Amplifying a Fiscal Stimulus: The Role of Banks *

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Abstract

Exploiting increases in US defense spending following the 9/11 attacks, we show that the procurement-driven fiscal stimulus led to lower non-performing loans at banks. In turn, constrained banks responded by increasing lending to small businesses in not-directly-impacted counties. We find the additional economic activity enabled by the increased lending capacity is quantitatively important – amplification from this 'credit multiplier' is about 10%-15% as large as typical fiscal multipliers estimated in the literature. Since intermediaries are more likely to face constraints in times when a fiscal stimulus is needed, this 'credit multiplier' can substantially amplify a stimulus' overall impact in downturns.

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1 Introduction

Understanding the effects of fiscal interventions has received substantial attention in recent years, following the massive fiscal stimulus in the aftermath of the global financial crisis and the COVID-19 pandemic. While recent work has greatly enhanced our understanding of the direct effects of fiscal interventions on firms and households, less attention has been paid to the role of the banking sector in the transmission and amplification of a fiscal stimulus.

In the absence of financing frictions, the banking sector should have little or no impact on the way a fiscal stimulus is transmitted through the economy. However, if banks face financial constraints, their role in the transmission of a fiscal stimulus is a priori unclear. This stems from the fact that constrained banks are unable to fund all profitable lending opportunities. If a fiscal stimulus increases demand for credit in areas most benefiting from the shock, capital-constrained banks may reduce lending elsewhere to supply this credit and thereby crowd-out financing of economic activity in other regions.[1] On the other hand, if the positive demand shock from the stimulus improves exposed borrowers' credit quality or reduces their reliance on bank financing, it can free up lending capacity on banks' balance sheet, which in turn can allow banks to reallocate lending towards other areas less exposed to government spending.[2]

Studying the transmission of a fiscal stimulus is inherently difficult due to the many confounding factors typically present during periods when there are fiscal interventions. Our empirical strategy therefore builds on previous studies that exploit plausibly exogenous shifts in military spending to identify the effects of government spending shocks (e.g., Nekarda and Ramey (2011); Nakamura and Steinsson (2014)). We use these shocks along the substantial heterogeneity in bank market shares across US counties (e.g., Gilje et al. (2016)) to identify the propagation role of the stimulus through the banking sector.

[1] Cortés and Strahan (2017) find that, following a natural disaster, banks increase lending in areas hit by the disaster due to increased credit demand and cut back on credit supply in other areas.

[2] Note that exposed borrowers could be firms directly benefiting from the stimulus or other borrowers in areas that benefit from the effects of a stimulus.

Specifically, we exploit the large increase in US defense spending following the September 2001 terrorist attacks, together with the substantial heterogeneity in defense spending activity and bank market shares across US counties, to study the mechanism through which a fiscal stimulus may transmit through the banking sector.

Our analysis proceeds in four steps. First, we show a large, discontinuous, and sustained increase in aggregate defense spending for the US in 2001 and subsequent years. A significant proportion of the increase in military spending can be attributed to increased procurement of military equipment, which we are able to trace to the county in which the benefiting firm was located. We show that a county's defense spending per capita in 2000 is a strong predictor of the county's defense spending growth over the following years due to the stickiness of government procurement across firms over time. We therefore use the year 2000 distribution of defense spending across counties interacted with the national increase in defense spending to instrument for the actual fiscal shock to each county.

Second, we find that this instrumented increase in defense spending at the county-level led to a decline in non-performing assets for banks with larger branch networks in shocked counties – defined as counties in the top 10% by defense spending exposure in 2000, which are the counties that experienced a significant boost in activity.[3] By relying on the pre-2001 distribution of bank branches, our empirical design also overcomes concerns about the endogeneity of bank-branch location in the post-2001 period. We interpret this finding as being consistent with the stimulus improving borrowers' credit quality in the shocked counties as economic activity increases. This in turn improved the health of balance sheets for banks with the greatest branch presence in these shocked counties.

Having traced the fiscal shock to an improvement in balance sheets for the most exposed banks, we next examine the consequences of their improved lending capacity. In our third step, we look within counties that were not directly shocked by the fiscal stim-

[3]Since banks lend primarily in areas where they have branches and firms tend to borrow from banks that are located close by, the geographic distribution of banks' branches provide a good proxy for the location of commercial lending activity (Petersen and Rajan (2002); Berger et al. (2005)).

ulus. Within these counties, we compare lending by banks with differential exposure to the fiscal stimulus shock through their branch network. By focusing within counties, this empirical design allows us to control for local credit demand and all other time-varying confounding factors at the county-year level. We rely only on across-bank variation in exposure to the defense spending shock to identify the transmission of the fiscal intervention to unexposed counties. In addition, our analysis controls for interactions between time fixed effects and pre-shock bank characteristics (including banks' exposure through their branch network to other county characteristics than defense spending) to address the possibility that bank characteristics correlated with their government spending exposure may drive the differential response of banks to the shock.

We show that within counties that were not directly shocked by the fiscal stimulus, banks with a greater exposure to the stimulus through their branch network increased both credit supply to small businesses and mortgage lending to households more than banks with a smaller exposure to the stimulus. A 10-percentage point increase in a bank's fraction of branches in shocked counties is related to a 7.5 to 9 percentage points increase in small business lending relative to other banks lending in the same non-shocked county. Consistent with bank financial constraints playing an important role in the propagation of a fiscal stimulus, the result is driven mainly by capital constrained banks. In fact, we do not find any significant effects for banks that are not constrained.

Our results suggest that the mechanism at work is as follows: The fiscal stimulus has a positive impact on firm health. This in turn helps banks with lending exposure to these firms to benefit from a reduction in non-performing loans. When banks are constrained, the quality improvement in these banks' loan book frees up additional lending capacity. Through their branch network, these banks then increase lending to small businesses in other areas not directly shocked by the initial fiscal stimulus.[4]

In our fourth and final step, we show that the spatial lending spillovers have a mul-

[4]Since the stimulus was procurement-driven, it most likely did not substantially increase overall reliance on credit in the directly-shocked-counties.

tiplicative effect on small business employment. Our estimates imply that an increase in national defense spending of about $350,000 is associated with an additional job in small businesses through the multiplicative effect of the banking system.[5] Our results suggest that this 'credit multiplier' can be as large as 10%-15% of the fiscal multipliers estimated in the literature when studying the direct effect of a stimulus.

Broadly, these results speak on the effect of 'targeted' – as opposed to broad – fiscal interventions. The results show that an exogeneous fiscal shock in one corner of the economy – that is exposed to government spending – generates spillovers to other sectors and areas through the banking system if banks are constrained. The effect of the propagation depends on the degree of constraints in the banking sector. An implication of this finding is that since fiscal interventions are typically undertaken when the economy is stressed, this is also likely to be a time when the banking sector faces greater constraints. The transmission of the fiscal stimulus through banks will be stronger precisely in those bad times, when banks are constrained and not doing well, suggesting a larger multiplier in bad times than in good ones.

The results provide novel insights for understanding the consequences of changes in government spending on the real economy. A long literature quantifies the multiplier associated with government spending, that is the increase in output generated by $1 of government spending (e.g., Ramey (2011); Nakamura and Steinsson (2014); Serrato and Wingender (2016); Adelino et al. (2017); Auerbach et al. (2022)).[6] Relative to these papers, we highlight how a fiscal stimulus is amplified by the banking system, and this leads to an increase in economic activity across various geographic areas and sectors not directly affected by the stimulus. Our results also suggest that even in the counties exposed to the

[5]These effects are a combination of partial equilibrium effects and local general equilibrium effects. See Guren et al. (2021) or Nakamura and Steinsson (2018).

[6]Several papers also specifically study the fiscal stimulus response to the 2008-09 financial crisis (American Reinvestment and Recovery Act) across states or counties (e.g., Wilson (2012); Chodorow-Reich et al. (2012); Leduc and Wilson (2017); Garin (2019)), with mixed findings on the effectiveness of the program in stimulating employment (Ramey (2019)). A nascent literature examines the effect of local government spending shocks on large listed firms, and generally find negative "crowding-out" effects on firm growth (e.g., Kim and Nguyen (2019)).

fiscal stimulus, a part of the local fiscal multiplier could be due to increased local lending by previously constrained banks.

Our paper also relates to the literature on state dependent multipliers (Christiano et al. (2011); Auerbach and Gorodnichenko (2012); Ramey and Zubairy (2018); Corsetti et al. (2012); Born et al. (2020); Bonfim et al. (2022)). The evidence presented in these papers is consistent with the models where financial frictions amplify fiscal multipliers (Eggertsson and Krugman (2012); Faria-e Castro (2022)). We add to this literature by showing that existing constraints in the banking system could play an important role regarding the size of the fiscal multiplier. More precisely, our findings suggest that a fiscal stimulus can alleviate constraints in the banking system and in turn stimulate more lending that increases the impact of the stimulus.

Our paper is also related to the literature on public procurement. Government spending via public procurement can stimulate firm growth (Ferraz et al. (2015)) and reduce firm sensitivity to an aggregate shock (Goldman (2020)). Bonfim et al. (2022), find that in the European sovereign debt crisis increase in constraints in the banking sector, due to reduction in public procurement, led to a further contraction in lending. Furthermore, payment efficiency by the government can reduce financial constraints for firms (Barrot and Nanda (2020)). We contribute to this literature by showing that the positive effects of procurement on firms can also pass on to the banking sector due firm-bank linkages. This in turn could lead to more lending, and spill over onto other non-targeted firms via the banking channel. The results also speak to the targeting of public procurement. Our results suggest that targeting procurement spending to sectors that are in distress, and also linked to constrained banks, could have dual benefits in terms of stimulating growth.

Finally, our paper contributes to the literature that highlights the importance of geographic footprint of bank branch network on the real economy. Gilje et al. (2016), find that after banks gain deposits due to their exposure to fracking shocks, they increase lending in other areas via their branch network.[7] Similarly, Cortés and Strahan (2017) show that

[7]In a similar way, see also Bird et al. (2019) who study a fiscal (tax) shock and find that banks increase

banks with presence in areas that are exposed to natural disasters cut lending in other areas to meet the increased lending demand in areas hit by the shock.[8] We add to this literature by examining the role of branch networks in amplifying a fiscal stimulus and the associated real effects. The mechanism in our paper is driven by a reduction in non-performing loans in banks while the prior literature emphasizes the role of deposits. In addition, we also document real effects at a more aggregate level (county level). More broadly, our paper links the literature that examines the impact of intermediary balance sheet on the real economy to the literature that analyzes fiscal multipliers.

2 Data and Background

2.1 Data

We assemble a comprehensive dataset of government defense procurement spending and bank small business lending between 1998 and 2005. Consolidated Federal Funds Reports (CFFR) provide information on yearly county-level procurement spending by the Department of Defense, including military agencies. Small business lending information comes from Community Reinvestment Act (CRA) disclosure data from the Federal Financial Institutions Examination Council (FFIEC). The CRA requires banks above a certain asset threshold to report small business lending by census tract each year. The asset threshold was $250 million between 2000 and 2005. Greenstone et al. (2020) estimate that, in 2007, CRA eligible banks accounted for approximately 86 percent of all loans under $1 million. These lending data are at the bank-county-year level, which will allow us studying changes in banks' lending across different counties. Our measure of small business lending follows Greenstone et al. (2020). It uses the dollar amount of small business loan originations to businesses with $1 million or less in annual gross revenues.

lending thereafter.

[8] Jayaratne and Strahan (1996) find that removal of branch restrictions in the US led to increase in economic activity. Similarly, Morgan et al. (2004) find more co-movement of state business cycles after banking deregulation.

We merge the lending and government spending data with banks' financial information from the Call Reports as of the fourth quarter of each year, with information on banks' branches in each county from Summary of Deposits data, and with employment and business dynamics information at the county level from the Quarterly Census of Employment and Wages, County Business Patterns, Quarterly Workforce Indicators and the 2000 US Census. Data on listed firms' financial information are from Compustat, and their government contractor status is from Compustat Customer Segments (as in Goldman (2020)).

2.2 Empirical Setting: Defense Spending after 9/11

The September 11, 2001 terrorist attacks led to a substantial military build-up in the United States. Following the attacks, the US government expanded military capacity for the wars in Afghanistan and Iraq and to strengthen national security. Figure 1 illustrates this growth in national defense procurement spending. Spending stayed constant in the five years to 2001 before increasing by 67% in real terms over the next five years. Figure 2 further shows that the aggregate growth in procurement defense spending in the US affected different counties to different extent, with some large increases around Washington DC, on the Louisiana coast, in southern California and Arizona, and in numerous other counties scattered across the country. Our identification strategy is based on the sudden increase in national military spending following 9/11, together with its spatial heterogeneity across areas.

[Include Figures 1 and 2]

At the micro level, government contracting activity is very persistent over time. Among listed firms, the one year auto-correlation in a variable indicating whether a firm derives more than 10% of sales from government procurement is over 0.8 (Goldman (2020)). Defense procurement spending is also very persistent at the county-level. Consequently, the level of a county's defense procurement spending per capita in 2000 is a strong predictor

of post-9/11 defense procurement spending growth that the same county will experience. A linear regression of county defense sending growth per capita between 2001 and 2006 on county defense spending exposure in year 2000 produces a coefficient of 0.61 statistically significant at the 1% level, indicating that for each dollar of defense spending in a county in 2000, the county receives on average an extra 61 cents after 9/11. Figure A.2 in the appendix illustrates the positive relationship between the level of local spending in 2000 and its post-9/11 growth in a scatter plot between the two variables.

Our empirical strategy exploits this persistence in local defense spending. We rely on the distribution of government spending in 2000 to measure counties' exposure to the national defense spending shock associated with 9/11. This method mitigates the possible concern that our results are driven by the Department of Defense endogenously allocating defense contracts after 2001 based on counties' economic activity. The stickiness of county-level defense spending, together with the spending growth generated by the unexpected 9/11 events represents a large plausibly exogenous shock in local defense spending for the counties that hosted a significant fraction of defense contracting activity in 2000.

3 Direct Effect of the Stimulus

3.1 Firms and Counties

We start by assessing the direct effect of the defense spending shock on firms and local activity. Panel A of Table 1 displays the results of firm-level regressions estimated on publicly listed firms, a set of firms for which accounting information is available. We assess the effect of 9/11 on firms' sales as a function firms' defense contractor status before the shock.[9] We compare the effect of 9/11 on the sales of defense contractors to those of

[9] As explained above, relying on the contractor status before the shock mitigates the possible concern that our results are driven by the Department of Defense endogenously allocating defense contracts after 2001 based on firms' activity, or that the results are driven by firms endogenously becoming government contractor after 9/11.

other firms with the following difference-in-differences regression:

$$\ln(\textit{Sales})_{it} = \beta(\textit{DoD Contractor}_i \times \textit{Post}_t) + \tau_t + \alpha_i + \epsilon_{it} \quad (1)$$

where i denotes firms and t denotes years, and α_i and τ_t are firm and year fixed effects. *DoD Contractor* is a dummy variable equal to one if the firm is a defense contractor – identified as a firm disclosing the Department of Defense as a major customer in the Compustat Segment Files – in 2000, and *Post* is a dummy variable equal to one after 2001. The results in column 1 indicate that on average defense contractors experience an 11% increase in sales following the defense spending shock, relative to other firms. Tightening the regressions with industry-year fixed effects that limit the comparisons to firms within the same industry produces a larger effect of 17% (column 2). In columns 3 and 4, we re-estimate the regressions on smaller firms with total assets below the sample median and find similar effects (a 14% to 21% increase in sales following 9/11). These results suggest that the increase in defense procurement spending following 9/11 is beneficial to defense contractors' sales.

Next, we move to the county-level to capture the effect on all firms in the local economy. We examine how county defense spending and county employment change from before to after 9/11 as a function of counties' exposure to defense spending before the shock. The regression that we estimate is:

$$Y_{ct} = \beta(\textit{County DoD Exposure}_c \times \textit{Post}_t) + \tau_t + \alpha_c + \epsilon_{ct} \quad (2)$$

where c denotes counties and t denotes years, and α_c and τ_t are county and year fixed effects. *County DoD Exposure* is the amount of defense spending per employee in the county in 2000 (scaled by its standard deviation to ease the interpretation of the coefficient), and *Post* is a dummy variable equal to one after 2001. As counties' exposure to defense spending is not random, we also augment this specification to allow counties with different characteristics that might be correlated with defense spending exposure (in-

cluding county population, average wage, share of employment in manufacturing, and unemployment rate) to be deferentially affected by 9/11. These saturated specifications ensure that any effect that we may find from defense spending exposure is not driven by these other characteristics.

Panel B of Table 1 displays the results of these regressions, equivalent to a first-stage in our empirical framework. The coefficient in column 1 indicates that, at the county-level, a one standard-deviation increase in defense spending exposure is associated with a statistically significant $27,000 increase in defense spending per employee after 9/11. Controlling for the interaction between pre-shock county characteristics and the *Post* variable does not affect this result. The results of columns 1 and 2 thus confirm the stickiness in the distribution of county-level defense spending. Moving to employment, we find, in columns 3 and 4 that a one standard-deviation increase in defense spending exposure is associated with a statistically significant 0.3% to 0.5% increase in employment after 9/11. Figure 3 illustrates the evolution of county employment over time as a function of county exposure to defense spending by plotting the coefficients on the interaction between *County DoD Exposure* and the year fixed effects. Until 2001 the coefficients are close to zero and insignficant, suggesting that the parallel trend assumption on which the test is based is likely met. Starting in 2002, the coefficients are positive, significant, and increasing, attesting of the positive effect of county defense spending exposure on local employment.

Appendix Figure A.3 further shows that the post-2001 employment effects of county defense spending exposure are concentrated in the 10% of counties most exposed to defense spending (i.e., counties in the 90th percentile of the DoD exposure distribution). The rest of the paper thus defines these 10% of counties most exposed to defense spending as "directly-shocked counties". These counties that are in the top 10% by defense spending exposure in 2000 received 75% of the defense spending increase between 2001 and 2005, and relative to other counties they experienced a 1.1% increase in employment after 9/11.

In sum, we find that the large and localized increase in defense procurement following

9/11 leads to measurable increase in sales and employment at the firm- and county-levels. In the next section, we assess how this increase in economic activity affects banks active in directly-shocked counties.

3.2 Bank Outcomes

After evaluating the direct effect of the defense stimulus on firms and counties, we assess how the increase in economic activity affects banks active in directly-shocked counties. In stimulating economic activity, the increase in defense spending at the local level could increase deposits (e.g., if firms accumulate extra cash stemming from an increase in local demand) and/or improve the quality of banks' loan book (e.g., if firms exploit the increase in demand to strengthen credit quality, for example by repaying debt or reducing reliance on debt funding).

Previous studies establish that local banks have an informational advantage in screening and monitoring borrowers, so that firms tend to borrow from local lenders (Petersen and Rajan (2002); Berger et al. (2005)). Banks with a strong branch presence in counties shocked by the defense stimulus are therefore particularly affected by the defense spending shock. We measure a bank's exposure to defense spending with the fraction of its branches located in directly-shocked counties.[10]

To assess how banks' non-performing loans and deposits change from before to after 2001 as a function of the banks' exposure to the defense spending shock, we estimate the following bank-level equivalent of equation 2:

$$Y_{bt} = \beta(\textit{Bank DoD Exposure}_b \times \textit{Post}_t) + \tau_t + \alpha_b + \Gamma X_i' \times \textit{Post}_t + \epsilon_{bt} \tag{3}$$

where b denotes banks and t denotes years, and α_b and τ_t are bank and year fixed effects. *Bank DoD Exposure* is the fraction of a bank's branches that are located in a directly-

[10] As discussed earlier, we classify counties in the top 10% of defense spending per capita in year 2000 as directly shocked. These counties account for 75% of the total defense spending increase.

shocked county in 2000. *Post* is a dummy variable that takes the value of one after 2001. The $\Gamma X' \times Post$ term controls for the possibility that various bank characteristics included in the vector X might be correlated with bank exposure to defense spending through the branch network (e.g., bank size) and deferentially affect banks' response to the shock. That is, we control for the interaction of each of bank size (logarithm of total assets), equity/assets, deposits/assets, real estate loans/assets, C&I loans/assets and the number of counties in which the bank has a branch presence – measured in 2000 – and the *Post* indicator. In addition, we allow banks' exposure to defense spending to be correlated with other types of county exposure stemming from the fact that counties exposed to defense spending may be different from other counties along several dimensions. We thus also calculate banks' exposure (through their branch network) to counties with differential proportion of employment in the manufacturing sector, size, unemployment rate, and average wage. Overall, the specification ensures that any significant effect we find on defense spending exposure through the branch network is not driven by these other banks' characteristics or other types of exposures.

Table 2 presents the results from these regressions. Columns 1 to 3 display the results for non-performing loans scaled by total assets, with and without the 'bank characteristics' and 'other exposures' controls interacted with the *Post* indicator. All three specifications indicate that at the bank-level a higher fraction of branches in directly-shocked counties is associated with a significant decline in non-performing loans after 2001. A 10 percentage point increase in the fraction of a bank's branches located in directly-shocked counties leads to a decrease in non-performing loans over assets of between 0.0012 and 0.0016 after the shock, which represents a 3% to 4% decrease in non-performing loans over assets relative to the sample mean. Figure 4 illustrates the dynamics of non-performing loans for banks with high and low exposures to directly-shocked counties. The average ratios of non-performing loans over assets for the two groups of banks follow parallel trends in the pre-shock period. The improvement in non-performing loans for highly exposed banks starts to become apparent in 2003.

Columns 4 to 6 of Table 2 assess the effect on deposits, also scaled by total assets. We do not find any discernible effect of banks' exposure to defense spending on deposits, possibly because firms do not respond to the local demand shock by accumulating cash. Across the three columns the coefficients are statistically insignificant and economically small relative to the sample mean.

In additional analyses presented in the appendix, we study from firms' perspective the improvement in credit quality that stems from the positive demand shock. Using accounting data available for publicly listed firms, we assess the evolution of financial leverage (debt financing over assets) and a proxy for credit quality (Altman's z-score, as in Chang et al. (2019)) as a function of firms' government contractor status. The results presented in Appendix Table A.2 indicate that defense contractors (defined, as above, as firms for which sales to the Department of Defense represent more than 10% of revenue) experience a statistically significant reduction in financial leverage and an increase in credit quality. These patterns also hold for the smaller firms of the sample (columns 2 and 4).

To sum up, the first stage of the analysis reveals a measurable effect of the 9/11 defense spending shock on county and bank outcomes. Directly-shocked counties experience increases in employment, and banks active in these counties through their branch network experience a significant reduction in non-performing loans. In the next section, we examine whether these banks further transmit the government spending shock to other counties.

4 Shock Transmission

4.1 Lending Spillovers

In this section, we evaluate how the government spending shock propagates through banks' branch networks, and affects lending to small businesses in counties that are not

directly shocked by the stimulus. From now on, the analysis therefore focuses on *not*-directly-shocked counties. The main test exploits the CRA lending data at the bank-county-year level and compares, in a given not-directly-shocked county, the local lending by banks as a function of their exposure to the defense spending shock through their branch network.

We start by presenting, in Table 3, the descriptive statistics for banks with high and low exposure to the defense spending shock through their branch network. As in Figure 4, banks with high (low) exposure are defined as those with an exposure greater (smaller) than the sample mean. The two groups of banks have similar levels of deposits/assets ratio, fraction of real estate loans, and non-performing loans/assets. Banks with high exposure are on average larger and have more C&I loans. The last column of Table 3 shows that, after controlling for the characteristics' levels, more and less exposed banks are on similar trends before the shock. Nonetheless, mirroring the bank-level tests of Section 3.2, the transmission tests below not only control for the level of the bank characteristics through fixed effects, but also for the possibility these characteristics are associated with a differential bank response to the defense spending shock through interacting the level of these characteristics before the shock and the *Post* indicator.

To assess if banks most exposed to the spending shock through their branch network transmit the shock to other counties where they operate branches, we estimate the following regression on the bank-county-year panel:

$$\ln\left(\textit{Credit Originated}\right)_{bct} = \beta(\textit{Bank DoD Exposure}_b \times \textit{Post}_t) + \Gamma Z_b' \times \textit{Post}_t + \zeta_{ct} + \eta_{bc} + \epsilon_{jst} \tag{4}$$

where b denotes banks, c denotes counties, and t denotes years. Thanks to the bank-county-year structure of the data, we absorb county-year fixed effects (ζ_{ct}), thus removing time-varying, county-level demand-side shocks related to business cycles, industry composition, housing, or industrial demand. As mentioned above, to further distinguish ef-

fects on credit supply from potentially confounding demand shocks, we restrict the analysis to *not*-directly-shocked counties that did not experience a substantial demand shock as a result of the national increase in defense spending after 9/11 (i.e., we focus on counties that are not in the top 10% by defense spending exposure in 2000). As in equation 3, the terms $\Gamma Z' \times Post$ control for the possibility that bank characteristics correlated with *Bank DoD Exposure* affect the results by interacting the post-shock indicator with bank characteristics measured in 2000 (including size, equity/assets, deposits/assets, real estate loans/assets, C&I loans/assets, initial amount of local small business loans, number of counties in which the bank has a branch presence, as well as the bank's exposure through the branch network to other county characteristics than defense spending exposure). Mirroring our empirical design for the county and bank level analyses, the treatment variable *Bank DoD Exposure* is measured before the shock and time-invariant for a given bank.

Table 4 reports the results of estimating equation 4. Across models, it displays a significant positive effect of bank exposure to the defense spending shock on the amount of small business credit originated in not-directly-shocked counties. Column 1 displays the result for models with only bank-county and year fixed effects. Column 2 estimates a more saturated model that control for county-level varying shocks through county-year fixed effects. Column 3 controls for the interactions between banks' initial characteristics and the 9/11 shock indicator, and column 4 controls for the interactions between banks' other exposures and the shock indicator. The within-county results in columns 3 and 4 indicate that in a given not-directly-shocked county, a 10 percentage point increase in a bank's fraction of branches located in directly-shocked counties increases local credit supply to small firms by about 9% after 9/11. Figure 5 illustrates the dynamics of the effect year-by-year and confirms the absence of apparent pre-trends by plotting the coefficients on the interaction between *Bank DoD Exposure* and the time fixed effects. Overall, these results suggest that banks transmit the initially localized spending shock through the wider economy via their branch network both across sectors and spatially.

One potential concern with these findings is that the increase in small business lending

by banks in not-directly-shocked counties stems from an increase in lending as a response to higher credit demand by local firms in the supply chain that cater to defense contractors. This is unlikely to be the case as it would require that the distribution of supply chain networks is systematically correlated with the distribution of bank branches. Nevertheless, we further tackle this concern in two ways. First, we incorporate in our regression finer controls for banks' exposure through branches to the industrial specialization of the counties in which they operate branches. That is, we compute the share of employment in each three-digit manufacturing industry and in construction in each county where the bank has branches, take the average weighted by the number of branches in the county, and interact these exposures with the $Post$ indicator. When adding these controls to our regressions, we effectively compare similar banks with similar exposure through the branch network to manufacturing and construction industrial structure (in addition to other county characteristics), but a differential exposure to the defense spending shock. For example, these regressions compare banks with similar exposure to areas specialized in, say, machinery manufacturing, but different exposure to defense spending. The results, presented in column 5, are robust to including these additional controls. The coefficient indicates that in these regressions a 10 percentage point increase in a bank's fraction of branches located in directly-shocked counties increases local credit supply to small firms by about 7.5% after 9/11.

Second, we examine the degree to which bank lending to the household sector – which is independent of the supply chain – increases. In Table 5 we show that the lending spillovers are not limited to small business lending. Using bank-county data on mortgage lending from HDMA disclosure files, we find that banks also transmit the spending shock in terms of mortgage lending. Like Gilje et al. (2016), we find particularly strong effects for the types of loans that are less frequently securitized (home purchases and home equity loans). This additional result further supports the notion that the increase in small business lending that we document in not-directly-shocked counties is unlikely to be driven by an increase in loan demand by small businesses supplying government

contractors and located in other counties, which would also benefit from an increased demand following the spending shock.

Overall, the results indicate that banks active in directly-shocked counties experience a reduction in non-performing loans and transmit the spending shock by increasing credit supply in other counties where they operate branches.

4.2 Banks' Constraints

A reduction in non-performing loans can particularly allow capital constrained banks to extend lending in not-directly-shocked areas. Lending by unconstrained banks is less likely to respond to the improvement in the loan book as these banks should be able to satisfy any unmet loan demand. This section investigates the differential effect of the transmission of the government spending shock by constrained and unconstrained banks.

Table 6 reports the results of estimating the credit supply regressions (equation 4) separately for constrained and unconstrained banks. Following Chakraborty et al. (2018), given the skewed distribution of bank capital, we divide banks into quintiles of equity over assets ratio and classify banks in the top quintile as unconstrained and the remaining as constrained. We find material differences in the transmission of the government spending shock across the two groups. For constrained banks, the coefficient on bank exposure is positive and significant: a 10-percentage point exposure to the government spending shock makes them expand originated credit by about 6-7% (columns 1 to 3). For unconstrained banks, we do not find any discernible effect: the coefficients are close to zero and statistically insignificant (columns 4 to 6). The difference in the effect between constrained and unconstrained banks is statistically significant at the 5% level. This finding is consistent with the notion that bank constraints play an important role in the transmission of the government spending shock.

These results on the role of bank constraints also suggest that the transmission of the spending shock is likely to be greater in bad times, when the banking sector is likely to be

more constrained. In other words, constraints in the intermediary sector can play a role for the transmission of a stimulus through the economy.

4.3 Real Effect and Multiplier Quantification

After establishing the propagation of the defense spending shock through banks' branch networks, we end by studying the real effects of the stimulus transmission in not-directly-shocked counties, and quantify the potential amplification of the national increase in defense spending through banks' branch networks. We focus on not-directly-shocked counties and assess these counties' growth in small business lending and small business employment through the exposure of their local banks to other directly-shocked counties.

For each not-directly-shocked county, we calculate the county's indirect exposure to defense spending that comes from the exposure of banks operating branches in that county. We weight each local bank's exposure to defense spending through their branch network by the share of the county's branches that the bank owns. Formally, a county's indirect exposure to defense spending through its banks' branch networks is:

$$\textit{County DoD exposure through banks}_c = \sum_{b=1}^{N_c} \omega_{b,c} \times \textit{Bank DoD exposure}_b$$

where b denotes banks and c denotes counties. ω represents the fraction of branches of county c that are owned by bank b, and *Bank DoD exposure* is the fraction of bank b's branches that are in directly-shocked counties.

Mirroring the regressions in Panel A of Table 1, we then regress county small business lending and small business employment on the interaction of the county indirect exposure to defense spending measure and the post-9/11 indicator, controlling for the interactions between pre-shock county characteristics (which, here, also includes the small direct exposure to defense spending that counties below the 90th percentile may have).

Formally, the regression we estimate takes the form:

$$Y_{ct} = \beta(\textit{County DoD exposure through banks}_c \times \textit{Post}_t) + \Psi Z_c' \times \textit{Post}_t + \nu_c + \tau_t + \epsilon_{ct} \quad (5)$$

where Y represents the outcome of interest, Z a vector a county characteristics – that includes, as before, population, average wage, share of employment in manufacturing, and unemployment rate – ν the county fixed effects, and τ the year fixed effects.

As county-year fixed effects cannot be included in these county-level regressions, identification is looser here than in the spillover analysis that could control for time-varying county characteristics through the fixed effects. For example, a possible concern in these regressions is that a not-directly-shocked county with a large presence of DoD-exposed banks may be geographically close to directly-shocked counties, such that county outcomes may be affected through, for example, a local trade channel. We address this concern by further controlling for the possible role of the industrial structure of the spillover counties (i.e., the interaction between the shock indicator and the shares of employment in each of the three-digit NAICS manufacturing industries and in construction) or the physical distance between the spillover county and the three nearest directly-shocked-counties.[11] These additional controls ensure that the results are not driven by a few industries directly supplying firms in a close directly-shocked county or by the possible correlation between local banks' exposure to directly-shocked counties and the physical distance to these counties.

Columns 1 to 5 of Table 7 presents the results of estimating these regressions. Panel A displays the effect of credit to small firms and Panel B the effect on small firm employment. In the not-directly-shocked spillover counties, a 10 percentage point increase in indirect exposure to defense spending through banks' branches is associated with a 2.1%-3.3% increase in small business lending (Panel A), and a 0.6%-1.2% increase in small business employment (Panel B) after the shock.

[11] Data on the physical distance between counties is from the NBER County Distance Database (www.nber.org/research/data/county-distance-database)

In column 5 of Table 7, we perform a further test and exclude – in addition to the directly-shocked counties themselves – the not-directly-shocked counties located geographically close to the directly-shocked counties to ensure that the amplification of the fiscal stimulus is not limited to the counties closest to the directly-shocked ones. Specifically, we remove from the sample the not-directly-shocked counties located within 50 kilometers of a directly-shocked county (as measured in the NBER County Distance database) which, at the median, is equivalent to removing counties that have a directly-shocked county among their five closest counties.[12] We find that the results are robust to such restriction. Finally, in Appendix Table A.3, we verify that the amplification effect is concentrated in counties for which local banks exposed to directly-shocked counties represent a substantial fraction of the local banking market, that is counties at the top of indirect exposure distribution (since *County DoD exposure through banks* will be closer to one when the exposed banks have a local greater market share), and show that the results are also robust to excluding counties that are geographically adjacent to a directly-shocked county.

To gauge of the multiplicative effect of defense spending on small business employment, we then relate the estimated effect on small business employment to the magnitude of the post-9/11 aggregate defense spending shock. Aggregate national defense procurement spending increased by \$34bn (or 51%) between the pre and post periods (from an average of \$67bn in the pre-period to an average of \$102bn in the post-period). Given that national small business employment is on average 17.3m during the pre-period, we estimate that in aggregate counties' indirect exposure to defense spending through banks' branches is associated with about 93,500 jobs in small firms.[13] This translates in a defense spending increase per job of about \$350,000 (i.e., each \$350,000 increase in defense spending is associated with an extra job in a small firm).[14] As noted by Guren et al. (2021)

[12] In the NBER County Distance database, for the median county the fifth nearest county is located 50 km away.

[13] Counties' mean indirect exposure to defense spending through their banks' branches is 9 percentage points. Using the estimates in column 4 of Table 7, the estimated number of jobs created in small businesses is thus estimated at about $0.06 \times 0.09 \times 17.3\text{m} = 93{,}420$.

[14] \$34bn/93,420 =\$363,948.

or Nakamura and Steinsson (2018), these effects represent a combination of partial equilibrium effects and local general equilibrium effects. The results thus suggest that the amplification of the fiscal stimulus through the banking system can be as large as 10%-15% of the fiscal multipliers estimated in the literature when studying the direct effect of a stimulus (see, for example, Chodorow-Reich (2019) for a survey of the literature on local multipliers).

5 Conclusion

In this paper, we study the propagation of a government spending shock on the real economy through the banking system. Exploiting shifts in US defense procurement spending that followed the 9/11 attacks, we first show that counties most exposed to the spending shock experience measurable increases in employment relative to other counties. Following the increase in local economic activity, banks with a larger branch network in the most exposed counties experience a decline in non-performing loans. This improvement in loan book quality then contributes to banks propagating the government spending shock to other areas. Banks active in directly-shocked counties increase lending to small firms in other counties initially unaffected by the shock. This response is driven capital-constrained banks. The spatial lending spillovers also have a multiplicative effect. Small business credit and small business employment increase in counties indirectly exposed to the spending shock through their local banks' branch networks. Overall, we estimate that an increase of about $350,000 in defense spending is associated with an additional job in small businesses through this credit multiplier.

Together, the findings suggest that targeted fiscal stimulus can generate spillovers to other areas of the economy through the banking system, thereby also amplifying the original intervention. The results also suggest that the extent of a stimulus transmission and its amplification is likely to depend on the degree of constraints in the banking sector. As financial intermediaries are typically more constrained when the economy is doing

poorly, the findings thus suggest an important potential role played by financial intermediaries in the transmission and the effectiveness of a fiscal stimulus.

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Table 1: First Stage: Direct of Effect of the 9/11 Defense Spending Shock

This table displays the results of firm-level (Panel A) and county-level (Panel B) regressions assessing the effect of the 9/11 Department of Defense (DoD) procurement spending shock as a function of firms' and counties' pre-existing DoD spending exposure in year 2000. In Panel A, we assess the effect of 9/11 on the evolution of firms' sales as a function of firms' defense contractor status before the shock. *DoD Contractor* is a dummy variable equal to one for firms deriving more than 10% of revenue from sales to the Department of Defense in year 2000. *Sales* is the dollar amount of yearly sales. Columns 1 and 3 of Panel A display the regression results for all firms. Columns 2 and 4 of Panel A display the regression results for small firms, defined as those with average total assets smaller than the sample median. In Panel B, we assess the effect of 9/11 on the evolution of county defense spending per employee (columns 1 and 2) and county employment (columns 3 and 4) as a function of counties' exposure to defense spending before the shock. *DoD spending per employee* is the dollar amount of defense procurement spending in the county scaled by the number of employees in the county. *County DoD exposure* is the amount of DoD spending per employee in year 2000. It is scaled by its standard deviation to ease the interpretation of the coefficients. County *employment* is the annual average of monthly employment for a given year. *Post* is a dummy variable equal to one for the years after 2001. Firm sales and county employment are expressed in natural logarithm. Abs.= Absorbed. Standard errors presented in parentheses are clustered at the firm level in the regressions underlying the results in Panel A and at the county level for those underlying the results in Panel B. *, **, and *** denote significance at 10%, 5%, and 1% level.

Panel A: Firm-level panel (listed firms)

	Sales			
	All Firms		Small Firms	
	(1)	(2)	(3)	(4)
DoD Contractor × Post	0.106**	0.174***	0.135*	0.206**
	(0.053)	(0.065)	(0.078)	(0.096)
Firm FE	Yes	Yes	Yes	Yes
Year FE	Yes	Abs.	Yes	Abs.
Industry FE × Year FE	-	Yes	-	Yes
Observations	31054	31054	15522	15522

Panel B: County-level panel (all firms)

	DoD Spending per Employee ($k)		Employment	
	(1)	(2)	(3)	(4)
County DoD exposure × Post	27.5***	27.1***	0.005***	0.003**
	(4.9)	(5.0)	(0.001)	(0.001)
County FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
County char. × Post	-	Yes	-	Yes
Observations	24544	24544	24544	24544

Table 2: Banks' Non-Performing Loans and Deposits

This table displays the results of bank-level panel regressions assessing the effect of the 9/11 Department of Defense (DoD) procurement spending shock on the evolution of banks' non-performing loans and deposits, as a function of banks' pre-existing DoD spending exposure in year 2000. *Bank DoD exposure* is measured as the fraction of a bank's branches that are in counties in the top 10% by *County DoD exposure* (measured as county DoD spending per employee in year 2000). These counties received 75% of the total defense procurement spending increase after 9/11. *NPL/Assets* is the ratio of dollar amount of non-performing loans (item rcfd1403 in the Call Reports) to total assets (item rcfd2170 in the Call Reports). *Deposits/Assets* is the ratio of bank deposits (item rcfd2200 in the Call Reports) to total assets. Bank pre-shock characteristics (denoted *Bank char.*) include year 2000 log(assets), equity/assets, deposits/assets, real estate loans/assets, C&I loans/assets and number of counties in which the bank has a branch presence. *Bank pre-shock other exposures* include each of the weighted averages of county unemployment rate, log(employment), share of employment in the manufacturing sector, and average wage, across all counties in which the bank has a branch and where the average's weights are the bank's fraction of branches located in each county. *Post* is a dummy variable equal to one for the years after 2001. NPL = Non-performing loans. Standard errors presented in parentheses are clustered at the bank level. *, **, and *** denote significance at 10%, 5%, and 1% level.

Bank-level panel

	NPL/Assets			Deposits/Assets		
	(1)	(2)	(3)	(4)	(5)	(6)
Bank DoD exposure × Post	-0.0016***	-0.0015***	-0.0012**	-0.0046	-0.0035	-0.0043
	(0.0006)	(0.0006)	(0.0005)	(0.0063)	(0.0062)	(0.0063)
Banks FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Bank char. × Post	-	Yes	Yes	-	Yes	Yes
Bank other exposure × Post	-	-	Yes	-	-	Yes
Observations	6378	6378	6352	6378	6378	6352
Mean of dependent variable		0.0040			0.7900	

Table 3: Banks' Descriptive Statistics for the Transmission Analysis

This table presents descriptive statistics for the sample of banks active in counties not directly shocked by the defense spending stimulus. The statistics are calculated on the average of the years between 1998 and 2000. *Total assets* is the dollar amount bank total assets (item rcfd2170 in the Call Reports). *Deposits/Assets* is the ratio of bank deposits (item rcfd2200 in the Call Reports) to total assets. *Fraction of real estate loans* is the ratio of dollar amount of real estate loans (item rcfd1410 in the Call Reports) to total loans (item rcfd1400 in the Call Reports). *Fraction of C&I loans* is the ratio of dollar amount of commercial and industrial loans (item rcfd1766 in the Call Reports) to total loans. *NPL/Assets* is the ratio of dollar amount of non-performing loans (item rcfd1403 in the Call Reports) to total assets. *Equity/Assets* is the ratio of dollar amount of bank equity (item rcfd3210 in the Call Reports) to total assets. *Credit to small firms* is the dollar amount of small business loan originations to businesses with $1 million or less in annual gross revenues. *Bank DoD exposure* is measured as the fraction of a bank's branches that are in counties in the top 10% by *County DoD exposure* (measured as county DoD spending per employee) in year 2000. Banks with high DoD exposure are defined as the banks with *Bank DoD exposure* greater than the sample mean (0.18). In the last two columns, we assess whether *Bank DoD exposure* is associated with differences in bank characteristics and trends in these characteristics on the pre-period. The last two columns report, respectively, the p-value for the coefficient on *Bank DoD exposure* in a regression of (i) the characteristic's level on *Bank DoD exposure* and (ii) the characteristic's pre-period growth rate on *Bank DoD exposure* and the level of banks' characteristics before the shock.

Bank-level statistics on the pre-period: Banks active in not-directly-shocked counties

	Banks with low DoD spending exposure through branches (N=692)			Banks with high DoD spending exposure through branches (N=285)			p-value for diff. in means	p-value for diff. in pre-period growth rates
	Mean	Median	S.D.	Mean	Median	S.D.		
Total assets ($m)	3327	506	19400	6230	778	34100	0.005	0.269
Deposits/Assets	0.80	0.81	0.09	0.79	0.81	0.09	0.528	0.787
Fraction of real estate loans	0.63	0.64	0.18	0.62	0.63	0.19	0.932	0.383
Fraction of C&I loans	0.18	0.16	0.12	0.21	0.18	0.13	0.001	0.512
NPL/Assets	0.003	0.003	0.003	0.004	0.003	0.003	0.325	0.136
Equity/Assets	0.088	0.083	0.022	0.089	0.084	0.023	0.414	0.297
Credit to small firms ($m)	50.00	23.03	150.764	68.66	22.94	168.38	0.034	0.809

Table 4: Transmission of the Spending Shock at the Bank-County Level: Lending Spillovers

This table displays the results of bank-county-level panel regressions assessing the spillover effects of banks' exposure to the 9/11 Department of Defense (DoD) spending shock on their small business lending in not-directly-shocked counties. The regressions compare small business lending of banks differently exposed to DoD spending. This comparison is done within the same county (through County×Year fixed effects). *Bank DoD exposure* is measured as the fraction of branches that a bank has in the directly shocked counties (i.e., counties in the top 10% by *County DoD exposure*) in 2000. The regressions are estimated on not-directly-shocked counties, that is counties that are not in the top 10% of *County DoD Exposure*. Bank pre-shock characteristics (denoted *Bank char.*) include year 2000 log(assets), equity/assets, deposits/assets, real estate loans/assets, C&I loans/assets, initial amount of local small business loans and number of counties in which the bank has a branch presence. *Bank other exposures* include each of the weighted averages of county unemployment rate, log(employment), share of employment in the manufacturing sector, and average wage, across all counties in which the bank has a branch and where the average's weights are the bank's fraction of branches located in each county. *Bank industrial structure exposure* includes the weighted averages of county share of employment in each 3-digit NAICS manufacturing industry and in construction, across all counties in which the bank has a branch and where the average's weights are the bank's fraction of branches located in each county. *Credit to small firms* is the dollar amount of small business loan originations to businesses with $1 million or less in annual gross revenues, measured at the bank-county-year level and expressed in natural logarithm. *Post* is a dummy variable equal to one for the years after 2001. Abs. = Absorbed. Standard errors presented in parentheses are clustered at the bank level. *, **, and *** denote significance at 10%, 5%, and 1% level.

Bank-county panel (in not-directly-shocked counties)

	Credit to small firms				
	(1)	(2)	(3)	(4)	(5)
Bank DoD exposure × Post	1.194**	1.252**	0.923***	0.908***	0.746***
	(0.505)	(0.517)	(0.300)	(0.259)	(0.193)
Bank-County FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Abs.	Abs.	Abs.	Abs.
County FE × Year FE	-	Yes	Yes	Yes	Yes
Bank char. × Post	-	-	Yes	Yes	Yes
Bank other exposures × Post	-	-	-	Yes	Yes
Bank indus. structure exposure × Post	-	-	-	-	Yes
Observations	204202	204202	204202	204152	204152

Table 5: Lending Spillovers: Mortgage Lending

This table displays the results of bank-county-level panel regressions assessing the spillover effects of banks' exposure to the 9/11 Department of Defense (DoD) spending shock on their mortgage lending in not-directly-shocked counties. The regressions compare mortgage lending of banks differently exposed to DoD spending. This comparison is done within the same county (through County×Year fixed effects). *Bank DoD exposure* is measured as the fraction of branches that a bank has in the directly exposed counties (i.e., counties in the top 10% by *County DoD exposure*) in 2000. The regressions are estimated on not-directly-shocked counties, that is counties that are not in the top 10% of *County DoD Exposure*. Bank pre-shock characteristics (denoted *Bank char.*) include year 2000 log(assets), equity/assets, deposits/assets, real estate loans/assets, C&I loans/assets, initial amount of local mortgage lending and number of counties in which the bank has a branch presence. *Bank pre-shock other exposures* include each of the weighted averages of county unemployment rate, log(employment), share of employment in the manufacturing sector, and average wage, across all counties in which the bank has a branch and where the average's weights are the bank's fraction of branches located in each county. *Mortgage lending* is the dollar amount of conventional mortgage originations made by a given bank in a given county, expressed in natural logarithm. Column 1 presents the results for mortgages originated for home purchase, column 2 for home equity loans, and column 3 for refinancing. *Post* is a dummy variable equal to one for the years after 2001. Abs. = Absorbed. Standard errors presented in parentheses are clustered at the bank level. *, **, and *** denote significance at 10%, 5%, and 1% level.

Bank-county panel (in not-directly-shocked counties)

	Mortgage lending		
	Home purchases	Home equity loans	Refinancing
	(1)	(2)	(3)
Bank DoD exposure × Post	0.446**	0.672***	0.151
	(0.214)	(0.239)	(0.290)
Bank-County FE	Yes	Yes	Yes
County FE × Year FE	Yes	Yes	Yes
Bank char. × Post	Yes	Yes	Yes
Bank other exposures × Post	Yes	Yes	Yes
Observations	260238	114670	275340

Table 6: The Role of Banks' Constraints in the Transmission of the Spending Shock

This table displays the results of bank-county-level panel regressions assessing the spillover effects of banks' exposure to the 9/11 Department of Defense (DoD) spending shock on their small business lending in not-directly-shocked counties. The regressions compare small business lending of banks differently exposed to DoD spending. This comparison is done within the same county (through County×Year fixed effects). *Bank DoD exposure* is measured as the fraction of branches that a bank has in the directly shocked counties (i.e., counties in the top 10% by *County DoD exposure*) in 2000. The regressions are estimated on not-directly-shocked counties, that is counties that are not in the top 10% of *County DoD exposure*. Columns 1 to 3 (4 to 6) report the results on the sub-sample of constrained (unconstrained) banks. Banks are classified as unconstrained if they are in the top quintile by equity/assets, measured in year 2000, and as constrained otherwise (Chakraborty et al. (2018)). Bank pre-shock characteristics (denoted *Bank char.*) include year 2000 log(assets), equity/assets, deposits/assets, real estate loans/assets, C&I loans/assets, initial amount of local small business loans and number of counties in which the bank has a branch presence. *Bank pre-shock other exposures* include each of the weighted averages of county unemployment rate, log(employment), share of employment in the manufacturing sector, and average wage, across all counties in which the bank has a branch and where the average's weights are the bank's fraction of branches located in each county. *Bank industrial structure exposure* includes the weighted averages of county share of employment in each 3-digit NAICS manufacturing industry and in construction, across all counties in which the bank has a branch and where the average's weights are the bank's fraction of branches located in each county. *Credit to small firms* is the dollar amount of small business loan originations to businesses with $1 million or less in annual gross revenues, measured at the bank-county-year level and expressed in natural logarithm. *Post* is a dummy variable equal to one for the years after 2001. Abs. = Absorbed. Standard errors presented in parentheses are clustered at the bank level. *, **, and *** denote significance at 10%, 5%, and 1% level.

Bank-county panel (in not-directly-shocked counties)

	Credit to small firms					
	Constrained banks			Unconstrained banks		
	(1)	(2)	(3)	(4)	(5)	(6)
Bank DoD exposure × Post	0.738**	0.696***	0.605***	0.042	0.034	0.002
	(0.294)	(0.241)	(0.178)	(0.186)	(0.190)	(0.194)
Bank-County FE	Yes	Yes	Yes	Yes	Yes	Yes
County FE × Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Bank char. × Post	Yes	Yes	Yes	Yes	Yes	Yes
Bank other exposures × Post	-	Yes	Yes	-	Yes	Yes
Bank indus. structure exposure × Post	-	-	Yes	-	-	Yes
Observations	163651	163634	163634	40551	40518	40518
t-stat col. 2 vs. col. 4	2.42**					

Table 7: Quantifying the Amplification of the Spending Shock through Banks' Branches at the County-Level

This table displays the results of county-level panel regressions assessing the effect of local banks' exposure to the 9/11 Department of Defense (DoD) spending shock on county small business lending and county employment. *County DoD exposure through banks* is measured as the weighted average of the county banks' fraction of branches in directly-shocked counties, where the average's weights are the market shares of each bank in the county. The regressions are estimated on not-directly-shocked counties, that is counties that are not in the top 10% of *County DoD Exposure*. County pre-shock characteristics (denoted *County char.*) include year 2000 county unemployment rate, log(employment), share of employment in manufacturing, average wage, and direct exposure to the spending shock (*County DoD Exposure*). County industrial structure (denoted *County indus. structure*) includes the county share of employment in each 3-digit NAICS manufacturing industry and in construction. County distance to the nearest directly-shocked counties is measured as the sum of the distance between the focal county and the three-nearest directly-shocked counties. Distances between counties are from the NBER County Distance database. Results in column 5 are based on regressions that also exclude the not-directly-shocked counties located within 50 kilometers of a directly-shocked county (as measured in the NBER County Distance database) which, at the median, is equivalent to removing counties that have a directly-shocked county among their five closest counties. *Credit to small firms* is the dollar amount of small business loan originations to businesses with $1 million or less in annual gross revenues, aggregated at the county-year level and expressed in natural logarithm. *Small firm employment* is the county fourth quarter employment count in firms with less than 20 employees. Standard errors presented in parentheses are clustered at the county level. *, **, and *** denote significance at 10%, 5%, and 1% level.

County-level panel (not-directly-shocked counties)

	County outcome				
	(1)	(2)	(3)	(4)	(5)
Panel A: Credit to small firms					
County DoD exposure through banks × Post	0.206**	0.324***	0.241***	0.333***	0.334***
	(0.088)	(0.095)	(0.095)	(0.097)	(0.122)
Observations	20096	20096	20096	20096	14912
Panel B: Small firm employment					
County DoD exposure through banks × Post	0.117***	0.092***	0.058**	0.092***	0.073**
	(0.023)	(0.025)	(0.025)	(0.025)	(0.031)
Observations	18182	18182	18182	18182	13499
County FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
County char. × Post	-	Yes	Yes	Yes	Yes
County indus. structure × Post	-	-	Yes	-	-
Distance to nearest shocked counties × Post	-	-	-	Yes	-
Exclude counties close to shocked counties	-	-	-	-	Yes

Figure 1: US Defense Procurement Spending

This figure plots the US aggregate national defense procurement spending in 2017 dollars.

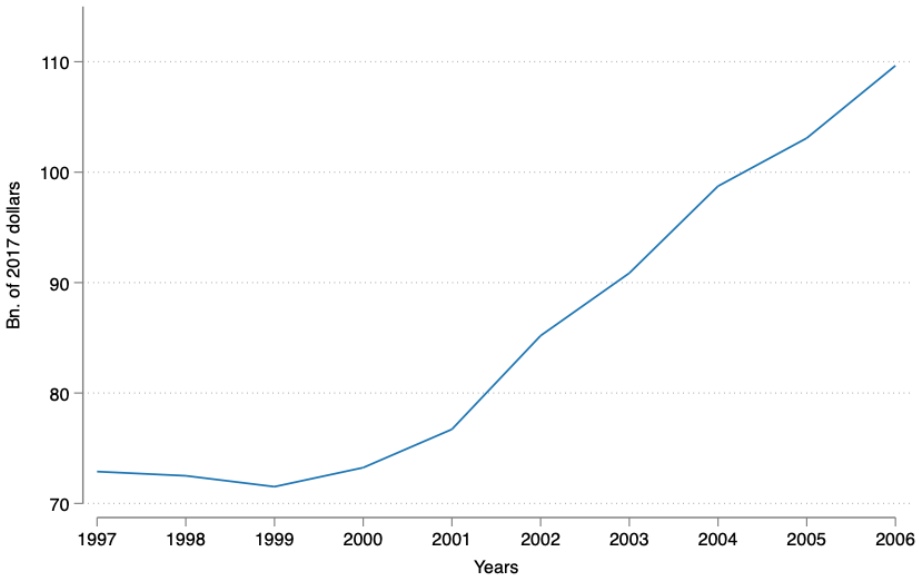


Figure 2: Spatial Distribution of Defense Spending Growth following 9/11

This figure displays the growth in defense procurement spending between 2001 and 2005 at the county level. Counties colored according to the decile in which such defense spending growth lies. Counties in the top decile are colored in red.

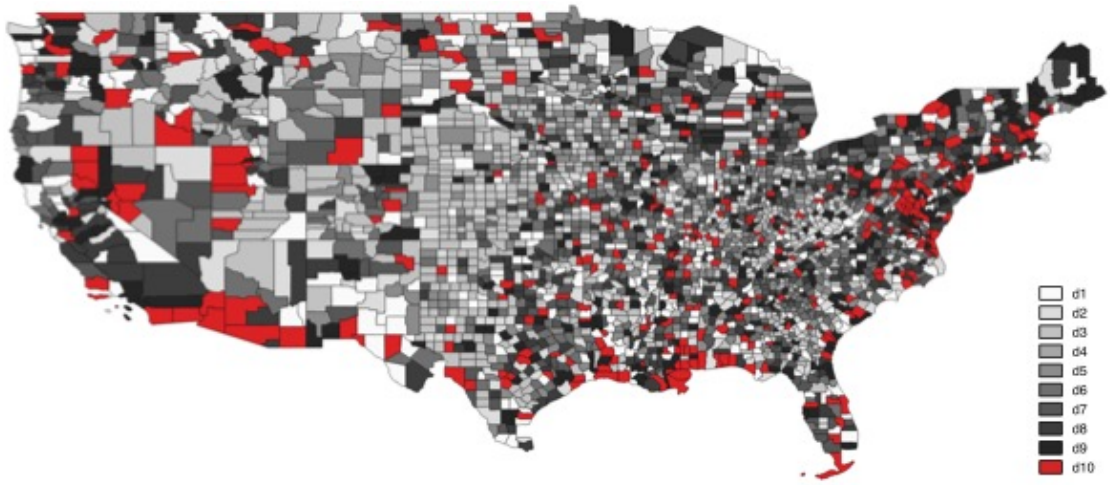


Figure 3: Defense Procurement Spending Shock: County Employment

This figure shows the coefficients on *County DoD exposure* interacted with the year fixed effects, in a regression of the logarithm of county employment on county fixed effects, year fixed effects, *County DoD exposure* interacted with year fixed effects, and pre-shock county characteristics interacted with the *Post* indicator. *County DoD exposure* is measured as local defense procurement spending per employee in 2000 and is scaled by its standard deviation to ease interpretation. County *employment* is the annual average of monthly employment for a given year. The vertical bars represent 95% confidence intervals, with standard errors clustered at the county level.

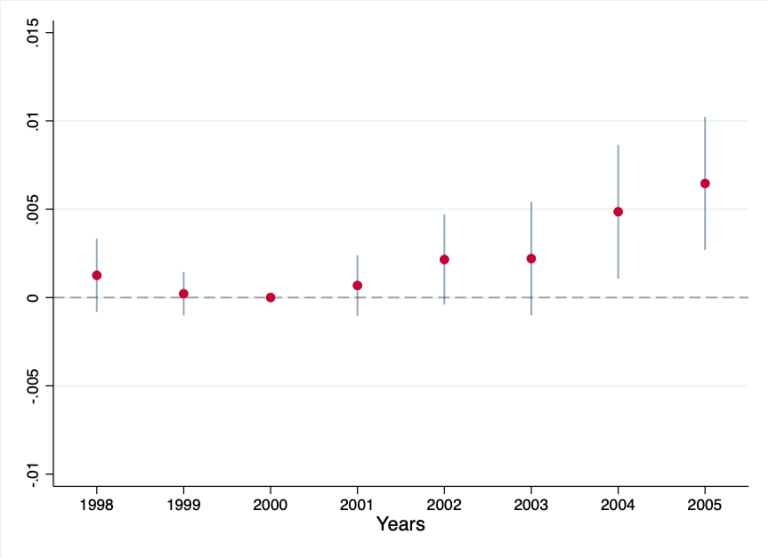


Figure 4: Banks' Non Performing Loans

This figure shows the coefficients on the year fixed effects, in a regression of banks' non-performing loans over assets (*NPL/Assets*) on year fixed effects. The regressions are estimated separately for banks with *Bank DoD exposure* higher and lower than the mean. *Bank DoD exposure* is measured as the fraction of branches that a bank has in the directly shocked counties (i.e., counties in the top 10% by *County DoD exposure*) in 2000. *NPL/Assets* is the ratio of dollar amount of non-performing loans to total assets. Dotted vertical lines represent 95% confidence intervals, with standard errors clustered at the bank level.

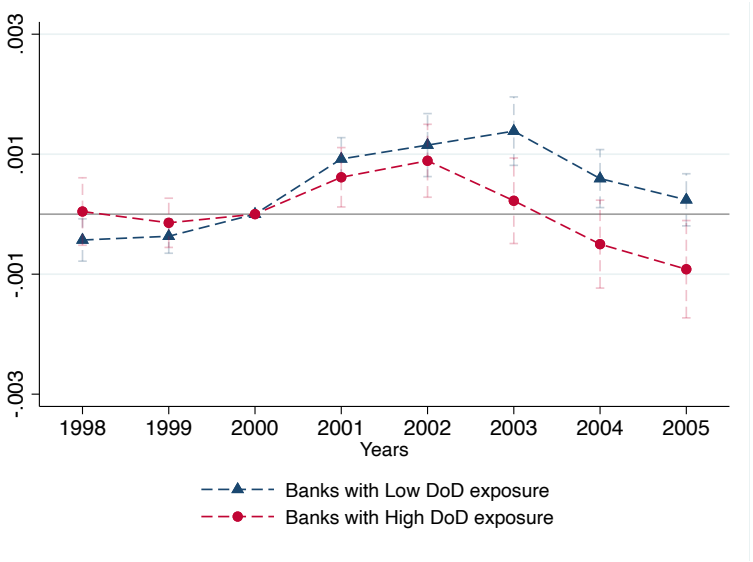
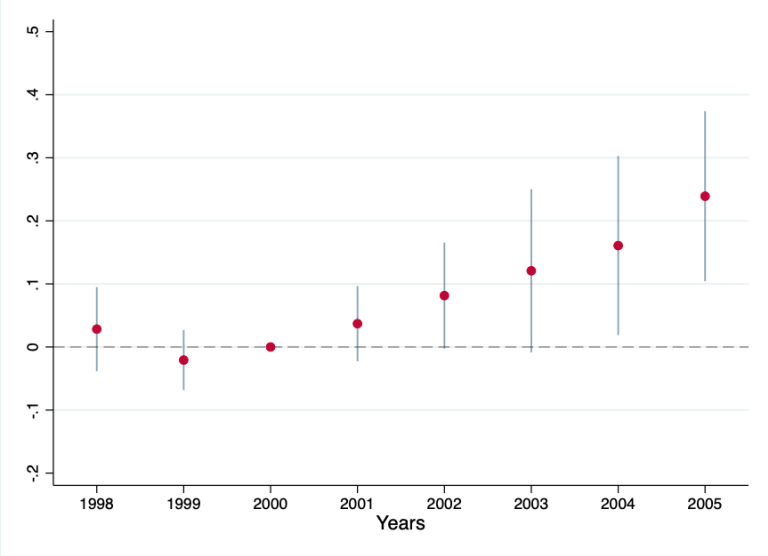


Figure 5: Defense Procurement Spending Shock: Lending Spillovers through Banks' Branch Networks

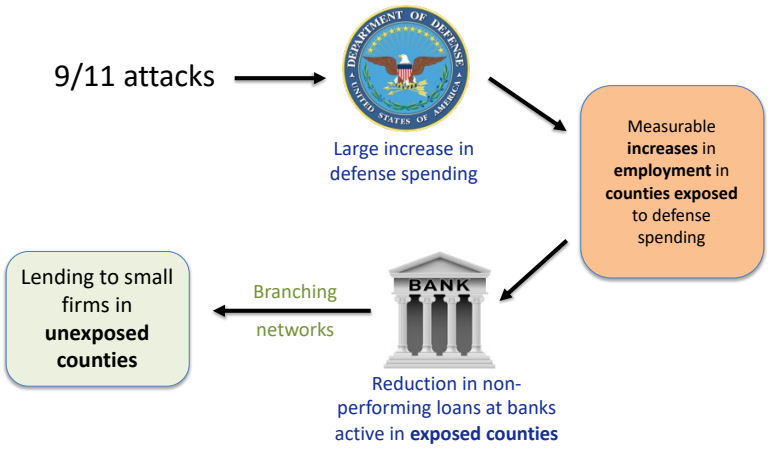
This figure shows the coefficients on *Bank DoD exposure* interacted with the year fixed effects, in a regression of the logarithm of small firm credit on bank-county fixed effects, county-year fixed effects, and *Bank DoD exposure* interacted with year fixed effects. *Bank DoD exposure* is measured as the fraction of branches that a bank has in the directly-shocked counties (i.e., counties in the top 10% by *County DoD exposure*) in 2000. The regressions are estimated on not-directly-shocked counties, that is counties that are not in the top 10% of *County DoD Exposure*. The coefficients are scaled to represent the effect of a 10 percentage point increase in *Bank DoD exposure*. The vertical bars represent 95% confidence intervals, with standard errors clustered at the county level.



ONLINE APPENDIX
for
"Amplifying a Fiscal Stimulus: The Role of Banks"

Figure A.1: Representation of the Framework

(a) Transmission of the 9/11 Spending Shock



(b) Econometric Framework

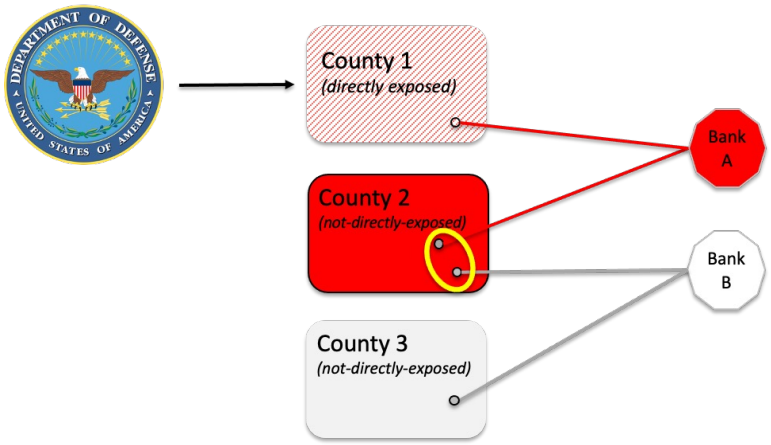


Figure A.2: County Defense Spending Exposure in 2000 and Post-9/11 Defense Spending Growth

This figure displays the scatter plot of *County DoD exposure* in 2000 (x-axis) and post-9/11 defense spending growth per capita between 2001 and 2006 (y-axis). *County DoD exposure* is measured as local defense procurement spending per employee in 2000. For clarity, the scatter plot represents each variable trimmed at 1% in each tail.

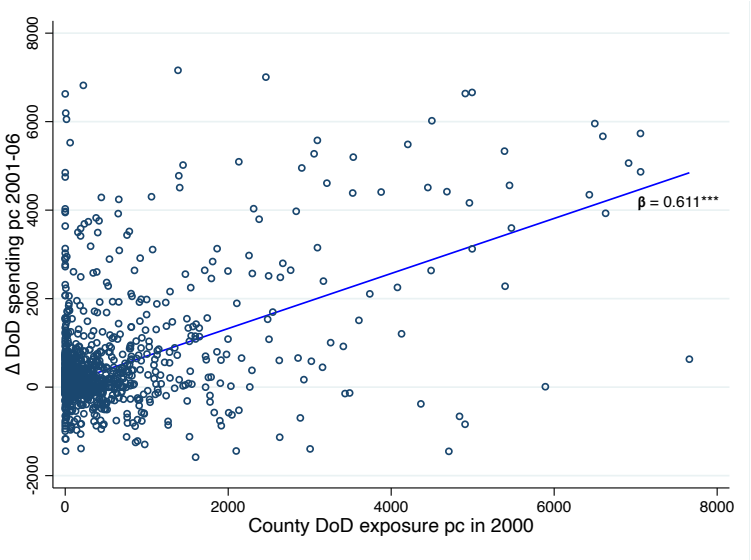


Figure A.3: Effect on County Employment with Discrete Exposure Definitions

This figure displays the coefficients of county-level panel regressions assessing the effect of the 9/11 Department of Defense (DoD) procurement spending shock on county employment with discrete county DoD shock definitions (i.e., dummy variables equal to one if *County DoD exposure* in year 2000 is, respectively, greater than the 50th, 75th, or 90th percentile of the county exposure distribution). *County DoD exposure* is measured as local defense procurement spending per employee in 2000. County employment is expressed in natural logarithm.

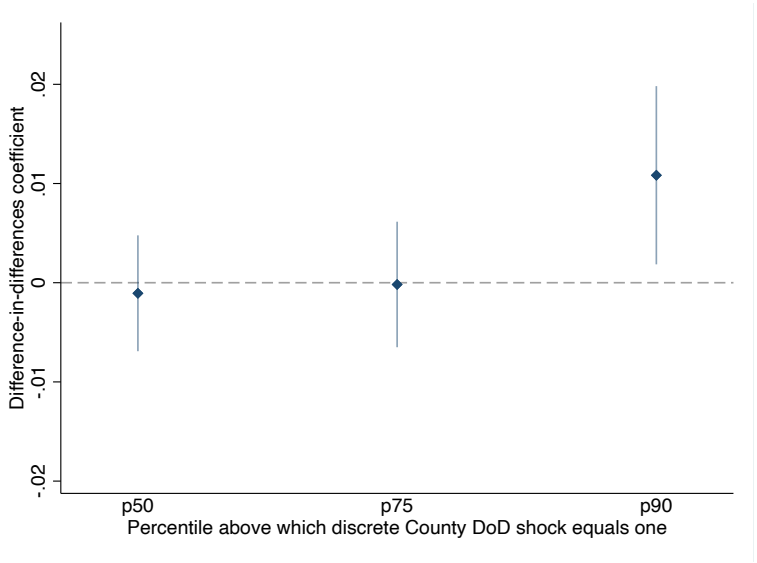


Table A.1: County Descriptive Statistics

This table presents descriptive statistics for the sample of counties not directly exposed to the defense spending shock. The employment and average wage statistics are calculated on the average of the years before the shock. *Employment* is the annual average of monthly employment for a given year. *Average wage* is the average wage based on the 12-monthly employment levels and total annual wage levels in the county. *Share of employment in the manufacturing sector* is the fraction of employee employed in the manufacturing sector in year 2000. *Unemployment rate* is the county unemployment rate in year 2000 as defined by the Bureau of Labor Statistics.

Not-directly-shocked counties (N=2761)

	Mean	S.D.
Employment	28317	71,268
Average wage	23825	4873
Share of employment in manufacturing sector	0.183	0.125
Unemployment rate	4.321	1.600

Table A.2: Direct Effect of Spending Shock on Firm Leverage and Credit Quality

This table displays the results of firm-level regressions assessing the effect of the 9/11 Department of Defense (DoD) procurement spending shock as a function of firms' pre-existing DoD spending exposure in year 2000. We assess the effect of 9/11 on the evolution of firms' leverage and credit quality as a function of firms' defense contractor status before the shock. *DoD Contractor* is a dummy variable that equals to one for firms deriving more than 10% of revenue from sales to the Department of Defense in year 2000. *Leverage* is the ratio of long-term debt over total assets. *Credit quality* is estimated with Altman's Z-score, as in Chang et al. (2019). *Post* is a dummy variable equal to one for the years after 2001. Industry is defined according to 3-digit SIC codes. Columns 1 and 3 display the regression results for all firms. Columns 2 and 4 display the regression results for small firms, defined as those with average total assets smaller than the sample median. Standard errors presented in parentheses are clustered at the firm level. *, **, and *** denote significance at 10%, 5%, and 1% level.

	Leverage		Credit Quality	
	All	Small	All	Small
	(1)	(2)	(3)	(4)
DoD Contractor × Post	-0.020**	-0.020*	0.550***	0.740***
	(0.009)	(0.010)	(0.156)	(0.261)
Firm FE	Yes	Yes	Yes	Yes
Industry FE × Year FE	Yes	Yes	Yes	Yes
Observations	30675	15369	29561	14961

Table A.3: Amplification of the Spending Shock through Banks: Robustness

This table displays the results of county-level panel regressions assessing the effect of local banks' exposure to the 9/11 Department of Defense (DoD) spending shock on county small business lending and county employment. *County DoD exposure through banks* is measured as the weighted average of the county banks' fraction of branches in directly-shocked counties, where the average's weights are the market shares of each bank in the county. This variable is then split in quintiles and interacted with the post-2001 indicator. The regressions are estimated on not-directly-shocked counties, that is counties that are not in the top 10% of *County DoD Exposure*. County pre-shock characteristics (denoted *County char.*) include year 2000 county unemployment rate, log(employment), share of employment in manufacturing, average wage, and direct exposure to the spending shock (*County DoD Exposure*). County industrial structure (denoted *County indus. structure*) includes the county share of employment in each 3-digit NAICS manufacturing industry and in construction. County distance to the nearest directly-shocked counties is measured as the sum of the distance between the focal county and the three-nearest directly-shocked counties. Distances between counties are from the NBER County Distance database. *Credit to small firms* is the dollar amount of small business loan originations to businesses with $1 million or less in annual gross revenues, aggregated at the county-year level and expressed in natural logarithm. *Small firm employment* is the county fourth quarter employment count in firms with less than 20 employees. *qtile2* to *qtile5* denote second to fifth quintiles. Standard errors presented in parentheses are clustered at the county level. *, **, and *** denote significance at 10%, 5%, and 1% level.

County-level panel (not-directly-shocked counties)

	Credit to small firms			Small firm employment		
	(1)	(2)	(3)	(4)	(5)	(6)
County DoD exposure through banks (qtile 2) × Post	0.028	0.032	-0.015	0.003	0.006	0.007
	(0.030)	(0.030)	(0.034)	(0.007)	(0.007)	(0.007)
County DoD exposure through banks (qtile 3) × Post	0.052*	0.057*	0.032	0.003	0.002	-0.001
	(0.030)	(0.030)	(0.033)	(0.007)	(0.007)	(0.008)
County DoD exposure through banks (qtile 4) × Post	0.080***	0.098***	0.100***	0.010	0.016**	0.010**
	(0.029)	(0.029)	(0.033)	(0.007)	(0.007)	(0.009)
County DoD exposure through banks (qtile 5) × Post	0.090***	0.117***	0.121***	0.015**	0.023***	0.021*
	(0.030)	(0.030)	(0.038)	(0.007)	(0.008)	(0.011)
County FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
County char. × Post	Yes	Yes	Yes	Yes	Yes	Yes
County indus. structure × Post	Yes	-	-	Yes	-	-
Distance to nearest shocked counties × Post	-	Yes	-	-	Yes	-
Excluding counties with adjacent shocked county	-	-	Yes	-	-	Yes
Observations	20096	20096	12600	18182	18182	11487