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.

Keywords: Government debt, Crowding out, Banks.

JEL Codes: E22, E44, E62, G21, G32, H74.

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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 debt 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 case of local government bank debt. This is an important finding given the surge in local government debt. This is also the first quantification of the financial crowding out effect for any type of government

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

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 2,080 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 multibank 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.

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

[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.

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

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$\times$industry$\times$time cell. These firms are therefore subject to a similar local-level change in local government debt, but differ in their exposure to crowding out because they borrow from different sets of banks. I also 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 €0.29 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 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

Schnabl (2017).

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

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. 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 problematic 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. Recent contributions by Priftis and Zimic (2021) and Broner et al. (2022) show that, across countries, fiscal multipliers increase 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.[6]

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. Relatedly, Becker and Ivashina (2018) show that banks' holdings of sovereign bonds crowd out corporate credit during the European sovereign debt crisis, and Williams (2018) and Önder et al. (2024) document this phenomenon in Colombia. These studies look at developing countries (typically characterized by shallow capital markets and a strong home bias for sovereign debt), or focus on state-owned banks and political interference.[7] In addition, except for Önder et al. (2024), these studies focus on micro-level effects. Önder et al. (2024) also quantify aggregate effects. The focus of their aggregate analysis is the total output response to an increase in government debt (accounting for the change in other fiscal variables implied by their model's fiscal rule). By contrast, the object of interest in this article is the output loss attributable to financial crowding out, holding constant other effects of fiscal policy.

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 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.[8] My results imply that, when the supply of debt is imperfectly elastic, debt-financed

[6]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; de Fraisse 2023), but have no direct implication for aggregate investment and output.

[7]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), benefit from a preferential treatment in case of default (Broner et al. 2014), or make losses on lending to governments (Hoffmann, Stewen and Stiefel 2022).

[8]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.

multipliers will be lower than transfer-financed multipliers.

My results 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). The wide range of estimates of debt multipliers (see Ramey 2019) likely reflects the fact that multipliers are not a structural parameter but rather depend on many forces, for instance the strength of aggregate demand effects or the stance of monetary policy. My article allows to "unpack" debt multipliers by precisely pinning down one important channel—financial crowding out—independently of these other forces.

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.

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.[9] 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.[10]

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 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.

[9]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 $\mathrm{d}G = \mathrm{d}T + \mathrm{d}D^g$.

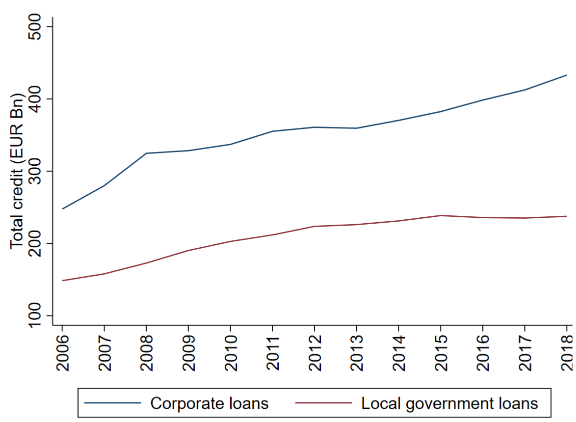
[10]This definition of financial crowding out is in the spirit of Diamond (1965).

3. Data and institutional setting

3.1. Data

My main data source is the 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. 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.[11] 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. For local governments, I have 61,881 unique local governments and 196,750 unique local government-bank pairs. 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 use banks' balance sheets from regulatory filings. More details on the data can be found in Appendix F.

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.

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

[11] 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.

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

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
$BankExposure_{bt}$ (%)	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} (€Mn)	1,010	1,437	3.76	574	3,225	909	1,459	0.33	245	2,961
Total loans C_{bt}^{tot} (€Mn)	6,858	9,896	717	2,745	28,901	6,906	10,140	358	2,643	29,819
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	1352.1	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.

Geographic units. The credit registry provides the location of borrowers. I sort borrowers across 2,080 time-invariant "municipalities", the geographic units defined by 2016 inter-*communes* cooperation structures (*EPCI*). Throughout the text, municipalities correspond to geographical units, not to layers of subnational governments. Municipalities are a good approximation of local lending markets: the average bank branch 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 13% 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 to local governments are also large from the point of view of banks: from Figure 1, 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 (Table A.1), the largest share goes to the four layers of elected local governments (communes, inter-*communes* cooperation structures, departements, and regions, accounting for 64% of the total), followed by public housing (21%) and public hospitals (11%). 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. Figure A.3 illustrates that new debt issuance is indeed strongly predicted by local government capital expenditures. This implies a 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 (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 large banks. The market is split between national and local banks (defined as banks operating in less than 20% of municipalities), the latter accounting for 44% of corporate credit. Most banks lend to both firms and local government, but there is heterogeneity across banks in the share of their lending going to local governments. Figure A.4 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, holding constant other effects of fiscal policy. I estimate the following specification:

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

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 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:

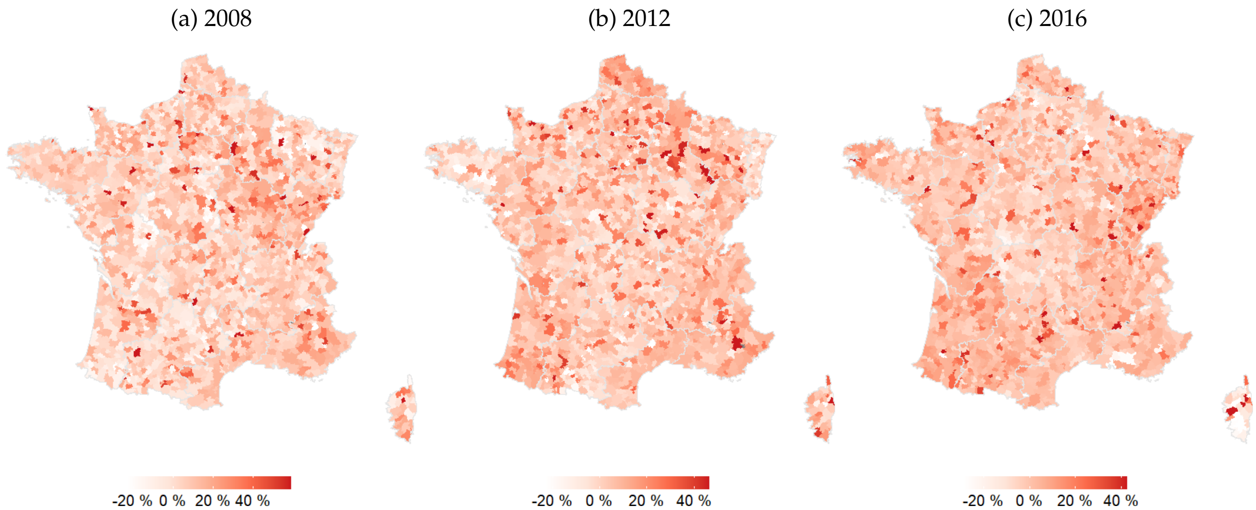
$$\Delta C^{gov}_{mbt} = \alpha^{gov}_{mt} + \alpha^{gov}_{bt} + \varepsilon_{mbt} \tag{3}$$

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).[12] The bank fixed effects α^{gov}_{bt} measure the variation in banks' lending that is common across municipalities, like bank-level supply factors. Similarly, the municipality fixed effects α^{gov}_{mt} measure the change in credit explained by municipality factors. Through the lens of the canonical Khwaja and Mian (2008) model, the parameters α^{gov}_{mt} provide estimates of structural municipality demand shocks. For my purpose, the assumption is that the α^{gov}_{mt} are estimates of municipality-level drivers of 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

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

FIGURE 2. Local government debt demand shocks by municipality



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.

across municipalities and within municipality across time, reflecting the lumpy nature of local government capital expenditure. In Appendix B.1, I link the estimated $\hat{\alpha}^{gov}_{mt}$ to local government debt demand shifters identified via a narrative approach. These demand shifters are well captured by the municipality fixed effects, demonstrating the effectiveness of the Amiti-Weinstein approach in systematically identifying municipality-level shifts in demand.

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

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

$\omega^{gov}_{bm,t-1}$ is bank b's exposure to local government credit in municipality m relative to its total credit.[13] *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.[14] The exposure weights $\omega^{gov}_{bm,t-1}$ sum to banks' local government loan share $\lambda^{gov}_{b,t-1} = C^{gov}_{b,t-1}/C^{tot}_{b,t-1}$ which is always included as a control (as recommanded by Borusyak, Hull

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

[14] 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 $BankExposure_{bt} =$

and Jaravel 2022). Table 2 reports descriptive statistics on the components of $BankExposure_{bt}$ by year.

Table 2. Bank exposure to local government debt demand shocks

	2007	2008	2009	2010	2011	2012	2013	2014	2015	2016	2017	2018
$BankExposure_{bt}$ (%)												
Mean	1.67	1.62	1.61	0.99	0.75	1.05	0.25	0.37	0.47	0.04	0.00	-0.13
Std. dev.	2.33	1.84	2.25	1.33	1.18	1.38	0.86	0.71	0.83	1.22	0.96	0.61
Std. dev., resid.	1.83	1.22	1.74	0.92	0.92	0.96	0.94	0.69	0.72	1.42	1.17	0.99
$\hat{\alpha}^{gov}_{mt}$ (%)												
Mean	9.71	9.14	10.44	6.19	4.44	5.26	0.34	2.03	2.49	0.20	-0.29	-0.96
Std. dev.	9.68	7.51	8.48	7.57	6.71	6.82	6.80	6.00	8.00	9.34	11.45	6.10
ω^{gov}_{bmt}(%)												
Mean	0.01	0.01	0.01	0.01	0.01	0.01	0.01	0.01	0.01	0.01	0.01	0.01
Std. dev.	0.18	0.17	0.16	0.18	0.19	0.19	0.20	0.18	0.18	0.19	0.18	0.17

Note: This table displays, for each year, the mean and standard deviation of $BankExposure_{bt}$ (weighted by bank credit), $\hat{\alpha}^{gov}_{mt}$ (weighted by municipality credit), and ω^{gov}_{bmt} (weighted by bank×municipality credit). "Std. dev., resid." is the standard deviation of $BankExposure_{bt}$ residualized on the local government loan share $\lambda^{gov}_{b,t-1}$.

4.2. Identifying assumptions

The goal is to identify the relative crowding out effect β^C. My empirical design is meant to address two main threats to identification that arise in this setting.[15] This design will be valid if the standard orthogonality condition is satisfied:

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

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. The main concern is that 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.[16]

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

$\frac{1}{C^{tot}_{b,t-1}} \sum_m \tilde{\omega}^{gov}_{mb,t-1} \times \hat{d}C^{gov}_{mt}$: 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.

[15] Model equation (D.28) in Appendix D.4.1 formalizes these identification concerns.

[16] Appendix B.3 further discusses the potential sources of correlation between *BankExposure* and firm demand.

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-specific demand for local government loans. Hence, the within-firm design allows to estimate the (bank-level) financial crowding out effect, holding other effects of government debt constant.

This design requires that any residual firm×bank demand shock not absorbed by the firm fixed effects is orthogonal to *BankExposure*. 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 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.2). Adding bank×time fixed effects increases the adjusted R-squared by only 6%. Section 4.4 presents additional tests supporting this assumption.

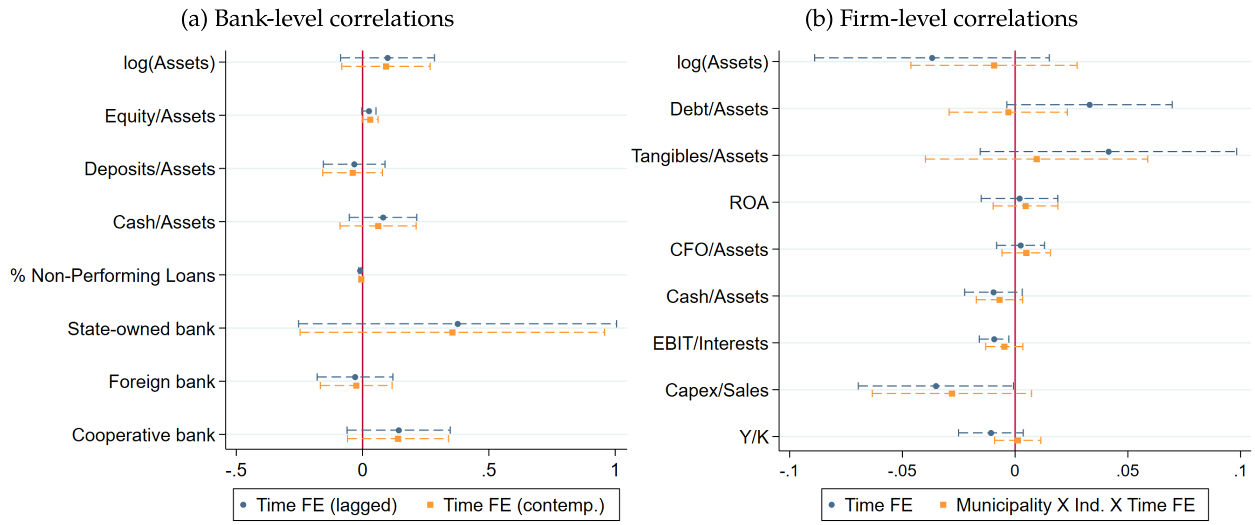
Correlated bank-level credit supply shocks. A second hurdle is that lending to local governments and corporates are jointly determined in banks' optimization problem and may be correlated. For instance, a bank-level liquidity shock will affect its lending to both local governments and firms. Banks may 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. $BankExposure_{bt}$ shifts the realized quantity, but the shift-share structure combined with the Amiti-Weinstein shocks is designed to purge $BankExposure_{bt}$ from bank b's supply factors that may also enter the residual ε_{fbt}.[17]

The first threat to this design is if the Amiti-Weinstein decomposition (3) does not correctly purge the estimated $\hat{\alpha}^{gov}_{mt}$ from banks' supply factors. Appendix B.1 provides a test, exploiting two large bank supply shocks identified from a narrative analysis. Municipality exposure to the supply shocks predicts credit growth but *not* the estimated municipality fixed effects, supporting the hypothesis that the procedure correctly filters out supply factors. Additionally, in Appendix B.2, I assess the ability of the Amiti-Weinstein decomposition to estimate municipality-specific demand shocks in a simulation study. The procedure performs well, even when the data-generating process deviates from the benchmark Khwaja and Mian (2008) model. I identify cases where it fails. For each case where it fails, I develop tests or refinements of the baseline strategy.

Second, even if the fixed effects $\hat{\alpha}^{gov}_{mt}$ perfectly estimate municipalities' true demand shocks,

[17]Figure C.1 plots the relationships between *BankExposure* and realized local government growth.

Figure 3. Balance tests



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$\times$industry$\times$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.

assumption (A1) will be violated 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).[18] One example of problematic story is if $\hat{\alpha}^{gov}_{mt}$ is correlated to corporate defaults in m, and if this affects banks' ability to lend through the same exposure weights $\omega^{gov}_{bm,t-1}$ (then, *BankExposure* would be correlated with bank-level corporate defaults, likely to act as a negative supply shock).

The most direct test supporting assumption (A1) is bank-level balance on observables. Figure 3(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.

Appendix B.3 provides a detailed discussion of identification with the shift-share design following the "shifters-based" approach in Borusyak, Hull and Jaravel (2022). I summarize the key intuitions here. First, the local government debt demand shocks $\hat{\alpha}^{gov}_{mt}$ are not correlated with other

[18]Given the firm$\times$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*.

municipality-level outcomes that could generate corporate credit supply shocks for banks located in these municipalities (like corporate defaults in the example above).[19] Second, I use shares specific to the local government credit market, that differ from shares in the corporate credit market. This reduces the risk that *BankExposure* picks up banks' exposure to unobserved municipality-level shocks in the corporate sector that could correlate with $\hat{\alpha}_{mt}^{gov}$. Third, the $\hat{\alpha}_{mt}^{gov}$ are not persistent, reflecting the lumpy nature of local government capital expenditure. Combined with the fact that the shares are 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 $\hat{\alpha}_{mt}^{gov}$ municipalities. Together, these facts support the hypothesis that high *BankExposure* banks are not subject to systematically different supply shocks, as reflected in the bank-level balance tests.

4.3. Baseline results

Table 3 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 4.4 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×time fixed effects as well as

[19] 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 $\hat{\alpha}_{mt}^{gov}$ across neighboring municipalities.

Table 3. 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.

controls. The point estimate remains similar, slightly higher in absolute value.

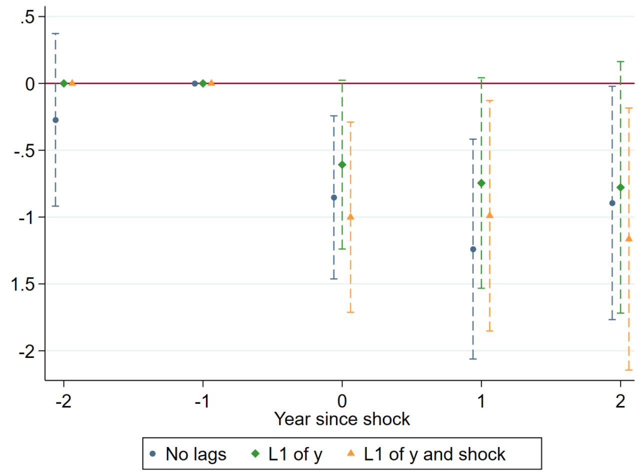
The point estimate implies that a one standard deviation increase in *BankExposure* reduces corporate mid-point credit growth by 1.22pp (or equivalently, the standard growth rate by 1.23pp). As a back-of-the-envelope computation assuming all variables are equal to their sample means, the coefficient 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).

Figure 4 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.

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.5 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.

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. The fact that the

FIGURE 4. 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 $(C_{f,b,t+h} - C_{f,b,t-1})/0.5(C_{f,b,t+h} + C_{f,b,t-1})$. "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 3. The dot is the point estimate and the bar is the 95% confidence interval.

coefficient in column (1) is less negative than that in columns (2)-(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 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.

4.4. 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

[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.

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 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) any residual firm×bank demand shock not absorbed by the firm fixed effects is orthogonal to *BankExposure*; and (A1-b) there are no other bank-level credit supply shocks that are systematically correlated with *BankExposure*.

More granular fixed effects: (A1-a) will be violated if firms have bank-specific demand shocks that are systematically correlated with *BankExposure*. To alleviate this concern, I further interact the firm×time fixed effect with a dummy equal to 1 if the bank is active in lending to local governments, a dummy equal to 1 if the bank is specialized in the firm's industry, or a dummy equal to 1 if the bank is specialized in the firm's area. I also consider excluding banks that account for more than 70% of the firm's credit to alleviate concerns that firms have demand shocks specific to their main bank. (A1-b) will be violated if banks lending to local governments receive different credit supply shocks. One concern 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×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: Table C.3 presents additional tests of the identifying assumption. First, to alleviate concerns that banks with negative corporate credit supply shocks strategically relocate in high $\hat{\alpha}^{gov}_{mt}$ municipalities, I fix exposure weights in 2006. Second, I present a placebo test where *BankExposure* is defined with corporate credit exposure weights, alleviating concerns that *BankExposure* picks up exposure to municipality-level shocks occurring on the corporate credit market. Third, I define $BankExposure_{bt,-m(f)}$ leaving out the shock $\hat{\alpha}^{gov}_{mt}$ of the municipality where f is located, i.e., the $\hat{\alpha}^{gov}_{mt}$ shock that most likely correlates with firm demand. Fourth, I estimate (2) including as a control the estimated fixed effect $\hat{\alpha}^{gov}_{bt}$, which provides an estimate for unobserved bank supply shocks.

Table C.3 additionally presents a series of tests related to the construction of the demand shocks $\hat{\alpha}^{gov}_{mt}$. To alleviate concerns that the estimated $\hat{\alpha}^{gov}_{mt}$ 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. To alleviate concerns that the Amiti-Weinstein procedure fails if bank supply shocks are heterogeneous across municipalities or if municipalities have bank-specific demand shocks, I estimate augmented versions of equation (3) further interacting bank fixed effects with municipalities' characteristics or including additional controls for bank$\times$municipality-level demand effects. These specifications produce coefficients similar to my baseline results.

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.2 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 various assumptions on 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 4 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. Liq-

Table 4. 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.

uidity 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 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 C.8. First, I document that the crowding out effect is asymmetric: increases in local government debt lead to a reduction in corporate credit, while reductions in local government debt do not significantly increase corporate credit. This is in line with the mechanism proposed: constrained banks have more leeway to adjust to a reduction in credit demand (e.g., by holding liquid assets instead of increasing credit) than to an increase. Second, splitting the sample in two subperiods, I find that crowding out is more severe over 2007-2013—corresponding to the Great Financial crisis and the Euro Area sovereign debt crisis—than over 2014-2018, a period with no notable financial turmoil and characterized by an accommodative monetary policy which likely relaxed banks' balance sheet constraints (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.10 shows that the price effect is positive, consistent with a reduction in credit supply.[21] 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 estimates of the price elasticity of corporate credit demand. This result is usually rationalized by concerns about adverse selection effects of higher interest rates (Stiglitz and Weiss 1981).[22]

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 affect firm-level credit and investment?

[21] 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.

[22] 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 (2024) yields an extensive margin elasticity of –109.

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:

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

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

$$\mathit{FirmExposure}_{ft} = \sum_b \omega_{fb,t-1} \mathit{BankExposure}_{bt} \tag{6}$$

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. $\mathit{FirmExposure}_{ft}$ 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): $\Delta C_{ft} = d_{ft} + \beta^C \mathit{FirmExposure}_{ft} + \varepsilon_{ft}$. 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.[23] This procedure precisely controls for the correlation between *FirmExposure* and d_{ft}. Identification of β^C in the firm-level credit growth regression then follows from identification in the firm×bank-level credit growth specification.

When looking at investment, the coefficient of interest β^K corresponds to $\beta^C \times \eta^K$, the effect on credit multiplied by the credit-to-investment sensitivity η^K. The identifying assumption be-

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

comes 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}. To alleviate potential concerns with this assumption, I further tighten my identification strategy by looking at the effect of *FirmExposure within* municipality×industry×time cells. Municipality×time fixed effects imply that I only compare firms experiencing a similar local-level increase in local government debt, partialling out the local-level macroeconomic relationship between local government debt and 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 banks. In addition, I exploit the panel structure of the data and include firm fixed effects that control for any firm-specific time-invariant determinant 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 3(b) tests whether firms with higher *FirmExposure* are systematically different on observed characteristics. I report unconditional correlations and correlations conditional on municipality×industry×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 6.3 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.[24] 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-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 6.3 provides results with alternative specifications.

[24]Figure A.5 provides a visual representation of the sample selection imposed by the tax-filings.

Table 5. 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*** (0.260)	-1.050*** (0.261)	-1.402*** (0.323)	-0.476*** (0.086)	-0.465*** (0.080)	-0.452*** (0.108)		
gr(credit)							0.288*** (0.057)	0.231*** (0.047)
Firm controls	–	✓	✓	–	✓	✓	✓	✓
Municipality×Ind.×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.							23.5	24.3

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 3 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.

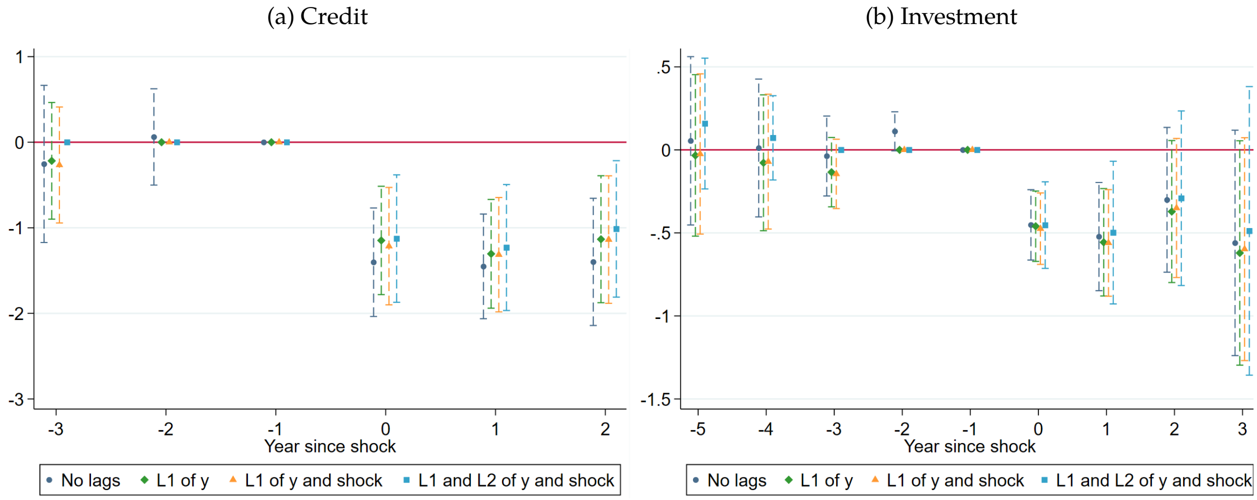
6.2. Results

I first repeat the within-firm estimation on the tax-filings subsample to obtain the relevant magnitudes. Table C.9 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 5 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

FIGURE 5. Firm-level effects of crowding out: dynamic effect



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 $(C_{f,t+h} - C_{f,t-1})/0.5(C_{f,t+h} + C_{f,t-1})$. For investment, the outcome is the h-horizon growth rate $(K_{f,t+h} - K_{f,t-1})/K_{f,t-1}$. "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 of the shock as controls. "L1 and L2 of y and shock" adds two lags of the outcome variable and of the shock as controls. All other elements of the specifications are as in Table 5. The dot is the point estimate and the bar is the 95% confidence interval.

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 €0.29 drop in corporate investment at firms borrowing from this bank (computation details are in Appendix C.3).

Figure 5 shows the effect of firm exposure to crowding out at longer horizons by estimating local projections. The effect of *FirmExposure* on credit and investment does not mean revert in the 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.

Other firm-level outcomes. Using the same empirical strategy, I investigate the effects of the crowding out-induced credit cut on other firm outcomes. First, I show that the reduction in corporate credit does not lead to a significant increase in bond or equity issuance (Fig. C.4). This suggests that firms only have a limited ability to substitute towards equity or bonds in response to a contraction in corporate credit, rationalizing the decline in investment. Second, I estimate the response of firm-level employment and wage bill (Fig. C.5). Both trend downwards after the shock, but the effects are too small to be statistically significant. 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 small effect is consistent with the expected size of the indirect effect due to capital-labor complementarities. Third, I investigate how the crowding out-induced reduction in credit and investment affects firm-level output (Fig. C.6). I find evidence of a negative effect from one year after the shock onward. The timing is consistent with the idea that reduced investment only impairs output with a lag: new capital may require time-to-build, and firms that do not invest may be able to maintain production using their old capital stock for a while until the effects of underinvestment kick in. In terms of magnitudes, the relationship between the effect on capital and that on output is as predicted by standard production function parameters.

6.3. Further tests and robustness checks

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

More granular fixed effects: Table C.11 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.[25]

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.11).

Robustness checks. I perform a variety of robustness checks of my results, detailed in Appendix C.3. Table C.12 reports results when progressively adding the baseline firm-level controls, when

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

including additional firm-level controls, when including additional controls related to banking relationships, or when imposing additional sample restrictions. Table C.13 explores the results with alternative weighting strategies. Table C.14 presents the results with an alternative definition of *FirmExposure*, different winsorization, and different assumptions on the appropriate level of clustering. Table C.15 reports results when using alternative versions of the estimated firm demand shock. The estimated coefficients are similar across all specifications. Finally, Figure C.3 presents robustness tests of the local projection results.

6.4. Heterogeneous effects

Table 6 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.

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

Table 6. 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	-1.427***	-1.412***	-1.515***	-1.264***	-1.371***	-1.736***	-1.253***	-1.523***
	(0.476)	(0.326)	(0.316)	(0.364)	(0.335)	(0.442)	(0.378)	(0.326)
Controls	✓	✓	✓	✓	✓	✓	✓	✓
Municipality×Ind.×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)		.015		.250		-.111		-.270
		(.433)		(.265)		(.256)		(.316)

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	0.210	-0.509***	-0.226	-0.583***	-0.518***	0.015	-0.166	-0.615***
	(0.370)	(0.108)	(0.144)	(0.136)	(0.134)	(0.362)	(0.176)	(0.130)
Controls	✓	✓	✓	✓	✓	✓	✓	✓
Municipality×Ind.×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	0.57	0.59	0.61	0.59	0.72	0.65	0.58
Credit-to-inv. IV	-.085	.248***	.134**	.303***	.274***	.002	.105	.270***
	(.131)	(.05)	(.056)	(.075)	(.072)	(.196)	(.08)	(.062)
High *minus* Low (RF)		-.721*		-.357**		.563		-.450**
		(.375)		(.158)		(.346)		(.225)
High *minus* Low (IV)		.336**		.169**		-.245		.165*
		(.136)		(.076)		(.239)		(.094)

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 75th 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.

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 %-change shortfall attributable to crowding out as $\mathcal{L}(Y_t) = \log(Y_t) - \log(Y_t(\mathbf{0}))$. I 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 and a fixed amount of equity. Local governments borrow from banks. There is a continuum of banks; they are funded via deposits and lend to firms and local governments. Banking relationships enter the model through the assumption that firms and local governments are assigned to a given bank. Imperfect capital mobility across banks enters the model through the assumption that depositors do not arbitrage across banks. An interbank market can be accessed at a cost. All decisions are static. 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 $Y = \left(\int_0^1 \int_0^1 Y_{fb}^{\frac{\sigma-1}{\sigma}} df db\right)^{\frac{\sigma}{\sigma-1}}$. 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 a fixed amount of equity $\overline{E}$ and bank loans C_{fb}: $K_{fb} = C_{fb} + \overline{E}$. Solving the firm's problem yields a demand curve for capital for firm f borrowing from bank b.

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

where $\bar{c}$ is a constant. This implicitly defines a corporate credit demand curve with an elasticity denoted $\epsilon^c \geq 0$, as well as a credit-to-investment elasticity denoted $\ell \leq 1$. 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 $\epsilon^g \geq 0$. 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. Access to the interbank market is subject to a quadratic cost indexed by ϕ.

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:

$$\hat{C}^c = \Upsilon^c Z^c + \underbrace{(1+\kappa^C_{\mathrm{GE}})\chi^C Z^g}_{\mathcal{L}(C^c)} \tag{8}$$

where Υ^C, χ^C, and κ^C_{GE} depend on model parameters (see Appendix equation (D.13)). The corporate credit shortfall due to crowding out is $\mathcal{L}(C^c) = (1+\kappa^C_{\mathrm{GE}})\chi^C Z^g$. 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? $\chi^C < 0$ 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^C tends to 0 and there is no crowding out. κ^C_{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:

$$\hat{C}_{fb} = \upsilon^C z_{fb} + (\Upsilon^C - \upsilon^C)Z^c + \underbrace{\kappa^C_{\mathrm{GE}}\chi^C Z^g + \chi^C(1-\nu)Z^g + \chi^C \nu Z^g_b}_{\mathcal{L}(C^c)} \tag{9}$$

Crowding out now corresponds to the last three terms. The term $\kappa^C_{\mathrm{GE}}\chi^C Z^g$ is as in equation (8). The direct crowding out effect $\chi^C Z^g$ is split into two terms: $\chi^C(1-\nu)Z^g$ depends on the aggregate shock, while $\chi^C \nu Z^g_b$ 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^g_b 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^C 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 $\chi^C(1-\nu)Z^g$ implies that each bank's corporate credit supply is negatively affected by the aggregate local government loan demand shock.

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 debt demand shock Z_b^g to my demand shifter $BankExposure_{bt}$. 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. That is, $\beta^C = \chi^C\nu$.

Characterization of the missing intercept. I rewrite the aggregate crowding out effect $\mathcal{L}(C^c)$ as:

$$\mathcal{L}(C^c) = \underbrace{\kappa_{\mathrm{GE}}^C\chi^C Z^g}_{\textit{GE feedback}} + \underbrace{\underbrace{\chi^C(1-\nu)Z^g}_{\textit{Spillover across banks}} + \underbrace{\chi^C\nu Z^g}_{\textit{Cross–sectional effect } \mathcal{L}^{Xsec}(C^c)}}_{\textit{Direct effect } \mathcal{L}^{direct}(C^c)} \tag{10}$$

$\mathcal{L}^{Xsec}(C^c) = \chi^c\nu Z^g$ is the credit shortfall implied by my cross-sectional estimate β^C. Quantifying the total effect $\mathcal{L}(C^c)$ requires to account for two components of the missing intercept. Adding the spillover across banks $\chi^c(1-\nu)Z^g$ yields the shortfall implied by the direct effect of crowding out $\mathcal{L}^{direct}(C^c) = \chi^c Z^g$. Adding the general equilibrium feedback yields the total effect $\mathcal{L}(C^c)$.

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 contains information. First, the fact that I estimate $\beta^C = \chi^C\nu \neq 0$ implies that $\chi^C \neq 0$: we can reject the null that crowding out has no direct effect on corporate credit. Second, $\chi^C \leq \chi^C\nu$, and hence, $\mathcal{L}^{direct}(C^c) \leq \mathcal{L}^{Xsec}(C^c)$: the cross-sectional effect captures only the part of the direct effect that has cross-sectional implications due to banking frictions, and thus underestimates the direct effect.

Other variables. I show that the same logic applies to other endogenous variables. For a generic variable X, the aggregate crowding out effect can be decomposed into $\mathcal{L}(X) = \kappa_{\mathrm{GE}}^X\chi^X Z^g + \chi^X(1-\nu)Z^g + \chi^X\nu Z^g$, where $\chi^X\nu$ corresponds to the effect identified by the cross-sectional regression, $\chi^X(1-\nu)$ is the spillover term, and $\kappa_{\mathrm{GE}}^X\chi^X$ is the general equilibrium feedback. For instance, for investment, the coefficient β^K identified in specification (5) corresponds to $\chi^K\nu = \ell\chi^C\nu$, and $\kappa_{\mathrm{GE}}^K = \kappa_{\mathrm{GE}}^C$. Appendix equations (D.21)-(D.23) detail the coefficients χ and κ_{GE} for all variables. An important result is that ν is the same for all variables.

7.3. Quantification

Corporate credit and capital. It is immediate that $\mathcal{L}^{direct}(C^c) = \frac{1}{\nu}\mathcal{L}^{Xsec}(C^c)$ and $\mathcal{L}(C^c) = (1 + \kappa^C_{GE})\mathcal{L}^{direct}(C^c)$. I thus quantify the aggregate crowding out effect by combining: (i) the cross-sectional effect identified from my empirical analysis $\mathcal{L}^{Xsec}(C^c)$; (ii) an estimate of ν; (iii) an estimate of κ^C_{GE}.

Shortfall from cross-sectional estimates. For each time t, 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:

$$\mathcal{L}^{Xsec}(C^c_t) = \hat{\beta}^C \sum_f \frac{C_{ft}(\mathbf{0})}{C^c_t(\mathbf{0})} FirmExposure_{ft} \tag{11}$$

This is the empirical counterpart to the model object $\chi^c \nu Z^g_t$ (which assumes a degenerate distribution of baseline firm and bank size). This quantity can be readily estimated from the data.[26] I can similarly estimate the investment shortfall $\mathcal{L}^{Xsec}(K_t)$ using the coefficient of the investment regression. Computation details are in Appendix D.3.

Spillover across banks. The extent of the spillover due to capital flows across banks depends on ν. 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, $\mathcal{L}^{Xsec}(.)$ underestimates the direct effect $\mathcal{L}^{direct}(.)$ by 15%.

General equilibrium feedback. Finally, the general equilibrium feedback κ^C_{GE} introduces a wedge between the direct effect χ^C 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

[26] I use β^C estimated in the firm-level specification (5)—as opposed to the firm×bank-specification—so that the estimated aggregate effects account for potential substitution across banks (see Appendix D.4.1).

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.3 in Appendix D.3 calibrates the general equilibrium term κ^C_{GE}. For plausible parameter values, general equilibrium effects can be positive or negative and are small in magnitude. To avoid introducing additional uncertainty related to calibrated parameter values, I thus use the approximation $\kappa^C_{\text{GE}} \approx 0$ and use my estimates of the direct effect χ^C as the total effect. $\kappa^K_{\text{GE}} = \kappa^C_{\text{GE}}$, so I use the same assumption for the capital shortfall.

Results. From this analysis, the yearly corporate credit shortfall due to crowding out implied by my cross-sectional estimates $\mathcal{L}^{Xsec}(C^c_t)$ is equal to 0.85% on average. 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 –0.54 on average across years. For capital, I find a multiplier equal to –0.32.

Accounting for the missing intercept, the aggregate corporate credit loss due to crowding out $\mathcal{L}(C^c_t)$ is equal to 1.00% on average across years. Equivalently, €1 of local government loans crowds out €0.64 of corporate credit. The capital shortfall is equal to 0.28%, corresponding to a multiplier equal to –0.38. These multipliers are summarized in Table 7. I provide point estimates and 95% confidence intervals implied by the cross-sectional estimates.

Table 7. Aggregate effects of crowding out

	Multiplier			
	Implied by cross-sectional estimates		Aggregate effect	
	Estimate	95% C.I.	Estimate	95% C.I.
Corporate credit	–0.54	[–0.75,–0.34]	–0.64	[–0.89,–0.39]
Capital	–0.32	[–0.48,–0.17]	–0.38	[–0.57,–0.20]
Aggregate output	–0.17	[–0.26,–0.09]	–0.21	[–0.30,–0.11]

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 two columns refer to the aggregation implied by the cross-sectional coefficients. The last two columns refer to the estimate of aggregate effects accounting for equilibrium effects of crowding out. Reported multipliers are averages of yearly multipliers. See appendix D.3 for details on the construction of the confidence intervals.

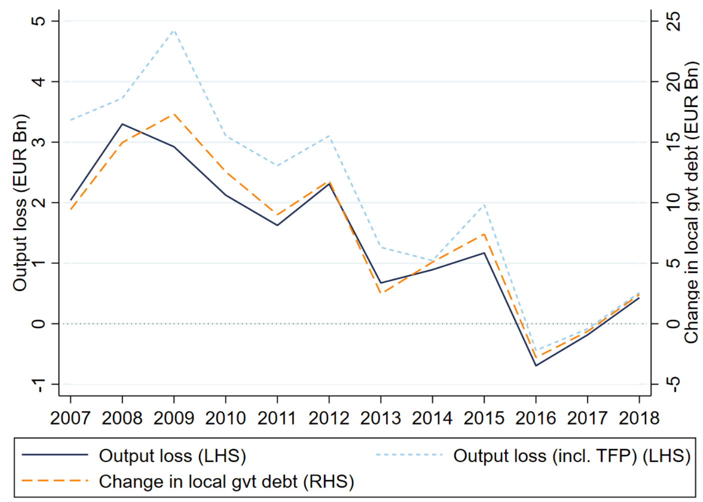
Ouptut. The aggregate output shortfall can be written as $\mathcal{L}(Y_t) = \alpha\mathcal{L}(K_t) + (1-\alpha)\mathcal{L}(L_t)$. When labor supply is elastic, $\mathcal{L}(L_t) < 0$ and the drop in the capital stock is amplified by a reduction in labor supply. Writing $\mathcal{L}(Y_t) = (1+\tilde{\kappa}^Y_{\text{GE}})\,\alpha\,\mathcal{L}^{direct}(K_t)$, $\tilde{\kappa}^Y_{\text{GE}}$ can be large when $\psi > 0$ (Table D.3). My empirical evidence does not provide moments to discipline $\mathcal{L}(L_t)$.[27] To avoid introducing uncertainty related to the calibration of the labor supply elasticity, my baseline quantification makes the conservative

[27] The reduced form evidence does not yield precise effects on employment outcomes. In addition, given the model assumption that labor is freely mobile across firms, the cross-sectional effect on labor is uninformative about the aggregate effect.

assumption $\mathcal{L}(L_t) = 0$ and $\mathcal{L}(Y_t) = \alpha\mathcal{L}(K_t)$.

I estimate that 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. Figure 6 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.

Figure 6. 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.

Robustness and extensions. 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.

Appendix D.3 provides an alternative quantification of the aggregate output loss based on the cross-sectional effect on firm-level output. I obtain an output multiplier equal to –0.29. It is reassuring that I find a similar effect starting from an entirely different empirical estimate.

Appendix D.4 presents a number of extensions of the baseline model. Appendix D.4.1, D.4.2, and D.4.3 present versions of the model with different assumptions on the firms' financing and investment decision: I consider firms borrowing from multiple banks and substituting across banks (Appendix D.4.1), firms substituting between debt and equity (Appendix D.4.2), and firms' dynamic financing and investment choices (Appendix D.4.3). In Appendix D.4.4, I introduce the possibility that depositors reallocate their savings across banks. Finally, Appendix D.4.5 and D.4.6

modify the banks' problem by introducing a cost of bank leverage and lending to households, respectively. Performing the aggregation exercise through the lens of these extended models does not affect the aggregation results and the quantitative magnitudes of estimates.[28]

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 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 $\tilde{\kappa}^{Y}_{GE} \approx 0$), which can be compared to 0.2 in Herreño (2021).

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 6 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

[28]One limitation is the case where depositors reallocate deposits across banks, in which case the aggregate effect is only set identified. The obtained bounds are tight, so that the quantitative conclusions of my analysis are unchanged.

proportional to the change in local government loans and hence is not included in my baseline multiplier quantifications.

8. 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, Priftis and Zimic (2021) and 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.[29] 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.[30] 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. In addition, even when local governments issue bonds, it is common that a large fractions of these bonds are held by banks. For instance, in the United States, domestic banks hold 15% of the $4 trillion municipal bond market at the national level, and this share rises to 40% when considering the average U.S. county (Yi 2021). In this case, similar crowding out effects can be expected.

[29] 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.

[30] 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.

Do my results teach us something about crowding out generated by central 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, they can be used as collateral, and are often held by agents not subject to bank regulation. Then, my quantification would provide an upper bound for the crowding out effect of government bonds.[31]

9. 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 mar-

[31] One reference point for government bonds is the crowding out effect implied by Priftis and Zimic (2021) and Broner et al. (2022). Using the Blanchard and Perotti (2002) methodology, Priftis and Zimic (2021) find that a $1 increase in debt financed by foreign investors raises output multipliers by 0.3 compared to the same increase financed by domestic investors. Broner et al. (2022) use the Guajardo, Leigh and Pescatori (2014) shocks and find much larger effects: the difference is around 2.5. While these studies use different methodologies, they also find large crowding out effects.

kets. 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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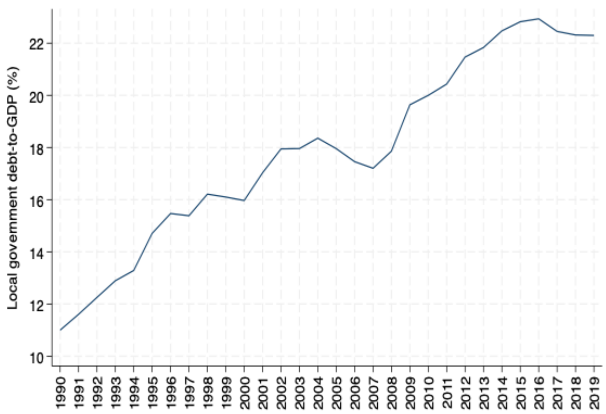
Appendix for online publication

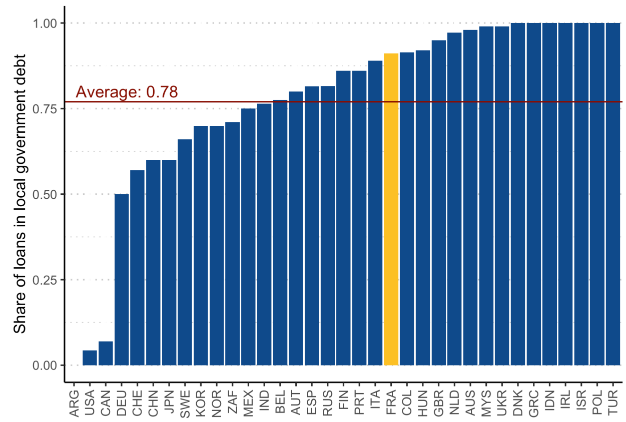
Appendix A. Additional tables and figures

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

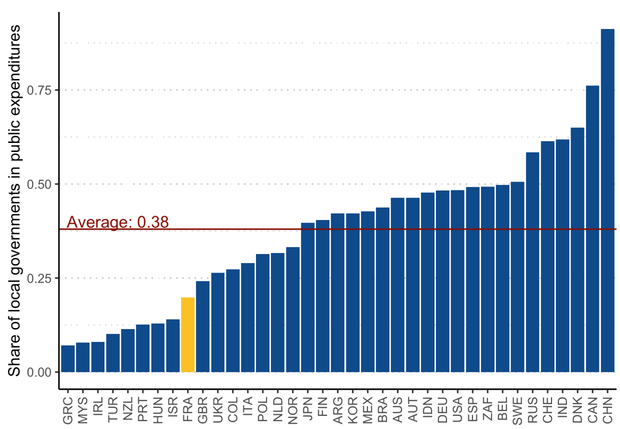
(a) Local government debt-to-GDP over time



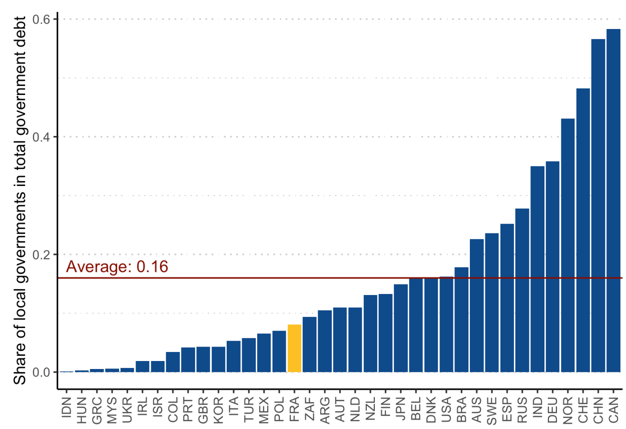
(b) Share of loans in local government debt



(c) Share of local governments in public expenditures

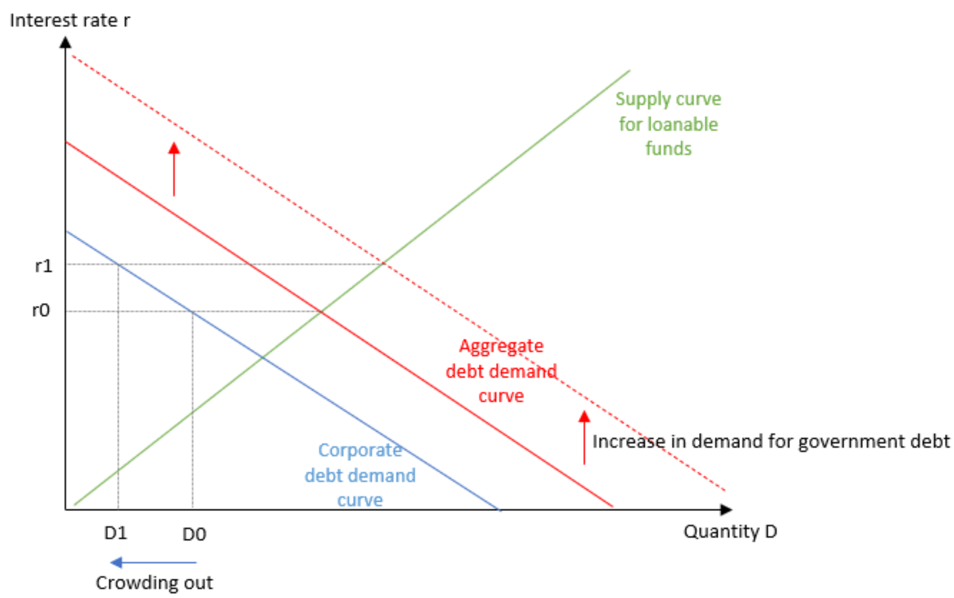


(d) Share of local in total government debt



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.

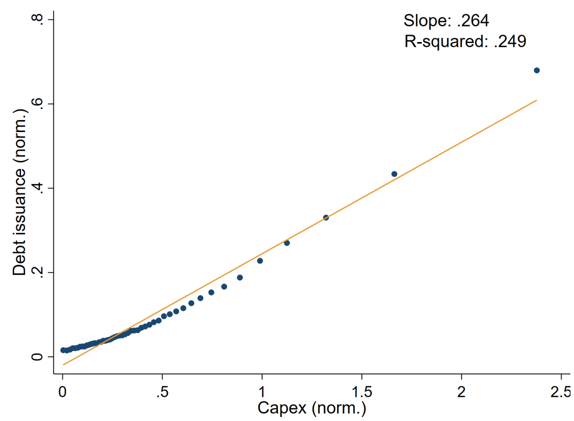
FIGURE A.2. Crowding out: simple supply and demand graph



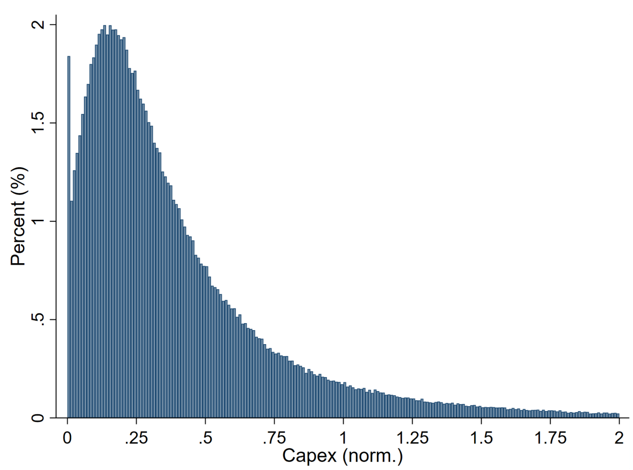
Note: This figure depicts the crowding out mechanism on a simple supply and demand graph.

Figure A.3. Local government debt and investment

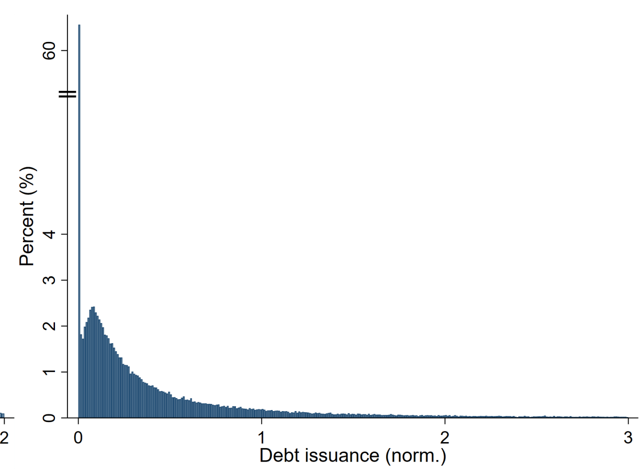
(a) Capital expenditures and debt issuance



(b) Distribution of capital expenditures



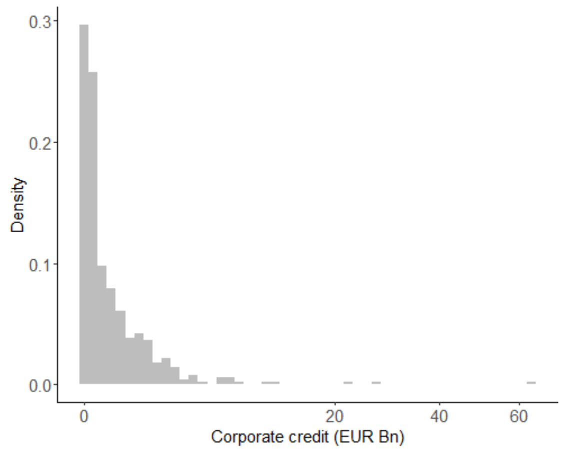
(c) Distribution of new debt issuance



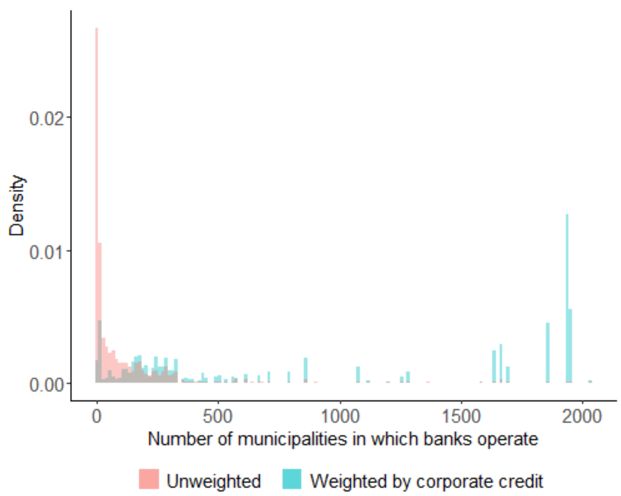
Note: This figure documents the relationship between debt issuance and capital expenditures using the local government accounts data. The analysis is performed on 477,893 local government×year observations. See Appendix F for more details on the data. Panel (a) shows a binscatter of local government debt issuance (normalized by revenues) on capital expenditures (normalized by revenues). Panel (b) shows the distribution of capital expenditures (normalized by revenues). Panel (c) shows the distribution of debt issuance (normalized by revenues). The two horizontal ticks on the y-axis indicate an axis break.

Figure A.4. Population of French banks

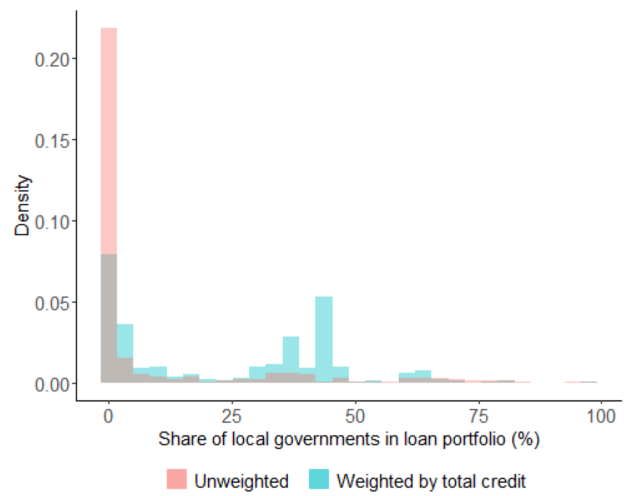
(a) Distribution by loan portfolio size



(b) Distribution by number of municipalities

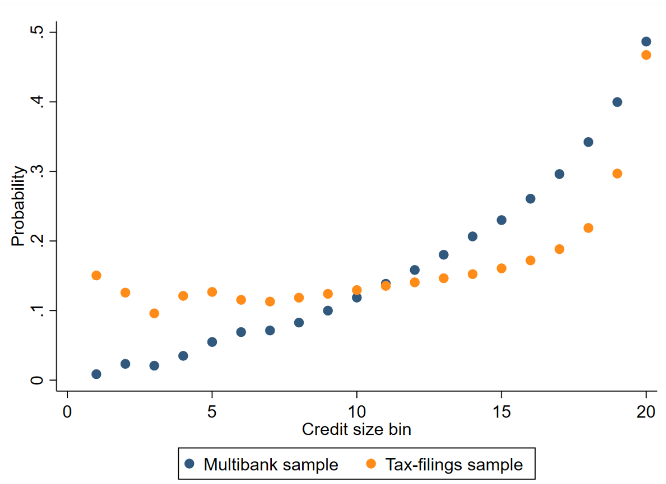


(c) Distribution by local government loan share



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.5. 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. Local government debt by category of local government

Panel A: Aggregate local government debt by category of local government

	Number	Credit share	Tax autonomy	Main responsibilities
Local governments	38,810	63.6%		
Communes	29,447	28.1%	Yes	Pre-schools, primary schools; local police; water management & sanitation; sport facilities.
EPCI (inter-*communes* cooperation structures)				Urban planning (zoning); waste management; local public transports; daycare centers.
General-purpose *EPCI*	2,327	11.7%	Yes	
Other syndicates	6,887	7.2%	No	
Departments	124	11.8%	Yes	Middle schools; roads; fire and safety; administration of social welfare programs.
Regions	25	4.8%	Yes	High schools, vocational training; regional train lines; airports; economic development; environmental protection.
Hospitals & other healthcare	3,261	11.2%	No	Hospitals; nursing homes; psychiatric care centers.
Public housing	293	21.3%	No	Public housing offices.
Others	2,787	3.9%	No	Local public service operators; management of state-owned land; other local public entities.

Panel B: Variation across municipalities in the share of different categories of local government

	mean	sd	p10	p50	p90
Local governments					
Communes	0.28	0.21	0.074	0.21	0.62
EPCI (inter-*communes* cooperation structures)					
General purpose *EPCI*	0.12	0.097	0.011	0.100	0.22
Other syndicates	0.072	0.091	0.0028	0.042	0.18
Departments	0.12	0.13	0	0.079	0.32
Regions	0.048	0.084	0	0	0.19
Hospitals & other healthcare	0.11	0.093	0.030	0.091	0.23
Public housing	0.21	0.16	0	0.23	0.42
Others	0.039	0.070	0.00029	0.022	0.067
Observations	2,080				

Note: This table provides details on local government debt by category of local government. In Panel A, I consider aggregate local government debt. The categories of local governments are defined as follows. Local governments (*collectivités territoriales*) refers to decentralized government entities elected by universal suffrage, enjoying tax autonomy, and that have a relatively general competence within their jurisdiction. I order the four tiers of local governments by increasing size. *Communes* refers to *communes* and related status (*communes nouvelles*, *communes associées*). *EPCI* stands for *Etablissement Public de Coopération Intercommunale* (inter-*communes* cooperation structures). General-purpose *EPCI* (*EPCI à fiscalite propre*) take five forms (*Communauté de communes*, *Communauté de villes*, *Communauté d'agglomération*, *Métropoles*) and have tax autonomy. Other syndicates are all the EPCI without tax autonomy (*syndicats intercommunaux*, *syndicats mixtes*). Departments includes departments and inter-department cooperation structures. Regions includes regions and inter-regions cooperation structures. Hospitals & other healthcare includes hospitals and other health-related local public entities. Public housing refers to public housing offices (*Office Public de l'Habitat*). The "Other" category covers all public entities not classified above. Panel B provides summary statistics on the shares of each local government entity type by municipality.

Table A.2. 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.

Appendix B. Additional details on identification strategy

Repeating equation (A1) the identifying assumption of the firm×bank specification (2), and substituting the definition of *BankExposure* yields:

$$\mathbb{E}\left[\left[\sum_m \omega^{gov}_{bm,t-1}\hat{\alpha}^{gov}_{mt}\right]\varepsilon_{fbt}\,\middle|\, d_{ft}\right]=0 \tag{B.1}$$

The error term ε_{fbt}—by construction orthogonal to the firm×time fixed effects d_{ft}—captures the firm×*bank*-specific determinants of credit flows.

Hence, identification requires that: (A1-a) any residual firm×bank demand shock not absorbed by the firm fixed effects is orthogonal to *BankExposure*; and (A1-b) there are no other bank-level credit supply shocks systematically correlated with *BankExposure*. This Appendix provides additional evidence in support of these assumptions.

It is useful to distinguish two types of potential issues. A first concern is that I do not observe municipalities' true demand shocks, but instead recover an estimate using the Amiti-Weinstein decomposition (3). If this procedure does not appropriately purge $\hat{\alpha}^{gov}_{mt}$ from banks' supply factors, it is more likely that (B.1) will be violated. Appendix B.1 and B.2 present evidence that alleviate this concern. Appendix B.1 validates the estimated fixed effects from the Amiti-Weinstein decomposition by linking them to local government debt demand and supply shifters identified via a narrative approach. Appendix B.2 assesses the performance of the Amiti-Weinstein procedure in a simulation study. Second, even if the estimated fixed effects $\hat{\alpha}^{gov}_{mt}$ are equal to municipalities' true demand shocks, assumption (B.1) requires that banks exposed to municipalities with large demand shocks are not subject to systematically different supply or demand shocks. This is the identifying assumption of the shift-share design, discussed in Appendix B.3.

B.1. Amiti-Weinstein decomposition and identification of demand shocks: narrative analysis

This section validates the estimated fixed effects from the Amiti-Weinstein decomposition by linking them to local government debt demand and supply shifters identified via a narrative approach.

I conduct an extensive analysis of administrative sources pertaining to local government debt: the parliamentary reports on local government finances that are part of the annual budget bill (*Jaunes Budgétaires*), the reports of the court of government auditors and other oversight bodies (*Cour des Comptes*, *Inspection Générale des Finances*), the reports of the Observatory on Local Finances and Public Management (*OFGL*), the reports of the French statistical office (*INSEE*), as well as the annual reports of the largest banks active in this market.

From this analysis, I identify significant episodes that drive local government debt dynamics. I classify each episode as either a demand shifter or a supply shifter, and exclude events that appear to involve both supply and demand components.

I identify three demand shifters, that is, episodes when changes in local government debt demand are primarily driven by changes in demand and plausibly uncorrelated to local credit supply conditions. I show that these demand shifters predict well the municipality fixed effects $\hat{\alpha}^{gov}_{mt}$ obtained from the Amiti-Weinstein decomposition. This demonstrates the effectiveness of the Amiti-Weinstein approach in systematically identifying shifts in demand.

Second, I identify two large bank-specific credit supply shocks. I show that municipality-level exposure to these supply shocks predicts credit growth (as expected) but is orthogonal to the estimated $\hat{\alpha}^{gov}_{mt}$. At the level of banks, the supply shocks predict the bank fixed effects $\hat{\alpha}^{gov}_{bt}$ but are orthogonal to my shift-share variable $BankExposure_{bt}$. This evidence provides direct support to my assumption that the Amiti-Weinstein decomposition recovers municipality-level drivers of credit flows purged from municipalities' exposure to banks' supply factors.

Table B.1 provides a summary of this study. This section often refers to the names of different categories of local governments in France. These categories are summarized in Table A.1.

TABLE B.1. Narrative analysis: summary of results

Panel A: Municipality-level demand shifters

Event	**Municipality exposure**	**Predicts $\hat{\alpha}^{gov}_{mt}$**	**Effect size**	**Results**
Public hospital modernization plan	Share of public hospitals	✓	10%	Table B.2
Creation of *métropoles*	Headquarter location × Post	✓	7%	Fig. B.2
Local elections	Share of *communes* × Pre-election year	✓	26%	Table B.3

Panel B: Bank-level supply shifters

Event	**Municipality-level tests**				**Bank-level tests**		**Results**
	Exposure	Predicts ΔC^{gov}_{mt}	Effect size	Orthogonal to $\hat{\alpha}^{gov}_{mt}$	Predicts $\hat{\alpha}^{gov}_{bt}$	Orthogonal to $BankExp_{bt}$	
2009 Dexia collapse	2008 Dexia share	✓	- 26%	✓	✓	✓	Table B.4
Banque Postale entry	2013 *Banque Postale* share	✓	10%	✓	✓	✓	Table B.5

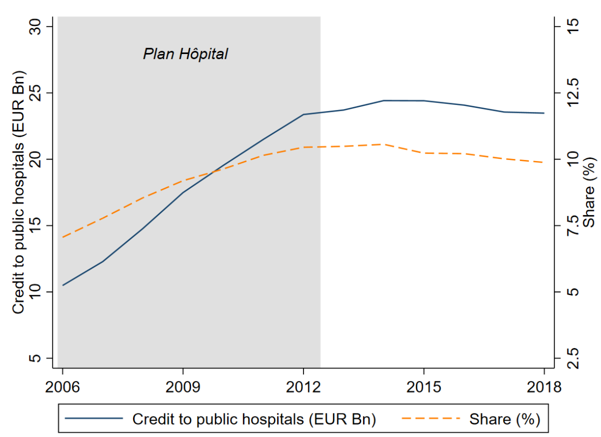
Note: This table provides a summary of the narrative analysis. The column "Effect size" provides a back-of-the-envelope computation of the change in the outcome variable in the counterfactual where the demand/supply shifter does not occur. It reports the predicted change in the outcome variable when the explanatory variable changes from its sample minimum to its sample mean, as a fraction of the sample mean of the outcome variable, using the most conservative point estimate.

B.1.1. Demand shifter #1: Plans Hôpital

In 2002, the French government launched a large investment program to modernize public hospitals. "Plan Hôpital 2007" was launched in 2002 for a duration of 5 years. "Plan Hôpital 2012" followed from 2007 to 2012. The programs consisted in central government subsidies for investment projects proposed by public hospitals. Over this decade, investment in public hospitals doubled to reach around €6 billion per year. The program was designed to encourage public hospitals to leverage. The subsidies covered half of project costs, requiring hospitals to seek additional financing sources. In addition, the subsidies primarily took the form of current expenditures subsidies, which could be used to cover interest payments but not the capital expenditures associated to the projects, providing a strong incentive to leverage.

As a result, public hospital debt was the fastest-growing category of local government debt from 2006 to 2013. Figure B.1 displays this trend in my credit registry data. By 2012, there was a realization that the program was ill-designed and led to high levels of public hospital indebtedness. The program was not renewed, and institutional changes were implemented to better monitor hospital investments and debt-taking. Hospital debt thus stagnated in the post-2013 period.

FIGURE B.1. Debt of public hospitals



Note: This figure plots total outstanding credit to public hospitals in the Banque de France credit registry on the left axis. The right axis shows credit to public hospitals as a share of total credit to local governments.

I interpret the two "Plans Hôpital" as public hospitals experiencing positive credit demand shocks between 2006 and 2013.[32] Are these demand shocks correctly identified by the Amiti-

[32] The plan ended in 2012, and I allow for a one year lag between the approval of the project subsidy and the contracting of a loan by the hospital.

TABLE B.2. The two "Plan Hôpital" and local government debt demand shocks

	Estimated municipality × time fixed effects $\hat{\alpha}^{gov}_{mt}$				
	(1)	(2)	(3)	(4)	(5)
$Hospital_{mt}$	0.069***	-0.027		0.134**	0.011
	(0.014)	(0.019)		(0.057)	(0.053)
$OtherHealth_{mt}$			0.017		
			(0.019)		
Time FE	✓	✓	✓	✓	✓
Indep. var. def.	Hospital share	Hospital share	Placebo	Project amount	Project amount
Sample	Pre-2013	Post-2013 placebo	Pre-2013	2009-2013	Post-2013 placebo
Observations	14,517	10,370	14,517	10,366	10,365
R-squared	0.15	0.028	0.14	0.15	0.027

Note: This table reports the results of municipality-level regressions of local government debt demand shocks $\hat{\alpha}^{gov}_{mt}$ on municipality-level variables. In columns (1) and (2), the independent variable is the municipality-level share of public hospitals in total local government borrowing. In column (3), the independent variable is the municipality-level share of other public healthcare establishments (excluding hospitals). In columns (4) and (5), the independent variable is plan Hôpital 2012 total investment amount in each municipality (projects selected in October 2008), normalized by initial total municipality borrowing. Regressions are weighted by municipality credit. Standard errors are clustered at the municipality level. ***, ** and * indicate significance at the 1%, 5% and 10% levels, respectively.

Weinstein procedure? I estimate a specification of the type:

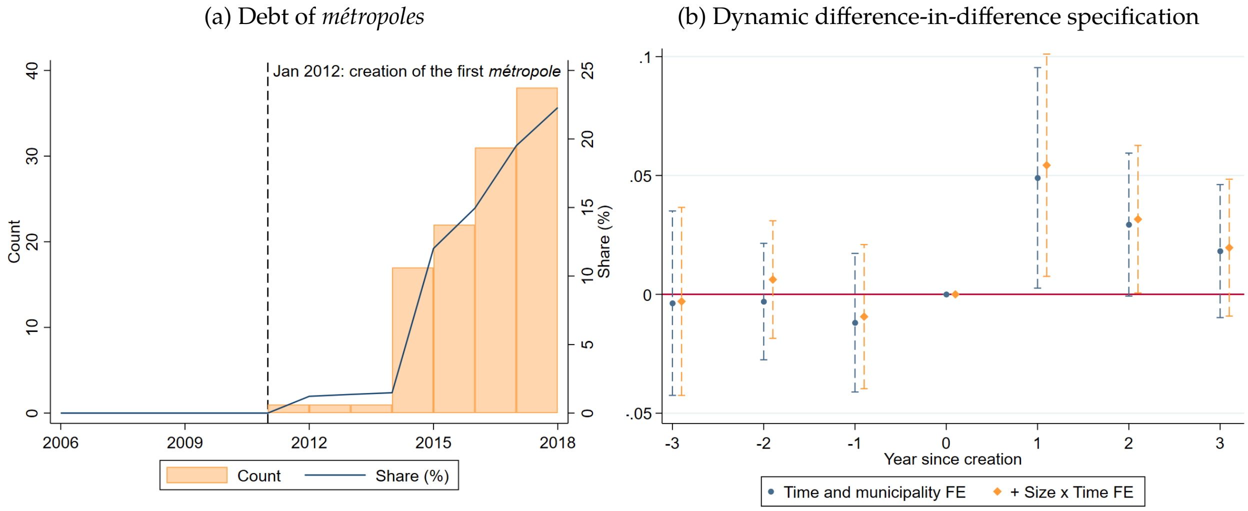
$$\hat{\alpha}^{gov}_{mt} = \delta_t + \beta Hopital_{mt} + \varepsilon_{mt} \quad \text{(B.2)}$$

where $Hopital_{mt}$ captures municipality m's exposure to the "Plans Hôpital".

Table B.2 presents the results. In my baseline test, I define $Hopital_{mt}$ as the municipality-level share of public hospitals in total local government borrowing. Column 1 shows that over 2007-2013, $\hat{\alpha}^{gov}_{mt}$ is systematically larger in municipalities where public hospitals are located. The magnitude is large: the point estimate implies that the average $\hat{\alpha}^{gov}_{mt}$ would be lower by 10% in the absence of the program. Columns (2)-(3) provide placebo tests: the correlation does not hold for the post-2013 period or for health-related public establishments other than hospitals, which do not benefit from the program. As an additional test, I obtain the list of the largest projects funded by the plan Hôpital 2012, which were selected in October 2008.[33] For each project, I obtain the municipality and the total investment amount. I then regress $\hat{\alpha}^{gov}_{mt}$ on the investment amount (normalized by initial total municipality borrowing) for the years 2009-2013. Column (4) shows that I identify positive demand shocks for municipalities with the largest investment projects.

[33] The list of project funded for the plan Hôpital 2007 is not publicly available. For the plan Hôpital 2012, the published list of flagship projects has amounts totaling to €1 billion, more than 20% of the total investment.

Figure B.2. The creation of *métropoles* and local government debt demand shocks



Note: Panel (a) plots descriptive statistics related to the creation of *métropoles*. The left axis plots the cumulative number of *métropoles* (including *pôles métropolitains*). The right axis plots outstanding credit to *métropoles*, as a share of total credit to inter-*communes* cooperation structures. Panel (b) plots the estimated coefficients resulting from estimating the dynamic difference-in-differences specification (B.3). Regressions are weighted by municipality credit. Standard errors are clustered at the municipality level. The dot is the point estimate and the bar is the 95% confidence interval.

B.1.2. Demand shifter #2: Creation of *métropoles*

In 2010, the French government created a new category of local government, called *métropoles* (metropolitan areas). This new entity belongs to the broader category of *EPCI* (inter-*communes* cooperation structures). *Métropoles* were created to be more integrated than the existing forms of inter-*communes* cooperation structures. The goal was to facilitate large investment projects in urban areas spanning multiple *communes*. The first *métropole* was created in 2012, and there was an acceleration in 2015 with the creation of 16 additional ones. The creation of *métropoles* catalyzed large local public investment projects. Examples of such projects include the €610 million renovation of the subway in Lille or the €207 million deepening of the harbor channel in Rouen. As Figure B.2 shows, the creation of *métropoles* is reflected in an increase in their share of local government debt.

I consider the creation of *métropoles* as a positive credit demand shocks in the municipalities where the *métropoles* are headquartered.[34] To test whether these demand shocks correctly picked up by the Amiti-Weinstein procedure, I estimate a difference-in-difference specification:

$$\hat{\alpha}^{gov}_{mt} = \gamma_m + \gamma_t + \sum_{\tau} \beta^{\tau}\, \mathbb{1}[Metropole]_m \times \delta^{\tau}_{mt} + \varepsilon_{mt} \tag{B.3}$$

[34] *Métropoles* typically cover several of the time-invariant municipalities that I define as geographic units in this article. The loans are contracted by the *métropoles* headquarter, located in one of the municipality.

$\mathbb{1}[Metropole]_m$ is a dummy equal to 1 if the municipality is the headquarter of a *métropole* and δ^{τ}_{mt} are indicators for the number of periods since the creation of the *métropole*. The results are presented in Figure B.2. I identify larger $\hat{\alpha}^{gov}_{mt}$ in municipalities where *métropoles* are headquartered, after the *métropole* is created. The differentials in $\hat{\alpha}^{gov}_{mt}$ (which are differences in growth rates, not levels) then slowly revert to zero. This result is robust to interacting time fixed effects with dummies for municipalities size quintiles to account for potential differential trends for larger municipalities. In terms of magnitude, the estimates imply that the creation of *métropoles* increased the demand shocks $\hat{\alpha}^{gov}_{mt}$ by 7%.

B.1.3. Demand shifter #3: Local elections

Fluctuations in investment expenditures of local governments can partly be linked to the local electoral cycle. *Communes* (the lowest tier of local government) elect their *communes* councils and mayors every six years. These elections also trigger the renewal of the boards of *EPCI* (inter-*communes* cooperation structures). These are very prominent elections: more than 36,000 mayors and 500,000 council members are elected. Because of electoral concerns, mayors have incentives to increase investment in pre-election years. Several official reports note this fact (e.g., INSEE 2019).

I interpret this as positive demand shocks for debt by *communes* and inter-*communes* cooperation structures in pre-election years. To test whether these demand shocks are appropriately captured by the $\hat{\alpha}^{gov}_{mt}$, I ask whether I find higher shocks in municipalities where *communes* represent a larger share of total debt, specifically in pre-election years.[35] That is, I estimate

$$\hat{\alpha}^{gov}_{mt} = \delta_t + \beta_0\, ShareCommunes_m + \beta_1\, ShareCommunes_m \times \mathbb{1}[PreElectionYear]_t + \varepsilon_{mt} \tag{B.4}$$

$ShareCommunes_m$ is equal to the share of *communes* in total local government debt in municipality m. $\mathbb{1}[PreElectionYear]_t$ is a dummy equal to 1 in 2007 and 2013.

Table B.3 presents the results. Column (1) shows that I estimate higher $\hat{\alpha}^{gov}_{mt}$ in pre-election years, for municipalities where a large share of the credit goes to *communes*. I find similar results with or without municipality fixed effects. I then repeat the same exercise, considering the debt of both *communes* and inter-*communes* cooperation structures (*EPCI*), since their electoral cycles are synchronized (columns 3 and 4). I find very similar results. The point estimates imply that $\hat{\alpha}^{gov}_{mt}$ would be lower by 26% in election years in the absence of politically-driven borrowing.

[35] See Table A.1 for the distribution of the share of communes in total debt across municipalities.

Table B.3. Local elections and local government debt demand shocks

	Estimated municipality × time fixed effects $\hat{\alpha}^{gov}_{mt}$			
	(1)	(2)	(3)	(4)
$ShareCommunes_m \times \times \mathbb{1}[PreElectionYear]_t$	0.048***	0.051***	0.047***	0.050***
	(0.013)	(0.013)	(0.010)	(0.010)
$ShareCommunes_m$	-0.017**	0.000	-0.014***	0.000
	(0.008)	(.)	(0.005)	(.)
Time FE	✓	✓	✓	✓
Municipality FE	–	✓	–	✓
Indep. var. def.	Communes	Communes	Communes + EPCI	Communes + EPCI
Observations	24,887	24,886	24,887	24,886
R-squared	0.17	0.26	0.17	0.26

Note: This table reports the results of estimating the municipality-level specification (B.4). Regressions are weighted by municipality credit. Standard errors are clustered at the municipality level. ***, ** and * indicate significance at the 1%, 5% and 10% levels, respectively.

B.1.4. Supply shifter #1: The 2009 collapse of Dexia

Before 2009, the Franco-Belgian bank Dexia was the main lender to French local governments, with a market share of 35%.[36] In September 2008, Dexia came under pressure. Its subsidiary FSA, a major player in credit enhancement for US municipalities, suffered large losses on its portfolio of US mortgage bonds. The French and Belgian governments were forced to intervene with a large bailout package. Unable to recover, the bank was eventually dismantled in 2013.

These events led to a major recomposition of the local government lending landscape in France. From a 35% market share, Dexia essentially stopped producing new loans to French local government in 2009. To avoid a major credit crunch for local governments, the investment arm of the French State (*Caisse des dépôts et Consignations*) entered the market at scale. Since then, the market has stabilized around a larger permanent role for state-owned actors: the *Caisse des dépôts et Consignations*, which was initially supposed to intervene temporarily, took a larger permanent role; and the state-owned bank *Banque Postale* entered in 2013.

The 2009 Dexia collapse constitutes a large unanticipated supply shock to local government credit. The supply shock affected more severely municipalities that had a larger share of their credit from Dexia. On the other hand, Dexia's distress emanated from activities unrelated to its lending French local governments, so that municipalities' exposure to the shock has no reason to be correlated to municipalities' demand for debt. I use this event to assess whether the Amiti-Weinstein

[36] Report from the public finance watchdog *Cour des Comptes* on the Dexia collapse.

TABLE B.4. Exit event and estimated fixed effects

	Municipality level				Bank level	
	ΔC^{gov}_{mt} (1)	$\hat{\alpha}^{gov}_{mt}$ (2)	ΔC^{gov}_{mt} (3)	$\hat{\alpha}^{gov}_{mt}$ (4)	$\hat{\alpha}^{gov}_{bt}$ (5)	$BankExposure_{bt}$ (6)
$Treated_m$	-0.076**	-0.014	0.004	-0.032		
	(0.032)	(0.031)	(0.031)	(0.035)		
$Treated_b$					-0.141***	0.003
					(0.027)	(0.003)
Time FE	✓	✓	✓	✓	✓	✓
Sample	2009	2009	2007 placebo	2007 placebo	2009	2009
Observations	2,078	2,074	2,077	2,074	129	149
R-squared	0.013	0.00038	0.000030	0.0014	0.18	0.0055

Note: Columns (1)-(4) report the results of estimating the municipality-level specification (B.5). $Treated_m$ is the 2008 market share of Dexia. In odd columns, the outcome variable is municipality-level local government debt growth. In even columns, the outcome variable is $\hat{\alpha}^{gov}_{mt}$ estimated from (3). Regressions are weighted by municipality-level credit. Columns (5)-(6) report the results of estimating the bank-level specification (B.6). $Treated_b$ is a dummy equal to 1 for Dexia. In column (5), the outcome variable is $\hat{\alpha}^{gov}_{bt}$ estimated from (3). In column (6), the outcome variable is $BankExposure_{bt}$ defined in (4), residualized on the sum of weights $\lambda^{gov}_{b,t-1}$. Regressions are weighted by bank-level credit. ***, ** and * indicate significance at the 1%, 5% and 10% levels, respectively.

procedure correctly isolates supply and demand factors. I estimate the specifications of the form:

$$\hat{\alpha}^{gov}_{m} = \beta_0 + \beta_1 Treated_m + \varepsilon_m \tag{B.5}$$

$$\hat{\alpha}^{gov}_{b} = \beta_0 + \beta_1 Treated_b + \varepsilon_b \tag{B.6}$$

$Treated_m$ is municipality-level exposure to the shock defined as the 2008 market share of Dexia. $Treated_b$ is an indicator for Dexia.[37]

The results are in Table B.4. Column (1) estimates equation (B.5) in 2009, with municipality-level local government debt growth as the outcome variable. The coefficient is negative and statistically significant, consistent with these municipalities experiencing negative supply shocks. I estimate that local government debt growth would have been higher by 26% in the absence of the shock. In column (2), I instead ask if the 2008 market share of Dexia can predict $\hat{\alpha}^{gov}_{mt}$. I find that the coefficient is very close to 0 and statistically and economically insignificant. This shows that the Amiti-Weinstein decomposition correctly purges $\hat{\alpha}^{gov}_{mt}$ from municipalities' exposure to the Dexia supply shock. Columns (3)-(4) provide a placebo test and show that that municipalities were not systematically different in 2007, before the start of the turmoil.

In columns (5)-(6), I assess the performance of the procedure at the level of banks. Regressing

[37] In the credit registry, banks' identities are anonymized. Therefore, I cannot formally identify the two legal entities that constitute Dexia (*Dexia Crédit Local* and Dexia Municipal Agency). Henceforth, I refer to Dexia as the two foreign banks with the largest market shares in 2008. All other foreign banks have market shares below 0.05%. I check that the loan volumes coincide with external sources.

the 2009 estimated bank fixed effect on $Treated_b$, I find a negative coefficient, in line with the idea that $\hat{\alpha}^{gov}_{bt}$ reflects supply shocks. Finally, I regress $BankExposure_{bt}$ (residualized on the sum of weights $\lambda^{gov}_{b,t-1}$) on $Treated_b$. The coefficient is very close to 0 and statistically and economically insignificant. This supports the hypothesis that *BankExposure* is orthogonal to bank-level supply shocks.

B.1.5. Supply shifter #2: The large-scale entry of *Banque Postale*

Under the Dexia resolution plan approved by the European Commission, Dexia's legacy portfolio was to be transferred to a government backed-entity, and a new entity was to be created to fill the gap in the production of new loans to local governments. The decision was to have the main state-owned bank *Banque Postale*, until then only active in the household segment, enter the market for local government loans. *Banque Postale* launched its activity dedicated to local governments in November 2012. With a large deposit base and the most extensive branch network of all French banks, *Banque Postale* could immediately enter at scale. In 2013, it already had a 15% market share in the production of new loans.[38]

I consider the entry of *Banque Postale* as a positive supply shock, affecting disproportionately the municipalities where the bank entered. Because the entry of *Banque Postale* relied on a pre-existing branch network, I make the hypothesis that the 2013 entry into municipalities was orthogonal to municipality-specific demand shocks.

Table B.5 shows the results of estimating equation (B.5) for this shock. $Treated_m$ is the market share of *Banque Postale* at the end of 2013 (option 1), or a dummy equal to 1 if that share is strictly positive (option 2).[39] The latter is likely to be even less correlated with municipality level demand factors. I stack the coefficients of the two specifications vertically. Column (1) shows that $Treated_m$ positively affects local government debt growth, consistent with these municipalities experiencing a positive supply shock. The magnitude is large: credit volumes would have been lower by 10% in the absence of the *Banque Postale* entry. In column (2), I instead ask if the entry of Banque Postale can predict $\hat{\alpha}^{gov}_{mt}$. I find that the coefficient is close to 0 and statistically insignificant. This shows that the Amiti-Weinstein decomposition properly purged the $\hat{\alpha}^{gov}_{mt}$ from the municipalities' exposure to the positive supply shock. In columns (3)-(4), I provide evidence that municipalities were not systematically different in 2010-12, before the entry of Banque Postale.

In the next panel, I assess the performance of this procedure at the level of banks. Regressing

[38] *La Banque Postale* 2013 annual report

[39] In the credit registry, banks' identities are anonymized. Therefore, I cannot formally identify *Banque Postale*. Henceforth, I refer to *Banque Postale* as the only state-owned bank entering the local government debt market in that year and check that loan volumes coincide with external sources.

Table B.5. Entry event and estimated fixed effects

	Municipality level				Bank level	
	ΔC^{gov}_{mt} (1)	$\hat{\alpha}^{gov}_{mt}$ (2)	ΔC^{gov}_{mt} (3)	$\hat{\alpha}^{gov}_{mt}$ (4)	$\hat{\alpha}^{gov}_{bt}$ (5)	$BankExposure_{bt}$ (6)
$Treated_m$ (1)	0.549**	0.205	0.238	0.144		
	(0.250)	(0.241)	(0.348)	(0.385)		
$Treated_m$ (2)	0.018***	0.004	0.002	0.001		
	(0.005)	(0.005)	(0.005)	(0.006)		
$Treated_b$					0.713***	-0.014
					(0.082)	(0.010)
Time FE	✓	✓	✓	✓	✓	✓
Sample	2013-2015	2013-2015	2010-2012 placebo	2010-2012 placebo	2013-2015	2013-2015
Observations	6,236	6,222	6,237	6,221	379	424
R-squared (1)	0.017	0.019	0.015	0.0097	0.17	0.062
R-squared (2)	0.027	0.019	0.015	0.0096		

Note: Columns (1)-(4) report the results of estimating the municipality-level specification (B.5). $Treated_m$ (1) is the 2013 market share of *Banque Postale*. $Treated_m$ (2) is a dummy equal to 1 if this share is positive. The coefficients from these alternative specifications are stacked vertically. In odd columns, the outcome variable is municipality-level local government debt growth. In even columns, the outcome variable is $\hat{\alpha}^{gov}_{mt}$ estimated from (3). Regressions are weighted by municipality-level credit. Columns (5)-(6) report the results of estimating the bank-level specification (B.6). $Treated_b$ is a dummy equal to 1 for *Banque Postale*. In column (5), the outcome variable is $\hat{\alpha}^{gov}_{bt}$ estimated from (3). In column (6), the outcome variable is $BankExposure_{bt}$ defined in (4), residualized on the sum of weights $\lambda^{gov}_{b,t-1}$. Regressions are weighted by bank-level credit. ***, ** and * indicate significance at the 1%, 5% and 10% levels, respectively.

the estimated bank fixed effects on an indicator for the bank being *Banque Postale*, I find a positive coefficient, in line with intuition. Finally, I regress $BankExposure_{bt}$ on $Treated_b$. The coefficient is very close to 0 and statistically and economically insignificant. This supports the hypothesis that $BankExposure_{bt}$ is orthogonal to bank-level supply shocks.

B.2. Amiti-Weinstein decomposition and identification of demand shocks: simulation study

In this section, I assess the ability of the Amiti-Weinstein decomposition to estimate municipality demand shocks in a simulation study. I simulate various equilibrium models of bank-municipality credit volumes and assess the properties of the Amiti-Weinstein procedure. In particular, I test whether the estimated fixed effects provide unbiased estimates of municipality demand shocks and if the obtained Amiti-Weinstein shift-share variable is orthogonal to bank supply shocks. I first consider the canonical Khwaja-Mian Amiti-Weinstein model, and then study deviations from this model. In what follows, I omit time subscripts to simplify notations.

B.2.1. Canonical version

Model. There are $\mathcal{N}_B$ banks and $\mathcal{N}_M$ municipalities.

Local government credit demand. Local governments borrow from one or several banks. The baseline version of the model assumes that local government demand directed to each bank are independent and given by $C^g_{mb} = g a_{mb} e^{z^g_m} (r^g_b)^{-\epsilon^g}$. r^g_b is the interest rate charged by bank b to local governments. z^g_m is a municipality-specific local government debt demand shifter. I will solve the model in log-deviations from a deterministic equilibrium where $z^g_m = 0\ \forall m$. a_{mb} is the lending share of bank b in the deterministic equilibrium. It satisfies $\sum_b a_{mb} = 1$ for each m. In addition, I impose that all the municipalities have the same size in the deterministic equilibrium, which implies that $\sum_m a_{mb} = \frac{\mathcal{N}_M}{\mathcal{N}_B}$. In log-deviation from the deterministic equilibrium:

$$\hat{C}^g_{mb} = z^g_m - \epsilon^g \hat{r}^g_b \tag{B.7}$$

Deposit supply. Each bank has an isoelastic supply of funds. In log-deviations, $\hat{S}_b = \epsilon^s \hat{r}^s_b$

Banks. Banks maximize the proceeds of lending minus the cost of funds $r^g_b C^g_b - r^s_b S_b$ subject to a balance sheet constraint $C^g_b = S_b + \tilde{\xi}_b$. $\tilde{\xi}_b$ is a bank-specific (expansionary) balance sheet shock, equal to 0 in the deterministic equilibrium. Optimality requires that banks set $r^g_b = r^s_b$.

Equilibrium. Imposing the balance sheet constraint of each bank yields each bank's interest rate:

$$\hat{r}^g_b = \frac{Z^g_b - \xi_b}{\epsilon^s + \epsilon^g} \tag{B.8}$$

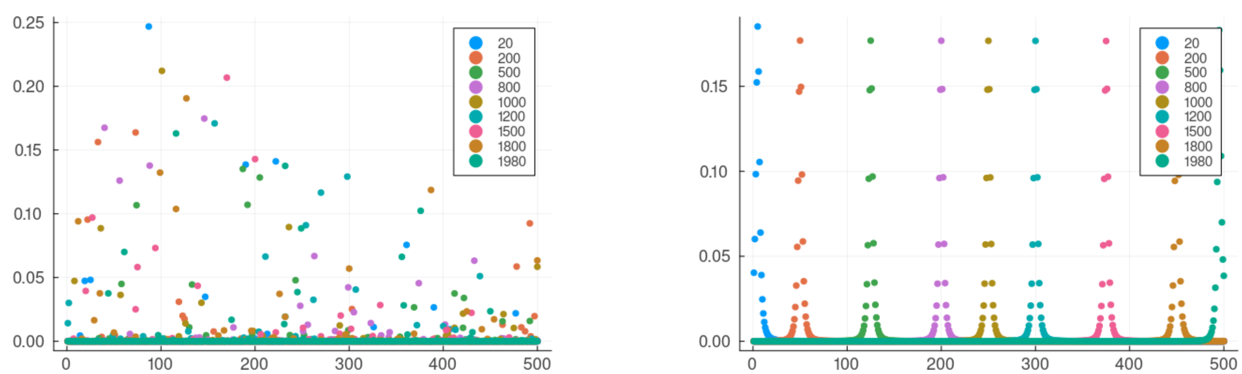
$Z^g_b = \sum_m a_{mb} z^g_m$ is the bank-specific local government debt demand shock. $\xi_b = \frac{1}{S^*}\tilde{\xi}_b$ is the bank-specific supply shock (rescaled by bank size in the deterministic equilibrium).

Simulations: data generating process. To simulate data from this model, I assume a statistical process for the shocks $\{z^g_m, \xi_b\}$ and a matrix of municipality-bank shares $\mathbf{A} = [a_{mb}]$. I solve the model, and I then assume that observed credit growth $\hat{C}^g_{mb}$ is equal to the model prediction plus some white noise ε_{mb} such that $\hat{C}^g_{mb} = z^g_m - \epsilon^g \hat{r}^g_b + \varepsilon_{mb}$. I choose parameters and processes for shocks to match key moments of the true data.

The number of banks is equal to 500 and the number of municipalities is equal to 2000. I consider two versions of $\mathbf{A}$, the matrix representing the municipality-bank network: a random network, where adjacent municipalities are equally likely to borrow from all banks, and a clustered network, where adjacent municipalities are more likely to borrow from the same banks. Specifically, I proceed as follows. Let $f(x; \mu, \tau^2, k)$ be the probability density function of a non-standardized Student's t distribution with location parameter τ^2 and k degrees of freedom. For the clustered network, I set $a_{mb} = f(b; \frac{m\mathcal{N}_B}{\mathcal{N}_M}, \tau^2_{\mathbf{A}}, k_{\mathbf{A}})$. This implies that for a municipality at the p-percentile of $[0, \mathcal{N}_M]$, the distribution of bank shares has a bell shape with a peak for the bank at the p-percentile of $[0, \mathcal{N}_B]$. Therefore, adjacent municipalities borrow from the same banks. I obtain the random network by randomizing the elements $\{a_{mb}\}$ for each municipality m. By design, $\sum_b a_{mb} = 1$.[40] $\tau^2_{\mathbf{A}}$ and $k_{\mathbf{A}}$ control the dispersion in bank shares within each municipality. I choose $\tau^2_{\mathbf{A}}$ and $k_{\mathbf{A}}$ to match the number of banks from which a municipality borrows more than 1% and more than 0.2% of its total, and the number of municipalities a bank lends to more than 0.1% and more than 0.02% of its total. Figure B.3 displays the obtained distribution of bank shares for various municipalities.

FIGURE B.3. Bank shares by municipality

(a) Random network (b) Clustered network



Note: This figure plots the bank shares $\{a_{mb}\}$ for a subset of municipalities. The x-axis indicates the bank indices b. The color of the markers refers to the indices of the municipalities, indicated in the legend box.

To discipline the choice of the statistical processes for z^g_m, ξ_b, and ε_{mb}, I target the standard deviation of bank-municipality credit growth ΔC^g_{mb} (using only the range $(-2, 2)$ and including all values), and the R-squared of the Amiti-Weinstein decomposition. I draw z^g_m and ξ_b from a non-standardized Student's t distribution.[41] Conditional on the network being clustered, I consider the possibility of sorting, i.e., the fact that different banks may lend to municipalities with a different distribution of shocks. For each municipality m (bank b), I draw the shocks z^g_m (ξ_b) from a distribution with mean $\rho_M(\frac{m}{\mathcal{N}_M} - 0.5)$ ($\rho_B(\frac{b}{\mathcal{N}_B} - 0.5)$). When $\rho_M \neq 0$ (or $\rho_B \neq 0$), $\mathbb{E}[z^g_m]$ ($\mathbb{E}[z^g_b]$) increases in m (b). Finally, I draw normal noise ε_{mb} to rationalize that observed data does not exactly fit the model prediction. I choose the variance of ε_{mb} to match the R-squared of the Amiti-Weinstein decomposi-

[40] Because of boundary effects, this is not true for municipalities close to 1 and 2000. I rescale the matrix to ensure it is doubly stochastic.

[41] Allowing for heavier tails than a normal distribution is important for two reasons. First, it helps to match the fact that in the data ΔC^g_{mb} has heavy tails, in the sense that the standard deviation including the $\{-2, 2\}$ values is significantly larger than excluding them. Second, because banks are well diversified across municipalities, normally distributed z^g_m shocks would imply virtually no variation in the bank-level aggregate Z^g_b.

tion in the true data. This ensures that the statistical power of the two-way fixed effects regression in the same as in the actual data. Panel B of Table B.6 compares the relevant moments in the actual data and in my simulated model and shows that they are very similar.[42]

TABLE B.6. Simulation study: calibration

Panel A: Parameter values

Parameter	Value
Number of banks $\mathcal{N}_B$	500
Number of municipalities $\mathcal{N}_M$	2000
Elasticity of credit demand ϵ^g	2
Elasticity of credit supply ϵ^s	3
PDF shares: d.f. $k_{\mathbf{A}}$	2.0
PDF shares: scale $\tau^2_{\mathbf{A}}$	2.0
PDF shocks: d.f. k	1.3
PDF shocks: scale τ^2	0.022
Variance of ε_{mb}	0.2

Panel B: Targeted moments

Moment	Simulated	Actual
Nb banks per municipality (>1%)	13.58	8.99
Nb banks per municipality (>0.2%)	24.29	11.90
Nb municipalities per bank (>0.1%)	155.82	126.86
Nb municipalities per bank (>0.02%)	196.34	267.41
Std. dev. credit growth	0.30	0.28
Std. dev. credit growth (int.)	0.27	0.22
R-squared AW reg.	0.57	0.55

Note: Panel A reports the parameter values used to generated the simulated data. Panel B reports key moments of the data-generating process. The first column reports the average value for this moment obtained from 100 simulations. The second column reports the value of the same moment in the true data.

Output. With the simulated data, I estimate the Amiti-Weinstein decomposition, obtain the fixed effects $\hat{\alpha}^{gov}_m$, and construct the shift-share variable $BankExposure^{\mathrm{AW}}_b = \sum_m a_{mb} \times \hat{\alpha}^{gov}_m$. I then ask two questions. First, is $BankExposure^{\mathrm{AW}}_b$ an unbiased estimate of the true bank-level demand shock Z^g_b? Second, is $BankExposure^{\mathrm{AW}}_b$ orthogonal to bank-specific supply shocks ξ_b? Throughout the exercise, I maintain the assumption that the true demand shock Z^g_b is orthogonal to ξ_b. This is the key identifying assumption of the shift-share design, discussed in section 4.2 and Appendix B.3. This simulation study asks whether, provided $Z^g_b \perp \xi_b$, the Amiti-Weinstein procedure yields $BankExposure^{\mathrm{AW}}_b \perp \xi_b$.

Panel A of Table B.7 reports the output. I perform 100 simulations and report means across simulations. Each line corresponds to a different data-generating process. Line (R) is the random network. (C) is the clustered network. (C & $\rho_M > 0$) is the clustered network with $\rho_M > 0, \rho_B = 0$, that is, municipalities borrowing from the same bank have correlated shocks. (C & $\rho_B > 0$) is the clustered network with $\rho_M = 0, \rho_B > 0$, that is, banks lending to adjacent municipalities have correlated supply shocks. Finally, (C & $\rho_M, \rho_B > 0$) is the clustered network with $\rho_M > 0, \rho_B > 0$, i.e., municipalities with larger demand shocks borrow from banks with larger supply shocks. This

[42]It is difficult to jointly match the number of banks per municipalities and the number of municipalities per bank because the model assumes that all banks and municipalities have the same total credit in steady state, while in the data there are large persistent differences in bank and municipality size.

last case violates the assumption $Z_b^g \perp \xi_b$, but is useful for illustrative purposes.

Columns (1) and (2) report the mean and the standard deviation of the estimation error for the municipality demand shocks $\hat{\alpha}_m^{gov} - z_m^g$. Column (3) reports the slope of the regression of $\hat{\alpha}_m^{gov}$ on z_m^g, which allows to assess if there is systematic (under-) over-estimation for (small) large shocks. Columns (4)-(6) perform the same exercise for the comparison of $BankExposure_b^{\text{AW}}$ and Z_b^g. These results show that the Amiti-Weinstein procedure allows to recover unbiased estimates of the true municipality-level demand shocks z_m^g, so that $BankExposure_b^{\text{AW}}$ is an unbiased estimate of Z_b^g. This is true irrespective of the structure of the network.

Columns (7)-(12) assess whether the shift-share variable is orthogonal to the (positive) credit supply shock ξ_b. I start by considering the "standard" shift-share variable: $BankExposure_b^{\text{std}} = \sum_m a_{mb} \times \hat{C}_m^g$. Columns (7)-(8) show the coefficient and p-value of the regression of $BankExposure_b^{\text{std}}$ on ξ_b. The correlation is positive and statistically significant. This is because the shifters $\hat{C}_m^{gov}$ aggregate the supply shocks of banks present in m, introducing a mechanical correlation between supply shocks and the shift-share variable.[43]

Columns (9)-(10) show the coefficient and p-value of the regression of $BankExposure_b^{\text{AW}}$ on ξ_b. For reference, columns (11)-(11) show these statistics for the regression of Z_b^g on ξ_b. The Amiti-Weinstein shift-share variable is orthogonal to ξ_b: the coefficient in column (9) is very close to 0 and not statistically significant. That is, the two-way fixed effects regression allows to appropriately purge municipality-level credit growth from municipalities differential exposure to bank supply shocks. The only case where it fails (line C & $\rho_M, \rho_B > 0$ in grey) is when the condition $Z_b^g \perp \xi_b$ for the true shock is violated.

Having established that the methodology works as desired in the canonical model, I now study several departures from this model.

[43]This concern with shift-share variables is well-known, see for instance Borusyak, Hull and Jaravel (2022).

Table B.7. Simulation results

	Extracted FE recover true shocks?						Bank-level variables orthogonal to ξ_b?						$\tilde{Z}_b^g$?
	$(\hat{\alpha}_m^g, z_m^g)$			$(BankExp_b^{\text{AW}}, Z_b^g)$			$BankExp_b^{\text{std}}$		$BankExp_b^{\text{AW}}$		Z_b^g		
	mean	std	β	mean	std	β	β	p-val	β	p-val	β	p-val	β
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)
Panel A: Canonical model													
R	-0.000	0.065	1.000	-0.000	0.043	1.000	0.078	0.042	0.002	0.608	0.003	0.617	
C	-0.000	0.066	1.000	-0.000	0.067	1.003	0.076	0.015	0.002	0.578	0.002	0.587	
C & $\rho_M > 0$	0.000	0.066	0.998	0.000	0.068	0.994	0.070	0.023	-0.009	0.544	-0.007	0.537	
C & $\rho_B > 0$	-0.000	0.066	0.999	-0.000	0.068	0.995	0.390	0.000	-0.015	0.190	-0.008	0.215	
C & $\rho_M, \rho_B > 0$	-0.000	0.066	1.000	-0.000	0.067	1.007	0.521	0.000	0.464	0.057	0.459	0.041	
Panel B: Transmission of shocks across banks													
R	0.000	0.065	1.000	0.000	0.043	1.000	0.067	0.075	0.002	0.608	0.003	0.617	
C	0.000	0.066	1.000	0.000	0.067	1.003	0.065	0.036	0.002	0.578	0.002	0.587	
C & $\rho_M > 0$	-0.000	0.066	0.998	-0.000	0.068	0.993	0.058	0.082	-0.009	0.544	-0.007	0.537	
C & $\rho_B > 0$	0.000	0.066	0.999	0.000	0.068	0.995	0.330	0.000	-0.015	0.192	-0.008	0.215	
Panel C: Heterogeneous loadings on supply shocks													
R	-0.000	0.065	1.000	-0.000	0.043	1.000	0.078	0.042	0.002	0.604	0.003	0.617	
C	0.000	0.070	1.000	0.000	0.070	1.003	0.076	0.015	0.003	0.576	0.002	0.587	
C & $\rho_M > 0$	0.000	0.123	1.065	0.000	0.358	1.117	0.058	0.367	-0.017	0.514	-0.011	0.501	
C & $\rho_B > 0$	-0.000	0.074	0.999	-0.000	0.129	0.998	0.422	0.000	-0.176	0.066	-0.006	0.121	
Panel D: Bank-specific demand shocks (additive)													
R	0.000	0.092	0.999	0.000	0.060	0.997	0.076	0.038	-0.001	0.616	-0.000	0.631	1.006
C	0.000	0.093	0.999	0.000	0.096	0.990	0.082	0.001	0.002	0.514	0.007	0.562	1.004
C & $\rho_M > 0$	-0.000	0.087	1.000	-0.000	0.091	1.005	0.078	0.101	0.005	0.499	0.003	0.528	2.023
C & $\rho_B > 0$	-0.000	0.087	1.000	-0.000	0.091	1.007	0.670	0.000	0.010	0.143	-0.005	0.285	0.990
Panel E: Bank-specific demand shocks (multiplicative)													
R	-0.000	0.073	1.000	-0.000	0.048	1.001	0.075	0.032	-0.002	0.633	-0.002	0.668	1.002
C	0.000	0.075	1.000	0.000	0.078	1.009	0.075	0.004	-0.001	0.592	-0.002	0.625	0.999
C & $\rho_M > 0$	0.000	0.112	0.998	0.000	0.095	0.985	0.072	0.030	-0.007	0.509	-0.008	0.500	0.996
C & $\rho_B > 0$	-0.000	0.114	1.005	-0.000	0.093	1.006	0.373	0.000	-0.016	0.172	0.001	0.329	1.001
Panel F: Substitution across banks (CES)													
R	-0.000	0.068	1.010	0.001	0.051	1.053	0.131	0.001	-0.046	0.242	-0.003	0.644	
C	0.000	0.078	1.013	-0.001	0.141	1.343	0.171	0.000	-0.058	0.235	-0.001	0.614	
C & $\rho_M > 0$	-0.000	0.110	1.041	0.000	0.298	1.512	0.170	0.005	-0.061	0.374	-0.003	0.552	
C & $\rho_B > 0$	-0.000	0.101	1.014	0.001	0.262	1.357	0.942	0.000	-0.293	0.069	-0.003	0.284	
Panel G: Substitution across banks (non-CES)													
(H1) R	0.000	0.065	1.001	-0.000	0.043	1.000	0.126	0.001	0.001	0.564	0.001	0.607	
(H1) C	0.000	0.066	1.001	-0.001	0.068	1.010	0.127	0.000	0.003	0.519	0.003	0.574	
(H1) C & $\rho_M > 0$	0.000	0.069	1.000	-0.002	0.078	0.992	0.127	0.000	0.001	0.534	0.002	0.524	
(H1) C & $\rho_B > 0$	0.000	0.067	1.000	-0.000	0.070	1.002	0.199	0.000	0.006	0.361	-0.003	0.502	
(H2) R	0.000	0.072	1.005	-0.001	0.053	1.023	0.112	0.003	-0.017	0.463	0.000	0.622	
(H2) C	0.000	0.069	1.004	-0.000	0.080	1.062	0.112	0.000	-0.014	0.511	0.000	0.570	
(H2) C & $\rho_M > 0$	-0.000	0.071	1.006	0.000	0.093	1.061	0.111	0.000	-0.016	0.480	0.002	0.530	
(H2) C & $\rho_B > 0$	-0.000	0.066	1.002	0.000	0.070	1.062	0.190	0.000	-0.006	0.345	0.007	0.449	

Note: This table presents the results of the simulation study. Each column is a given statistic, averaged across 100 simulations. Columns (1) and (2) report the average and standard deviation of $\hat{\alpha}_m^{gov} - z_m^g$ across m. Column (3) reports the regression coefficient of $\hat{\alpha}_m^{gov}$ on z_m^g. Columns (4) and (5) report the average and standard deviation of $BankExposure_b^{\text{AW}} - Z_b^g$ across b. Column (6) reports the regression coefficient of $BankExposure_b^{\text{AW}}$ on Z_b^g. Columns (7)-(8) report the regression coefficient and p-value of the regression of $BankExposure_b^{\text{std}}$ on ξ_b. Columns (9)-(10) and (11)-(12) report the same statistics for $BankExposure_b^{\text{AW}}$ and Z_b^g, respectively. Column (13) reports the coefficient of the regression of $Z_b^g + \tilde{Z}_b^g$ on Z_b^g, when applicable. Columns (1) to (6) are shaded in dark (light) orange if the test deviates from its benchmark value by more than 10% (5%). Columns (9)-(10) are shaded in dark orange if the p-value is below 0.1. Column (13) is shaded in dark orange if the coefficient deviates from 1 by more than 10%.
Each line is a data-generating process. R (C) refers to the random (clustered) network. In the case of sorting ($\rho_M > 0$ or $\rho_B > 0$), I use $\rho_M = 0.15$ and $\rho_B = \rho_M \mathcal{N}_M / \mathcal{N}_B$ unless otherwise specified. In panel B, $\nu = 0.85$. For panel C, I generate the municipality loadings as $\epsilon_m^g = \epsilon^g + \zeta_m$ where ζ_m is drawn from a Normal distribution with mean $\rho_\epsilon(m/\mathcal{N}_M - 0.5)$ and variance $\sigma_\epsilon^2 = 0.25$ (truncated to ensure $\epsilon_m^g > 0$). In lines (R) and (C), $\rho_\epsilon = 0$. In line $\rho_M > 0$, $\rho_M = 1$ and $\rho_\epsilon = 1$. In line $\rho_B > 0$, $\rho_B = 10$ and $\rho_\epsilon = 10$. In panel D, $\tilde{z}_{mb}^g$ is drawn from a Normal distribution with mean $\rho_{\tilde{z}}(b/\mathcal{N}_B - 0.5)$ and variance $\sigma_{\tilde{z}}^2 = 0.08$. In lines (C) and (R), $\rho_{\tilde{z}} = 0$. In lines with $\rho_M > 0$ or $\rho_B > 0$, $\rho_{\tilde{z}} = 1$. In panel E, λ_{bm} is drawn from a Normal distribution with mean $\rho_\lambda(b/\mathcal{N}_B - 0.5)$ and variance $\sigma_\lambda^2 = 0.05$. In lines (C) and (R), $\rho_\lambda = 0$. In lines with $\rho_M > 0$ or $\rho_B > 0$, $\rho_\lambda = 0.1$. In panel F and G, $\theta = 1.33\epsilon^g$. In panel G, (H) refers to the definition of the substitution variable $\xi_{m,-b}$, detailed in the text.

B.2.2. Introducing some transmission of shocks across banks

Model. I study the case where bank-specific shocks are transmitted to other banks via the interbank market. Banks now solve:

$$\max_{\{C_b^g,\, S_b,\, B_b\}} r_b^g C_b^g - r_b^s S_b - iB_b - \frac{\phi}{2} iB_b^2 \quad \text{subject to} \quad C_b^g = S_b + B_b$$

where B_b is net interbank borrowing and i is the interbank rate. Each bank's interest rate is now given by $\hat{r}_b = (1-\nu)\frac{Z^g}{\epsilon^s+\epsilon^g} + \nu\frac{Z_b^g - \xi_b}{\epsilon^s+\epsilon^g}$. $\nu = \frac{\epsilon^s+\epsilon^g}{\epsilon^s+\epsilon^g+\frac{1}{\phi S^*}} \in [0,1]$ indexes the degree of segmentation across banks, which controls the extent to which bank-specific shocks affect bank-specific interest rates.

Output. Panel B of Table B.7 reports the output. $BankExposure_b^{AW}$ is an unbiased estimate of Z_b^g and $BankExposure_b^{AW} \perp \xi_b$. Intuitively, allowing for an interbank market affects the equilibrium distribution of the bank-specific interest rates, but does not interfere with the identification of municipality and bank fixed effects.

B.2.3. Heterogeneous effects of credit supply shocks

Model. I consider the case where bank-level supply shocks have heterogeneous effects on different borrowers. For instance, it may be the case that following a supply shock, banks primarily cut credit supply to riskier or smaller borrowers. This contradicts equation (B.7) which assumes that all municipalities m are similarly affected by a given bank-specific interest rate shock $\hat{r}_b^g$. I generalize the Amiti-Weinstein model by introducing municipality-specific loadings on bank supply shocks. Equation (B.7) becomes $\hat{C}_{mb}^g = z_m^g - \epsilon_m^g \hat{r}_b^g$. ϵ_m^g may capture differences in true demand elasticities, or be a reduced form for different loadings on a supply shock.

Output. Panel C of Table B.7 reports the output. In lines (C) and (R), ϵ_m^g varies but is independent of other municipality characteristics. In this case, the results that $BankExposure_b^{AW}$ is an unbiased estimate of Z_b^g and $BankExposure_b^{AW} \perp \xi_b$ remain true. In line (C & $\rho_M > 0$), I assume that ϵ_m^g is positively correlated with municipality demand shocks z_m^g. Now, the coefficient in column (3) is larger than 1, i.e., $\hat{\alpha}_m^g$ overestimates (underestimates) z_m^g for high (low) values of z_m^g. Municipalities' differential responses to $\hat{r}_b^g$ are picked up by the $\hat{\alpha}_m^g$, and this differential response systematically correlates with z_m^g. This translates to the same pattern for $BankExposure_b^{AW}$ (col. 6). Despite this measurement error, $BankExposure_b^{\text{AW}} \perp \xi_b$ remains true.[44] Hence, the problem in this case is non-classical measurement error in $BankExposure_b^{\text{AW}}$, which would induce a bias in my main specification (2): if $BankExposure_b^{\text{AW}} = \kappa Z_b^g$, then the coefficient estimated in this regression will be equal to the true coefficient divided by κ. Finally, in line (C & $\rho_B > 0$), I consider the case where municipalities with low elasticities sort into banks with low supply shocks ξ_b. Now, $\hat{\alpha}_m^g$ overestimates z_m^g for low-ϵ_m^g municipalities borrowing from low-ξ_b banks.[45] As a result, $BankExposure_b^{\text{AW}}$ is negatively correlated with ξ_b (col. 10), a violation of the identifying restriction.

To alleviate this concern, I perform a number of additional tests of my baseline specification, reported in Appendix C.1 Table C.3.

B.2.4. Bank-specific demand shocks.

Additive model. I now consider the case where municipalities' credit demand shocks are heterogeneous across banks. Equation (B.7) becomes $\hat{C}_{mb}^g = z_m^g + \tilde{z}_{mb}^g - \epsilon^g \hat{r}_b^g$ where $\tilde{z}_{mb}^g$ is a bank-specific demand shock. I normalize $\tilde{z}_{mb}^g$ to be have a mean equal to 0 within each municipality. Each bank's interest rate is now given by:

$$\hat{r}_b^g = \frac{Z_b^g + \tilde{Z}_b^g - \xi_b}{\epsilon^s + \epsilon^g} \tag{B.9}$$

where $\tilde{Z}_b^g = \sum_m a_{mb}\tilde{z}_{mb}^g$ is the bank-level aggregated bank-specific component of the demand shock.

I test if $BankExposure_b^{\text{AW}}$ provides an unbiased estimate of Z_b^g. I use Z_b^g as opposed to the total shock $Z_b^g + \tilde{Z}_b^g$ as the bank-level demand shock used to identify crowding out for two reasons. First, the municipality-bank component of demand cannot be separately identified from idiosyncratic noise. Second, the bank-specific component of demand is more likely to be correlated to bank characteristics themselves correlated to supply shocks. For this approach to yield an unbiased estimate of the coefficient in my baseline specification (2), an additional requirement is that the regression coefficient of $Z_b^g + \tilde{Z}_b^g$ on Z_b^g equals 1 (or equivalently, $Z_b^g \perp \tilde{Z}_b^g$).

Output. Panel D of Table B.7 shows the results. Lines (R) and (C) consider the case where the $\tilde{z}_{mb}^g$ vary across municipality×bank cell, but are uncorrelated to other municipality or bank characteristics, for the random and the clustered network. Line (C & $\rho_M > 0$) indicates the clustered

[44] The intuition is that the measurement error leads to $BankExposure_b^{\text{AW}} = Z_b^g + \zeta_b$ where ζ_b is a function of Z_b^g. Since $Z_b^g \perp \xi_b$, $BankExposure_b^{\text{AW}} \perp \xi_b$ remains true.

[45] Banks with low ξ_b have high $\hat{r}_b$. They lend to municipalities with low ϵ_m^g, so that the high $\hat{r}_b$ only leads to a small change in $\hat{C}_{mb}^{gov}$, which is rationalized by higher z_m^g for these municipalities.

network where $\mathbb{E}[z_m^g]$ increases in m and $\mathbb{E}[\tilde{z}_{mb}^g]$ increases in b, i.e., municipalities with larger demand shocks z_m^g borrow from banks with larger bank-specific demand shocks $\tilde{z}_{mb}^g$. In line (C & $\rho_B > 0$), $\mathbb{E}[\xi_b]$ increases in b and $\mathbb{E}[\tilde{z}_{mb}^g]$ increases in b, i.e., bank-specific demand shocks $\tilde{z}_{mb}^g$ are correlated with bank supply shocks ξ_b.

Columns (1)-(6) show that additive bank-specific demand shocks do not bias the estimates of z_m^g and Z_b^g. This is true even with sorting: even when some banks face systematically larger bank-specific demand shocks, this does not bias the borrower fixed effects.[46] Columns (9)-(10) show that in all cases, $BankExposure^{AW} \perp \xi_b$, consistent with the fact that $Z_b^g \perp \xi_b$. It is worth noting that in line (C & $\rho_B > 0$), this condition would be violated if we were instead using $Z_b^g + \tilde{Z}_b^g$ as our estimate of the demand shock. Columns (13) tests the additional requirement that the regression coefficient of $Z_b^g + \tilde{Z}_b^g$ on Z_b^g equals 1. This holds true except in line (C & $\rho_M > 0$). If banks lending to high z_m^g municipalities also have large bank-specific demand shocks, then there is a component of bank-specific demand that varies systematically with Z_b^g and is omitted from my specification, creating an omitted variable bias.

Multiplicative model. I then consider a model of the type $\hat{C}_{mb}^g = z_m^g \lambda_{mb} - \epsilon^g \hat{r}_b^g$ where λ_{mb} is a multiplicative bank-municipality specific demand loading. For instance, if municipalities experiencing demand shocks always direct these shocks towards their main bank, then λ_{mb} is high for b the main bank of m. I normalize the loadings to have a mean equal to 1 within each municipality. Each bank's interest rate is now given by:

$$\hat{r}_b^g = \frac{Z_b^g + \tilde{Z}_b^g - \xi_b}{\epsilon^s + \epsilon^g}$$

where $\tilde{Z}_b^g = \sum_m a_{mb} z_m^g (\lambda_{mb} - 1)$. As before, $\tilde{Z}_b^g$ is the bank-level aggregated bank-specific component of the demand shock. I again test if $BankExposure^{AW}$ estimates Z_b^g.

Output. The results are presented in Panel E of Table B.7. The four lines are as above. The results show that the procedure works well in this case: $BankExposure^{AW}$ is an unbiased estimate of Z_b^g, $BankExposure^{AW} \perp \xi_b$, and the coefficient of $Z_b^g + \tilde{Z}_b^g$ on Z_b^g is 1. This is now true also in the case (C & $\rho_M > 0$), even when my simulation implies a high correlation coefficient between Z_b^g and λ_{mb} (equal to 0.68). The intuition is that with a multiplicative structure, for $\tilde{Z}_b^g$ to be correlated to Z_b^g, one would need not only that $\mathbb{E}[z_m^g]$ increases in m and $\mathbb{E}[\lambda_{mb}]$ increases in b, but also that the

[46] In this case, the bank fixed effects cannot be interpreted as only capturing supply factors. My procedure does not require that the bank fixed effects produce unbiased estimates of bank supply shocks.

slope of the latter relationship increases in m. Finally, one can note that in line (C & $\rho_B > 0$), the p-value associated to the regression coefficient of $BankExposure^{\text{AW}}$ on ξ_b is twice lower than that of the regression coefficient of Z_b^g on ξ_b. That is, even if $BankExposure^{\text{AW}} \perp \xi_b$ holds for usual statistical significance levels in simulations with a high correlation between ξ_b and λ_{mb} (0.62), the noise in the estimation of $BankExposure^{\text{AW}}$ works in the direction of inducing a correlation with ξ_b.

To alleviate this concern, I perform a number of additional tests of my baseline specification, reported in Appendix C.1 Table C.3.

B.2.5. Municipalities substituting across banks

CES model. I study the case where local governments consider loans from different banks as imperfect substitutes, as implied by a CES aggregator. Each local government m minimizes: $\sum_b r_b^g C_{mb}^g$ subject to $\left(\sum_b a_{mb}^{\frac{1}{\theta}} C_{mb}^{g\,\frac{\theta-1}{\theta}}\right)^{\frac{\theta}{\theta-1}} \geq C_m^g$. Solving this problem, we obtain that:

$$C_{mb}^g = a_{mb} \left(\frac{r_b^g}{r_m^g}\right)^{-\theta} C_m^g \quad \text{where} \quad r_m^g = \left[\sum_b a_{mb} (r_b^g)^{1-\theta}\right]^{\frac{1}{1-\theta}}$$

Total local government loan demand is given by: $C_m^g = g e^{z_m^g} (r_m^g)^{-\epsilon^g}$. A natural assumption is $\theta > \epsilon^g$, i.e., substitution across banks is easier than substitution away from bank credit. The rest of the model is as above.

Let $\hat{\mathbf{r}}$, $\mathbf{Z}^g$, and $\hat{\boldsymbol{\xi}}$ be the $\mathcal{N}_B \times 1$ vectors of bank-specific interest rates $\hat{r}_b^g$, local government debt demand shocks Z_b^g, and supply shock ξ_b, respectively. Let $\mathbf{I}$ be the identity matrix. In equilibrium, $\hat{\mathbf{r}} = \left(\epsilon^s \mathbf{I} - ((\theta - \epsilon^g)\ {}^t\mathbf{A^g} \mathbf{A^g} - \theta \mathbf{I})\right)^{-1} [\mathbf{Z}^g - \hat{\xi}]$. We then obtain municipality$\times$bank-level credit using:

$$\hat{C}_{mb}^g = z_m^g - \theta \hat{r}_b^g + (\theta - \epsilon^g) \hat{r}_m^g \quad \text{with} \quad \hat{r}_m^g = \sum_b a_{mb} \hat{r}_b^g \tag{B.10}$$

Equation (B.10) makes the identification issue immediately apparent: the municipality fixed effects will absorb both z_m^g and the average municipality-level interest rate shock $\hat{r}_m^g$. Equation (B.10) also clarifies that this issue arises only to the extent that $\theta - \epsilon^g \neq 0$. That is, if municipalities substitute across banks to the same degree that they substitute away from overall credit, this term disappears and the model becomes equivalent to the canonical model.

Output. Panel F of Table B.7 shows the simulation results. Line (R) shows the results for the random bank-municipality network. In this case, the presence of substitution adds noise but the bias is small (the coefficient in column 6 is close to 1). However, when the network is clustered (following lines), the bias can become large, as can be seen from the coefficients in column (6) being significantly above 1. The demand shocks z_m^g are overestimated for municipalities with large $\hat{r}_m^g$, i.e. municipalities that borrow from banks with large Z_b^g, so that we overestimate Z_b^g for these banks. A highly clustered network makes this issue acute: each municipality borrows from a set of adjacent banks that are all facing the demand shocks of the same adjacent municipalities. In the case (C & $\rho_B > 0$) where adjacent banks have correlated supply shocks, this also leads to $BankExposure^{\text{AW}} \not\perp \xi_b$. I perform additional tests to quantify the severity of this concern in the true data at the end of this section.

Beyond CES. In the CES case, the term capturing substitution patterns enters as a municipality-level effect that is perfectly collinear with the municipality demand shock, inducing an identification problem. While the CES assumption is analytically convenient, it may not be the most realistic in practice. For instance, substitution patterns may not be perfectly symmetric if some banks are closer substitute than others in response to a bank-specific shock. I now explore how the Amiti-Weinstein procedure performs in non-symmetric substitution cases. To do so, I make several plausible ad-hoc assumptions about the substitution patterns, and show the simulations output in each case. The data-generating process is:

$$\hat{C}_{mb}^g = z_m^g - \theta\xi_b + (\theta - \epsilon^g)\xi_{m,-b} + \varepsilon_{mb} \tag{B.11}$$

$-\theta\xi_b$ is the effect of bank b's contractionary shock ξ_b on its credit to municipality m and $(\theta-\epsilon^g)\xi_{m,-b}$ captures the substitution towards bank b due to the shocks of municipality m's other banks.

I consider two different functional forms for $\xi_{m,-b}$. In hypothesis (H1), $\xi_{m,-b} = \sum_{b'} a_{mb'}\theta_{bb'}\xi_{b'}$ where $\theta_{bb'}$ is a proxy for the elasticity of substitution between bank b and bank b'. This captures the idea that a shock to bank b' will generate more substitution towards bank b than a shock to bank b'' if (b, b') are closer substitutes than (b, b''). I propose one implementation where $\theta_{bb'} = \frac{\mathbf{A_b} \cdot \mathbf{A_{b'}}}{\|\mathbf{A_b}\|\|\mathbf{A_{b'}}\|}$: two banks are close substitutes if the cosine similarity between their vectors of municipality shares is high. In hypothesis (H2), borrowers systematically turn towards their main bank $b^1(m)$ when other banks are shocked. Substitution towards the main bank is the average shock of other banks $\xi_{m,-b^1(m)} = \sum_{b' \neq b^1(m)} a_{mb'}\xi_{b'}$. Substitution towards non-main banks occurs only if the relationship bank is shocked: for $b \neq b^1(m)$, $\xi_{m,-b} = a_{mb^1(m)}\xi_{b^1(m)}$.

Panel G of Table B.7 shows the simulation results. I again report results for the random network, the clustered network, and the clustered network with sorting. The procedure works well: the estimated $BankExposure_b^{\text{AW}}$ is close to the true Z_b^g and $BankExposure_b^{\text{AW}} \perp \xi_b$. That is, breaking the symmetry of the CES assumption allows the procedure to correctly parse out the municipality-specific demand shocks from the substitution term.

How large could the bias be? The results above show that municipalities substituting across banks can induce a large bias in the estimation of demand shocks if municipalities substitute across banks as predicted by a CES assumption, $\theta >> \epsilon^g$, and the network is highly clustered. While the bias can theoretically be very large, I now show that in the context of my analysis the bias is likely negligible. First, I provide a separate estimation of the degree of substitution and obtain that $\theta \approx \epsilon^g$. Second, I show that the true municipality-bank network exhibits only a mild degree of clustering, so that even if $\theta > \epsilon^g$, the bias will be small.

Estimation of θ. First, I estimate $\epsilon^g - \theta$ in equation (B.10). Estimating the degree of substitution requires a bank-specific supply shifter that affects $\hat{r}^g_b$. This specification cannot be estimated with municipality fixed effects, which would absorb $\hat{r}^g_m$. Therefore, to obtain an unbiased estimate of $\epsilon^g - \theta$, it is critical that the average supply shock faced by a municipality $\hat{r}^g_m$ is orthogonal to the municipality demand shock z^g_m. I exploit the supply shocks identified in my narrative analysis: the 2009 collapse of Dexia and the 2013 entry of Banque Postale (see sections B.1.4 and B.1.5 for details). In both cases, it is plausible that municipality exposure to the shock is orthogonal to z^g_m. I estimate:

$$\Delta C^{gov}_{mbt} = \delta_t + \beta Treated_{bt} + \gamma ShareTreated_{mt} + \varepsilon_{mbt} \tag{B.12}$$

$Treated_{bt}$ is a dummy equal to 1 for banks experiencing the supply shock. Consistent with (B.10), $ShareTreated_{mt}$ is the municipality-level average of $Treated_{bt}$, weighted by bank shares. β identifies $-\theta$ multiplied by a scalar. γ identifies $\theta - \epsilon^g$ multiplied by the same scalar. Table B.8 presents the results. Columns (1) and (3) confirm that the supply shocks have strong effects, and thus are susceptible of causing discernible substitution effects. Columns (2) and (4) add $ShareTreated_{mt}$. In both cases, the estimated coefficient is statistically insignificant and very close to 0 in magnitude. In the case of column (4), the coefficient even has the wrong sign. These results show that the elasticity of substitution θ is not significantly different from the elasticity of corporate credit demand ϵ^g.[47]

Here, I use the two supply shocks to estimate the elasticity of substitution across banks, and show that substitution across banks is unlikely to generate a bias in the estimated fixed effects $\hat{\alpha}^{gov}_{mt}$. In the narrative analysis sections B.1.4 and B.1.5, I use these shocks to provide a more direct test: I show that municipality-level exposure to these supply shocks strongly predicts municipality-level

[47] One potential reason why substitution is limited is because municipalities aggregate many individual local governments, some of which borrow from a single bank.

Table B.8. Estimation of elasticity of substitution across banks

	Credit growth ΔC^{gov}_{mbt}			
	Dexia		Banque Postale	
	(1)	(2)	(3)	(4)
Treated$_{bt}$	-0.127***	-0.128***	1.889***	1.887***
	(0.006)	(0.006)	(0.037)	(0.037)
ShareTreated$_{mt}$		0.016		0.113
		(0.025)		(0.267)
Time FE	✓	✓	✓	✓
Sample	2009	2009	2013	2013
Observations	13,452	13,451	14,048	14,048
R-squared	0.032	0.032	0.16	0.16

Note: This table reports the results of estimating specification (B.12). In columns (1)-(2), *Treated$_{bt}$* is a dummy equal to 1 for Dexia and *ShareTreated$_{mt}$* is the 2008 market share of Dexia. In columns (3)-(4), *Treated$_{bt}$* is a dummy equal to 1 for Banque Postale and *ShareTreated$_{mt}$* is the 2013 market share of Banque Postale. Regressions are weighted by credit volumes. ***, ** and * indicate significance at the 1%, 5% and 10% levels, respectively.

credit growth (as expected) but does *not* predict the estimated fixed effects $\hat{\alpha}^{gov}_{mt}$. This provides direct support to the hypothesis that the $\hat{\alpha}^{gov}_{mt}$ are not biased due to bank supply shocks.

Bias for empirically-relevant degree of clustering. I now perform the following exercise: assuming that $\theta - \epsilon^g$ is not equal to 0, how large would the resulting bias be, given the degree of clustering of the municipality-bank network observed in the true data? I quantify the degree of clustering as follows. I define clusters of municipalities based on the identity of their main bank (the bank with the largest share). I then ask: do municipalities belonging to the "main bank b"-cluster tend to borrow from the same other banks? Formally, let $\mathcal{M}_b$ denote the set of municipalities with main bank b. For each municipality m, define $\mathbf{A_{m,-b}}$ the $1 \times \mathcal{N}_B - 1$ vector of bank shares excluding bank b. For each pair of municipalities (m, m'), let $\cos_{mm',-b}(\mathbf{A}) = \frac{\mathbf{A_{m,-b}} \cdot \mathbf{A_{m',-b}}}{\|\mathbf{A_{m,-b}}\| \|\mathbf{A_{m',-b}}\|}$. Define:

$$
\begin{aligned}
&\mathcal{P}^{in}_b(\mathbf{A}) = \frac{1}{|\mathcal{M}_b|} \sum_{m \in \mathcal{M}_b} \frac{1}{|\mathcal{M}_b| - 1} \sum_{m' \in \mathcal{M}_b, m' \neq m} \cos_{mm',-b}(\mathbf{A}) && \text{(in-cluster proximity for } \mathcal{M}_b\text{)} \\
&\mathcal{P}^{out}_b(\mathbf{A}) = \frac{1}{|\mathcal{M}_b|} \sum_{m \in \mathcal{M}_b} \frac{1}{\mathcal{N}_M - |\mathcal{M}_b|} \sum_{m' \notin \mathcal{M}_b} \cos_{mm',-b}(\mathbf{A}) && \text{(out-cluster proximity for } \mathcal{M}_b\text{)} \\
&\mathcal{C}(\mathbf{A}) = \frac{1}{\mathcal{N}_B} \sum_b \frac{\mathcal{P}^{in}_b(\mathbf{A})}{\mathcal{P}^{out}_b(\mathbf{A})} && \text{(degree of clustering)}
\end{aligned}
\tag{B.13}
$$

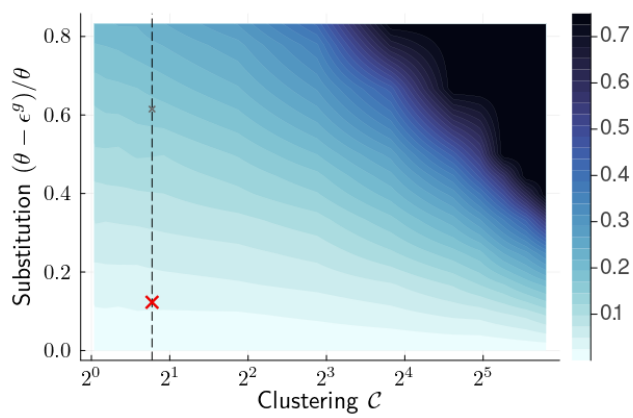
For each cluster $\mathcal{M}_b$, $\frac{\mathcal{P}^{in}_b(\mathbf{A})}{\mathcal{P}^{out}_b(\mathbf{A})}$ is the average pairwise similarity of bank shares of municipalities in $\mathcal{M}_b$, compared to the average pairwise similarity of municipalities in $\mathcal{M}_b$ with municipalities out of $\mathcal{M}_b$. $\mathcal{C}(\mathbf{A})$ averages this metric across banks. When the network is random (as in Fig. B.3(a)), $\mathcal{C}(\mathbf{A}) = 1$. A larger $\mathcal{C}(\mathbf{A})$ indicates a higher degree of clustering. For the fully deterministic clustered network

(Fig. B.3(b)) we get $\mathcal{C}(\mathbf{A}) = 55.79$.

Computing this metric in the true data, I find $\mathcal{C}(\mathbf{A}) = 1.71$. The true municipality-bank network is not random. However, clustering is much less extreme than assumed in my simulations.

I then simulate a municipality-bank network $\mathbf{A}$ corresponding to this degree of clustering and quantify the bias in $BankExposure^{\mathrm{AW}}$. The result of this exercise is reported in Figure B.4. The x-axis is $\mathcal{C}$, the degree of clustering in the simulated data (on a log2 scale). The y-axis is $\frac{\theta-\epsilon^g}{\theta}$, the size of the substitution effect compared to the direct effect of the bank shock. The shading indicates the severity of the bias, which I define as $abs(\kappa - 1)$ where κ is the regression coefficient of $BankExposure_b^{\mathrm{AW}}$ on the true shock Z_b^g (as in col. 6 of Table B.7). The dashed black line indicates the where the actual data lies, given the observed value of $\mathcal{C}$. Given the observed extent of clustering, the bias is well below that shown in Table B.7.

Figure B.4. Bias due to municipalities substituting across banks



Note: This figure plots a measure of the bias in $BankExposure_b$ for different data-generating processes, obtained from the simulation model described in section B.2. The x-axis is $\mathcal{C}(\mathbf{A})$, the degree of clustering of the bank-municipality network, defined in the text. The y-axis is $\frac{\theta-\epsilon^g}{\theta}$. The color shading indicates the severity of the bias, which I define as $abs(\kappa - 1)$ where κ is the regression coefficient of $BankExposure_b$ on the true shock Z_b^g. The dashed line indicates the value of $\mathcal{C}$ in the actual data. The two markers indicate the value of the bias for various assumptions on $\frac{\theta-\epsilon^g}{\theta}$.

To further quantify the severity of the bias, I need an assumption for $\frac{\theta-\epsilon^g}{\theta}$. I take seriously the fact that the point estimate of the coefficient for the substitution effect in column (2) of Table B.8 is not exactly 0 and compute $\frac{\theta-\epsilon^g}{\theta}$ using this value (divided by the coefficient of the direct effect). This yields $\theta = 1.14\epsilon^g$, marked by the red cross. As the figure indicates, the bias is very small (3%). I also show the result for a more conservative assumptions: I assume the substitution effect is equal to five times the point estimate in Table B.8 (implying $\theta = 2.60\epsilon^g$). The grey cross shows that even with a conservative assumption on the degree of substitution, the bias remains around 15%. In sum, even if municipalities substitute across banks following the predictions of a CES, $\theta \approx \epsilon^g$ and $\mathcal{C}(\mathbf{A})$ small imply that the bias is negligible.

B.3. Identification with the shift-share design

This section discusses the identifying assumption (B.1), repeated here for convenience:

$$\mathbb{E}\left[\left[\sum_m \omega_{bm}^{gov}\hat{\alpha}_m^{gov}\right]\varepsilon_{fb}\,|\,d_f\right]=0 \tag{B.14}$$

where I omit time subscripts to simplify notations.

The necessary requirement for identification 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:

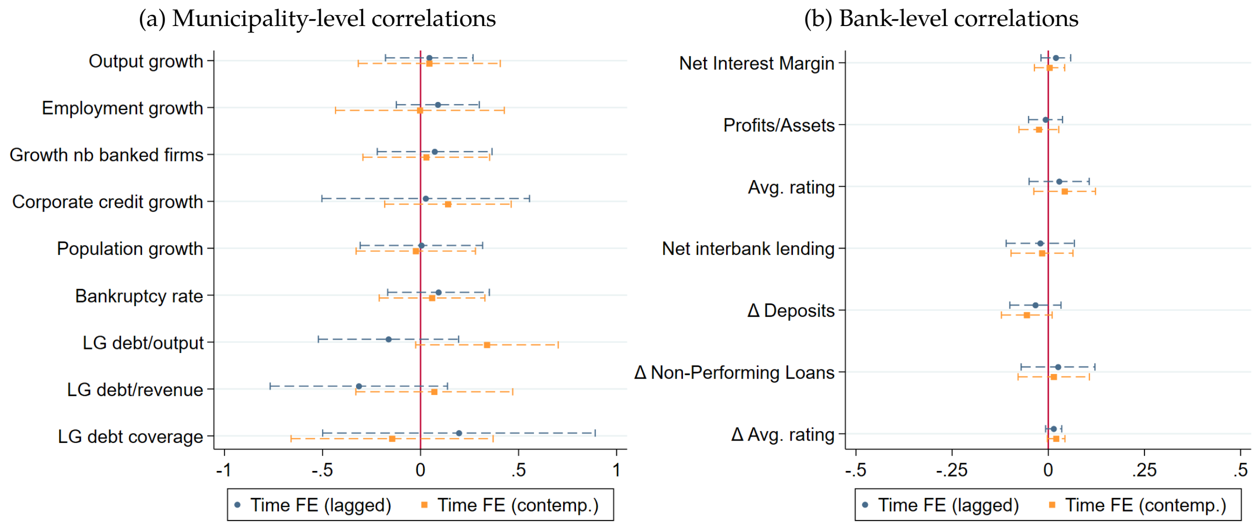
$$\mathbb{E}\left[\sum_m \hat{\alpha}_m^{gov}\left(\sum_{f,b}\tilde{\omega}_{bm}^{gov,f}\varepsilon_{fb}\right)\right]=0 \tag{B.15}$$

$\hat{\alpha}_m^{gov}$ 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 $\hat{\alpha}_m^{gov}$ 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.5 shows that $\hat{\alpha}_m^{gov}$ is not correlated with the lagged or contemporaneous municipality-level output 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 economic outcomes. However, this re-

Figure B.5. Additional balance tests



Note: Panel (a) shows the coefficient of municipality-level regressions of local government debt demand shocks $\hat{\alpha}^{gov}_{mt}$ on municipality-level variables. LG debt-to-output is total local government bank debt from the credit registry divided by municipality output. LG debt-to-revenue and LG debt coverage are obtained from the individual local governments accounts (aggregated at the municipality-level, see Appendix F for details). LG debt-to-revenue is debt divided by revenues (tax revenues and central government transfers). LG debt coverage equals debt obligations (interests and principal repayment) divided by net income (total revenues minus current expenditures, excluding interests). 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. Panel (b) shows the coefficients of bank-level regressions of bank exposure to local government debt demand (defined in (4)) on bank characteristics. Avg. rating refers to the credit ratings issued by Banque de France (transformed into numeric values, higher rating meaning lower risk). Regressions are weighted by bank-level corporate credit. Standard errors are clustered at the bank level. All regressions include time fixed effects. The blue (orange) dots correspond to correlations with lagged (contemporaneous) characteristics. The dot is the point estimate and the bar is the 95% confidence interval. All variables are standardized.

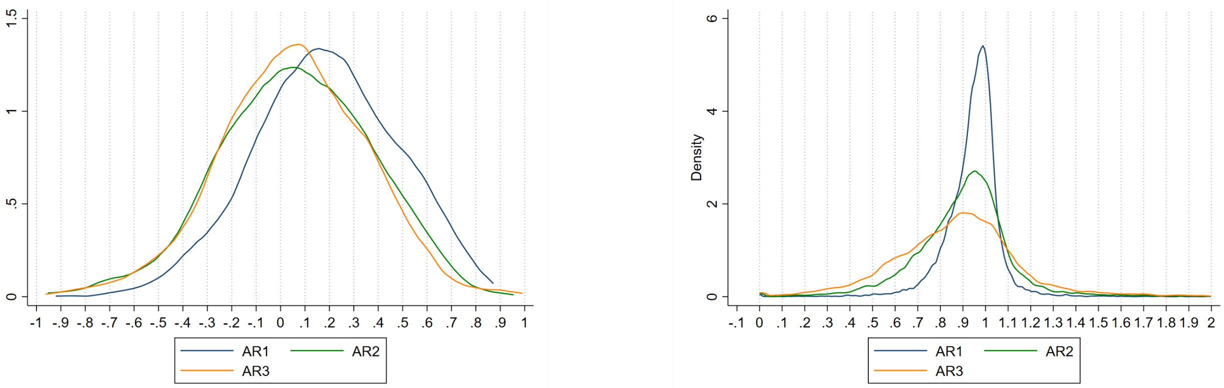
lationship is unlikely to operate at the municipality level: municipalities are small and are not the relevant economic scale for stimulus spending effects, and there is high dispersion in α^{gov}_{mt} across neighboring municipalities (Fig. 2). Figure B.5 shows that $\hat{\alpha}^{gov}_{m}$ is not correlated with measures of local government creditworthiness (local government debt-to-output, debt-to-revenue, or debt coverage), alleviating concerns that high $\hat{\alpha}^{gov}_{m}$-municipalities are risky. In addition, Figure B.6 show that the $\hat{\alpha}^{gov}_{m}$ 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 (see Fig. A.3 for the distributions of capital expenditures and debt).

Necessary condition for identification. While reassuring, these municipality-level orthogonality conditions are not necessary. 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, generating a bank-level shock correlated to *BankExposure*. Several features of the shares support the identifying assumption. First, I use shares specifically in the local government credit market. Any municipality-level shock emanating from corporates would affect banks via their ex-

FIGURE B.6. 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.

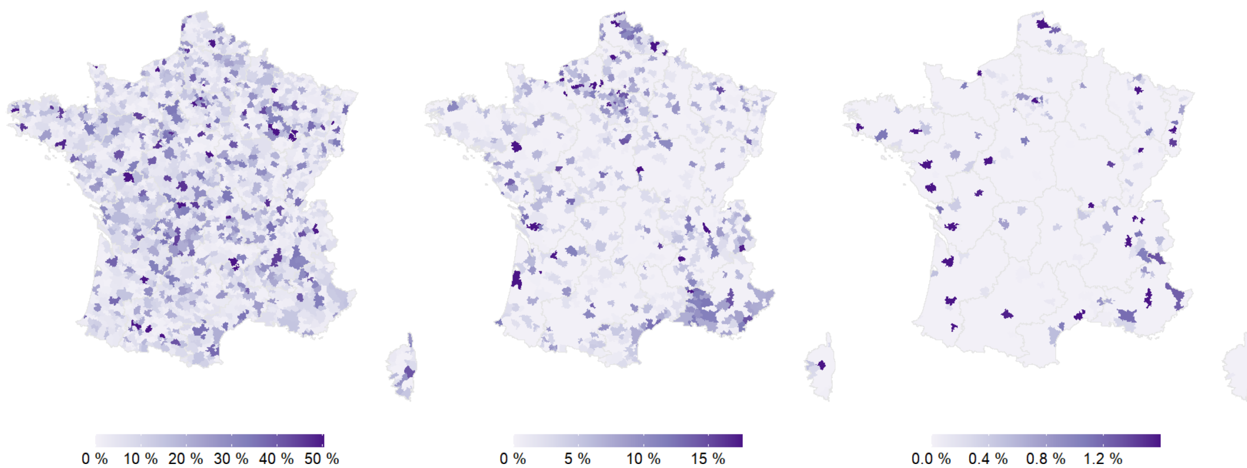
posure to the corporate credit market. Conversely, bank-specific corporate credit shocks would affect municipality-level outcomes (like local government debt demand) of municipalities with large corporate credit presence of affected banks. As a placebo test, Table C.3 shows that *BankExposure* constructed with corporate credit exposure weights does not predict a decline in corporate credit. Second, the maps in Figure B.7 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.6 shows that shares are highly persistent. This rules out banks on declining corporate credit supply trends strategically increasing their shares in high $\hat{\alpha}_m^{gov}$ municipalities in every period. As a further check, Table C.3 shows that my results are virtually identical when I fix shares in 2006.

Together, the fact that $\hat{\alpha}_m^{gov}$ does not correlate with municipalities variables and the specificity of shares rationalize that *BankExposure* does not appear correlated with other bank characteristics in Figure 3. I complement this evidence in Figure B.5(b) with another set of bank-level balance tests. In particular, it is not the case that higher *BankExposure* banks have riskier borrowers or face a degradation of the quality of their portfolio.

Role of firm×time fixed effects. Including firm×time fixed effects is critical for the assumption (B.15) to plausibly hold. Otherwise, obtaining an unbiased estimate of β^C would require that

Figure B.7. 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.

$BankExposure_b$ is orthogonal to firm demand shocks d_f. The most obvious threat is sorting of banks across municipalities, along with a correlation between local government debt demand shocks and firm demand shocks. To see this, re-write the required orthogonality condition (B.15) in the absence of fixed effects:

$$\mathbb{E}\left[\sum_m \hat{\alpha}_m^{gov}\left(\sum_f \bar{\omega}_{fm}^{gov} d_f + \sum_{f,b} \omega_{bm}^{gov} \varepsilon_{fb}\right)\right] = 0 \tag{B.16}$$

where $\bar{\omega}_{fm}^{gov}$ is the sum of ω_{bm}^{gov} for the set of banks b lending to f. $\sum_f \bar{\omega}_{fm}^{gov} d_f$ is 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, $\sum_f \bar{\omega}_{fm}^{gov} d_f$ will put a large weight on the corporate credit demand shocks of firms located in m. $\sum_f \bar{\omega}_{fm}^{gov} d_f$ is then likely to be correlated with $\hat{\alpha}_m^{gov}$. Hence, this condition is unlikely to hold.[48]

There may be other reasons why (B.15) holds only conditional on fixed effects. For instance, Figure 3 shows that banks with higher *BankExposure* tend to be slightly larger and have a higher probability to be state-owned. Even if these differences are not statistically significant, one could be concerned that larger banks (or state-owned banks) lend to firms with different demand shocks, for instance if larger banks lend to larger, healthier firms. If the latter correlation is very strong, this

[48]This equation also clarifies that what matters is not the demand shocks of firms located in m, but rather the average demand shock of the firms borrowing from banks exposed to m.

Table B.9. Shock-level summary statistics

Panel A: Summary statistics on municipality-level shocks

	count	mean	sd	p25	p50	p75
Municipality-level shock $\hat{\alpha}_{mt}^{gov}$	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 $\hat{\alpha}_{mt}^{gov}$. Panel B presents summary statistics of municipality-level weights $s_{mt} = \sum_b C_{bt-1}^{corp} \omega_{bm,t-1}^{gov}$ where C_{bt-1}^{corp} 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.

would bias my estimate of β^C. Another channel could be an information effect. Firms may interpret the reduction in credit supply from high exposure banks as a negative signal on their productivity or investment opportunities. These firms would then reduce their credit demand from all of their banks.[49]

Consistency. Exposure to common municipality-level shocks induce dependencies across banks with similar exposure shares, so that the setting is not *iid*. Borusyak, Hull and Jaravel (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.9 documents a large dispersion in $\hat{\alpha}_m^{gov}$, which persists when residualizing on fixed effects. Besides, exposure shares are not too concentrated. Define municipality-level weights as $s_{mt} = \sum_b C_{bt-1}^{corp} \omega_{bm,t-1}^{gov}$ where C_{bt-1}^{corp} 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.[50]

[49] Dessaint et al. (2019) document a phenomenon of this type on the stock market: firms significantly reduce investment in response to non-fundamental drops in the stock price of their peers, due to managers' limited ability to filter out the noise in stock prices when using them as signals about their investment opportunities.

[50] 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%.

Alternative interpretation of identification based on shares. As shown by Goldsmith-Pinkham, Sorkin and Swift (2020), $\mathbb{E}[\varepsilon_{fb}\omega_{bm}^{gov} \mid d_f] = 0$ for all m with $\hat{\alpha}_m^{gov} \neq 0$ is a sufficient condition for the shift-share variable to yield an unbiased and consistent estimate. This assumption is credible in my setting, but shares exogoneity is a less intuitive source of identification. 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.[51,52] 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.

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

[52] These 5 weights 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. From the results in Table 3, I estimate the (partial equilibrium) corporate credit shortfall compared to a counterfactual where local government debt demand shocks α^{gov}_{mt} are all equal to 0. For this back-of-the-envelope computation, 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).

For the average firm borrowing from the average bank, actual credit growth $\widehat{\Delta C}_{fbt}$ is lower by $\hat{\beta}^C \times \overline{BankExposure}_{bt}$ compared to the counterfactual growth rate $\Delta C_{fbt}(\mathbf{0})$ when $\forall m\ \alpha^{gov}_{mt} = 0$, implying a euro shortfall equal to $\hat{C}_{fb,t} - C_{fb,t-1}(\mathbf{0}) = \hat{\beta}^C \times \overline{BankExposure}_{bt} \times \overline{C}_{fb,t-1}$. Aggregating over (identical) firms and banks, the aggregate corporate credit shortfall is: $\hat{C}^c_t - C^c_t(\mathbf{0}) = \hat{\beta}^C \times \overline{BankExposure}_{bt} \times \overline{C}^c_{t-1}$. Substituting the definition of $BankExposure_{bt}$ (when all banks and municipalities are identical), $\hat{C}^c_t - C^c_t(\mathbf{0}) = \hat{\beta}^C \times \overline{\alpha}^{gov}_{mt} \times \frac{\overline{C}^{gov}_{t-1}}{\overline{C}^{tot}_{t-1}} \times \overline{C}^c_{t-1}$, where $\overline{C}^{tot}_{t-1}$ is total credit (corporate and local government combined), as used in the denominator of the exposure weights $\omega^{gov}_{bm,t-1}$.

To obtain a euro-for-euro effect, defined as $m^C = \frac{\hat{C}^c_t - C^c_t(\mathbf{0})}{\hat{C}^{gov}_t - C^{gov}_t(\mathbf{0})}$, I divide this quantity by the corresponding change in local government debt. In the counterfactual where $\forall m\ \alpha^{gov}_{mt} = 0$, local government debt growth is lower by $\overline{\alpha}^{gov}_{mt}$, and the corresponding euro change is $\hat{C}^{gov}_t - C^{gov}_t(\mathbf{0}) = \overline{\alpha}^{gov}_{mt} \times \overline{C}^{gov}_{t-1}$.

Therefore, $m^C = \hat{\beta}^C \times \frac{\overline{C}^c_{t-1}}{\overline{C}^{tot}_{t-1}}$. The multiplier is equal to the estimated effect, multiplied by the ratio of corporate credit on total credit. Using $\hat{\beta}^C = -0.853$ (Table 3) and $\frac{\overline{C}^c_{t-1}}{\overline{C}^{tot}_{t-1}} = 0.63$ (text in section 3.2), I obtain a euro-for-euro effect equal to $m^C = -0.54$.

C.1.1. 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***	-0.911**	-1.233**	-0.897**	-1.120***	-1.112***	-0.732**
	(0.327)	(0.458)	(0.485)	(0.441)	(0.291)	(0.392)	(0.317)
× High Powerful Exp		-0.136					
		(0.576)					
× High Contested Exp			0.416				
			(0.558)				
× High (Contested×Powerful) Exp				-0.186			
				(0.576)			
× High Powerful Exp					0.083		
					(0.664)		
× High Contested Exp						0.250	
						(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 3 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 level, taking the mayor of the largest *commune* in each municipality. 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.

C.1.2. Additional tests of identifying assumptions.

Additional fixed effects and heterogeneity by strength of demand effect. Table C.2 presents further tests that support the identifying assumptions of my main results, described in the main text.

TABLE C.2. Firm×bank-level effects: Additional tests of identifying assumptions (1)

	Credit growth							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
BankExposure	-0.983***	-0.972***	-0.963***	-0.950***	-1.301***	-1.074***	-0.808***	-0.910***
	(0.315)	(0.306)	(0.311)	(0.320)	(0.294)	(0.314)	(0.298)	(0.318)
BankExposure× Pub. Proc.								0.248
								(0.486)
Controls	✓	✓	✓	✓	✓	✓	✓	✓
Firm×Time FE	✓	✓	✓	✓	✓	✓	✓	✓
Firm×Active bank×Time FE	✓	–	–	–	–	–	–	–
Firm×Ind. spe.×Time FE	–	✓	–	–	–	–	–	–
Firm×Local spe.×Time FE	–	–	✓	–	–	–	–	–
Bank FE	–	–	–	–	✓	–	–	–
Regional shares (pub)×Time FE	–	–	–	–	–	✓	–	–
Regional shares (all)×Time FE	–	–	–	–	–	–	✓	–
Excl. large bank share	–	–	–	✓	–	–	–	–
Observations	2,595,432	2,402,585	2,141,157	1,834,160	2,731,067	2,598,842	2,731,110	2,731,110
R-squared	0.54	0.54	0.55	0.52	0.54	0.54	0.54	0.54

Note: This table presents robustness checks of the main results presented in Table 3. "Active bank" is a dummy equal to 1 if the bank has a non-zero share of local government loans in its portfolio. "Ind. spe." is a dummy equal to 1 if the firm's industry belongs to the top 3 industries for the bank. "Local spe." is a dummy equal to 1 if the firm's municipality belongs to the top 10 municipalities for the bank. "Excl. large bank share" excludes banks accounting for more than 70% of the firm's credit. "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.

Additional tests related to shift-share structure. Table C.3 presents further tests related to the shift-share structure of *BankExposure*. To alleviate the concern that banks on declining corporate credit supply trends strategically increasing their shares in high $\hat{\alpha}_m^{gov}$ municipalities in every period, I fix exposure weights in 2006 (col. 1). In columns (2)-(4), I conduct a placebo test where *BankExposure* is computed with exposure weights based on banks' exposure to corporates $\omega_{bmt-1}^{corp} = C_{bmt-1}^{corp}/C_{bt-1}^{tot}$ instead of local governments. 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. This placebo test thus further alleviates concerns that *BankExposure* is picking up bank exposure to municipality-level shocks occurring on the corporate credit market.[53] In column (5), 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.

Concerns due to imperfect measurement of demand shocks: general considerations. I then conduct a series of tests related to the measurement of municipalities' local government debt demand shocks. A generic concern with shift-share designs is that the true shifters (here, the demand shock of municipality m) are proxied by realized values. In the standard design, the shifters are realized changes (here, ΔC_m^{gov}), and may be contaminated by the supply shocks of the large banks in m which also enter the residual of my bank-level regression. The simulation study in section B.2 shows that defining the shift-share with the fixed effects $\hat{\alpha}_m^{gov}$ as shifters largely alleviates this issue. I now report additional tests that alleviate residual concerns related to this point.

First, $\hat{\alpha}_m^{gov}$ is most likely to be contaminated by the supply shocks of the large banks in m. Column (6) of 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.

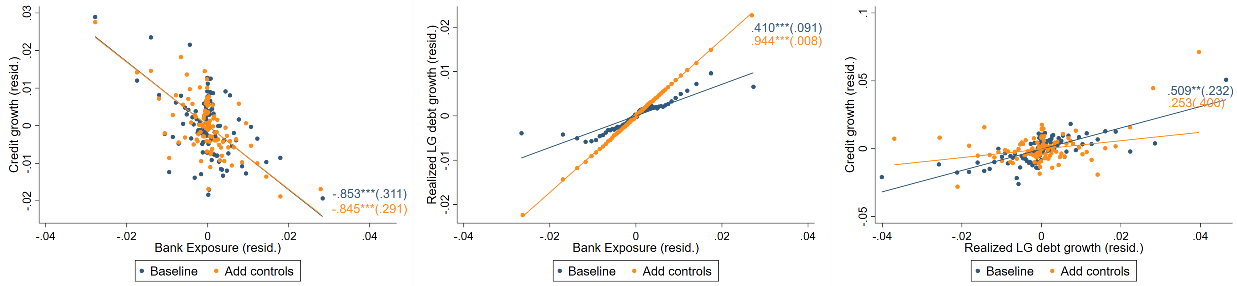
Second, if this were an issue, the coefficient on the shift-share variable *BankExposure* would be biased towards the coefficient with its "realized" quantity equivalent as an explanatory variable. Define $\mathbb{d}C_{bt}^{gov} = \sum_m \omega_{bmt-1}^{gov} \Delta C_{bmt}^{gov} = \frac{C_{bt}^{gov} - C_{bt-1}^{gov}}{C_{bt-1}^{tot}}$ 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 12). 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 C.1 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 (panel c).[54] These considerations are robust to controlling for $\hat{\alpha}_{bt}^{gov}$ and $\lambda_{bt-1}^{gov} \hat{\alpha}_{bt}^{gov}$, where $\hat{\alpha}_{bt}^{gov}$ provides an estimate for any unobservable bank-specific supply shock.

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

[54] 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.

Figure C.1. 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$\times$time fixed effects and controls. In the "baseline" specification, included controls are the baseline bank-level controls. In the "add controls" specification, additional controls are $\hat{\alpha}^{gov}_{bt}$ and $\lambda^{gov}_{bt-1}\hat{\alpha}^{gov}_{bt}$. Corresponding regression coefficients and standard errors are printed.

Concerns due to imperfect measurement of demand shocks: refinements of the Amiti-Weinstein estimation. The simulation results in Table B.7 show that the estimated demand shocks $\hat{\alpha}^{gov}_{m}$ will be biased if municipalities have heterogeneous loadings on bank-specific credit supply shocks, in a way that is correlated to municipality or bank-level shocks. A problematic example would be if smaller municipalities have a higher loading on bank-level shocks and banks lending to small municipalities have a different distribution of supply shocks. To alleviate this type of concern, I propose an improvement over the standard Amiti-Weinstein decomposition. Let us assume that the municipality-specific loadings depend on an observable characteristic X_{mt} taking discrete values $x \in \Omega_X$. I estimate:

$$\Delta C^{gov}_{mbt} = \alpha^{gov}_{mt} + \sum_{x \in \Omega_X} \alpha^{gov}_{bt} \mathbb{1}[X = x]_{mt} + \varepsilon_{mbt} \tag{C.1}$$

This specification allows the effect of the credit supply shock α^{gov}_{bt} to vary for municipalities with different values of X_{mt}. Panel B of Table C.3 presents the results of my main specification when $BankExposure_{bt}$ is constructed using $\hat{\alpha}^{gov}_{mt}$ estimated from equation (C.1). I interact α^{gov}_{bt} with quartiles of municipality size, proxies for municipalities' creditworthiness (local government debt-to-output, debt-to-total revenues, debt coverage), output growth, and local government debt growth. I also consider the possibility that banks pass on supply shocks differentially to municipalities in their core geographic area by interacting α^{gov}_{bt} with a dummy equal to 1 if the municipality is among the top 10 for that bank. Across specifications, the estimated effect is highly similar to my baseline.

Table C.3. Firm×bank-level effects: Additional tests of identifying assumptions (2)

Panel A: Additional tests

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

Panel B: Estimation of $\hat{\alpha}^{gov}_{mt}$: robustness to heterogeneous effects of bank supply shocks

	Credit growth						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
BankExposure	-0.931***	-0.866***	-0.994***	-0.958***	-0.933***	-0.812**	-0.967***
	(0.356)	(0.301)	(0.332)	(0.338)	(0.339)	(0.319)	(0.325)
Controls	✓	✓	✓	✓	✓	✓	✓
Firm×Time FE	✓	✓	✓	✓	✓	✓	✓
Indep. var. def.	× Size	× LG debt-to-output	× LG debt-to-revenue	× LG debt cov.	× Output gr.	× LG debt gr.	× Top muni
Observations	2,731,110	2,731,110	2,731,110	2,731,110	2,731,110	2,731,110	2,731,110
R-squared	0.54	0.54	0.54	0.54	0.54	0.54	0.54

Panel C: Estimation of $\hat{\alpha}^{gov}_{mt}$: robustness to bank-specific demand shocks

	Credit growth							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
BankExposure	-0.833***	-0.820***	-0.705**	-0.796***	-0.872***	-0.858***	-0.751***	-0.833***
	(0.308)	(0.304)	(0.280)	(0.305)	(0.310)	(0.311)	(0.290)	(0.312)
Controls	✓	✓	✓	✓	✓	✓	✓	✓
Firm×Time FE	✓	✓	✓	✓	✓	✓	✓	✓
Indep. var. def.	+ Main bank (1)	+ Main bank (2)	+ % LG (1)	+ % LG (2)	+ Top muni (1)	+ Top muni (2)	+ Large (1)	+ Large (2)
Observations	2,731,110	2,731,110	2,731,110	2,731,110	2,731,110	2,731,110	2,731,110	2,731,110
R-squared	0.54	0.54	0.54	0.54	0.54	0.54	0.54	0.54

Note: This table presents robustness checks of the main results presented in Table 3. "Controls" include the bank's lagged local government loan share, assets (log), equity ratio, and dummies for state-owned and foreign banks. "Add $\hat{\alpha}^{gov}_{bt}$" indicates that $\hat{\alpha}^{gov}_{bt}$ estimated from (3) and its interaction with λ^{gov}_{bt-1} are included as controls. "Indep. var. def." refers to the definition of *BankExposure*. "Excl. largest banks" indicates that the α^{gov}_{mt} 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 $\omega^{corp}_{bmt-1} = C^{corp}_{mbt-1}/C^{tot}_{bt-1}$. "Active" refers to banks with a non-zero share of local government loans in their portfolio. In panel B, *BankExposure* is constructed using $\hat{\alpha}^{gov}_{mt}$ estimated from equation (C.1). For columns (1)-(6), the bank×time fixed effects are interacted with quartiles of municipality size (defined as total local government credit), municipality local government debt-to-output, municipality local government debt-to-revenue, municipality local government debt coverage, municipality output growth, municipality local government debt growth, respectively. In column (7), the bank×time fixed effects is interacted with a dummy equal to 1 if the municipality is among the top 10 municipalities for the bank. See notes of Figure B.5 for the definition of fiscal variables. In Panel C, *BankExposure* is constructed using $\hat{\alpha}^{gov}_{mt}$ estimated from equations (C.2) (columns labelled (1)) or (C.3) (columns labelled (2)). The added variables are a main bank dummy (col. 1-2), quartiles of banks' local government loan shares (col. 3-4), a dummy equal to 1 if the municipality is among the top 10 municipalities for the bank (col. 5-6), quartiles of bank size (7-8). 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.

The simulation results in Table B.7 show that another threat to identification is if municipalities have bank-specific demand shocks, and there is sorting such that banks with high exposure to the municipality-level component of demand also have larger bank-specific demand shocks. I re-estimate the Amiti-Weinstein decomposition by further controlling for potential heterogeneous demand effects. I consider characteristics of the match X_{mbt} taking discrete values that could be associated to bank-specific demand shocks. Let $Positive_{mt}$ be a dummy equal to 1 if $\Delta C^{gov}_{mt} > 0$. I estimate the following models:

$$\Delta C^{gov}_{mbt} = \alpha^{gov}_{mt} + \sum_{x \in \Omega_X} \mathbb{1}[X = x]_{mbt} Positive_{mt} + \sum_{x \in \Omega_X} \mathbb{1}[X = x]_{mbt} + \alpha^{gov}_{bt} + \varepsilon_{mbt} \tag{C.2}$$

$$\Delta C^{gov}_{mbt} = \alpha^{gov}_{mt} + \sum_{x \in \Omega_X} \gamma_m \mathbb{1}[X = x]_{mbt} Positive_{mt} + \sum_{x \in \Omega_X} \gamma_m \mathbb{1}[X = x]_{mbt} + \alpha^{gov}_{bt} + \varepsilon_{mbt} \tag{C.3}$$

Take the case where X_{mbt} is a dummy for a municipalities' main bank. Equation (C.2) allows for bank-specific demand shocks that systematically differ for the main bank, and allows this main bank effect to differ during credit expansions and contractions. Equation (C.3) further allows the main bank loading in case of expansion and contraction to be municipality specific. Panel C of Table C.3 presents the results. In columns (1)-(2), I consider different demand shocks for municipalities' main bank. In columns (3)-(4), I allow municipalities to have differentiated demand shocks towards banks more specialized in lending to local governments. In columns (5)-(6), I allow municipalities to have differentiated demand shocks towards banks more specialized in lending to their geographic area. In columns (7)-(8), I allow municipalities to have differentiated demand shocks towards larger banks. In all these cases, the estimated effect is highly similar to my baseline.

C.1.3. Robustness checks

Controls and sample restrictions. 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.2, I further test the sensitivity of my results by showing the estimated coefficients for various perturbations of my baseline specification. Panel A displays estimated coefficients when

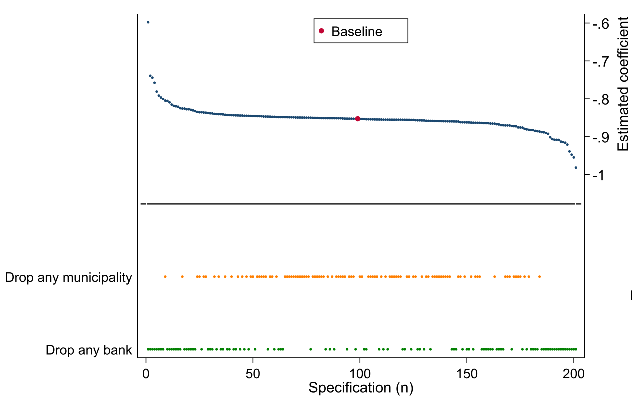
I drop any of the 100 largest banks or any of the 100 largest municipalities from my estimating sample. Panel B shows coefficients estimated in regressions with each control individually and 30 random draws of two to four controls within the set of available controls, for two different fixed effects 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

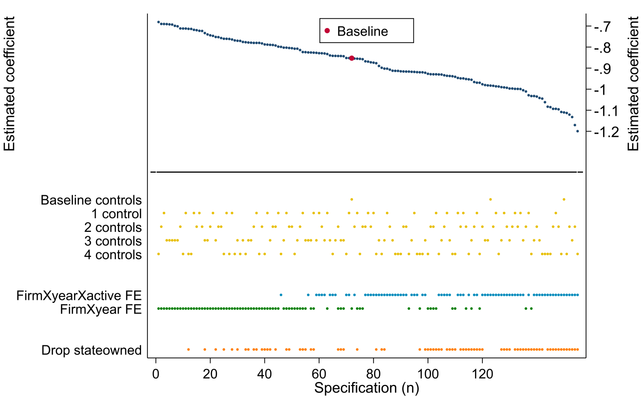
	Credit growth				
	(1)	(2)	(3)	(4)	(5)
BankExposure	-0.723**	-0.853***	-0.855***	-0.902***	-0.983***
	(0.310)	(0.311)	(0.306)	(0.313)	(0.316)
Baseline controls	–	✓	✓	✓	✓
Add. bank controls	–	–	✓	–	–
Firm×Time FE	✓	✓	✓	✓	✓
Sample	Full	Full	Full	$\geq 50€M$	Active
Observations	2,731,110	2,731,110	2,731,110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

Alternative variable definitions. 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).[55] 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. In column (7), I present results when excluding outliers in *BankExposure*. *BankExposure* is bounded, since it is the average of estimated fixed effects $\hat{\alpha}^{gov}_{mt}$ comprised between -2 and 2. That said, the results may be influenced by extreme values of *BankExposure*. To alleviate this concern, I winsorize the extreme values of *BankExposure*, defined as exceeding p50 $\pm$ 2.5(p90-p10). Results are robust to these alternative definitions.

Standard errors. Table C.6 presents results for various assumptions on standard errors. 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.

To account for the fact that $BankExposure_{bt}$ is constructed from estimates of α^{gov}_{mt} and hence contains sampling variation, I bootstrap standard errors. A challenge of the standard bootstrap in models with granular fixed effects is that re-sampling observations may create many singletons. To circumvent this issue, I use the wild bootstrap. The wild bootstrap is implemented by multiplying the residuals from the original model by Rademacher weights (equal to -1 or 1 with probability 0.5) to construct the dependent variable in each bootstrap sample. In addition to desirable properties

[55]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.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	(7) MPGR
BankExposure	-0.605**	-0.188*	-0.201**	-1.051***	-1.113***	-0.617**	-0.772**
	(0.264)	(0.108)	(0.081)	(0.319)	(0.320)	(0.310)	(0.305)
Controls	✓	✓	✓	✓	✓	✓	✓
Firm×Time FE	✓	✓	✓	✓	✓	✓	✓
Indep. var. def.	–	–	–	Incl. bank mergers	*Communes* level	*Bassin de vie* level	Winsor.
Observations	2,731,110	1,982,477	2,579,749	2,731,110	2,731,110	2,731,110	2,731,110
R-squared	0.60	0.53	0.42	0.54	0.54	0.54	0.54

Note: This table presents robustness checks of the main results presented in Table 3. "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.

(Horowitz 1997), this implies that all the original observations remain in all the bootstrap samples and avoids the estimation problem.

Because correct inference requires to account for cross-sectional dependence within clusters, I implement the clustered version of the wild bootstrap proposed in Cameron, Gelbach and Miller (2008) and draw the Rademacher weights at the cluster level. My baseline standard errors are two-way clustered at the bank and municipality-level. Clustered bootstrap procedures can preserve the pattern of correlations within each cluster for one-way clustering, but there is no straightforward way to preserve the correlations in two or more dimensions at once (see, e.g., the discussion of this point in MacKinnon, Nielsen and Webb 2021). Therefore, I compute bootstrapped standard errors for one-way clustering at the bank or at the municipality level.

The results are reported in columns (3) and (4). I produce two version of bootstrapped standard errors. In the first version (line Bootstrap se 1), $BankExposure_{bt}$ is kept constant and only the dependent variable varies in each bootstrap replication (i.e., I implement the standard wild bootstrap procedure). In the second version (line Bootstrap se 2), in each bootstrap iteration, I estimate the α_{mt}^{gov} from (3), construct $BankExposure_{bt}$, which is then used to estimate β^C in (2), so that both the independent and the dependent variable vary. I use the same cluster-level Rademacher weights to multiply the residuals in the two steps. I always use the unrestricted version of the wild bootstrap.[56]

When clustering at the municipality level, the standard error (1) is very similar to the standard cluster-robust estimate reported above in the same column. Bootstrapping both steps (line 2) in-

[56] The restricted version of the wild bootstrap, which imposes the null hypothesis, is not amenable to obtain bootstrap estimates of the α_{mt}^{gov} fixed effects.

creases the standard error, but the coefficient remains highly significant. When clustering at the bank level, the standard error (1) is smaller than the cluster-robust estimate.[57] Bootstrapping both steps again increases the standard error, but the coefficient remains significant at the 5% level.

Table C.6. Firm×bank-level effects: Standard errors

	Credit growth			
	(1)	(2)	(3)	(4)
BankExposure	-0.853***	-0.853***	-0.853***	-0.853**
	(0.311)	(0.122)	(0.142)	(0.402)
Controls	✓	✓	✓	✓
Firm×Time FE	✓	✓	✓	✓
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
Bootstrap se (1)			(.132)	(.288)
Bootstrap se (2)			(.174)	(.405)

Note: This table presents the results of estimating the baseline specification (2) with different assumptions on clustering of standard errors. The line Cluster indicates the level of clustering. Baseline standard errors are double-clustered at the bank and municipality level. Lines Bootstrap se (1) and (2) report standard errors obtained from the wild clustered bootstrap. Bootstrap se (1) bootstraps the estimation of the coefficient in equation (2). Bootstrap se (2) bootstraps both the estimation of the α^{gov}_{mt} from (3) and the coefficient in equation (2). 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). ***, ** and * indicate significance at the 1%, 5% and 10% levels, respectively.

Weighting. 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.

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

	Baseline weighting			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 3. 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.

[57] One potential explanation for the fact that the bootstrapped standard error is smaller than implied by the cluster-robust variance estimator is that inference relying on asymptotic theory has been shown to be problematic when there are few clusters or cluster size is highly variable (Hansen and Lee 2019; Djogbenou, MacKinnon and Nielsen 2019).

C.1.4. Other additional results

TABLE C.8. 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 C.9. 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.

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 total new lending volumes in each quarter. It contains information on the interest rate. The empirical specification is:

$$i_{lfbt} = d_{ft} + \beta BankExposure_{bt} + \Phi \cdot \mathbf{X}_{bt} + \Lambda \cdot \mathbf{W}_l + \varepsilon_{lfbt} \qquad \text{(C.4)}$$

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 more exposed banks. The estimation requires that the firm takes on new loans of the same type from two different banks in a year. 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.10. 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.10. 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	≤ 25 loans	Add leasing	Add subsidized
Observations	472,213	472,182	472,171	310,690	593,233	658,432
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.4). 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 credit and investment

Euro-for-euro crowding out computation. From the results in Table 5, I estimate the (partial equilibrium) capital shortfall compared to a counterfactual where local government debt demand shocks α_{mt}^{gov} are all equal to 0. I assume all variables are equal to their sample means, denoted with an upper bar. From the firm×bank results, the crowding out effect on corporate credit is equal to –0.54 per euro of local government debt (computations details are in section C.1). To obtain the effect on investment, I can use the fact that the euro decline in capital is given by $d\bar{K}_{ft} = \eta^K \frac{\bar{K}_{ft}}{\bar{C}_{ft}} d\bar{C}_{ft}$ where η^K is the credit-to-investment elasticity. Using η^K is estimated in Table 5 (equal to 0.23) and the sample means in Table 1, I obtain that the the crowding out effect on capital is equal to 0.29 per euro of local government debt.[58]

Additional tests of identifying assumptions. Table C.11 presents further tests that support the identifying assumptions of my main results. Columns (1) to (7) display results for various fixed effects structure. Column (1) has the coarsest fixed effects structure with only time fixed effects. Column (6) has the finest fixed effects structure: ISIC 2-digit industries × 2080 municipalities × year, size × year, as well as firm fixed effects. Column (8) controls for lagged credit growth, which restricts the comparison to firms on a similar credit trajectory. Column (9) looks at the differential effect of exposure to crowding out for firms in industries highly reliant on public procurement.

Robustness checks. I report several robustness checks on the firm-level specification.

Controls and sample restrictions. Table C.12 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) restricts the sample to firms borrowing from at least two banks. Column (6) 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.

Weighting. 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 weight-

[58]One could instead repeat the computations in section C.1, using the fact that when all variables are equal to their sample mean $\overline{FirmExposure_{ft}} = \overline{BankExposure_{bt}}$. My computations implicitly use the fact that the reduction in credit by a bank is equivalent to the reduction in credit for the borrowers of this bank—consistent with the fact that I find that firms are unable to significantly substitute across banks.

Table C.11. Firm-level effects: Tests of identifying assumptions

Panel A: Credit

	gr(credit)								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
FirmExposure	-1.183***	-1.191***	-1.116***	-1.050***	-1.050***	-1.402***	-1.401***	-1.148***	-1.379***
	(0.365)	(0.316)	(0.277)	(0.261)	(0.260)	(0.323)	(0.322)	(0.322)	(0.345)
FirmExposure× Pub. Proc.									-0.081
									(0.266)
Controls	✓	✓	✓	✓	✓	✓	✓	✓	✓
Time FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
Ind.(base)×Municipality×Time FE	–	–	–	✓	✓	✓	✓	✓	✓
Firm FE	–	–	–	–	–	✓	✓	–	✓
Ind.(12)×Region×Time FE	–	✓	–	–	–	–	–	–	–
Ind.(38)×Municipality×Time FE	–	–	✓	–	–	–	–	–	–
Size×Time FE	–	–	–	–	✓	–	✓	–	–
Lagged credit growth	–	–	–	–	–	–	–	✓	–
Observations	936,822	936,822	845,293	807,979	807,974	780,138	780,135	683,665	770,739
R-squared	0.93	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)	(9)
FirmExposure	-0.415***	-0.449***	-0.511***	-0.465***	-0.463***	-0.452***	-0.452***	-0.478***	-0.491***
	(0.095)	(0.081)	(0.081)	(0.080)	(0.080)	(0.108)	(0.109)	(0.123)	(0.128)
FirmExposure× Pub. Proc.									0.154
									(0.259)
Controls	✓	✓	✓	✓	✓	✓	✓	✓	✓
Time FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
Ind.(base)×Municipality×Time FE	–	–	–	✓	✓	✓	✓	✓	✓
Firm FE	–	–	–	–	–	✓	✓	–	✓
Ind.(12)×Region×Time FE	–	✓	–	–	–	–	–	–	–
Ind.(38)×Municipality×Time FE	–	–	✓	–	–	–	–	–	–
Size×Time FE	–	–	–	–	✓	–	✓	–	–
Lagged credit growth	–	–	–	–	–	–	–	✓	–
Observations	913,373	913,372	822,281	785,314	785,311	757,023	757,021	670,136	747,811
R-squared	0.18	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 5. 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.

ing ensures that the coefficients are directly comparable across specifications, in particular when I estimate the credit-to-input IV regressions. Table C.13 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

Table C.12. Firm-level effects: Additional controls and sample restrictions

Panel A: Credit

	gr(credit)					
	(1)	(2)	(3)	(4)	(5)	(6)
FirmExposure	-0.756***	-1.402***	-1.366***	-1.344***	-0.978***	-1.233***
	(0.259)	(0.323)	(0.318)	(0.315)	(0.330)	(0.334)
Wgt bank controls	✓	✓	✓	✓	✓	✓
$\hat{d}_{ft}$	–	✓	✓	✓	✓	✓
Firm controls (base)	–	✓	✓	✓	✓	✓
Firm controls (add)	–	–	✓	✓	–	–
Rel. controls	–	–	–	✓	–	–
FE	✓	✓	✓	✓	✓	✓
Observations	1,023,539	780,138	730,820	730,820	228,292	545,175
R-squared	0.39	0.97	0.96	0.96	0.98	0.97

Panel B: Investment

	gr(capital)					
	(1)	(2)	(3)	(4)	(5)	(6)
FirmExposure	-0.334***	-0.452***	-0.428***	-0.426***	-0.550**	-0.368***
	(0.110)	(0.108)	(0.109)	(0.110)	(0.225)	(0.127)
Wgt bank controls	✓	✓	✓	✓	✓	✓
$\hat{d}_{ft}$	–	✓	✓	✓	✓	✓
Firm controls (base)	–	✓	✓	✓	✓	✓
Firm controls (add)	–	–	✓	✓	–	–
Rel. controls	–	–	–	✓	–	–
FE	✓	✓	✓	✓	✓	✓
Observations	866,142	757,023	713,794	713,794	221,909	527,417
R-squared	0.48	0.57	0.57	0.58	0.64	0.59

Note: This table presents robustness checks of the main results presented in Table 5. "Wgt bank controls" refers to the firm-level average of the bank-specific controls included in Table 3. $\hat{d}_{ft}$ refers to the estimated firm-level credit demand shock. "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.

baseline across all these specifications.

Variable definitions and clustering of standard errors. Table C.14 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

Table C.13. Firm-level effects: Alternative weighting schemes

Panel A: Credit

	gr(credit)						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
FirmExposure	-1.361***	-1.394***	-1.407***	-1.166***	-1.349***	-1.361***	-1.321***
	(0.307)	(0.331)	(0.329)	(0.319)	(0.271)	(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	-0.557***	-0.450***	-0.360***	-0.231	-0.265*	-0.294**	-0.252***
	(0.161)	(0.099)	(0.092)	(0.184)	(0.147)	(0.136)	(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 5. 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$\times$industry$\times$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.

year. Estimated coefficients are again similar to the baseline.

Concerns related to the identification of the firm demand shocks $\hat{d}_{ft}$. My baseline specification includes as a control the firm$\times$time fixed effect estimated from an Amiti-Weinstein decomposition, following the approach in Cingano, Manaresi and Sette (2016) and Jiménez et al. (2019). A potential caveat is if the Amiti-Weinstein decomposition improperly estimates demand shocks. The simulation study of section B.2 shows that the Amiti-Weinstein decomposition performs well in general, but documents cases where it fails. Table C.15 presents tests to alleviate concerns related to this point.

First, the estimation of demand effects may be biased because credit supply shocks have heterogeneous effects across firms, and firm-specific loadings are correlated with demand or supply shocks. To address this concern, I augment the standard model to allow bank-supply shocks to have heterogeneous effects across firms with different characteristics, as in equation (C.1). Second, I re-estimate the Amiti-Weinstein decomposition by further controlling for potential heterogeneous

Table C.14. 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***	-1.455***	-1.402***	-1.402***	-1.402***
	(0.367)	(0.365)	(0.330)	(0.326)	(0.114)	(0.162)	(0.342)
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***	-0.408***	-0.493***	-0.455***	-0.452***	-0.452***	-0.452***
	(0.120)	(0.117)	(0.128)	(0.111)	(0.084)	(0.093)	(0.107)
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 5. "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 3 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.

demand effects, as in equations (C.2)-(C.3). Table C.15 shows that using ten different estimates of the demand shock $\hat{d}_{ft}$ (described in the table notes) produces highly similar results. Third, a concern is that the estimation of demand effects is biased because firms substitute across banks. As shown in Appendix B.2.5, this problem occurs in the case where substitution patterns are CES (implying that the substitution term is perfectly collinear with the firm demand shock), the elasticity of substitution across banks is larger than the elasticity of corporate credit demand, and the network is strongly clustered. In section D.4.1, I propose an estimation strategy for the elasticity of substitution across banks and find that is not significantly larger than the elasticity of corporate credit demand. Finally, I estimate the degree of clustering of the firm-bank network, as defined in equation (B.13). I find that $\mathcal{C} = 3.7$, well below the range of values where the bias becomes significant.

Table C.15. Firm-level effects: Alternative estimation of firm demand shock used as control

Panel A: Credit

	gr(credit)									
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
FirmExposure	-1.377***	-1.343***	-1.629***	-1.385***	-1.074***	-1.396***	-0.968***	-1.314***	-1.104***	-1.225***
	(0.303)	(0.325)	(0.305)	(0.319)	(0.330)	(0.325)	(0.323)	(0.311)	(0.270)	(0.206)
Controls	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
$\hat{d}_{ft}$	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Observations	779,430	780,038	780,084	780,138	315,656	780,138	382,319	777,113	320,668	780,119
R-squared	0.96	0.96	0.92	0.97	0.95	0.97	0.94	0.96	0.88	0.98

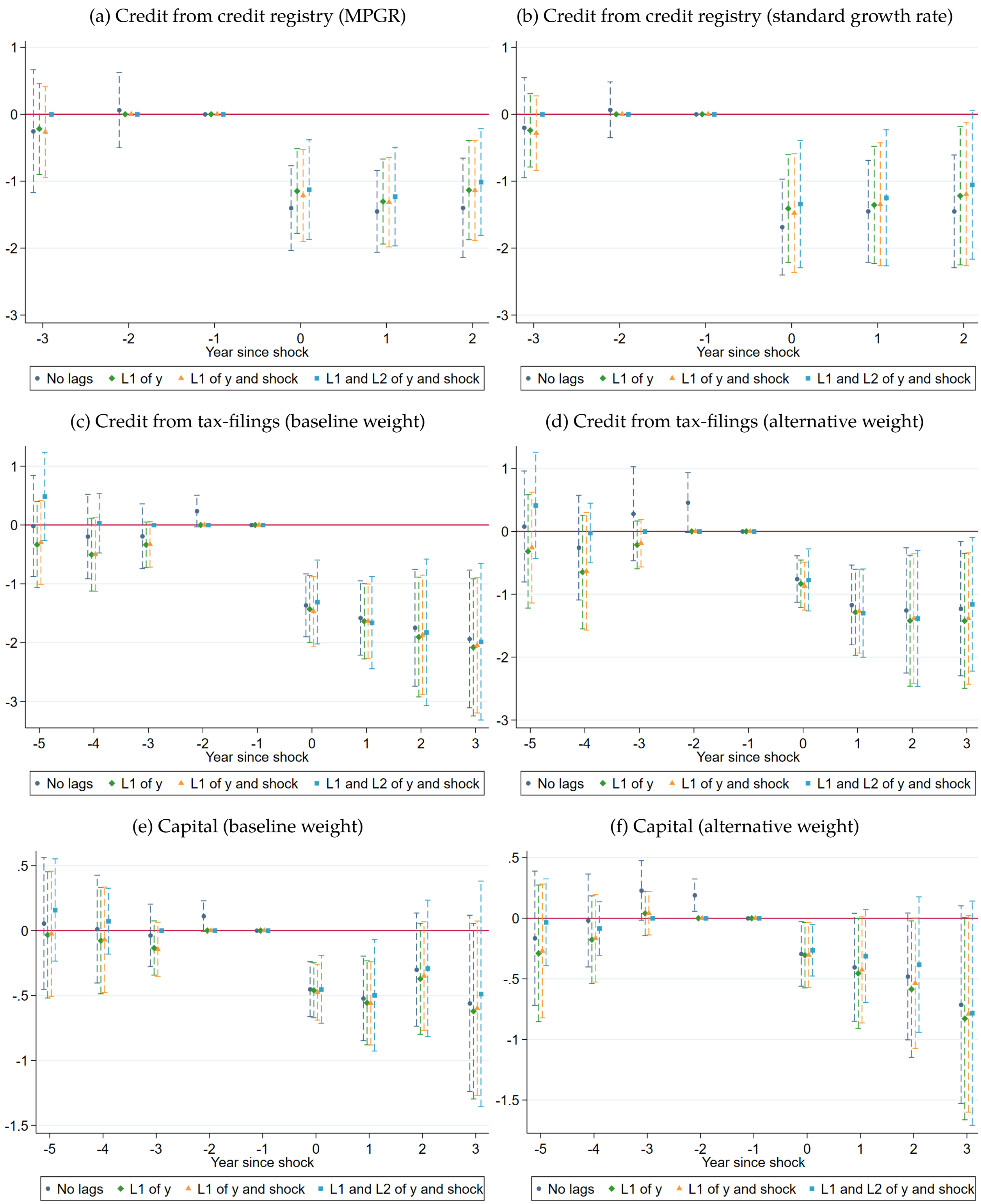
Panel B: Investment

	gr(capital)									
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
FirmExposure	-0.450***	-0.439***	-0.502***	-0.449***	-0.558***	-0.450***	-0.484***	-0.442***	-0.596**	-0.421***
	(0.107)	(0.106)	(0.096)	(0.108)	(0.181)	(0.108)	(0.163)	(0.109)	(0.239)	(0.102)
Controls	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
$\hat{d}_{ft}$	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Observations	756,346	756,932	756,971	757,023	308,281	757,023	372,122	754,148	312,891	757,006
R-squared	0.57	0.57	0.56	0.57	0.61	0.57	0.60	0.57	0.60	0.57

Note: This table presents robustness checks of the main results presented in Table 5. The line $\hat{d}_{ft}$ refers to the definition of the estimated firm×time fixed effects. In columns (1)-(3), $\hat{d}_{ft}$ is estimated from equation (C.1) to allow for bank fixed effects to have heterogeneous effects across firms. The firm characteristics considered are quartiles of firm size (total credit volume) in column (1), a dummy for risky firms (from French central bank credit ratings) in column (2), and a dummy indicating firms that are "strategic" from the point of view of the bank (share of the firm in the bank's total credit higher than 95th percentile) in column (3). In columns (4)-(9), $\hat{d}_{ft}$ is estimated from equations (C.2)-(C.3) to allow for heterogeneous demand effects across banks. In columns (4)-(5), the additional variable is a dummy for a firm's main bank. In columns (6)-(7), the additional variable is a dummy for whether the firm's industry is among the top 10 industries for that bank. In columns (8)-(9), the additional variable is quartiles of banks' "average" interest rates, obtained from a regression of interest rates on new loans on maturity×index×time, firm size×credit rating×type of loan×time, and bank fixed effects. In column (10), $\hat{d}_{ft}$ is the firm-level estimated in the baseline within-firm specification, as recommended in Cingano, Manaresi and Sette (2016) and Jiménez et al. (2019). 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.

Additional evidence on dynamic effects. Finally, Figure C.3 presents robustness checks of the local projection results in Figure 5. Panels (a) and (b) compare the effects on the mid-point growth rate (my baseline) and the standard growth rate, both obtained from the credit registry. Panels (c) and (d) presents results on firm-level credit growth when bank credit is obtained from the tax-filings (as opposed to the credit registry). Panels (e) and (f) show results for investment. In panels (a-c) and (e) regressions are weighted by the baseline weight (mid-point credit volume from the credit registry). In panels (d) and (f) regressions are weighted by the lagged outcome variable.

Figure C.3. Dynamic effects on credit and investment: additional evidence



Note: This figure plots the estimated coefficients β_h resulting from estimating equation (5). The outcome is the h-horizon mid-point growth rate $(Y_{f,t+h} - Y_{f,t-1})/0.5(Y_{f,t+h} + Y_{f,t-1})$ (panel a) or growth rate $(Y_{f,t+h} - Y_{f,t-1})/Y_{f,t-1}$ (other panels). “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 of the shock as controls. “L1 and L2 of y and shock” adds two lags of the outcome variable and of the shock as controls. Baseline weight is mid-point credit (credit registry), alternative weight is the lagged dependent variable. All other elements of the specifications are as in Table 5. The dot is the point estimate and the bar is the 95% confidence interval.

C.4. Cross-sectional effects on other firm-level outcomes

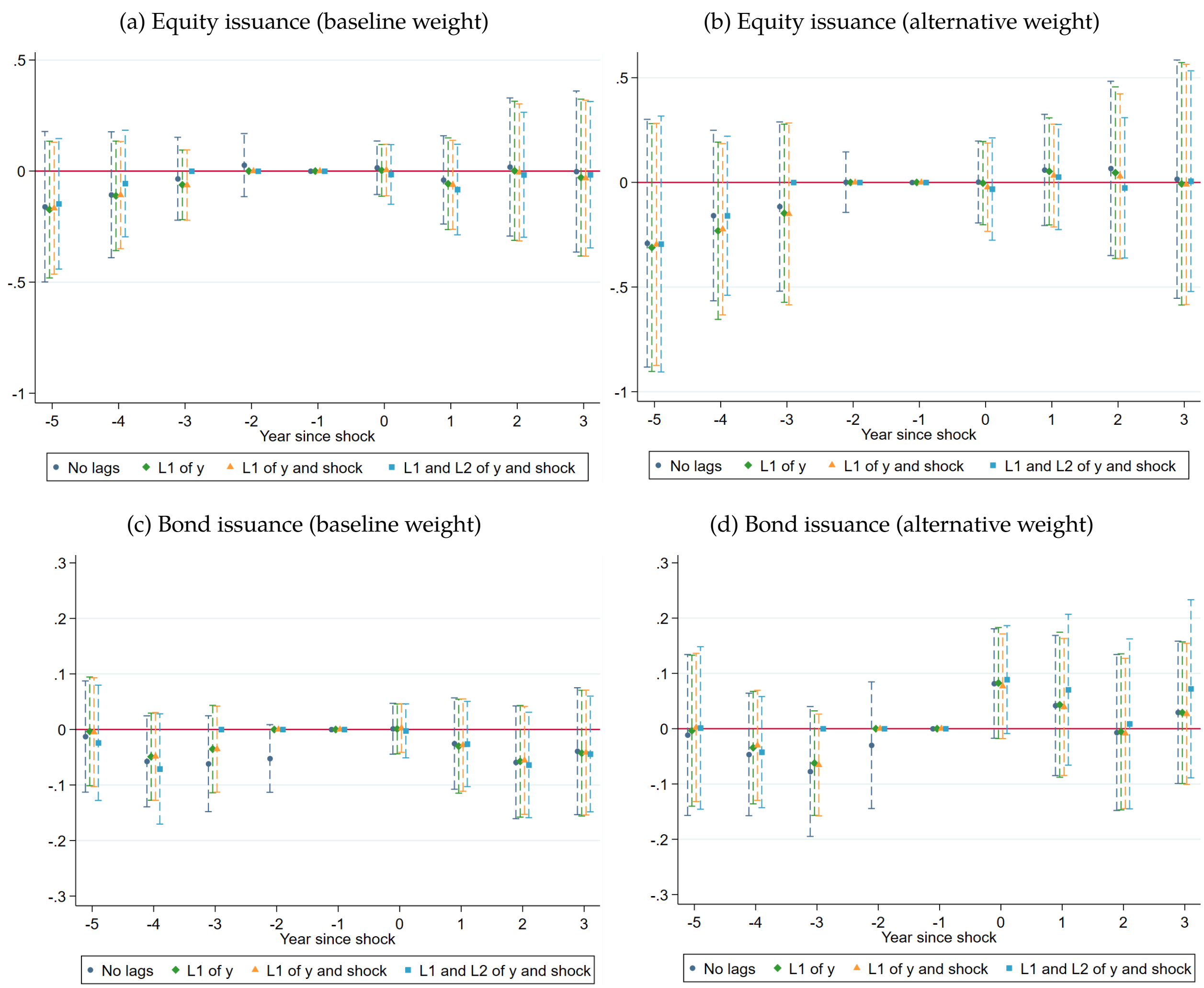
Other forms of financing. Figure C.4 presents the effect of firm exposure to the credit supply shock induced by crowding out on firm book equity and firm bond issuance. In panels (a) and (b), the dependent variable is the growth rate of book equity. In panels (c) and (d), the dependent variable is the growth rate of bonds. Less than 2% of firms in my sample have bonds. To avoid creating missing values, I assign a growth rate of 0 for firms with no bonds in t and $t-1$. In the left panel, I use the baseline weights (mid-point credit volume), and in the right panel, for each outcome variable, I weight regressions by the lagged value of the outcome. For bonds, to avoid losing most of the sample, I assign a weight of 1 to firms with $t-1$ bond outstanding equal to 0. I find no evidence of a large response of equity or bond issuance.[59] Interestingly, in panel (d), where the estimation puts virtually all the weight on firms that have outstanding bonds, I see a positive on-impact response of bond issuance significant at the 10% level for the specification "L1 and L2 of y and shock" (but I cannot identify significant effects at longer horizons).

Employment. Figure C.5 presents effects on firm employment outcomes. In panels (a) and (b), the dependent variable is the growth rate of the number of employees. In panels (c) and (d), the dependent variable is the growth rate of the total wage bill. While in some specifications we see a downward trend from $h = 0$ onwards, most coefficients are too small to be statistically significant. Out of 16×4 coefficients from $h = 0$ onwards, only one is statistically significant.

There are three possible explanations for this result. First, the magnitude of the adjustment may be too small to be detected as significantly different from 0. 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. Therefore, the effect on labor would come from capital-labor complementarities. In the model with CES demand and Cobb-Douglas production, the relationship between the effect on capital and the effect on labor is given by: $\frac{\beta^K}{\beta^L} = \frac{1+\alpha(\sigma-1)}{\alpha(\sigma-1)}$. I use $\alpha = 1/3$. For $\sigma \in [3, 6]$, this implies that $\frac{\beta^K}{\beta^L} \in [1.6, 2.5]$, so that we should observe $\beta^L \in [-0.18, -0.28]$. The $h = 3$ estimates of β^L fall in the upper part of this interval. Hence, the absence of statistically significant effects appears linked to insufficient power. Second, it could also be that the Cobb-Douglas assumption is not correct and that capital and labor have an elasticity of substitution larger than 1. Third, the French labor market is subject to numerous rigidities that may hinder the adjustment of labor.

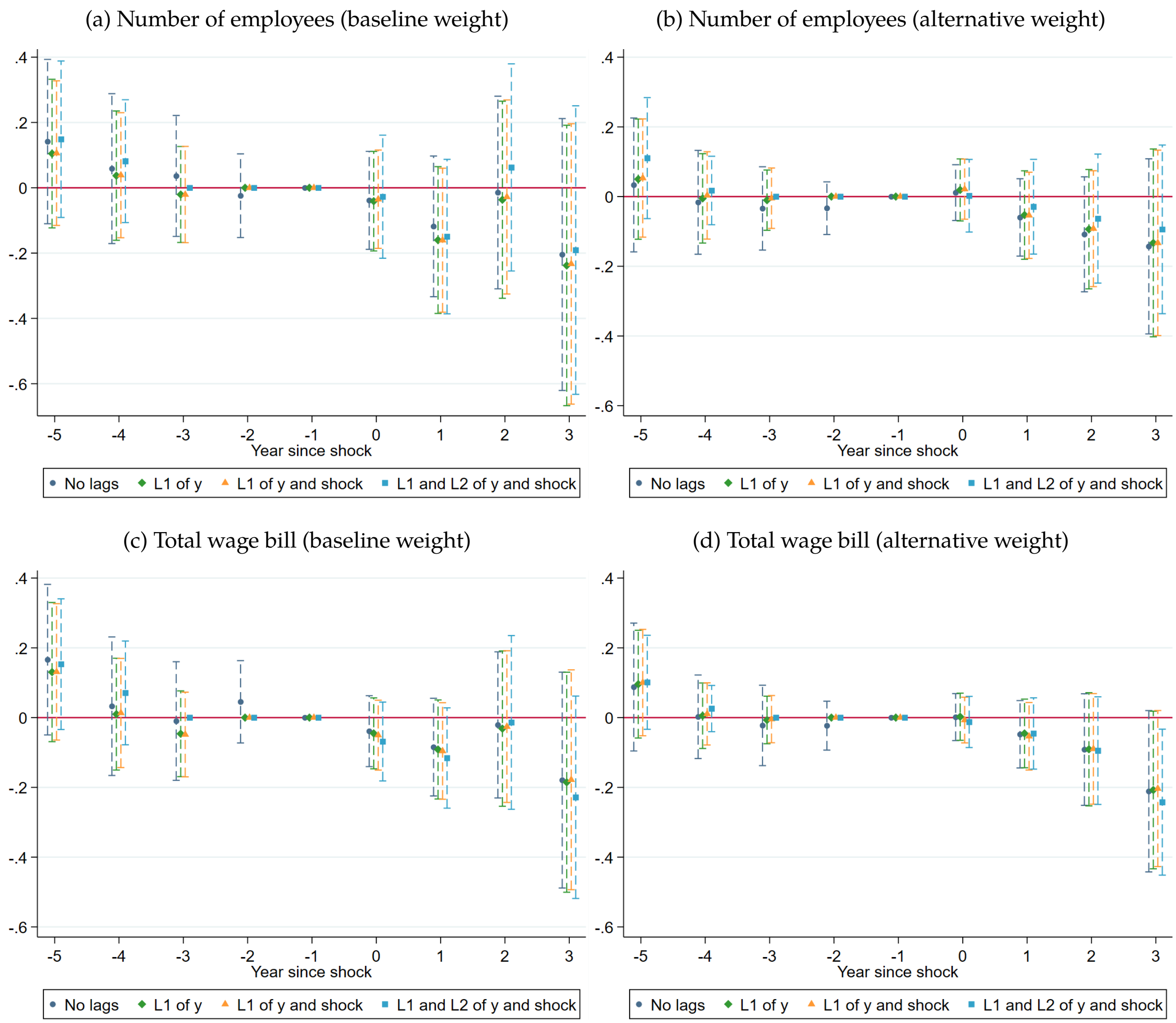
[59]This is consistent with the theoretical literature on the special role of banks in finance provision (e.g., Holmstrom and Tirole 1997). On the empirical side, this is in line with the literature documenting large real effects of credit supply

Figure C.4. Effect on other sources of financing



Note: This figure plots the estimated coefficients β_h resulting from estimating equation (5). The outcome is the h-horizon growth rate $(Y_{f,t+h} - Y_{f,t-1})/Y_{f,t-1}$ where $Y_{f,t}$ is firm-level book equity or outstanding bonds. "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 of the shock as controls. "L1 and L2 of y and shock" adds two lags of the outcome variable and of the shock as controls. Baseline weight is mid-point credit, alternative weight is the lagged dependent variable. When the outcome variable is bonds, I assign a growth rate of 0 for firms with no bonds in t and $t-1$, and in panel (d) I assign a weight of 1 for firms with lagged bonds equal to 0. All other elements of the specifications are as in Table 5. The dot is the point estimate and the bar is the 95% confidence interval.

FIGURE C.5. Effect on employment outcomes



Note: This figure plots the estimated coefficients β_h resulting from estimating equation (5). The outcome is the h-horizon growth rate $(Y_{f,t+h} - Y_{f,t-1})/Y_{f,t-1}$ where $Y_{f,t}$ is firm-level number of employees or firm-level wage bill. "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 of the shock as controls. "L1 and L2 of y and shock" adds two lags of the outcome variable and of the shock as controls. Baseline weight is mid-point credit, alternative weight is the lagged dependent variable. All other elements of the specifications are as in Table 5. The dot is the point estimate and the bar is the 95% confidence interval.

Output. Figure C.6 presents the effect of firm exposure to the credit supply shock induced by crowding out on firm output, defined as value added. In the baseline weight case, we see a downward trend from $h = 0$ onwards, with a statistically significant coefficient at $h = 1$ for three of the four specifications (p = 0.083). The pre-trends have some coefficients at $t = -1$ significant at the 10% level, but reassuringly, adding more controls for lags does not affect the dynamics of the effect after the shock. Weighting regressions by value added (which puts less weights on firms with value added close to 0, and potentially uninformative growth rates) allows to obtain much more precise estimates: the coefficients at $h = 1$, $h = 2$, $h = 3$ are all significant at conventional levels, with p-values equal to 0.099, 0.036, and 0.023 respectively for the "No lags" specification.

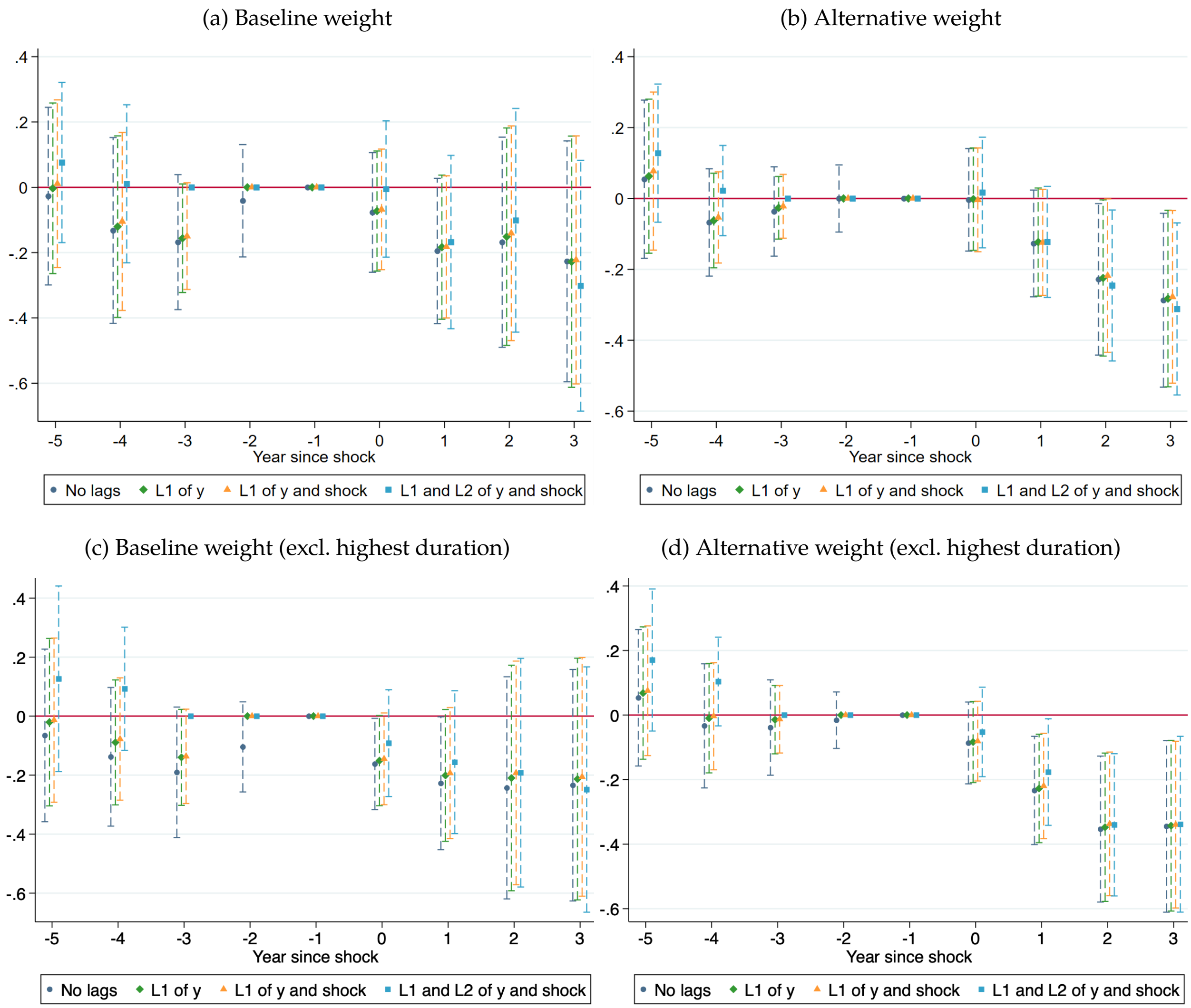
Does the magnitude of this effect make sense? Taking the average of all coefficients after $h = 0$ in the figure, I obtain $\beta^{PY} = -.231$. In the model with CES demand and Cobb-Douglas production, the relationship between the effect on capital and the effect on value added is given by: $\frac{\beta^K}{\beta^{PY}} = \frac{1+\alpha(\sigma-1)}{\alpha(\sigma-1)}$. For $\alpha = 1/3$ and $\sigma \in [3, 6]$, this implies that $\frac{\beta^K}{\beta^{PY}} \in [1.6, 2.5]$. Empirically, this ratio is equal to 1.97, right in the middle of this range. The results on output are thus quantitatively consistent with those on capital and a standard production function.

The timing is consistent with the idea that reduced investment only impairs output with a lag. The installation of new capital may require time-to-build. In addition, firms that do not invest may be able to maintain production using their old capital stock for a while (e.g., by increasing repairs) until we see the effects of underinvestment kick in. As a further check on the timing, I estimate the same specification excluding observations in the top quintile of capital duration (panels c and d). Firms with very long-lived capital are most likely to have long time-to-build or to be able to maintain production with their old capital stock, dampening the short-run effects on output. In line with intuition, I find more negative point estimates and smaller standard errors at $h = 0$ and $h = 1$.

These delayed effects suggest that the $h = 3$ coefficient may not capture the full extent of the long-run output effect. In this case, my estimates will tend to be conservative.

shocks, as well as with the literature documenting large equity issuance costs (e.g., Hennessy and Whited 2007).

Figure C.6. Effect on output value



Note: This figure plots the estimated coefficients β_h resulting from estimating equation (5). The outcome is the h-horizon growth rate $(Y_{f,t+h} - Y_{f,t-1})/Y_{f,t-1}$ where $Y_{f,t}$ is firm-level value added. "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 of the shock as controls. "L1 and L2 of y and shock" adds two lags of the outcome variable and of the shock as controls. Baseline weight is mid-point credit, alternative weight is the lagged dependent variable. Excl. highest duration excludes observations in the top quintile of capital duration (defined as the industry-level median of fixed assets/depreciation). Observations with negative $t-1$ value added are excluded. All other elements of the specifications are as in Table 5. The dot is the point estimate and the bar is the 95% confidence interval.

Appendix D. Model

D.1. Model

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 and a fixed amount of equity. Local governments borrow from banks. There is a continuum of banks of mass 1 indexed by $b \in [0,1]$; they are funded via deposits and lend to firms and local governments. Banking relationships enter the model through the assumption that firms and local governments are assigned to a given bank. Imperfect capital mobility across banks enters the model through the assumption that depositors do not arbitrage across banks. An interbank market can be accessed at a cost. All decisions are static.

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 fixed and 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:

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

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

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

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

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

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

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

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:

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

with $\epsilon^g \geq 0$. $\tilde{z}_{mb}^g$ 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 $\tilde{Z}^g = \int_0^1 \int_0^1 \tilde{z}_{mb}^g dmdb$.

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

$\epsilon^s \geq 0$.[60] 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.[61] 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 functioning of the interbank market, I assume that banks face a quadratic cost. The problem of the bank is:

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

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:

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

2. Bank's optimization: Taking $(\{r_b^c\}, \{r_b^g\}, \{r_b^s\}, i)$ as given, banks maximize profits;

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

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

5. 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 $\int_0^1 B_b db = 0$.

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

Solution. I solve the model by log-linearisation around the deterministic equilibrium (DE), characterized by $z_{fb} = 0$ for all f, b and $\tilde{z}_{mb}^g = 0$ for all m, b. I denote $\hat{x}$ the relative change of variable x

[60] This deposit supply function corresponds to the solution of a static optimization problem where depositors maximize the returns on deposits and have a convex cost of savings: $\max_{S_b}\ r_b^s S_b - \tilde{s} S_b^{1+\frac{1}{\epsilon^s}}$. In a dynamic consumption-savings problem, ϵ^s would reflect households' elasticity of intertemporal substitution.

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

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 $Z_b^g = \lambda \tilde{Z}_b^g$. 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:

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

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

The firm capital and corporate credit demand functions write:

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

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

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:

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

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 $\hat{Y}$, $\hat{w}$, $\hat{\imath}$, $\hat{K}$, $\hat{L}$, $\hat{C}^c$. The solution for $\hat{\imath}$ writes:

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

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

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

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

D.2. Aggregate and relative crowding out effect

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:

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

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

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

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

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 decompose the coefficient in front of Z^g into two terms. χ^C 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^C tends to 0 and there is no crowding out. χ^C does not depend on interbank market frictions. κ^C_{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^C and the general equilibrium feedback $\kappa^C_{\mathrm{GE}}\chi^C$ 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:

$$\hat{C}_{fb} = \upsilon^C z_{fb} + (\Upsilon^C - \upsilon^C)Z^c \; + \; \underbrace{\kappa^C_{\mathrm{GE}}\chi^C Z^g}_{\textit{GE feedback}} \; + \; \underbrace{\underbrace{\chi^C(1-\nu)Z^g}_{\textit{Spillover across banks}} \; + \; \underbrace{\chi^C \nu Z^g}_{\textit{Cross–sectional effect } \mathcal{L}^{Xsec}(C^c)}}_{\textit{Direct effect } \mathcal{L}^{direct}(C^c)} \tag{D.14}$$

where the additional parameters are $\upsilon^c = \frac{\sigma-1}{\ell}$ 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$\times$bank-level, the direct effect of crowding out $\chi^c Z^g$ is split into two terms: $\chi^c(1-\nu)Z^g$ and $\chi^c\nu Z^g_b$. $\nu \in [0,1]$ 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 cross-sectional effect of a bank-specific local government loan demand shock Z^g_b on bank-specific corporate credit supply is given by $\chi^c\nu$. 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^g_b corresponds to *BankExposure*. In terms of units, $Z^g_b = \lambda\tilde{Z}^g_b$ 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:

$$\Delta C_{fbt} = \upsilon^c z_{fbt} + (\Upsilon^c - \upsilon^c)Z^c_t + \chi^c(\kappa^c_{\text{GE}} + 1 - \nu)\mathit{BankExposure}_t + \chi^c\nu \mathit{BankExposure}_{bt} \tag{D.15}$$

The β^c coefficient that I estimate in the regression specification (2) corresponds to $\chi^c\nu$.

Missing intercept. Equation (D.14) clarifies that the cross-sectional coefficient $\chi^c\nu$ 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 $\kappa^c_{\text{GE}}\chi^c + \chi^c(1-\nu)$ multiplied by the aggregate shock. It can be decomposed into two channels: (i) a spillover effect due to capital mobility across banks $\chi^c(1-\nu)$, (ii) a general equilibrium feedback $\kappa^c_{\text{GE}}\chi^c$.

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

$\tilde{z}^g_m$ is 0, as implied by my cross-sectional coefficient. For each observation fb, the credit shortfall is given by $\mathcal{L}^{Xsec}(C_{fb}) = \chi^C \nu Z^g_b$.[62] Aggregating across firms, we obtain:

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

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

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

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

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

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

Other variables. The same logic applies to other firm-level variables. For a generic variable X, the aggregate crowding out effect can be decomposed into $\mathcal{L}(X) = \kappa^X_{\mathrm{GE}}\chi^X Z^g + \chi^X(1-\nu)Z^g + \chi^X \nu Z^g$, where $\chi^X \nu$ corresponds to the effect identified by the cross-sectional regression, $\chi^X(1-\nu)$ is the spillover term, and $\kappa^X_{\mathrm{GE}}\chi^X$ is the general equilibrium feedback.

Take the case of investment. Starting from (D.15), the firm-level equation for capital writes:

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

The cross-sectional coefficient I estimate in specification (5) corresponds to $\ell\chi^C\nu$. Denoting $\chi^K = \ell\chi^C$ and $\kappa^K_{\mathrm{GE}} = \kappa^C_{\mathrm{GE}}$, I obtain the decomposition above. I report below the values of χ^X and κ^X_{GE} for all the relevant firm-level variables:

$$\chi^C = \frac{-\epsilon^c}{\epsilon^s + \lambda\epsilon^g + (1-\lambda)\epsilon^c} \qquad \kappa^C_{\mathrm{GE}} = \frac{\frac{1}{\ell}\frac{1+\alpha\psi}{1-\alpha}}{\frac{1}{\ell}(1+\alpha(\sigma-1))}\frac{\epsilon^s + \lambda\epsilon^g + (1-\lambda)\frac{1}{\ell}(1+\alpha(\sigma-1))}{\epsilon^s + \lambda\epsilon^g + (1-\lambda)\frac{1}{\ell}\frac{1+\psi\alpha}{1-\alpha}} - 1 \tag{D.20}$$

$$\chi^K = \frac{-\ell\epsilon^c}{\epsilon^s + \lambda\epsilon^g + (1-\lambda)\epsilon^c} \qquad \kappa^K_{\mathrm{GE}} = \frac{\frac{1}{\ell}\frac{1+\alpha\psi}{1-\alpha}}{\frac{1}{\ell}(1+\alpha(\sigma-1))}\frac{\epsilon^s + \lambda\epsilon^g + (1-\lambda)\frac{1}{\ell}(1+\alpha(\sigma-1))}{\epsilon^s + \lambda\epsilon^g + (1-\lambda)\frac{1}{\ell}\frac{1+\psi\alpha}{1-\alpha}} - 1 \tag{D.21}$$

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

$$\text{(D.22)} \qquad \chi^L = \frac{-\alpha(\sigma-1)}{\epsilon^s + \lambda\epsilon^g + (1-\lambda)\epsilon^c} \qquad \kappa^L_{\mathrm{GE}} = \frac{\psi}{(\sigma-1)(1-\alpha)} \frac{\epsilon^s + \lambda\epsilon^g + \frac{(1-\lambda)}{\ell}(1+\alpha(\sigma-1))}{\epsilon^s + \lambda\epsilon^g + \frac{1-\lambda}{\ell}\frac{1+\alpha\psi}{1-\alpha}} - 1$$

$$\text{(D.23)} \qquad \chi^Y = \frac{-\alpha\sigma}{\epsilon^s + \lambda\epsilon^g + (1-\lambda)\epsilon^c} \qquad \kappa^Y_{\mathrm{GE}} = \frac{1+\psi}{\sigma(1-\alpha)} \frac{\epsilon^s + \lambda\epsilon^g + \frac{(1-\lambda)}{\ell}(1+\alpha(\sigma-1))}{\epsilon^s + \lambda\epsilon^g + \frac{1-\lambda}{\ell}\frac{1+\alpha\psi}{1-\alpha}} - 1$$

An important result is that the parameter ν that determines the share of the direct effect that appears in the cross-sectional coefficient is the same for all variables.

D.3. Quantification

For each variable X and each period t, I can estimate the three versions of the shortfall: $\mathcal{L}^{Xsec}(X_t)$, $\mathcal{L}^{direct}(X_t)$, $\mathcal{L}(X_t)$. It is immediate that $\mathcal{L}^{direct}(X_t) = \frac{1}{\nu}\mathcal{L}^{Xsec}(X_t)$ and $\mathcal{L}(X_t) = (1+\kappa^X_{\mathrm{GE}})\mathcal{L}^{direct}(X_t)$. I thus quantify the aggregate crowding out effect by combining: (i) the cross-sectional effect identified from my empirical analysis $\mathcal{L}^{Xsec}(X_t)$; (ii) an estimate of ν; (iii) an estimate of κ^X_{GE}.

These shortfalls are expressed in % difference compared to the level of the variable in the no crowding out counterfactual. For each version, the % shortfall can be translated into a multiplier using:

$$\text{(D.24)} \qquad m^X_t = \frac{X_t - X_t(\mathbf{0})}{C^g_t - C^g_t(\mathbf{0})} = \frac{\mathcal{L}(X_t)X_t(\mathbf{0})}{C^g_t - C^g_t(\mathbf{0})}$$

$C^g_t - C^g_t(\mathbf{0})$ is the difference between actual local government debt and the zero-local government debt growth counterfactual so that $C^g_t - C^g_t(\mathbf{0}) = C^g_t - C^g_{t-1}$. $X_t(\mathbf{0})$ can be estimated as $X_t(\mathbf{0}) = \frac{X_t}{1+\mathcal{L}(X_t)}$.[63]

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:

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

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:

$$\text{(D.25)} \qquad \mathcal{L}^{Xsec}(C^c_t) = \hat{\beta}^C \sum_f \frac{C_{ft}(\mathbf{0})}{C^c_t(\mathbf{0})} \mathit{FirmExposure}_{ft}$$

[63]Results are highly similar using $X_t(\mathbf{0}) = X_{t-1}$.

and proceed similarly for other firm-variables. For credit, I use $\hat{\beta}^C$ estimated from the firm-level specification (5). 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 appropriate coefficient for the aggregation exercise is the coefficient of the firm-level regression. The specification with firm fixed effects yields a point estimate higher than all the other specifications of Table 5 and Figure 5, which are very consistent among themselves. To avoid inflating the aggregate effect, I thus use the coefficient without firm fixed effects. To be consistent with weighting by the initial level, I use the coefficient of the specification with the standard growth rate as the outcome. For $\hat{\beta}^K$, the coefficient remains highly similar in columns (4) to (6) of Table 5 and for the various lag specifications displayed in Figure 5. I thus use the coefficient of the baseline specification. For results on firm output, I acknowledge that there is a lag before effects materialize and I take the local projection coefficient at horizon $h = 1$. Weighting by DE credit C_f^* corresponds to weighting by counterfactual credit $C_{ft}(\mathbf{0})$, which can be estimated from regression results (similarly for other variables). This yields the estimates presented in the first two column of Table D.1.

Table D.1. Aggregate effects

	Cross-sectional effect $\mathcal{L}^{Xsec}(.)$		Direct effect $\mathcal{L}^{direct}(.)$		Average κ_{GE}	Total effect $\mathcal{L}(.)$	
	$\mathcal{L}^{Xsec}(.)$ (%)	Multiplier	$\mathcal{L}^{direct}(.)$ (%)	Multiplier		$\mathcal{L}(.)$ (%)	Multiplier
Corporate credit	–0.85%	–0.54	–1.00%	–0.64	–0.04	–0.96%	–0.61
	(0.16%)	(0.10)	(0.20%)	(0.13)	(0.08)	(0.20%)	(0.13)
Capital	–0.24%	–0.32	–0.28%	–0.38	–0.04	–0.27%	–0.37
	(0.06%)	(0.08)	(0.07%)	(0.09)	(0.08)	(0.07%)	(0.09)
Output (baseline)	–0.07%	–0.17	–0.08%	–0.21	0.33	–0.11%	–0.27
	(0.02%)	(0.04)	(0.02%)	(0.05)	(0.42)	(0.04%)	(0.11)
Output (alt.)	–0.14%	–0.38	–0.16%	–0.45	–0.35	–0.11%	–0.29
	(0.08%)	(0.22)	(0.09%)	(0.26)	(0.23)	(0.07%)	(0.20)

Note: This table reports the effects of crowding out on aggregate variables. The reported quantities are % shortfalls and multipliers (as defined in equation D.24). The first two columns report the aggregation implied by the cross-sectional coefficients. The next two columns report aggregate effects accounting for the spillover across banks. Column 5 reports the average general equilibrium feedback coefficient across the calibration scenario in Table D.3. The last two columns report aggregate effects accounting for the (average) general equilibrium feedback term from column 5. Output (baseline) refers to the baseline estimation of the output shortfall based on the aggregate capital shortfall (in this line, column 5 reports $\tilde{\kappa}_{GE}^{Y}$). Output (alt) refers to the alternative estimation based on the cross-sectional effect on output (in this line, column 5 reports κ_{GE}^{Y}). All figures are averages across years. Standard errors are reported in parenthesis (construction detailed 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:

$$\frac{B_b}{S^*} = (1-\nu)\left[\lambda(\tilde{Z}^g_b - \tilde{Z}^g) + (1-\lambda)(\tilde{Z}^c_b - \tilde{Z}^c) - \frac{1}{S^*}\xi_b\right] \tag{D.26}$$

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 $\tilde{Z}^g_b$ may be correlated with corporate credit demand shocks $\tilde{Z}^c_b$ 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 implies that the orthogonality condition regarding bank-level corporate credit supply shocks is more stringent as it needs 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: $\alpha^D_{bt} = \sum_i \frac{C_{ibt-1}}{C_{bt-1}}\hat{\alpha}^D_{it}$. to proxy 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}. $\frac{B_b}{S^*}$ corresponds to the change in interbank borrowing normalized by the banks' lagged assets, denoted ΔB_{bt}. I thus estimate

$$\Delta B_{bt} = \delta_t + \beta\alpha^D_{bt} + \varepsilon_{bt} \tag{D.27}$$

I can control for the estimated α^S_{bt}, other bank variables, and bank fixed effects.
Results. The results are presented in Table D.2. 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 coefficient in column (4), which is equal to the average coefficient across the five specifications (0.15). I recover $\mathcal{L}^{direct}(.)$ by dividing their cross-sectional counterparts $\mathcal{L}^{Xsec}(.)$ by $\hat{\nu}$. This yields the estimates in columns (3)-(4) of Table D.1.

Calibration of the general equilibrium feedback. For corporate credit, equation (D.21) shows that κ^C_{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^C_{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 reallo-

Table D.2. 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	3,896	3,434	3,423	3,401	3,363
R-squared	0.064	0.11	0.11	0.13	0.21

Note: This table reports the results of estimating equation (D.27). 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.

cation 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 $\kappa^C_{GE} = 0$. These general equilibrium forces similarly apply to capital ($\kappa^K_{GE} = \kappa^C_{GE}$).

Calibrating κ^C_{GE} only requires to calibrate ψ, α, σ. χ^C 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.[64] This is a desirable feature since ψ, α and σ are common parameters for which the literature provides estimates.

Table D.3 shows the value of κ^C_{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^C_{GE} varies from -16.5% to +8.0%. This suggests that the general equilibrium feedback on corporate credit and capital is modest in magnitude. While the general equilibrium feedback does vary depending on the parameter choices, considering only the direct effect χ^C and χ^K 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 $\mathcal{L}^{direct}(C^c)$ and $\mathcal{L}^{direct}(K)$. These baseline estimates are shaded in blue in Table D.1.

[64]From our estimates of β^C and ν, we have estimated $\chi^C = \frac{-\frac{1}{\ell}(1+\alpha(\sigma-1))}{\epsilon^s+\lambda\epsilon^g+(1-\lambda)\frac{1}{\ell}(1+\alpha(\sigma-1))}$. With estimates for χ^C and ℓ, and for any value of α and σ for which we wish to calibrate κ^C_{GE}, we can recover an estimate of $\epsilon^s + \lambda\epsilon^g$.

Table D.3. 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^C_{GE}	-2.9%	-11.4%	-16.5%	1.5%	-6.3%	-11.0%	8.0%	1.6%	-2.4%
κ^K_{GE}	-2.9%	-11.4%	-16.5%	1.5%	-6.3%	-11.0%	8.0%	1.6%	-2.4%
κ^L_{GE}	-40.0%	-77.8%	-100.0%	-28.9%	-73.4%	-100.0%	8.0%	-58.9%	-100.0%
κ^Y_{GE}	-23.8%	-48.9%	-63.6%	-14.7%	-42.1%	-58.5%	8.0%	-25.3%	-45.8%
$\tilde{\kappa}^Y_{\mathrm{GE}}$	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 parameters in (D.21)-(D.23) for values of the elasticity of substitution across goods σ and the labor supply elasticity ψ reported in the first two lines. 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.

Output loss. To go from the shortfall in corporate credit and capital to the shortfall in aggregate output, I proceed as follows.

Using only the result on capital (baseline). The output loss is equal to $\mathcal{L}(Y) = \alpha\mathcal{L}(K) + (1-\alpha)\mathcal{L}(L)$. Estimating $\mathcal{L}(L)$ is subject to two caveats. First, the cross-sectional effects on firm employment are imprecisely estimated. Most importantly, κ^L_{GE} tends to be negative and large in magnitude. With no frictions on labor mobility across firms, workers at exposed firms reallocate to non-exposed firms. Therefore, the cross-sectional effects carry little information on $\mathcal{L}(L)$, which mostly depends on the labor supply elasticity.[65] 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. First, I present estimates that assume $\mathcal{L}(L) = 0$ and $\mathcal{L}(Y) = \alpha\mathcal{L}(K)$. This also corresponds to the case $\psi = 0$. Second, in the case where $\psi > 0$, I assume that the aggregate labor shortfall is as predicted by the model. In this case, we can write $\mathcal{L}(Y) = (1+\tilde{\kappa}^Y_{\mathrm{GE}})\alpha\mathcal{L}^{direct}(K)$ where $1+\tilde{\kappa}^Y_{\mathrm{GE}} = (1+\kappa^K_{\mathrm{GE}})\frac{1+\psi}{1+\alpha\psi}$. The last line of Table D.3 shows if labor is allowed to respond, we instead observe a large further amplification ($\tilde{\kappa}^Y_{\mathrm{GE}} >> 0$).

In my baseline quantification, I remain as close as possible to estimated moments and consider that $\mathcal{L}(L) = 0$, or equivalently $\tilde{\kappa}^Y_{\mathrm{GE}} = 0$. Starting from by baseline estimation of the capital loss $\mathcal{L}^{direct}(K)$, I obtain $\mathcal{L}^{direct}(Y) = \alpha\mathcal{L}^{direct}(K)$.[66] This produces the estimate shaded in blue in Table D.1. The assumption $\mathcal{L}(L) = 0$ implies that this estimate is likely to be conservative.

Using the result on output (alternative). I provide an alternative quantification of the aggregate output loss using as a starting point the cross-sectional effect on firm output documented in Figure C.6.

[65] One could introduce labor market segmentation, similar to the assumption of banking market segmentation. However, there is no empirical moment that would inform the degree of this friction. I therefore choose the free labor mobility assumption, which gives me a more conservative estimate on the strength of general equilibrium effects.

[66] In practice, I use industry-specific α coefficients obtained from estimating Cobb-Douglas production functions at the 2-digit level using the cost shares method (i.e., $1-\alpha$ is the ratio of labor compensation over value added).

The empirical results are for output value and do not coincide with the effect on physical output because of the counteracting movement in prices. The first step is thus to obtain an estimate of the cross-sectional output effect $\beta^{Y} = \nu\chi^{Y}$ by adjusting the observed effect on value added $\beta^{PY} = \nu\chi^{PY}$. From the model, this adjustment writes $\nu\chi^{Y} = \frac{\sigma}{\sigma-1}\nu\chi^{PY}$. With an estimate of $\nu\chi^{Y}$ in hand, I proceed as before: estimate the cross-sectional shortfall $\mathcal{L}^{Xsec}(Y)$, account for the interbank spillover to estimate the direct effect, and calibrate the general equilibrium feedback κ^{Y}_{GE}. In the case of output, the general equilibrium feedback κ^{Y}_{GE} tends to be negative and cannot be ignored at the risk of over-estimating the aggregate output loss. This is because κ^{Y}_{GE} reflects both the general equilibrium effect on capital (which is small) and that on labor (which can be very negative). For this alternative quantification, I account for κ^{Y}_{GE} and use the mean value across calibration (–0.35). This yields the estimate in the fourth line of Table D.1.

Standard errors on aggregate effects. I quantify the uncertainty around these estimates as follows. For the cross-sectional effect $\mathcal{L}^{Xsec}(.)$, estimated using formula (D.25), I can rely on the standard error of the firm-level crowding out coefficients for each variable (multiplied by the integral of *FirmExposure* across firms). Since the baseline estimation of the output loss uses the capital coefficient, the standard error for $\mathcal{L}^{Xsec}(Y_t)$ is based on that of the capital coefficient. I then go from a loss expressed in percentage points to a multiplier using equation (D.24).

The direct effect $\mathcal{L}^{direct}(.)$ depends on the cross-sectional crowding out coefficient divided by an estimate of ν. To construct a standard error for the ratio of regression coefficients, I rely on the Delta method. Let us assume that the parameters $(\hat{\beta}, \hat{\nu})$ are bivariate normally distributed with mean (β, ν) and variance-covariance matrix $\begin{pmatrix} \sigma^2_\beta & \sigma_{\beta\nu} \\ \sigma_{\beta\nu} & \sigma^2_\nu \end{pmatrix}$. Let $\hat{\chi} = \frac{\hat{\beta}}{\hat{\nu}}$. Then, the standard error for $\hat{\chi}$ is given by: $\sigma^2_\chi = \frac{1}{\hat{\nu}^2}(\sigma^2_\beta + \hat{\chi}^2\sigma^2_\nu - 2\hat{\chi}\sigma_{\beta\nu})$. In my context, there is no well-defined covariance between the firm-level crowding out effect $\hat{\beta}$ estimated in the firm×year panel, and $\hat{\nu}$ estimated using the bank×year data on interbank lending. Hence, I assume $\sigma_{\beta\nu} = 0$. Finally, obtaining a standard error for the total effect requires accounting for the uncertainty related to the calibration of the κ_{GE} terms. I do so by accounting for the standard deviation of κ_{GE} across calibration scenario.[67] Using again the Delta method, $\sigma^2_{(1+\kappa)\chi} = \hat{\chi}^2\sigma^2_\kappa + (1+\hat{\kappa})^2\sigma^2_\chi + 2\hat{\chi}(1+\hat{\kappa})\sigma_{\kappa\chi}$. I again assume $\sigma_{\kappa\chi} = 0$.

This yields the standard errors reported in parenthesis in Table D.1. The results show that the aggregate effects on corporate credit and capital are estimated with good precision. This is true even for the total effect, since $\kappa^{C}_{\mathrm{GE}} = \kappa^{K}_{\mathrm{GE}}$ is relatively stable across calibrations. My baseline estimate of the output loss is $\mathcal{L}^{direct}(Y_t) = \alpha\mathcal{L}^{direct}(K_t)$. Because the effect on capital is estimated with good

[67] The level of obtained standard errors obviously reflects the range of parameters considered in my calibration scenario. The relative size of standard errors across variables is meaningful.

precision, $\mathcal{L}^{direct}(Y_t)$ is also estimated with good precision. However, the standard deviation of $\tilde{\kappa}^{Y}_{\text{GE}}$ is large, reflecting the potential large amplification due to the labor supply response, which feeds into a large standard error for the total effect $\mathcal{L}(Y_t)$. This supports the choice of using the more conservative estimate $\mathcal{L}^{direct}(Y_t)$ as baseline.

Robustness checks. The quantification depends on the estimated relative crowding out effect $\hat{\beta}^C$ 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 $\hat{\beta}^C$ 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.2. These figures show that my baseline quantifications fall well in the middle of the estimated ranges.

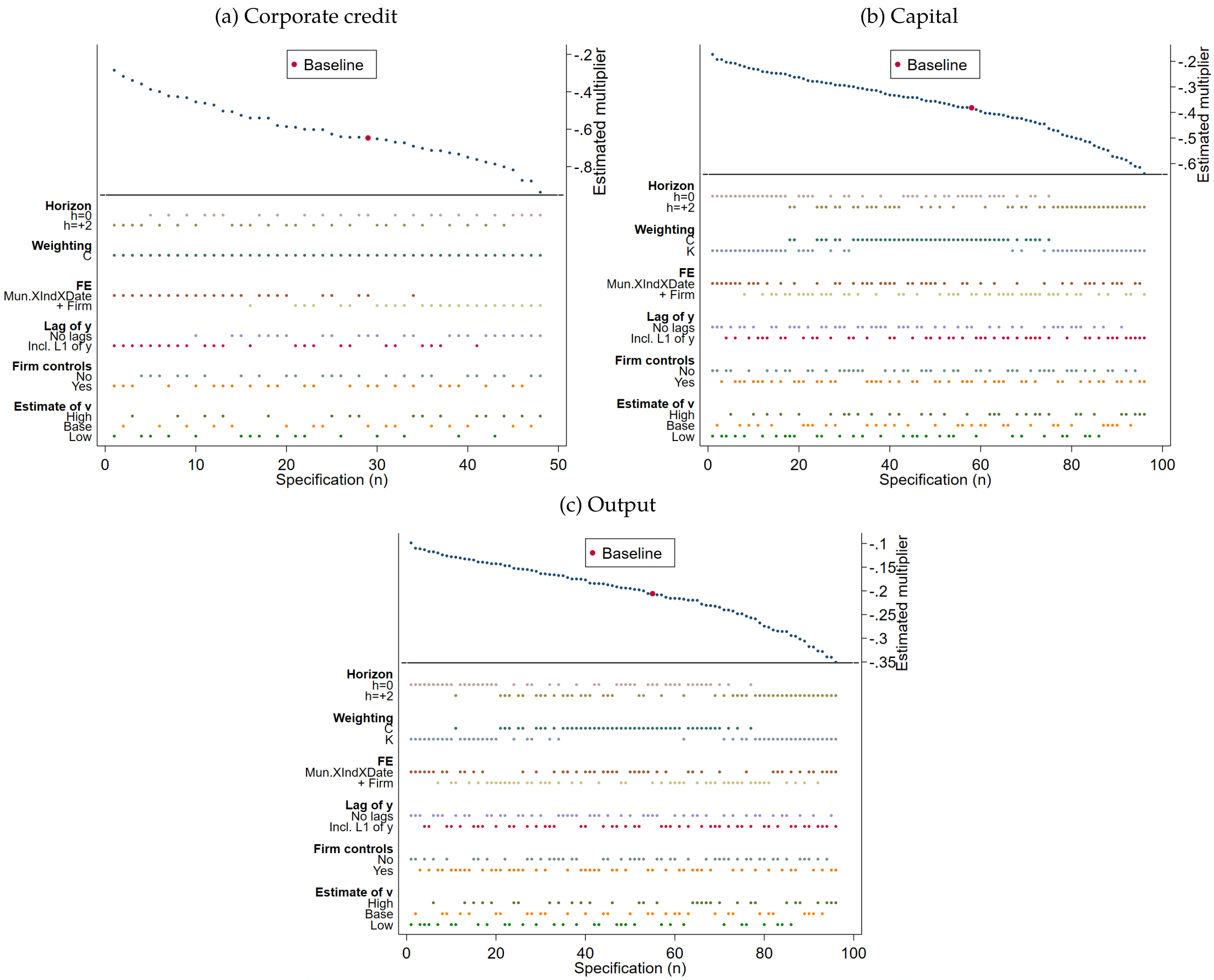
D.4. Extensions

This section performs the aggregation exercise through the lens of alternative models. Appendix D.4.1, D.4.2, and D.4.3 present versions of the model with different assumptions on the firms' financing and investment decision: I consider firms borrowing from multiple banks and substituting across banks, firms substituting between debt and equity, and firms' dynamic financing and investment choices. In Appendix D.4.4, I introduce the possibility that depositors reallocate their savings across banks. Finally, Appendices D.4.5 and D.4.6 modify the banks' problem by introducing a cost of bank leverage and lending to households, respectively.

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

This section presents a version of the model with two 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 $C^c_b + C^g_b = S_b + B_b + \xi_b$. Second, I introduce multibank firms. I assume that each firm borrows from a set of banks denoted $\mathcal{B}_f$ of mass μ_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 $[0, 1]$. The rest of the model is as in the baseline.

FIGURE D.1. Aggregate effects: Specification curves



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.

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:

(D.28) $$\hat{C}_{fb} = \upsilon^c z_f + (\Upsilon^c - \upsilon^c)Z^c + \chi^c(\kappa^c_{\text{GE}} + 1 - \nu)Z^g + \chi^c \nu Z^g_b + \iota^c \nu \xi_b$$

$\chi^C, \nu, \kappa^C_{GE}$ are as before. The effect of the bank-specific supply shock is $\iota^C = -\frac{\chi^C}{S^*}$. Assimilating log-deviations to growth rates and the demand shock Z^g_b 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^g_b may be correlated with firm-level corporate credit demand shocks z_f or with other bank-level corporate credit supply shocks ξ_b. The equation for net interbank borrowing is unchanged. Conditional on obtaining unbiased estimates of the relevant parameters, the mapping from reduced form to aggregate effects remains identical in this extended model.

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

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

The first-order condition writes:

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

Equation (D.10) now corresponds to the demand for firm-level credit C_f. Let $Z^g_f = \frac{1}{\mu_f}\int_{b\in\mathcal{B}_f} Z^g_b db$. Z^g_f is the average local government debt demand shocks for the set of banks f borrows from. Similarly, define $\xi_f = \frac{1}{\mu_f}\int_{b\in\mathcal{B}_f} \xi_b db$. Solving the model with these modified assumptions yields:

$$\hat{C}_{fb} = \upsilon^c z_f + (\Upsilon^c - \upsilon^c)Z^c + \chi^c(\kappa^C_{GE} + 1 - \nu)Z^g + (\chi^c\nu - \tilde{\chi}^c\tilde{\nu})Z^g_f + \tilde{\chi}^c\tilde{\nu}Z^g_b + (\iota^c\nu - \tilde{\iota}^c\tilde{\nu})\xi_f + \tilde{\iota}^c\tilde{\nu}\xi_b \tag{D.29}$$

$$\hat{C}_f = \upsilon^c z_f + (\Upsilon^c - \upsilon^c)Z^c + \chi^c(\kappa^C_{GE} + 1 - \nu)Z^g + \chi^c\nu Z^g_f + \iota^c\nu\xi_f \tag{D.30}$$

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

When firms can substitute across banks, the within-firm specification with firm fixed effects provides an estimate of $\tilde{\chi}^c\tilde{\nu}$, the coefficient of the bank-specific term Z^g_b in equation (D.29), where Z^g_b is the model equivalent of $BankExposure_{bt}$. By contrast, the firm-level specification provides an estimate of $\chi^c\nu$, the coefficient of the firm-specific shock Z^g_f in equation (D.30), where Z^g_f is the model equivalent of $FirmExposure_{ft}$.

If $\theta > \epsilon^c$ (loans from different banks are highly substitutable), then $\tilde{\chi}^C < \chi^C \leq 0$. That is, the estimate in the within-firm specification $\tilde{\chi}^C$ overestimates the firm-level effect χ^C. This is an instance of SUTVA violation: a negative shock to bank b has a causal (positive) effect on the firm's borrowing from other banks via substitution effects, as captured by the term $(\chi^C\nu - \tilde{\chi}^C\tilde{\nu})Z_f^g$ in equation (D.29).

In the firm-level specification (D.30), these substitution effects cancel out, and we recover χ^C. Intuitively, if substitution effects are strong, this will be picked up by a smaller coefficient in the firm-level regression.

This distinction highlights that the right coefficient to use in the aggregation exercise is the firm-level coefficient $\chi^C\nu$, since the aggregate effect depends on χ^C (as opposed to $\tilde{\chi}^C$). All my estimates of aggregate effects are based on results from the firm-level specification (5).

How important is this distinction quantitatively? This depends on the difference between the elasticity of bank-specific credit demand θ and the firm-level elasticity ϵ^c. If $\theta = \epsilon^c$, then $\chi^C\nu = \tilde{\chi}^C\tilde{\nu}$ and the substitution term in equation (D.29) disappears, so that the model becomes equivalent to the model with independent demand. Empirically, I find that the coefficient of the firm-level specification (5) is approximately equal to the coefficient of the firm×bank-level specification (2) (the results on the same sample are in Table C.9). This suggests that $\tilde{\chi}^C\tilde{\nu}$ is approximately equal to $\chi^C\nu$, or equivalently, that the elasticity of bank-specific credit demand θ is approximately equal to the firm-level elasticity ϵ^c. Therefore, the model with independent demand, which implicitly assumes $\theta = \epsilon^c$, provides a good approximation of the data.

Robustness: independent estimation of the elasticity of substitution across banks. To validate the result that the elasticity of bank-specific credit demand θ is approximately equal to the firm-level elasticity ϵ^c, I propose an independent estimation of the elasticity of substitution across banks. In the model where firms substitute across banks with a CES demand system, firm×bank demand writes:

$$\hat{C}_{fb} = z_f - \theta\hat{r}_b^c + (\theta - \epsilon^c)\hat{r}_f^c \quad \text{with} \quad \hat{r}_f^c = \sum_b \omega_{fb}\hat{r}_b^c \tag{D.31}$$

Taking this equation to the data provides a direct way to estimate the substitution term $\theta - \epsilon^c$. Obtaining an unbiased estimate of $\theta - \epsilon^c$ requires a bank-specific supply shock shifting $\hat{r}_b^c$ such that the firm-level average of this shock $\hat{r}_f^c$ is orthogonal to the firm-level demand shock z_f. The supply shock caused by bank exposure to local government debt demand does not satisfy this condition.

Here, I exploit a distinct supply shock that plausibly satisfies this condition and provide a direct estimate of $\theta - \epsilon^c$.

During the Great Financial crisis, several foreign banks historically implanted in France closed or significantly shrunk their French operations. Prominent cases include Commerzbank and WestLB. These banks suffered large losses from exposure to U.S. mortgage-backed securities. These losses forced these banks to drastically shrink their balance sheets and exit of their non-core markets. Because the cause of these banks' exit decisions lay outside of the French corporate loan sector, we can treat these events and the resulting negative supply shocks as plausibly orthogonal to the characteristics of each banks' precrisis borrowers. This design is similar to the seminal work of Peek and Rosengren (1997, 2000).

In the data, I identify four such foreign bank distress events over the Great Financial crisis period, defined as a year-on-year drop in credit volume larger than 60% (one in 2008, one in 2009, and two in 2010).[68] For each event, I define $Treated_b$ as a dummy equal to 1 for the distressed bank and $Share_f$ as the firm-level average of $Treated_b$ weighted by the share of each bank in the firms' total credit. To account for the fact that there may be systematic differences between firms borrowing from foreign banks and other firms, I use a difference-in-differences design. The identifying assumption is that firms borrowing from foreign banks do not experience differential demand dynamics after the shock. For a single event, the specification is:

$$\Delta C_{fbt} = \alpha_t + \beta_0 Treated_b + \beta_1 Treated_b \times Post_t + \gamma_0 Share_f + \gamma_1 Share_f \times Post_t + \varepsilon_{fbt} \tag{D.32}$$

where $Post_t$ is a dummy equal to 0 in the year before the exit and 1 the year of the exit. I estimate the effects using all four events using a stacked event-study design.

The results are presented in Table D.4. The first coefficient shows that the direct effect of the shock is large and statistically significant. This confirms that these exiting foreign banks reduce credit more than the average bank. The second coefficient shows that the substitution effect is not statistically different from 0 in all specifications. From this analysis, $\theta - \epsilon^c \approx 0$ seems to provide a good approximation of the data.

By providing an independent estimation of the extent of substitution, these results support my finding that the firm-level effect of crowding out is approximately equal to the firm$\times$bank-level effect. This limited ability to substitute across banks is in line with other reduced-form studies of corporate credit supply shocks (see, e.g., Khwaja and Mian 2008; Chodorow-Reich 2014; Huber

[68]Banks are anonymized in the credit registry, hence I cannot report the identity of exiting banks.

Table D.4. Elasticity of substitution across banks

	Credit growth				
	(1)	(2)	(3)	(4)	(5)
$Post_t \times Treated_b$	-1.470***	-1.245***	-1.266***	-1.263***	-1.147***
	(0.314)	(0.271)	(0.278)	(0.255)	(0.168)
$Post_t \times ShareTreated_f$	0.445	0.002	0.072	0.121	0.194
	(0.535)	(0.273)	(0.287)	(0.274)	(0.186)
Incl. $Treated_b$, $ShareTreated_f$	✓	✓	✓	✓	✓
Add. controls	–	–	✓	✓	✓
Time×Stack FE	✓	✓	✓	✓	✓
Industry×Time×Stack FE	–	–	–	✓	✓
Bank×Stack FE	–	✓	✓	✓	✓
Bank×Firm×Stack FE	–	–	–	–	✓
Observations	2,787,484	2,787,442	2,698,931	2,698,916	2,258,734
R-squared	0.0087	0.095	0.11	0.12	0.59

Note: This table presents additional evidence on firms' elasticity of substitution across banks. I use a stacked event-study design where the estimating equation is (D.32). Any control and fixed effect is estimated separately for each event stack. $Treated_b$ is a dummy equal to 1 for the distressed bank (defined in the text). $Share_f$ as the firm-level average of $Treated_b$ weighted by the share of each bank in the firms' total credit. $Post_t$ is a dummy equal to 0 in the year before the exit and 1 the year of the exit. Add. controls refers to lagged firm size (total credit, in logs). Regressions are weighted by firm×bank-level mid-point credit (top 0.5% winsorized). Standard errors are clustered at the bank-event stack level. ***, ** and * indicate significance at the 1%, 5% and 10% levels, respectively.

2018).

D.4.2. Adding substitution between corporate credit and other sources of firm financing

My baseline model assumes that firm equity is fixed, so that bank credit is the marginal source of financing. In this section, I show that relaxing this restriction does not affect my key conclusions.

Model. I consider a model where firms can substitute between bank credit and other forms of financing (which I group under the term equity) to finance their capital stock. As in Whited and Zhao (2021), I assume that debt and equity are imperfect substitutes. Let:

$$K_{fb} = \left[\ell C_{fb}^{\frac{\theta-1}{\theta}} + (1-\ell)E_{fb}^{\frac{\theta-1}{\theta}}\right]^{\frac{\theta}{\theta-1}}$$

where θ is the elasticity of substitution between bank debt and equity. I assume that equity is supplied by equity-holders who can freely reallocate equity across firms. The aggregate equity supply function writes $E = \bar{e}(r^E)^{\epsilon^E}$ where r^E denotes the rate of return on equity. The rest of the model is as in the baseline. The definition of equilibrium is as before, with the additional condition that the equity market must clear.

In this modified model, capital, credit and equity demand are given by:

$$\hat{K}_{fb} = (\sigma-1)z_{fb} + \hat{Y} - (1-\alpha)(\sigma-1)\hat{w} - (1+\alpha(\sigma-1))(\ell\hat{r}_b^c + (1-\ell)\hat{r}^E)$$
$$\hat{C}_{fb} = -\theta\hat{r}_b^c + (\sigma-1)z_{fb} + \hat{Y} - (1-\alpha)(\sigma-1)\hat{w} - (1+\alpha(\sigma-1)-\theta)(\ell\hat{r}_b^c + (1-\ell)\hat{r}^E)$$
$$\hat{E}_{fb} = -\theta\hat{r}^E + (\sigma-1)z_{fb} + \hat{Y} - (1-\alpha)(\sigma-1)\hat{w} - (1+\alpha(\sigma-1)-\theta)(\ell\hat{r}_b^c + (1-\ell)\hat{r}^E)$$

With these modifications, I solve for all endogenous variables as in the baseline case.

Characterization of crowding out. I then characterize aggregate and relative crowding out. I consider a shock to government loan demand and decompose the response of each aggregate variable X into the effect captured by the coefficient of my cross-sectional regressions $\chi^X\nu Z^g$, a spillover term $\chi^X(1-\nu)Z^g$, and a general equilibrium feedback $\kappa_{GE}^X\chi^X Z^g$.

How do the coefficients κ_{GE}^X, χ^X and ν differ in this extended version of the model, and how does that impact the conclusions on the aggregate effect of crowding out? It is useful to define $\tilde{\epsilon}^c = (1-\ell)\theta + \ell(1+\alpha(\sigma-1))$, the elasticity of corporate credit demand in the extended model. It is a weighted average of the elasticity of substitution between debt and equity θ and the elasticity of demand for capital $(1+\alpha(\sigma-1))$. The coefficients κ_{GE}^X, χ^X and ν are then given by:

$$\nu = \frac{\epsilon^s + \lambda\epsilon^g + (1-\lambda)\tilde{\epsilon}^c}{\epsilon^s + \lambda\epsilon^g + (1-\lambda)\tilde{\epsilon}^c + \frac{1}{\phi S^*}}$$

$$\chi^C = \frac{-\tilde{\epsilon}^c}{\epsilon^s + \lambda\epsilon^g + (1-\lambda)\tilde{\epsilon}^c} \qquad \kappa_{GE}^C = \frac{(1-\ell)\theta + \ell\frac{1+\alpha\psi}{1-\alpha}}{(1-\ell)\theta + \ell(1+\alpha(\sigma-1))}\frac{\epsilon^s + \lambda\epsilon^g + (1-\lambda)\left[(1-\ell)\theta + \ell(1+\alpha(\sigma-1))\right]}{\epsilon^s + \lambda\epsilon^g + (1-\lambda)\left[(1-\ell)\theta + \ell\frac{1+\alpha\psi}{1-\alpha}\right]}\delta_1\delta_2 - 1$$

$$\chi^K = \frac{-\ell(1+\alpha(\sigma-1))}{\epsilon^s + \lambda\epsilon^g + (1-\lambda)\tilde{\epsilon}^c} \qquad \kappa_{GE}^K = \frac{\frac{1+\alpha\psi}{1-\alpha}}{1+\alpha(\sigma-1)}\frac{\epsilon^s + \lambda\epsilon^g + (1-\lambda)\left[(1-\ell)\theta + \ell(1+\alpha(\sigma-1))\right]}{\epsilon^s + \lambda\epsilon^g + (1-\lambda)\left[(1-\ell)\theta + \ell\frac{1+\alpha\psi}{1-\alpha}\right]}\delta_1 - 1$$

$$\chi^E = \frac{\ell(\theta-(1+\alpha(\sigma-1)))}{\epsilon^s + \lambda\epsilon^g + (1-\lambda)\tilde{\epsilon}^c} \qquad \kappa_{GE}^E = \frac{\frac{1+\alpha\psi}{1-\alpha} - \theta}{1+\alpha(\sigma-1)-\theta}\frac{\epsilon^E}{\epsilon^E+\theta}\frac{\epsilon^s + \lambda\epsilon^g + (1-\lambda)\left[(1-\ell)\theta + \ell(1+\alpha(\sigma-1))\right]}{\epsilon^s + \lambda\epsilon^g + (1-\lambda)\left[(1-\ell)\theta + \ell\frac{1+\alpha\psi}{1-\alpha}\right]}\delta_1 - 1$$

$$\chi^Y = \frac{-\ell\alpha\sigma}{\epsilon^s + \lambda\epsilon^g + (1-\lambda)\tilde{\epsilon}^c} \qquad \kappa_{GE}^Y = \frac{1+\psi}{(1-\alpha)\sigma}\frac{\epsilon^s + \lambda\epsilon^g + (1-\lambda)\left[(1-\ell)\theta + \ell(1+\alpha(\sigma-1))\right]}{\epsilon^s + \lambda\epsilon^g + (1-\lambda)\left[(1-\ell)\theta + \ell\frac{1+\alpha\psi}{1-\alpha}\right]}\delta_1 - 1$$

$$\text{with} \quad \delta_1 = \frac{(\theta+\epsilon^E)\left[\epsilon^s + \lambda\epsilon^g + (1-\lambda)\left[(1-\ell)\theta + \ell\frac{1+\alpha\psi}{1-\alpha}\right]\right]}{(\theta+\epsilon^E)\left[\epsilon^s + \lambda\epsilon^g + (1-\lambda)\left[(1-\ell)\theta + \ell\frac{1+\alpha\psi}{1-\alpha}\right]\right] + (1-\ell)(\epsilon^s + \lambda\epsilon^g + (1-\lambda)\theta)(\frac{1+\alpha\psi}{1-\alpha} - \theta)}$$

$$\delta_2 = \frac{(\theta+\epsilon^E)\left((1-\ell)\theta + \ell\frac{1+\alpha\psi}{1-\alpha}\right) + (1-\ell)\theta(\frac{1+\alpha\psi}{1-\alpha} - \theta)}{(\theta+\epsilon^E)\left((1-\ell)\theta + \ell\frac{1+\alpha\psi}{1-\alpha}\right)}$$

The key novelty of this extended model is that a decline in aggregate corporate credit does not immediately imply a decline in aggregate capital. When bank credit contracts, firms can substitute towards equity, and if the elasticity of substitution is high, capital need not fall. In the extreme case where $\theta \to +\infty$, $\chi^C = -\frac{1}{1-\lambda}$, i.e. the euro increase in local government loans equals the euro reduction in corporate lending, but $\chi^K = 0$ and the aggregate capital stock is unaffected.

Importantly, this margin of adjustment is fully reflected in the difference between the cross-sectional coefficient on corporate credit and that on investment, or, in other terms, in the credit-to-investment elasticity estimated in Table 5:

$$\frac{\beta^K}{\beta^C} = \frac{\ell(1+\alpha(\sigma-1))}{(1-\ell)\theta + \ell(1+\alpha(\sigma-1))}$$

If θ is high, this will be reflected in a reduced-form coefficient on investment β^K and a credit-to-investment sensitivity η^K close to 0. Therefore, the capital shortfall $\mathcal{L}^{Xsec}(K)$ estimated from the cross-sectional coefficient β^K already accounts for this margin of adjustment.

Mapping from cross-sectional to aggregate effect. How does the introduction of substitution between debt and equity affect the gap between the cross-sectional effect $\mathcal{L}^{Xsec}(K)$ and the total effect $\mathcal{L}(K)$? The spillover term $\chi^K(1-\nu)Z^g$ has the same interpretation as before, and the estimation strategy for ν remains valid.

The formulas for the general equilibrium feedback terms are modified. I develop the intuition for the expression for κ^K_{GE}. The first two fractions capture the same intuition as that of the baseline model: κ^K_{GE} depends on the difference between $\frac{1+\alpha\psi}{1-\alpha}$ and $1+\alpha(\sigma-1)$, which capture the countervailing forces of the labor supply response and of substitution towards the goods of non-exposed firms. When $\frac{1+\alpha\psi}{1-\alpha} = 1+\alpha(\sigma-1)$, this term collapses to 1. In this version of the model, these forces are compounded by a factor δ_1. When $\ell = 1$, $\delta_1 = 1$ and we are back to the baseline model. When $\ell < 1$, δ_1 can be above or below 1, and $sign(\delta_1 - 1) = sign(\theta - \frac{1+\alpha\psi}{1-\alpha})$. Hence, when θ is high, the response of equity will amplify the other general equilibrium forces.[69]

Calibration of the missing intercept. Calibrating the κ^X_{GE} coefficients requires values for two additional parameters: θ and ϵ^E. I estimate θ by considering an additional empirical moment: the response of firms' equity issuance to the credit supply shock, β^E. My analysis does not provide any moment that allows to discipline equity-holders' elasticity of equity supply ϵ^E; hence this parameter is calibrated. The procedure works as follows. I calibrate α, ψ, σ, ϵ^E. Using my empirical estimates for β^E and β^K

[69] Take the example where σ is high and the product of the first two fractions is below 1. Less exposed firms benefit from increased demand for their goods and increase their credit demand. Without equity, this increase in credit demand would push up their cost of capital, dampening their relative expansion. However, when they can easily substitute towards equity, the supply of which is unsegmented, they suffer less from this relative dampening effect. This makes the missing intercept more positive.

and the relationship

$$\frac{\beta^E}{\beta^K} = 1 - \frac{\theta}{1+\alpha(\sigma-1)}$$

I can recover a value for θ. I then use the relationship $\frac{\beta^K}{\beta^C} = \frac{\ell(1+\alpha(\sigma-1))}{(1-\ell)\theta+\ell(1+\alpha(\sigma-1))}$ to recover ℓ. Finally, combining my empirical estimates of β^K and ν provides an estimate of χ^K; and as before I invert the formula for χ^K to recover $\epsilon^s + \lambda\epsilon^g$. This yields all the parameters that enter the κ^X_{GE} coefficients.

In Figure C.4, $\beta^E \approx 0$, which implies that $\theta = 1 + \alpha(\sigma - 1)$. That is, the elasticity of substitution between debt and equity is approximately equal to the elasticity of capital demand. I use this value as baseline. To make the point that substitution towards equity is primarily picked up by a low credit-to-investment elasticity, as opposed to a dampening through the general equilibrium feedback, I also provide a calibration using $\beta^E = 0.2$. For ϵ^E, I use the estimate of the price elasticity of demand of the aggregate equity market of Gabaix and Koijen (2021). They find an elasticity equal to 0.2. I take this value as my baseline calibration and provide sensitivity analysis using $\epsilon^E = 1$.

Table D.5. Calibration of the general equilibrium feedback in model with endogenous equity

	Parameter values for σ and ψ								
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
Baseline: $\beta^E = 0$ and $\epsilon^E = 0.2$									
κ^C_{GE}	-1.3%	-5.3%	-7.8%	0.7%	-2.9%	-5.1%	3.5%	0.7%	-1.1%
κ^K_{GE}	-1.8%	-7.3%	-10.7%	1.0%	-4.1%	-7.3%	5.6%	1.1%	-1.7%
$\tilde{\kappa}^Y_{\text{GE}}$	76.8%	22.7%	-10.7%	81.7%	26.9%	-7.3%	90.0%	33.9%	-1.7%
Alternative 1: $\beta^E = 0.2$ and $\epsilon^E = 0.2$									
κ^C_{GE}	-2.5%	-7.9%	-11.3%	0.3%	-4.6%	-7.6%	4.2%	0.3%	-2.1%
κ^K_{GE}	15.6%	7.6%	2.7%	19.4%	12.0%	7.5%	25.4%	19.1%	15.1%
$\tilde{\kappa}^Y_{\text{GE}}$	108.1%	42.4%	2.7%	114.9%	48.3%	7.5%	125.6%	57.6%	15.1%
Alternative 2: $\beta^E = 0$ and $\epsilon^E = 1$									
κ^C_{GE}	-1.3%	-5.0%	-7.2%	0.7%	-2.8%	-4.8%	3.6%	0.7%	-1.1%
κ^K_{GE}	-3.1%	-12.4%	-17.8%	1.8%	-7.5%	-12.8%	11.4%	2.2%	-3.3%
$\tilde{\kappa}^Y_{\text{GE}}$	74.3%	15.9%	-17.8%	83.2%	22.5%	-12.8%	100.5%	35.3%	-3.3%

Note: This table reports the value of the general equilibrium feedback parameters in the model with substitution between debt and equity, for values of the elasticity of substitution across goods σ and the labor supply elasticity ψ reported in the first two lines. 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.

The calibrated coefficients in Table D.5 show that considering this extended model does not alter the conclusions of my main analysis. Since my quantification of the output loss follows from the capital loss, I focus on κ^C_{GE}, κ^K_{GE}, and $\tilde{\kappa}^Y_{\text{GE}}$. Across all calibrations, κ^C_{GE} and κ^K_{GE} remains modest in magnitude. Again, this is because substitution between debt and equity will primarily be reflected in χ^K close to 0, as opposed to changes in κ^K_{GE}.

I therefore make the same assumptions that in the baseline case: $\kappa^{K}_{\text{GE}} = \kappa^{C}_{\text{GE}} = 0$, and I assume $\mathcal{L}(L_t) = 0$. Then, this alternative model leads to the same quantification of aggregate effects.

Robustness. I consider a model where equity-holders can freely reallocate equity across firms, which intuitively gives the most room for large general equilibrium effects. To check that my results are not sensitive to this modeling choice, I solve the model under the opposite polar case, i.e. assuming that equity supply is fully segmented across firms. The key conclusions are unchanged: a high degree of substitution between credit and equity will be reflected in a low estimate of the credit-to-investment elasticity, and, consistent with intuition, the extent of the general equilibrium feedback tends to be smaller in magnitude.

D.4.3. Dynamic model

In the baseline model, the problem of the firm is static. I now consider a model where firms' investment and credit demand are determined by dynamic decisions. I start from the workhorse model of the quantitative corporate finance literature (taken from Bazdresch, Kahn and Whited 2018). I make simplifying assumptions that allow me to obtain closed-form solutions, and discuss the general case afterwards. Everything but the firms' problem is kept as in the baseline model.

Firms' problem. Firms are owned by risk-neutral shareholders with time discount rate r^E. Capital accumulation is subject to depreciation, time to build, and adjustment costs. Gross investment I_{fbt} is given by $K_{fb,t+1} = (1-\delta)K_{fbt} + I_{fbt}$ where δ is the depreciation rate. In period t, investing I entails a convex cost $c(I)$. In addition, in period t the firm pays for capital that will only be used in production in period $t+1$. The firm finances investment out of retained earnings, bank debt, and equity issuance. I consider one-period maturity risk-free debt contracts that pay an interest rate r^c_{bt}, determined in equilibrium. I assume that $\forall b, t,\ r^c_{bt} \leq r^E$. A firm issuing debt C_{fbt} at date t repays $(1+r^c_{bt})C_{fbt}$ at $t+1$. r^c_{bt} is known at t. Firm profits net of interest payments and capital depreciation are taxed at rate τ.

There are two financing frictions. First, firms face a collateral constraint $C_{fbt} \leq \bar{\theta}K_{fbt}$ where $\bar{\theta} < 1$. Second, equity issuance is costly: if preissuance cash flows are x, cash flows net of issuance costs are given by $G(x) = x(1 + e\mathbb{1}_{x<0})$ where $e > 0$ parameterizes the cost of equity issuance.

Labor is a static input. Hence, profits (net of labor input and before taxes) are given by:

$$\pi(z_{fbt}, K_{fbt}) = \max_{L_{fbt}} P_{fbt}e^{z_{fbt}}K^{\alpha}_{fbt}L^{1-\alpha}_{fbt} - w_tL_{fbt} = be^{\frac{\tilde{\alpha}}{\alpha}z_{fbt}}K^{\tilde{\alpha}}_{fbt}Y^{1-\tilde{\alpha}}_{t}w_t^{-\frac{1-\alpha}{\alpha}\tilde{\alpha}}$$

where b is a scaling constant and $\tilde{\alpha} = \frac{\alpha(\sigma-1)}{1+\alpha(\sigma-1)}$.

Every period, physical capital and debt are chosen optimally to maximize a discounted sum of per-period cash flows, subject to the financing constraint. The firm takes as given its productivity, interest rate, and the aggregate variables. Cash flows are given by:

$$\begin{aligned}&\mathrm{CF}(K_{fbt}, C_{fbt}, z_{fbt}, K_{fb,t+1}, C_{fb,t+1}) = \\ &G\Big(\pi(z_{fbt}, K_{fbt}) - (K_{bf,t+1} - (1-\delta)K_{fbt}) - c(K_{bf,t+1} - (1-\delta)K_{fbt}) + C_{bf,t+1} - (1+r^c_{bt})C_{fbt} - \tau\big(\pi(z_{fbt}, K_{fbt}) - r^c_{bt}C_{fbt} - \delta K_{fbt}\big)\Big)\end{aligned}$$

Define $V(K_{fbt}, C_{fbt}, z_{fbt})$ the value of the discounted sum of cash flows given the exogenous state variable z_{fbt} and the past endogenous state variables (K_{fbt}, C_{fbt}). V solves the Bellman equation:

$$V(K_{fbt}, C_{fbt}, z_{fbt}) = \max_{K_{fb,t+1}, C_{fb,t+1}} \left\{ \mathrm{CF}(K_{fbt}, C_{fbt}, z_{fbt}, K_{fb,t+1}, C_{fb,t+1}) + \frac{1}{1+r^E}\mathbb{E}_t[V(K_{fb,t+1}, C_{fb,t+1}, z_{fb,t+1})] \right\}$$

subject to $C_{fbt} \leq \bar{\theta}K_{fbt}$. Optimal financial structure trades off the benefits of debt (the tax shield) with the costs of debt (the collateral constraint reflects the agency costs of debt and implies reduced ability to invest when firms are too close to the constraint).

Closed-form solutions. I make three simplifying assumptions that allow to solve the problem in closed form. First, firm productivity is constant: $z_{fbt} = z_{fb}$. Second, there are no capital adjustment costs: $c(I) = 0$. Third, there is no equity issuance cost: $e = 0$. I discuss the more general case at the end of this section. With these simplifying assumptions, one can guess and verify that: (i) the steady state-level of capital satisfies:

$$(1-\tau)be^{\frac{\tilde{\alpha}}{\alpha}z_{fb}}Y^{*1-\tilde{\alpha}}w^{*-\frac{1-\alpha}{\alpha}\tilde{\alpha}}\tilde{\alpha}K_{fb}^{*\tilde{\alpha}-1} = \delta(1-\tau) + r^E(1-\bar{\theta}) + \bar{\theta}(1-\tau)r^*_b \ ;$$

(ii) optimal leverage satisfies $C^*_{fb} = \bar{\theta}K^*_{fb}$; and (iii) for any (K_{fb}, C_{fb}) the firm will only take one period to reach its optimal levels of capital and leverage so that the value function is:

$$\begin{aligned}V(K_{fbt}, C_{fbt}, z_{fb}) =&(1-\tau)(be^{\frac{\tilde{\alpha}}{\alpha}z_{fb}}K_{fbt}^{\tilde{\alpha}}Y_t^{1-\tilde{\alpha}}w_t^{-\frac{1-\alpha}{\alpha}\tilde{\alpha}} - r_{bt}C_{fbt}) - (K^*_{bf} - (1-\delta)K_{fbt}) + \bar{\theta}K^*_{bf} - C_{fbt} + \tau\delta K_{fbt} \\ &+ \frac{be^{\frac{\tilde{\alpha}}{\alpha}z_{fb}}K_{fb}^{*\tilde{\alpha}}Y^{*1-\tilde{\alpha}}w^{*-\frac{1-\alpha}{\alpha}\tilde{\alpha}} - \delta K^*_{fb} - r^*_b\bar{\theta}K^*_{fb}}{r^E}\end{aligned}$$

The intuition for this solution is that at its steady state the firm will maximize its tax shield, hence $C^*_{fb} = \bar{\theta}K^*_{fb}$. The optimal capital stock will equalize the marginal product of capital to the cost of capital, accounting for the fact that the firm's financing structure is $\bar{\theta}$ bank credit and $1-\bar{\theta}$ equity. Hence, the weighted average cost of capital (accounting for depreciation) is $\delta(1-\tau) + r^E(1-\bar{\theta}) + \bar{\theta}(1-$

$\tau)r_b^*$. In this simplified version of the model, capital structure is determined by maximizing the tax shield, subject to the agency friction that determines the collateral constraint.

Solution. The rest of the model is the same as in the baseline. I study a small, unanticipated, permanent shock to local government demand for debt, which will induce a change in the interest rate charged by banks. The economy is in steady state until time $t-1$. I consider a shock that affects r_{bt}^c, the interest rate on bank credit that applies to C_{fbt}. In period $t-1$, the firm optimally chooses capital and debt given this new interest rate, and reaches its new optimal levels of capital and leverage at t. I denote $\hat{x}_t$ the log-difference in variable x in the new time-t steady state compared to the $t-1$ steady state. Starred variables denote values in the initial steady-state.

From the firm's problem solution, the change in capital and bank credit demand are given by:

$$\begin{aligned}
\hat{K}_{fbt} &= \hat{Y}_t - (1-\alpha)(\sigma-1)\hat{w}_t - \frac{\bar{\theta}(1-\tau)i^*}{\delta(1-\tau) + r^E(1-\bar{\theta}) + \bar{\theta}(1-\tau)i^*}(1+\alpha(\sigma-1))\hat{r}_{bt} \\
\hat{C}_{fbt} &= \hat{K}_{fbt}
\end{aligned}$$

Let $\ell = \frac{\bar{\theta}(1-\tau)i^*}{\delta(1-\tau)+r^E(1-\theta)+\bar{\theta}(1-\tau)i^*}$. $\tilde{\epsilon}^c = \ell(1+\alpha(\sigma-1))$ is the elasticity of corporate credit demand in this modified model. The rest of the model solves as in the baseline. The bank interest rate $\hat{r}_{bt}$ writes:

$$\hat{r}_{bt} = \frac{Z_t^g}{\epsilon^s + \lambda\epsilon^g + (1-\lambda)\ell\frac{1+\alpha\psi}{1-\alpha}} + \frac{Z_{bt}^g - Z_t^g}{\epsilon^s + \lambda\epsilon^g + (1-\lambda)\tilde{\epsilon}^c + \frac{1}{\phi S^*}}$$

Aggregate and relative crowding out. As in the baseline model, I decompose the change in each aggregate variable X into the effect captured by the coefficient of my cross-sectional regressions $\chi^X \nu Z^g$, a spillover term $\chi^X(1-\nu)Z^g$, and a general equilibrium feedback $\kappa_{\mathrm{GE}}^X \chi^X Z^g$. These coefficients are given by:

$$\nu = \frac{\epsilon^s + \lambda\epsilon^g + (1-\lambda)\tilde{\epsilon}^c}{\epsilon^s + \lambda\epsilon^g + (1-\lambda)\tilde{\epsilon}^c + \frac{1}{\phi S^*}}$$

$$\chi^K = \chi^C = \frac{-\tilde{\epsilon}^c}{\epsilon^s + \lambda\epsilon^g + (1-\lambda)\tilde{\epsilon}^c} \qquad \kappa_{\mathrm{GE}}^K = \kappa_{\mathrm{GE}}^C = \frac{\ell\frac{1+\alpha\psi}{1-\alpha}}{\ell(1+\alpha(\sigma-1))}\frac{\epsilon^s + \lambda\epsilon^g + (1-\lambda)\ell(1+\alpha(\sigma-1))}{\epsilon^s + \lambda\epsilon^g + (1-\lambda)\ell\frac{1+\alpha\psi}{1-\alpha}} - 1$$

$$\chi^Y = \frac{-\ell\alpha\sigma}{\epsilon^s + \lambda\epsilon^g + (1-\lambda)\tilde{\epsilon}^c} \qquad \kappa_{\mathrm{GE}}^Y = \frac{1+\psi}{(1-\alpha)\sigma}\frac{\epsilon^s + \lambda\epsilon^g + (1-\lambda)\ell(1+\alpha(\sigma-1))}{\epsilon^s + \lambda\epsilon^g + (1-\lambda)\ell\frac{1+\alpha\psi}{1-\alpha}} - 1$$

The key difference compared to the baseline model is that the interest rate on bank credit is not the only determinant of the marginal cost of capital. Hence, the interest rate elasticity of capital demand is lower than in the baseline model, and is determined by $\ell < 1$. Because firms are at the constraint, the credit-to-capital elasticity is 1 and the effect on credit is the same as that on capital.

Through the lens of this model, my empirical exercise estimates $\chi^C \nu$. The identification of ν remains valid in this model. The formulas for the κ_{GE} terms are highly similar to my baseline model but now account for the ℓ coefficient. Since $\ell < 1$, for any values of α, σ, ψ, the κ_{GE} terms are dampened compared to my baseline model.

I therefore make the same assumptions that in the baseline case: $\kappa_{\text{GE}}^K = \kappa_{\text{GE}}^C = 0$, and I assume $\mathcal{L}(L_t) = 0$. Then, this extended model yields the same implications for the mapping from the cross-sectional estimates to the aggregate effects.

How would these conclusions change away from the simplifying assumptions made above? It is known that when solving this model under more general assumptions, firms choose a leverage ratio strictly below the constraint. Therefore, the credit-to-investment elasticity may be lower than 1. If so, this would be reflected in χ^K less negative than χ^C. However, there is no reason to expect that the general equilibrium forces that shape the κ_{GE} terms would differ.

D.4.4. Introducing depositors substituting across banks

In the baseline model, households attached to a bank can only invest deposits at their bank and cannot reallocate their savings to exploit interest rate differentials across banks. I now consider a model where this assumption is relaxed. Everything is as in the baseline model except for the deposit supply function.

I now consider the case where households attached to each bank b can borrow to or lend from other households, which is equivalent to allowing them to invest their savings at other banks. I call this debt instrument IOUs. I model the functioning of this market symmetrically to the interbank market: each household can be a net borrower or a net saver on the IOU market, the rate is j, and I impose a quadratic friction on the trading of IOUs. Now, deposits at bank b S_b will equal the savings of household b A_b plus the net IOU borrowing of household b I_b. To obtain the supply of deposits at bank b, I extend the optimization problem that generates the deposit supply function in the baseline model (see footnote 60) and consider the optimization problem of households b who maximize the proceeds of deposits $S_b = A_b + I_b$ invested at bank b minus the cost of borrowing on the IOU market and their disutility cost of saving: $\max_{A_b, I_b}\ r_b^s(A_b + I_b) - jI_b - \frac{\varphi}{2} j I_b^2 - \tilde{s} A_b^{1+\frac{1}{\epsilon^s}}$. The solution of this problem yields $A_b = s(r_b^s)^{\epsilon^s}$ and $r_b^s = j(1 + \varphi I_b)$. Therefore,

$$S_b = s(r_b^s)^{\epsilon^s} + \frac{1}{\varphi}\frac{r_b^s - j}{j}$$

This formulation nests the baseline model when $\varphi \to +\infty$. With $\varphi < +\infty$, $\left.\frac{\partial \log S_b}{\partial \log r_b^s}\right|_{\text{DE}} = \epsilon^s + \frac{1}{\varphi S^*} > \epsilon^s$. Hence, the cross-sectional (across banks) elasticity of deposit supply may be arbitrarily larger than the aggregate elasticity. This functional form is particularly convenient because it nests the baseline model and yields expressions that are symmetric in the interbank market friction ϕ and the IOU market friction φ. Beyond this specific functional form, the intuitions developed below will be valid as long as the cross-sectional elasticity of deposit supply is larger than the aggregate elasticity.

The problem of banks is the same as before. The only difference is that the balance sheet constraint now writes $C_b^c + C_b^g = S_b + B_b$ where $S_b = A_b + I_b$. The definition of equilibrium is the same as before, with the additional condition that the IOU market must clear.

Characterization. The solution for aggregate variables is unchanged. The bank-specific interest rate is now equal to:

$$\hat{r}_b = \frac{Z^g + \frac{1-\lambda}{\ell}\frac{1+\psi}{1-\alpha} Z^c}{\epsilon^s + \lambda\epsilon^g + (1-\lambda)\frac{1}{\ell}\frac{1+\psi\alpha}{1-\alpha}} + \frac{Z_b^g - Z^g}{\epsilon^s + \lambda\epsilon^g + (1-\lambda)\epsilon^c + \frac{1}{S^*}\left(\frac{1}{\phi} + \frac{1}{\varphi}\right)}$$

Comparing this equation with its counterpart in the baseline model (D.12), allowing households to substitute across banks ($\varphi < +\infty$) reduces the effect of bank-specific demand shocks on the bank-level interest rates. In the extreme case where $\varphi = 0$, banks facing positive demand shocks draw in deposits from other banks, until interest rates are perfectly equalized across banks.

Aggregate and relative crowding out. As in the baseline model, we can decompose crowding out using (10) into a cross-sectional effect $\chi^{\text{C}}\nu$, a spillover across banks term $\chi^{\text{C}}(1-\nu)$, and a general equilibrium feedback $\kappa_{\text{GE}}^{\text{C}}\chi^{\text{C}}$. Allowing deposits to reallocate across banks does not affect the solution for aggregate variables, thus the coefficients χ^{C} and $\kappa_{\text{GE}}^{\text{C}}$ are unchanged.

The key difference lies in the effect captured by the cross-sectional term as opposed to spillovers across banks. We now have $\nu = \frac{\epsilon^s + \lambda\epsilon^g + (1-\lambda)\epsilon^c}{\epsilon^s + \lambda\epsilon^g + (1-\lambda)\epsilon^c + \frac{1}{S^*}\left(\frac{1}{\phi} + \frac{1}{\varphi}\right)}$. $\nu \in [0,1]$ jointly captures the degree of interbank frictions ϕ and IOU market frictions φ. For any $\phi \geq 0$, it is monotonically increasing in φ. When frictions to reallocate deposits across banks are small, a bank-specific local government debt demand shocks leads to a large increase in deposits at exposed banks, limiting the extent of bank-specific crowding out. When $\varphi \to 0$ (no IOU frictions), $\nu = 0$ and there is no bank-specific crowding out. However, in this case, non-exposed banks experience a large deposit outflow in response to the aggregate increase in local government debt, so that the missing intercept $(1-\nu)$ will be large. In sum, the cross-sectional crowding out effect reflects the cross-sectional elasticity of deposit supply, not the aggregate elasticity.

Estimation of the spillover across banks. Mapping the cross-sectional effect into the direct effect requires to estimate $1-\nu = \frac{\frac{1}{S^*}(\frac{1}{\phi}+\frac{1}{\varphi})}{\epsilon^s+\lambda\epsilon^g+(1-\lambda)\epsilon^c+\frac{1}{S^*}(\frac{1}{\phi}+\frac{1}{\varphi})}$. In this extended version of the model, the effect of bank-specific credit demand shocks on net interbank borrowing does not allow to estimate $1-\nu$. Net interbank lending is given by:

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

The coefficient estimated is $\beta^B = \frac{\frac{1}{S^*}\frac{1}{\phi}}{\epsilon^s+\lambda\epsilon^g+(1-\lambda)\epsilon^c+\frac{1}{S^*}(\frac{1}{\phi}+\frac{1}{\varphi})}$. If $\varphi < +\infty$, then $\beta^B < 1-\nu$. The intuition is that in this case, β^B captures only part of the capital flows from control to treated banks. That is, there is a part of the missing intercept not captured by β^B. Estimating the elasticity of deposits to a demand shock does not solve the problem:

$$\hat{S}_b = \epsilon^s\hat{i} + \frac{(\epsilon^s + \frac{1}{S^*}\frac{1}{\varphi})(Z_b^g - Z^g)}{\epsilon^s + \lambda\epsilon^g + (1-\lambda)\epsilon^c + \frac{1}{S^*}(\frac{1}{\phi} + \frac{1}{\varphi})} \tag{D.33}$$

The estimated coefficient is $\beta^S = \frac{\epsilon^s+\frac{1}{S^*}\frac{1}{\varphi}}{\epsilon^s+\lambda\epsilon^g+(1-\lambda)\epsilon^c+\frac{1}{S^*}(\frac{1}{\phi}+\frac{1}{\varphi})}$. It does not allow to recover the part of the missing intercept due to deposit flows across banks. The intuition is that β^S captures the overall elasticity of deposit supply faced by individual banks. However, what matters for the missing intercept term $1-\nu$ is only the part of this response that is "zero-sum" (the term $\frac{1}{S^*}\frac{1}{\varphi}$), in the sense that the inflow of deposits at treated banks comes from an outflow at control banks.

Bounds on $1-\nu$. In this extended model, I cannot point identify $1-\nu$. There is no empirical moment to separately identify the cross-sectional elasticity of deposit supply and the aggregate elasticity. However, I now show that $1-\nu$ is set identified.

First, note that $\beta^B \leq 1-\nu$. That is, considering only capital flows across banks occurring on the interbank market underestimates the negative spillover exerted by treated banks on control banks. This parallels the finding that the cross-sectional crowding out effect underestimates the direct effect. The lower bound argument due to unobserved capital flows from control to treated units is a robust intuition of my aggregation exercise. $\beta^B \leq 1-\nu$ implies that my baseline quantification is conservative.

Second, the coefficient of the cross-sectional regression of bank total liabilities $\hat{S}_b + \frac{B_b}{S^*}$ on a bank-specific demand shock is $\beta^L = \beta^B + \beta^S = \frac{\epsilon^s+\frac{1}{S^*}(\frac{1}{\phi}+\frac{1}{\varphi})}{\epsilon^s+\lambda\epsilon^g+(1-\lambda)\epsilon^c+\frac{1}{S^*}(\frac{1}{\phi}+\frac{1}{\varphi})}$. Hence, $\beta^L \geq 1-\nu$, allowing me

Table D.6. Effect on total bank liabilities

	Change in total bank liabilities				
	(1)	(2)	(3)	(4)	(5)
Credit demand shock	0.044	0.315***	0.223***	0.264***	0.210***
	(0.045)	(0.060)	(0.052)	(0.049)	(0.059)
Time FE	✓	✓	✓	✓	✓
Bank FE					✓
Est. supply shock		✓			
Est. supply shock (pub/private)			✓	✓	✓
Add. controls				✓	✓
Observations	3,925	3,444	3,433	3,409	3,374
R-squared	0.055	0.11	0.099	0.17	0.30

Note: This table reports the results of estimating equation (D.27). All elements of the specification are as in Table **??**, except that the outcome variable is the bank-level change in total liabilities normalized by lagged assets. ***, ** and * indicate significance at the 1%, 5% and 10% levels, respectively.

to provide an upper bound on the aggregate effect. The reason why $\beta^L \geq 1 - \nu$ is that total flows are larger than the purely cross-sectional "zero-sum" flows. Put differently, $\beta^L \geq 1 - \nu$ reflects the fact that $\epsilon^s + \frac{1}{S^*}\frac{1}{\varphi} \geq \epsilon^s$, i.e., the fact that the cross-sectional elasticity of deposit supply is (weakly) larger than the aggregate elasticity.

To estimate β^L, I use the empirical specification (D.27), with total bank liabilities as outcome variable. The results are provided in Table D.6. As predicted by the model, $\beta^L \geq \beta^B$, with effects larger by 30% on average across specifications. Estimating the direct effect using this upper bound on $1 - \nu$, I find that the capital (output) multiplier is equal to –0.44 (–0.24). Combining the lower bound and the upper bound on $1-\nu$ implies a range for the capital multiplier equal to [–0.44, –0.39] and for the output multiplier equal to [–0.24, –0.21].

D.4.5. 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:

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

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 $\chi^C = \frac{-\epsilon^c}{\epsilon^s + \lambda\epsilon^g + (1-\lambda)\epsilon^c}$. 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 $\varphi \to +\infty$, $\chi^C = \frac{1}{1-\lambda}$, 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.26) 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.4.6. 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 $\chi^C = \frac{\epsilon^c}{\epsilon^s + \lambda^g\epsilon^g + \lambda^c\epsilon^c + \lambda^h\epsilon^h}$, $\nu = \frac{\epsilon^s + \lambda^g\epsilon^g + \lambda^c\epsilon^c + \lambda^h\epsilon^h}{\epsilon^s + \lambda^g\epsilon^g + \lambda^c\epsilon^c + \lambda^h\epsilon^h + \frac{1}{\phi S^*}}$ and $\kappa_{\text{GE}}^C = \frac{\frac{1}{\ell}\frac{1+\alpha\psi}{1-\alpha}}{\frac{1}{\ell}(1+\alpha(\sigma-1))} \frac{\epsilon^s + \lambda^g\epsilon^g + \lambda^h\epsilon^h + \lambda^c\frac{1}{\ell}(1+\alpha(\sigma-1))}{\epsilon^s + \lambda^g\epsilon^g + \lambda^h\epsilon^h + \lambda^c\frac{1}{\ell}\frac{1+\psi\alpha}{1-\alpha}} - 1$. 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.26) 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 κ_{GE}^C, from –35.2% to 25.6%.

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:

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

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

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

where the variance is taken over all firms within each sector and $\mathrm{TFP}_s^* = (\sum A_{fs}^{\sigma-1})^{\frac{1}{\sigma-1}}$. I used the approximation $\mathrm{Var}(\log(\mathrm{MRPK}_{fs})) = \mathrm{Var}(\log(1 + \tau_{fs}^K)) = \mathrm{Var}(\tau_{fs}^K)$. 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.[70] Heterogeneous cross-sectional exposure to crowding out may thus imply a change

[70] 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 5) is to be understood as the reaction to this shock to wedges.

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:

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

The outer product denotes that I include all interacted and non-interacted terms. 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 6.

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. $\hat{\tau}^K_{ft} - \tau^K_{ft}(\mathbf{0}) = \hat{\beta}_0 \mathit{FirmExposure}_{ft} + \hat{\beta}_1 \mathit{FirmExposure}_{ft} \mathbb{1}[\mathit{High}\ \tau^K_{f,t-1}]$ yields $\tau^K_{ft}(\mathbf{0})$. The TFP loss is then given by:

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

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	-1.402***	0.362**	-1.363***	-1.468***	0.078	0.709***
	(0.323)	(0.158)	(0.292)	(0.347)	(0.226)	(0.188)
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	0.60
Credit-to-wedge IV		-.183**			-.094	-.282***
		(.081)			(.143)	(.102)
High *minus* Low (RF)				-.109		.598***
				(.325)		(.226)
High *minus* Low (IV)						-.206
						(.145)

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.

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 6. 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.[71] 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.

[71] 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.[72]

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, 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 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

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

above 750,000 euros.

New contracts (NCE). I obtain data on interest rates for a representative sample of new loans each quarter from the dataset *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.

Local government accounts. French local government accounts are obtained from the publicly available *Comptes individuels des collectivités*.[73] The data covers 2006-2018 for *communes*, 2007-2018 for general-purpose *EPCI*, and 2008-2018 for *départements*. I do not use regions as the series have a break in 2015 due to the redistricting. To aggregate the data at the level of my 2,080 time-invariant municipalities, I assign each *EPCI* to the largest *commune* within the *EPCI*, each *départements* to its capital *commune*, and then use the mapping from *communes* to the 2,080 time-invariant municipalities. When aggregating at the municipality level, to avoid having breaks in 2007 and 2008, I extrapolate the series for *EPCI* (*départements*) using growth rates in the *communes* belonging to the *EPCI* (*départements*).

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.

[73] https://www.data.gouv.fr/fr/datasets/comptes-individuels-des-collectivites/

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