
Controls	✓	✓	✓	✓	✓	✓	✓
FE	✓	✓	✓	✓	✓	✓	✓
Weighting	C	C (1%)	C (10%)	K	K (0.5%)	K (1%)	K (10%)
Observations	780,138	780,138	780,138	778,691	778,691	778,691	778,691
R-squared	0.97	0.97	0.97	0.99	0.98	0.98	0.98

Panel B: Investment

	gr(capital)						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
FirmExposure	-0.563***	-0.453***	-0.360***	-0.236	-0.269*	-0.297**	-0.251***
	(0.164)	(0.101)	(0.093)	(0.184)	(0.147)	(0.135)	(0.065)
Controls	✓	✓	✓	✓	✓	✓	✓
FE	✓	✓	✓	✓	✓	✓	✓
Weighting	C	C (1%)	C (10%)	K	K (0.5%)	K (1%)	K (10%)
Observations	757,023	757,023	757,023	757,023	757,023	757,023	757,023
R-squared	0.60	0.56	0.51	0.55	0.53	0.52	0.46

Note: This table presents robustness checks of the main results presented in Table 4. The line Weighting refers to the weighting scheme. C indicates weighting by firm-level mid-point credit. K indicates weighting by firm-level lagged fixed assets. The number in parenthesis indicates the top-winsorization of weights. Controls are the firm-level average of the bank-specific controls, the estimated firm-level credit demand shock, the firm's revenues (log), debt/assets, EBIT/sales and capex/sales ratios (all lagged). "FE" corresponds to baseline municipality×industry×time and firm 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 C.13. Firm-level effects: Alternative variable definitions and clustering

Panel A: Credit

	gr(credit)						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
FirmExposure	-1.766***	-1.760***	-1.462***	-1.456***	-1.403***	-1.403***	-1.403***
	(0.367)	(0.365)	(0.330)	(0.327)	(0.115)	(0.163)	(0.343)
Firm controls	–	✓	–	✓	✓	✓	✓
FE	✓	✓	✓	✓	✓	✓	✓
Indep. var. def.	Alt. shares	Alt. shares	Winsor.	Winsor.	Baseline	Baseline	Baseline
Cluster	Baseline	Baseline	Baseline	Baseline	Firm	Municipality	Main bank
Observations	706,403	706,403	780,138	780,138	780,138	780,138	780,138
R-squared	0.96	0.96	0.97	0.97	0.97	0.97	0.97

Panel B: Investment

	gr(capital)						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
FirmExposure	-0.439***	-0.411***	-0.493***	-0.458***	-0.455***	-0.455***	-0.455***
	(0.121)	(0.118)	(0.129)	(0.114)	(0.084)	(0.093)	(0.109)
Firm controls	–	✓	–	✓	✓	✓	✓
FE	✓	✓	✓	✓	✓	✓	✓
Indep. var. def.	Alt. shares	Alt. shares	Winsor.	Winsor.	Baseline	Baseline	Baseline
Cluster	Baseline	Baseline	Baseline	Baseline	Firm	Municipality	Main bank
Observations	693,378	693,378	757,023	757,023	757,023	757,023	757,023
R-squared	0.57	0.58	0.56	0.57	0.57	0.57	0.57

Note: This table presents robustness checks of the main results presented in Table 4. "Indep. var. def." refers to the definition of *FirmExposure*. "Alt. shares" indicates that *FirmExposure* is constructed using lagged bank shares. "Winsor" indicates that *FirmExposure* is winsorized at the p50 $\pm$ 2.5(p90-p10) level. Columns (5)-(7) cluster standard errors alternatively at the firm, municipality and main 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 revenues (log), debt/assets, EBIT/sales and capex/sales ratios (all lagged). "FE" corresponds to baseline municipality$\times$industry$\times$time and firm fixed effects. 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

D.1. Baseline 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 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.

Firms. 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 of the firm f borrowing from bank b is assumed to be differentiated from all the varieties produced by the firms borrowing from bank b'.

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

The demand for intermediate input fb is given by:

$$Y_{fb} = P_{fb}^{-\sigma} Y \tag{D.1}$$

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 using a Cobb-Douglas production technology:

$$Y_{fb} = e^{z_{fb}} K_{fb}^{\alpha} L_{fb}^{1-\alpha} \tag{D.2}$$

z_{fb} are i.i.d. firm-level productivity shocks with mean Z^c. Intermediate input firms finance their stock of capital using equity and bank loans: $K_{fb} = C_{fb} + \overline{E}$. $\overline{E}$ is the same for all firms. A firm borrowing from bank b borrows at rate r_b^c. Profits are distributed to households.

Firms maximize profits, given by:

$$\max_{Y_{fb}, L_{fb}, C_{fb}} P_{fb} Y_{fb} - w L_{fb} - r_b^c C_{fb}$$

taking the demand curve (D.1) as given. The first-order conditions are:

$$\alpha \frac{\sigma - 1}{\sigma} P_{fb} Y_{fb} = r_b^c K_{fb} \tag{D.3}$$

$$(1 - \alpha) \frac{\sigma - 1}{\sigma} P_{fb} Y_{fb} = w L_{fb} \tag{D.4}$$

From these equations, we obtain the firms' input demand functions:

$$K_{fb} = e^{(\sigma-1) z_{fb}} \left(\frac{\sigma - 1}{\sigma}\right)^{\sigma} Y \left(\frac{1-\alpha}{w}\right)^{(1-\alpha)(\sigma-1)} \left(\frac{\alpha}{R_b^c}\right)^{1+\alpha(\sigma-1)} \tag{D.5}$$

$$L_{fb} = e^{(\sigma-1) z_{fb}} \left(\frac{\sigma - 1}{\sigma}\right)^{\sigma} Y \left(\frac{1-\alpha}{w}\right)^{\alpha+(1-\alpha)\sigma} \left(\frac{\alpha}{R_b^c}\right)^{\alpha(\sigma-1)} \tag{D.6}$$

Using (D.5) and $K_{fb} = C_{fb} + \overline{E}$ defines a credit demand function C_{fb} for each firm. Aggregating across the firms f, we obtain corporate credit demand at bank b:

$$C_b^c = \int_0^1 C_{fb} df$$

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 bank loans:

$$C_{mb}^g = g e^{\tilde{z}_{mb}^g} (r_b^g)^{\epsilon^g}$$

with $\epsilon^g \leq 0$. $\tilde{z}_{mb}^g$ is a demand shifter. I do not model the use of these funds, which is irrelevant for the quantification of crowding out. Total demand for local government loans directed to bank b is given by:

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

I define $\tilde{Z}_b^g = \int_0^1 \tilde{z}_{mb}^g dm$ and $\tilde{Z}^g = \int_0^1 \int_0^1 \tilde{z}_{mb}^g dm db$.

Households. For each bank b, there is a representative household depositing their savings at the 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:

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

Banks. Banks maximize the revenues from lending minus the cost of funds. They are price-takers.[36] They 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:

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

subject to: $C_b^c + C_b^g = S_b + B_b$. 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 $(\{Y_{fb}\}, \{K_{fb}\}, \{C_{fb}\}, \{L_{fb}\}, \{S_b\}, \{C_b^g\}, \{B_b\})$ and prices $(\{P_{fb}\}, \{r_b^c\}, \{r_b^g\}, \{r_b^s\}, i, w)$ such that:

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

(b) Bank's optimization: Taking $(\{r_b^c\}, \{r_b^g\}, \{r_b^s\}, i)$ as given, 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, the demand for funds equals the supply of funds $C_b^c + C_b^g = S_b + B_b$; the labor market clears $L = \int_0^1 \int_0^1 L_{fb} df db$; the interbank market clears $\int_0^1 B_b db = 0$.

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

In equilibirum, I obtain all prices and quantities as a function of the exogenous shocks $(\{z_{fb}\}, \{\tilde{z}^{g}_{mb}\})$.

Solution. I solve the model by log-linearisation around the deterministic equilibrium (DE), characterized by $z_{fb} = 0$ for all f, b and $\tilde{z}^{g}_{mb} = 0$ for all m, b. I denote $\hat{x}$ the relative change of variable x with respect to its DE value x^*. In the DE, quantities are the same for all firms, local governments and banks. Therefore, there is no interbank market borrowing.

Let us denote λ the share of local government loans in the bank loan portfolio in the DE, equal for all banks. I define $Z^g_b = \lambda \tilde{Z}^g_b$. Let $\ell = \frac{C^{corp*}}{K^*}$ be the share of capital financed by bank loans in the DE, equal for all firms.

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

$$\hat{r}_b = \hat{i} + \phi B_b \tag{D.7}$$

$$\lambda \hat{C}^g_b + (1-\lambda)\hat{C}^c_b = \hat{S}_b + \frac{1}{S^*} B_b \tag{D.8}$$

The firm capital and corporate credit demand functions write:

$$\hat{K}_{fb} = \ell \hat{C}_{fb} \tag{D.9}$$

$$\hat{C}_{fb} = \frac{1}{\ell}[(\sigma-1)z_{fb} + \hat{Y} - (1-\alpha)(\sigma-1)\hat{w} - (1+\alpha(\sigma-1))\hat{r}^c_b] \tag{D.10}$$

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

Starting from (D.8) and substituting the corporate credit demand (aggregated across firms borrowing from bank b), local government credit demand, the deposit supply function, aggregating across banks, and using the interbank market clearing condition yields:

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

Combining this equation with the aggregate versions of the firm first-order conditions ((D.3) and (D.4)) and the production function (D.2) yields the solution for all aggregate variables $\hat{Y}$, $\hat{w}$, $\hat{i}$, $\hat{K}$, $\hat{L}$, $\hat{C}^c$. The solution for $\hat{i}$ writes:

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

Finally, differencing the aggregate and bank-level balance sheet constraints (D.8) yields:

$$B_b = \frac{1}{\phi}\frac{Z^g_b - Z^g}{\epsilon^s - \lambda\epsilon^g - (1-\lambda)\epsilon^c + \frac{1}{\phi S^*}} \tag{D.11}$$

$$\hat{r}_b = \frac{Z^g + \frac{1-\lambda}{\ell}\frac{1+\psi}{1-\alpha}Z^c}{\epsilon^s - \lambda\epsilon^g + (1-\lambda)\frac{1}{\ell}\frac{1+\psi\alpha}{1-\alpha}} + \frac{Z^g_b - Z^g}{\epsilon^s - \lambda\epsilon^g - (1-\lambda)\epsilon^c + \frac{1}{\phi S^*}} \tag{D.12}$$

D.2. Aggregate and relative crowding out

I use the solution of the model to (i) formally define financial crowding out, and (ii) contrast the aggregate and the relative "across banks" crowding out effect.

Aggregate crowding out. In the presence of both firm and local government debt demand shocks, equilibrium change in corporate credit is given by:

$$\hat{C}^c = \Upsilon Z^c + (1+\kappa^{GE})\chi Z^g \tag{D.13}$$

where $\Upsilon = \frac{1}{\ell}\frac{\frac{1+\psi}{1-\alpha}(\epsilon^s - \lambda\epsilon^g)}{\epsilon^s - \lambda\epsilon^g + (1-\lambda)\frac{1}{\ell}\frac{1+\psi\alpha}{1-\alpha}}$, $\chi = \frac{\epsilon^c}{\epsilon^s - \lambda\epsilon^g - (1-\lambda)\epsilon^c}$, $\kappa^{GE} = \frac{\frac{1}{\ell}\frac{1+\alpha\psi}{1-\alpha}}{\frac{1}{\ell}(1+\alpha(\sigma-1))}\frac{\epsilon^s - \lambda\epsilon^g + (1-\lambda)\frac{1}{\ell}(1+\alpha(\sigma-1))}{\epsilon^s - \lambda\epsilon^g + (1-\lambda)\frac{1}{\ell}\frac{1+\psi\alpha}{1-\alpha}} - 1$.

The change in aggregate corporate credit attributable to crowding out is given by:

$$\mathcal{L}(C^c) = (1+\kappa^{GE})\chi Z^g$$

It corresponds to the change in corporate credit due to the local government debt demand shock directed to banks, compared to a counterfactual that keeps everything else constant (here, the aggregate shock hitting firms Z^c), but where banks do not need to absorb the local government debt demand shock.

What determines the size of this effect? I conveniently decompose the coefficient in front of Z^g into two terms. χ corresponds to the direct crowding out effect. It captures the extent of the interest rate increase in response to the demand shock (the denominator), and the extent of the decline in corporate credit for a given interest rate change (the elasticity of credit demand at the numerator). When $\epsilon^s \to +\infty$, χ tends to 0 and there is no crowding out. χ does not depend on interbank market frictions. κ^{GE} captures the general equilibrium feedback ocurring on the labor and product markets. 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 and the general equilibrium feedback $\kappa^{GE}\chi$ sum to the aggregate effect.

Crowding out at the aggregate and at the bank×firm-level. At the firm×bank-level, the counterpart of equation (D.13) writes:

$$\hat{C}_{fb} = \upsilon z_{fb} + (\Upsilon - \upsilon)Z^c + \kappa^{GE}\chi Z^g + \chi(1-\nu)Z^g + \chi\nu Z_b^g \tag{D.14}$$

where the additional parameters are $\upsilon = \frac{\sigma-1}{\ell}$ and $\nu = \frac{\epsilon^s - \lambda\epsilon^g - (1-\lambda)\epsilon^c}{\epsilon^s - \lambda\epsilon^g - (1-\lambda)\epsilon^c + \frac{1}{\phi S^*}}$.

Comparing equation (D.14) to equation (D.13) shows that at the firm×bank-level, the direct effect of crowding out χZ^g is split into two terms: $\chi(1-\nu)Z^g$ and $\chi\nu Z_b^g$. $\nu \in [0,1]$ captures the degree of interbank market frictions. It is monotonically increasing in ϕ. When $\phi \to 0$ (no interbank frictions), $\nu = 0$, and when $\phi \to +\infty$ (complete segmentation), $\nu = 1$. The effect of a bank-specific local government loan demand shock Z_b^g on bank-specific corporate credit supply is given by $\chi\nu$. When banks are perfectly integrated, corporate credit by bank b does not depend on the bank-specific shock, but only on the aggregate shock. Conversely, when banks are fully segmented, corporate credit by bank b only depends on the bank-specific shock, and not on the aggregate shock. As long as $\nu < 1$, banks not directly exposed to to local government loan demand shock lend to other banks on the interbank market, so that corporate credit also falls at these banks.

Link with the empirical specification. Equation (D.14) 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 assimilate observed growth rates ΔC_{fb} to log-deviations from the deterministic equilibrium $\hat{C}_{fb}$. The local government loan demand shock Z_b^g corresponds to *BankExposure*. In terms of units, $Z_b^g = \lambda\tilde{Z}_b^g$ is the change in local government credit demand normalized by banks' loan portfolio, consistent with the normalization of *BankExposure*. Aggregate variables are defined accordingly. Equation (D.14) then writes:

$$\Delta C_{fbt} = \upsilon z_{fbt} + (\Upsilon - \upsilon)Z_t^c + \chi(\kappa^{GE} + 1 - \nu)BankExposure_t + \chi\nu BankExposure_{bt} \tag{D.15}$$

The β coefficient that I estimate in the regression specification (2) corresponds to $\chi\nu$. A more general version of the model where firm productivity shocks differ across banks and with other bank-specific supply shocks is presented in section D.4.1 and highlights the identification challenges mentioned in the main text.

Missing intercept. Equation (D.14) clarifies that the cross-sectional coefficient $\chi\nu$ only accounts for part of the aggregate effect, because it misses equilibrium effects uniformly affecting all firms and banks. This is the usual "missing intercept" problem.

The model yields a closed form prediction for the missing intercept: it is equal to $\kappa^{GE}\chi + \chi(1-\nu)$ multiplied by the aggregate shock. 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 and the aggregate effect, consider the exercise consisting in cumulating corporate credit shortfalls relative to a situation in which all $\tilde{z}_m^g$ is 0, as implied by my cross-sectional coefficient. For each observation *fb*, the credit shortfall is given by $\mathcal{L}^{Xsec}(C_{fb}) = \chi\nu Z_b^g$.[37] Aggregating across firms, we obtain:

(D.16) $$\mathcal{L}^{Xsec}(C^c) = \int_0^1 \int_0^1 \chi\nu Z_b^g dfdb = \chi\nu Z^g$$

Next, consider the corporate credit shortfall taking into account the spillover effect due to capital mobility across banks:

(D.17) $$\mathcal{L}^{direct}(C^c) = \chi Z^g = \frac{1}{\nu}\mathcal{L}^{Xsec}(C^c)$$

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

(D.18) $$\mathcal{L}(C^c) = (1+\kappa^{GE})\chi Z^g = \frac{1+\kappa^{GE}}{\nu}\mathcal{L}^{Xsec}(C^c)$$

Unless $\kappa^{GE} = 0$ and $\nu = 1$, $\mathcal{L}^{Xsec}(C^c)$ differs from $\mathcal{L}(C^c)$.

The same reasoning applies for investment. The firm-level equation for capital writes:

(D.19) $$\hat{K}_{fb} = \ell\upsilon z_{fb} + \ell(\Upsilon - \upsilon)Z^c + \ell\kappa^{GE}\chi Z^g + \ell\chi(1-\nu)Z^g + \ell\chi\nu Z_b^g$$

[37] Using the notations of the empirical sections, ΔC_{fbt} would be higher by $\hat{\beta}BankExposure_{bt}$ if $BankExposure_{bt}$ were 0 instead of its actual value.

The cross-sectional coefficient I estimate in specification (5) corresponds to $\ell\chi\nu$. The difference between the reduced-form and the aggregate effect is again given by: $\mathcal{L}(K) = (1+\kappa^{GE})\mathcal{L}^{direct}(K) = \frac{1+\kappa^{GE}}{\nu}\mathcal{L}^{Xsec}(K)$.

D.3. Estimation of aggregate effect.

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

Aggregation using cross-sectional estimates. I first quantify $\mathcal{L}^{Xsec}(C^c)$ (equation (D.16)). When the distribution of firm and bank size is non-degenerate, $\mathcal{L}^{Xsec}(C^c)$ is:

$$\mathcal{L}^{Xsec}(C^c) = \chi\nu\sum_f\sum_b \frac{C^*_{fb}}{C^{c*}}Z^g_b = \chi\nu\sum_f \frac{C^*_f}{C^{c*}}Z^g_f$$

where $Z^g_f = \sum_b \frac{C^*_{fb}}{C^*_f}Z^g_b$ is the model equivalent of *FirmExposure*. For each time period, I estimate this quantity as:

$$\mathcal{L}^{Xsec}(C^c_t) = \hat{\beta}\sum_f \frac{C_{ft}(\mathbf{0})}{C^c_t(\mathbf{0})}FirmExposure_{ft} \tag{D.20}$$

I use $\hat{\beta}$ estimated from the firm-level specification (5) with credit growth as the outcome variable. 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. Weighting by DE credit C^*_f corresponds to weighting by counterfactual credit $C_{ft}(\mathbf{0})$, which can be estimated from the regression. I proceed similarly for capital:

$$\mathcal{L}^{Xsec}(K_t) = \hat{\beta}^K\sum_f \frac{K_{ft}(\mathbf{0})}{K_t(\mathbf{0})}FirmExposure_{ft} \tag{D.21}$$

$\hat{\beta}^K$ is the coefficient of the investment specification (5). Note that the credit-to-investment IV provides an estimate of ℓ.

I then 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.

These quantities depend on the estimated coefficients for credit $\hat{\beta}$ and capital $\hat{\beta}^K$. For $\hat{\beta}^K$, the coefficient remains highly similar in columns (4) to (6) of Table 4 and for the various lag specifications displayed in Figure 6. I thus use the coefficient of the baseline specification, including controls and firm fixed effects. For credit $\hat{\beta}$, the specification with firm fixed effects yields a point estimate higher than all the other specifications of Table 4 and Figure 6, which are very consistent among themselves. To avoid inflating the credit multiplier, I thus use the coefficient without firm fixed effects. In addition, to be consistent with weighting by the initial level, I use the coefficient of the specification with the standard growth rate (as opposed to mid-point growth rate) as the outcome. This yields the estimates presented in the main text.

Estimation of the interbank market spillover. To estimate ν, I use an additional prediction of the model. Namely, equation (D.11) can be rewritten as:

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

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 more general version of the model where firm productivity shocks differ across banks and where we allow for other bank-specific supply shocks ξ_b (extension D.4.1), this equation writes:

$$\frac{B_b}{S^*} = (1-\nu)\left[\lambda(\tilde{Z}_b^g - \tilde{Z}^g) + (1-\lambda)(\tilde{Z}_b^c - \tilde{Z}^c) - \frac{1}{S^*}\xi_b\right] \tag{D.22}$$

where $\tilde{Z}^c$ rescales firm productivity shocks into corporate credit demand shocks. This equation highlights two identification concerns: bank-level local government debt demand shocks $\tilde{Z}_b^g$ may be correlated with corporate credit demand shocks $\tilde{Z}_b^c$ or other corporate credit supply shocks ξ_b. I cannot resort to the within-firm identification strategy to control for firm-specific credit demand shocks. This also imply that the orthogonality condition regarding bank-level corporate credit supply shocks is more stringent as it 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 firms. I decompose credit flows into bank and borrower fixed effects by estimating $\Delta C_{ibt} = \alpha^D_{it} + \alpha^S_{bt} + \varepsilon_{ibt}$ where i can be either a firm or a municipality. Again following the Amiti and Weinstein (2018) logic, α^D_{it} captures borrower-specific (demand) factors, while α^S_{bt} captures bank-specific (supply) factors. I then aggregate the borrower fixed effects at the bank level using the share of each borrower as weights: $\alpha^D_{bt} = \sum_i \frac{C_{ibt-1}}{C_{bt-1}} \hat{\alpha}^D_{it}$. α^D_{bt} proxies for $\left[\lambda \tilde{Z}^g_{bt} + (1-\lambda)\tilde{Z}^c_{bt}\right]$. I also recover $\hat{\alpha}^S_{bt}$ which proxies for ξ_{bt}.

To estimate (D.22), I assimilate $\frac{B_b}{S^*}$ to the change in interbank borrowing normalized by the banks' lagged assets, denoted ΔB_{bt}.[38] I estimate

$$\Delta B_{bt} = \delta_t + \beta \alpha^D_{bt} + \varepsilon_{bt} \tag{D.23}$$

I can control for the estimated α^S_{bt}, other bank variables, and bank fixed effects.

Results. The results are presented in Table D.1. As predicted by the model, banks facing larger than average demand shocks borrow from other banks on the interbank market. In my baseline quantification, I use the average coefficient across the five specifications, equal to 0.15. With my estimates of $\chi\nu$ and ν, I obtain an estimate of the direct crowding out effect χ. I recover $\mathcal{L}^{direct}(C^c)$, $\mathcal{L}^{direct}(K)$, and $\mathcal{L}^{direct}(Y)$ by dividing their cross-sectional counterparts $\mathcal{L}^{Xsec}(.)$ by $\hat{\nu}$.

Other flows of funds across banks? Note that if the interbank market is not the only way to move capital across banks, my estimate of $1-\nu$ will underestimate the extent of capital reallocation across banks, and hence the size of the (negative) spillover effect. Then, my estimate of χ would be conservative.

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

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

[38] 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.

Table D.1. Estimation of the interbank market spillover

	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 (D.23). The outcome variable is the bank-level change in net interbank lending normalized by lagged assets. The main independent variable is the bank-level credit demand shock α^D_{bt} (defined in the text). "Est. supply shock" indicates that the estimated α^S_{bt} is included as a control. "Est. supply shock (pub/private)" indicates that 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.

κ^{GE} is increasing in labor supply elasticity ψ. The direct effect of the shock reduces the wage, and is amplified by the subsequent 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 increases, triggering a reallocation of demand toward less 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 is 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 since ψ, α and σ are common parameters for which the literature provides estimates.

Table D.2 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, I use ψ equal to 2 (Hall 2009), ψ equal to 0.58 (Chetty 2012) and ψ equal to 0 (to mute the labor supply response). For these parameter values, κ^{GE} varies from -16.5% to +8.0%. That is, $\mathcal{L}(C^c) \in [0.84\mathcal{L}^{direct}(C^c), 1.08\mathcal{L}^{direct}(C^c)]$. 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 is equal to $\mathcal{L}(Y) = \alpha\mathcal{L}(K) + (1 - \alpha)\mathcal{L}(L)$. The

TABLE D.2. 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}	-2.9%	-11.4%	-16.5%	1.5%	-6.3%	-11.0%	8.0%	1.6%	-2.4%
$\tilde{\kappa}^{GE}$	74.8%	17.2%	-16.5%	82.7%	24.1%	-11.0%	94.4%	34.5%	-2.4%

Note: This table reports the 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 that general equilibrium dampens the direct effect. In all cells, the capital share α is set to 1/3.

cross-sectional evidence does not reveal any effect on labor: $\mathcal{L}^{Xsec}(L) \approx 0$.[39] 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. In the case where $\psi > 0$, I assume that the aggregate labor shortfall is as predicted by the model. 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 D.2 shows that the general equilibrium feedback to go from $\mathcal{L}^{direct}(Y)$ to $\mathcal{L}(Y)$ is the same as for capital if labor supply is inelastic. If labor is allowed to respond, we instead observe a large further amplification.

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 the direct effect χ, and provide estimates that are likely to be on the conservative side.

Robustness checks. The quantification depends on the estimated relative crowding out effect $\hat{\beta}$ and $\hat{\beta}^K$, as well as on the estimated $\hat{\nu}$. Figure D.1 assesses the sensitivity of the aggregate multiplier estimate to the choice of empirical specifications. For $\hat{\beta}$ and $\hat{\beta}^K$, I use the coefficients obtained with various controls, fixed effects, weighting scheme, for the on-impact time t effect and the effect at $t + 2$. For $\hat{\nu}$, I use my baseline estimate, as well as the upper bound and lower bound of the coefficients in Table D.1. These figures show

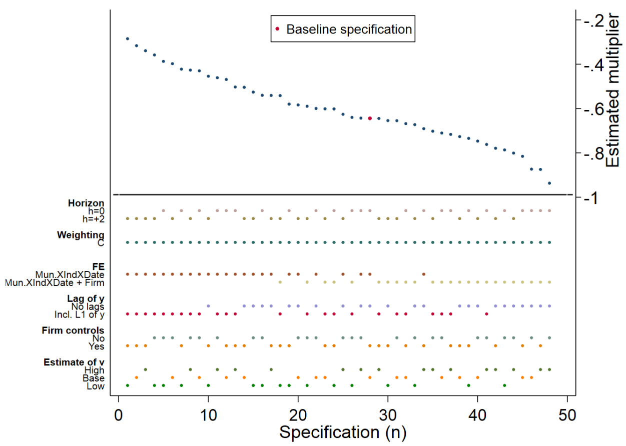
[39] Note that this contradicts model equation (D.6), which predicts a negative cross-sectional effect on labor due to capital-labor complementarities. The absence of cross-sectional effect may reveal that the drop in the marginal product of labor takes some time to materialize, even as investment falls, or may reflect the high degree of labor market frictions in France.

that my baseline quantifications fall well in the middle of the estimated ranges.

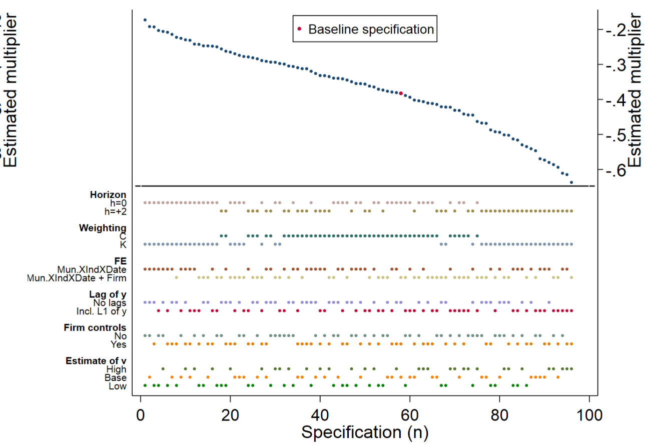
The quantification depends on the joint distribution of the shock and of firm size, which may not be the result of an invariant economic mechanism but rather of luck. I also provide the quantification of the output shortfall based on the assumption that all firms are symmetric, which neutralizes this effect. I obtain that the multipliers for corporate credit, capital, and output are equal to -0.66, -0.47, and -0.25, respectively.

FIGURE D.1. Aggregate effects: Specification curves

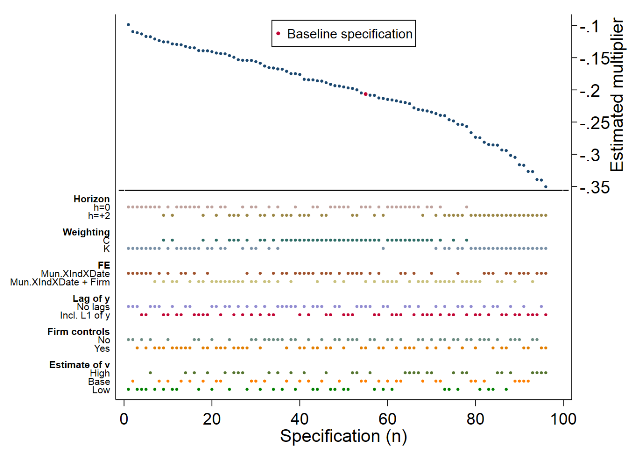
(a) Corporate credit



(b) Capital



(c) Output



Note: This figure shows the aggregate multipliers obtained depending on the specification choice. Panel (a) is the corporate credit multiplier. The specification elements refer to the credit coefficient obtain from the firm-level specification (5). Panel (b) is the capital multiplier. The specification elements refer to the capital coefficient obtained from the firm-level specification (5). Panel (c) is the output multiplier. The specification elements refer to the capital coefficient obtained from the firm-level specification (5). The red dot is the estimate provided in the main text.

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. This extended model provides a closer mapping to the empirical sections of the article. First, banks receive bank-specific liquidity shocks ξ_b. The balance sheet constraint of banks becomes $C_b^c + C_b^g = S_b + B_b + \xi_b$. Second, the firm productivity shocks are not i.i.d. across banks, so that there may be a correlation between firm-specific and bank-specific shocks. Third, 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 is 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 demand credit from each of their banks using an independent and identical demand function. This is the assumption in Khwaja and Mian (2008). Here, 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 (D.10). Solving the model with these modified assumptions yields:

$$\hat{C}_{fb} = \upsilon z_f + (\Upsilon - \upsilon) Z^c + \chi(\kappa^{GE} + 1 - \nu) Z^g + \chi\nu Z_b^g + \iota\nu\xi_b \tag{D.24}$$

All parameters are as before, except for $\upsilon = \frac{\sigma-1}{\ell}(1 + (1-\lambda)\chi\nu)$ and $\iota = -\frac{\chi}{S^*}$. Assimilating log-deviations to growth rates and the demand shock Z_b^g to *BankExposure*$_b$, this equation corresponds to my empirical specification (2). This equation clarifies the two identification concerns highlighted in Section 4.1: bank-level local government debt demand shocks Z_b^g may be correlated with firm-level corporate credit demand shocks z_f or with other bank-level corporate credit supply shocks ξ_b.

We can also write the equation for net interbank borrowing:

$$\frac{B_b}{S^*} = (1-\nu)\left[\lambda(\tilde{Z}_b^g - \tilde{Z}^g) + (1-\lambda)(\tilde{Z}_b^c - \tilde{Z}^c) - \frac{1}{S^*}\xi_b\right] \tag{D.25}$$

The notation $\tilde{Z}^c = \frac{\sigma-1}{\ell} Z^c$ rescales productivity shocks in corporate credit demand shocks.[40] This equation again highlights the identification challenges to estimate $1 - \nu$.

Conditional on obtaining unbiased estimates of the relevant parameters, the mapping frop reduced form to aggregate effects remains identical in this extended model.

Firms substitute across banks. I now assume that firms optimize on 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 (D.10) now corresponds to the demand for firm-level credit C_f. Let $Z_f^g = \int_{b \in \mathcal{B}_f} Z_b^g db$, $\xi_f = \int_{b \in \mathcal{B}_f} \xi_b db$. Solving the model with these modified assumptions yields:

$$\hat{C}_{fb} = \upsilon z_f + (\Upsilon - \upsilon) Z^c + \chi(\kappa^{GE} + 1 - \nu) Z^g + (\chi\nu - \tilde{\chi}\tilde{\nu}) Z_f^g + \tilde{\chi}\tilde{\nu} Z_b^g + (\iota\nu - \tilde{\iota}\tilde{\nu})\xi_f + \tilde{\iota}\tilde{\nu}\xi_b \tag{D.26}$$

$$\hat{C}_f = \upsilon z_f + (\Upsilon - \upsilon) Z^c + \chi(\kappa^{GE} + 1 - \nu) Z^g + \chi\nu Z_f^g + \iota\nu\xi_f \tag{D.27}$$

$\tilde{\chi}$ 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 $\tilde{\iota} = -\frac{\tilde{\chi}}{S^*}$.

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

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

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.

Empirically, I find that $\tilde{\chi}\tilde{\nu}$ (the coefficient of the firm$\times$bank-level relationship) is approximately equal to $\chi\nu$ (the coefficient of the firm-level relationship). This suggests that the elasticity of bank-specific credit demand $-\theta$ is approximately equal to the firm-level elasticity ϵ^c, so that omitting this distinction is innocuous.

D.4.2. Introducing a cost of bank leverage

I now assume that on top on the interbank market friction, banks face a cost to increase their total debt-taking. This could be due to regulatory leverage constraints that limit banks ability to take on debt. Banks now maximize:

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

subject to: $C_b^c + C_b^g = S_b + B_b + E_b$. I include a fixed equity amount per bank $E_b = E^*$ so that the problem makes sense in the limit $\varphi \to +\infty$. Let us denote $\mathcal{E}(\varphi)$ the ratio of bank equity to total balance sheet size in the DE, which is a function of φ.

Let us define $\tilde{\epsilon}^s(\varphi) = \frac{\epsilon^s(1-\mathcal{E}(\varphi))}{1+\frac{\epsilon^s \varphi S^*}{1+\varphi S^*}}$. In this alternative model, equations (D.13) and (D.14) are unchanged but one has to substitute $\tilde{\epsilon}^s(\varphi)$ for ϵ^s in the definition of χ, ν, and κ^{GE}.

The aggregate crowding out parameter is now a function of $\tilde{\epsilon}^s(\varphi)$. When $\varphi = 0$ and $E^* = 0$, we recover $\chi = \frac{\epsilon^c}{\epsilon^s - \lambda\epsilon^g - (1-\lambda)\epsilon^c}$. When $\varphi \to +\infty$, the aggregate supply of lending is fixed and determined by the amount of equity. To see this, take the simplest case where local government debt demand is inelastic. Then, when $\varphi \to +\infty$, $\chi = \frac{1}{1-\lambda}$, i.e. the euro increase in local government loans equals the euro reduction in corporate lending.

ν has the same interpretation as before. Equation (D.22) remains unchanged: as before, ν can be estimated using interbank flows. Therefore, the estimation of the direct effect χ combining the reduced form coefficient and the estimate of ν remains exact. Finally, since I do not need to separately estimate ϵ^s, the procedure to recover κ^{GE} is unchanged. Therefore, the quantification provided in the main text is fully consistent with this alternative model.

D.4.3. Adding bank lending to households

I assume that households have the following credit demand function:

$$C_b^h = h(r_b^h)^{\epsilon^h}$$

The problem of the bank now writes:

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

subject to: $C_b^c + C_b^g + C_b^h = S_b + B_b$. Let λ_g, λ_c, and λ_h be the shares of local government loans, corporate loans, and household loans in the bank loan portfolio in the DE, respectively.

In this case, equations (D.13) and (D.14) are unchanged but the parameters are given by $\chi = \frac{\epsilon^c}{\epsilon^s - \lambda^g \epsilon^g - \lambda^c \epsilon^c - \lambda^h \epsilon^h}$, $\nu = \frac{\epsilon^s - \lambda^g \epsilon^g - \lambda^c \epsilon^c - \lambda^h \epsilon^h}{\epsilon^s - \lambda^g \epsilon^g - \lambda^c \epsilon^c - \lambda^h \epsilon^h + \frac{1}{\phi S^*}}$ and $\kappa^{GE} = \frac{\frac{1}{\ell}\frac{1+\alpha\psi}{1-\alpha}}{\frac{1}{\ell}(1+\alpha(\sigma-1))} \frac{\epsilon^s - \lambda^g \epsilon^g - \lambda^h \epsilon^h + \lambda^c \frac{1}{\ell}(1+\alpha(\sigma-1))}{\epsilon^s - \lambda^g \epsilon^g - \lambda^h \epsilon^h + \lambda^c \frac{1}{\ell}\frac{1+\psi\alpha}{1-\alpha}} - 1$. The direct crowding out coefficient χ now also depends on the share of lending to households and on their elasticity of demand. As before, the share of the effect that is captured by the cross-sectional term depends on $\nu \in [0, 1]$. Equation (D.22) remains true and ν can be estimated using interbank flows. Hence, the estimation of the total direct effect provided in the main text remains exact. Introducing household loans affects the general equilibrium feedback term. I obtain a wider range for $\tilde{\kappa}^{GE}$, from -35.2% to 126.0%.

Appendix E. Details on the TFP loss derivation

This Appendix quantifies the TFP loss attributable to crowding out.

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 $r_{fs} = r(1+\tau_{fs}^K)$. In my model, the dispersion in τ_f^K fully comes from dispersion in interest rates across firms borrowing from different banks. The derivation of the TFP loss that follows is very general and holds for any distortion in firm-level actual or allocative input prices (such as distortionary regulation or taxation, financial constraints, or imperfect competition). The modified first-order condition for capital writes:

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

Write sector-level output as $Y_s = \text{TFP}_s\, K_s^{\alpha_s} L_s^{1-\alpha_s}$ where $K_s = \sum_f K_{fs}$ and $L_s = \sum_f L_{fs}$. Using a second order approximation around zero wedges or a log-normality assumption on $\log(A_{fs})$ and τ_{fs}^K, Hsieh and Klenow (2009) show that sector-level TFP is given by:

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

where the variance is taken over all firms within each sector and $\text{TFP}_s^* = (\sum A_{fs}^{\sigma-1})^{\frac{1}{\sigma-1}}$. I used the approximation $\text{Var}(\log(\text{MRPK}_{fs})) = \text{Var}(\log(1+\tau_{fs}^K)) = \text{Var}(\tau_{fs}^K)$. The first term corresponds to TFP under the optimal allocation of resources and the second term to misallocation. When wedges are highly dispersed, there are large gains from reallocating inputs away from firms with low MRPK toward firms with high MRPK.

I consider firm exposure to the credit supply shock generated by crowding out as a shock to the wedges.[41] 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 $\mathcal{L}(\text{TFP}_t) = \log(\text{TFP}_t) - \log(\text{TFP}_t(\mathbf{0}))$.

E.2. Quantification

Measurement of wedges. Nominal output $P_{fs}Y_{fs}$ is defined as value added. The capital stock is defined as the value of fixed assets, net of depreciation. Then, $\text{MRPK}_{fs} = \alpha_s \frac{P_{fs}Y_{fs}}{K_{fs}}$. To obtain α_s, I estimate industry-specific Cobb-Douglas production functions at the 2-digit level using the cost shares method, where the labor share is the ratio of sectoral labor compensation over value added.

Reduced-form effect of crowding out on wedges. Quantifying the TFP loss requires estimates of the counterfactual wedges $\tau^K_{ft}(\mathbf{0})$. I follow Bau and Matray (2023) and estimate the effect of *FirmExposure* on wedges using the specification for firm-level inputs (equation (5)) with $\Delta\tau^K_{ft}$ as the dependent variable, allowing for heterogeneity by ex-ante wedge:

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

The outer product denotes that I include all interacted and non-interacted terms. The results are reported in Table E.1. 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 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 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. This corroborates the findings of Table 5.

[41] In considering a shock to financing conditions as a shock to wedges, I follow Larrain and Stumpner (2017) and Blattner, Farinha, and Rebelo (2023). The observed reduction in firms' input usage (Table 4) is to be understood as the reaction to this shock to wedges.

TABLE E.1. 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	-1.403***	0.361**	-1.363***	-1.470***	0.077	0.707***
	(0.324)	(0.161)	(0.291)	(0.349)	(0.226)	(0.190)
Controls	✓	✓	✓	✓	✓	✓
Municipality×Industry×Time FE	✓	✓	✓	✓	✓	✓
Firm FE	✓	✓	✓	✓	✓	✓
Observations	780,138	763,319	135,657	561,037	130,266	553,609
R-squared	0.97	0.57	0.96	0.97	0.65	0.61
Credit-to-wedge IV		-.183**			-.093	-.282***
		(.081)			(.141)	(.101)
High *minus* Low (RF)				-.112		.589***
				(.326)		(.225)
High *minus* Low (IV)						-.201
						(.144)

Note: This table examines the crowding out effect of local government debt on corporate credit and on the capital wedge. It reports the results of estimating specification (5). The outcome variables are the firm-level mid-point growth rate of credit and the change in the capital wedge. The main independent variable is firm exposure to crowding out (defined in (6)). In columns (3) to (6), the sample is splitted along a dummy equal to 1 if the lagged capital wedge is above the first within-industry quartile. The line labeled IV shows the credit-to-wedge elasticities, obtained by instrumenting firm-level credit growth by *FirmExposure*. The lines High *minus* Low report the coefficient on the interaction term in the full sample specification and its standard error. Controls include the firm-level average of the bank-specific controls, the firms' revenues (log), debt/assets, EBIT/sales and capex/sales ratios (all lagged), 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. 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. $\hat{\tau}^K_{ft} - \tau^K_{ft}(\mathbf{0}) = \hat{\beta}_0 FirmExposure_{ft} + \hat{\beta}_1 FirmExposure_{ft}\mathbb{1}[High\ \tau^K_{f,t-1}]$ yields $\tau^K_{ft}(\mathbf{0})$. The TFP loss is then given by:

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

I compute the TFP loss for each industry and aggregate across industries using industry shares in value added. I use $\sigma = 3$.

I find that crowding out reduces aggregate TFP by 0.037% 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.

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 generate a larger drop in capital for firms with higher ex-ante wedges. Second, because banks are segmented, the distribution of local government lending across banks generates variation in credit supply shocks across firms. To assess the relative importance of these channels, I decompose the TFP loss as:

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

where $\overline{\mathbf{F}}_t$ is the counterfactual where changes in local government debt are equal at all banks. 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 (<€0.01 per €1 of local government loans). 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.[42] Second, the aggregate cost of the distributive effects induced by bank segmentation is negligible.

Limitations and robustness This computation is subject to several caveats. First, the previous computation assumes that $\log(\text{TFP}_t^*)$ is unaffected by the shock. This assumption would be violated if credit shocks affect firm-level productivity A_{ft}. Unfortunately, this cannot be tested in the absence of data on firm-level product quantities. Second, 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.

As robustness checks, quantifications of the TFP loss accounting for the presence of labor wedges or using the alternative estimation strategy developed in Sraer and Thesmar (2023) yield very similar results.

[42] In contrast, Blattner, Farinha, and Rebelo (2023) quantify misallocation induced by a credit shock concentrated on high-wedge firms.

Appendix F. Data

This article uses data collected by Banque de France. The data was accessed through the Banque de France virtual Open Data Room, then transferred to CASD.[43]

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 (SCR). 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 2012. I classify entities as local government entities based on their legal status (4xxx and 7xxx). All other entities are considered private corporations.

The French banking sector experienced a significant consolidation over the sample period, which is reflected by the number of banks decreasing from 455 in 2006 to 307 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). Locations correspond to the geographical identifier of the borrower. The

[43] The application procedure is detailed at https://www.casd.eu/en/your-project/procedures-dhabilitation/

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

Corporate tax-filings (FIBEN). 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.

New contracts (NCE). I obtain data on interest rates for a representative sample of new loans in each quarter from the dataset "New Contracts" (*Nouveaux Credits aux Entreprises)* collected by Banque de France.

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* (tables *SITUATION* and *CPTE RESU*) 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 only covers 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.