Policy Externalities and Banking Integration*

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Abstract

Can policy changes directed at the banking sector in one jurisdiction spill over and affect real economic activity in other regions? To empirically investigate this question, I exploit changes in tax rates imposed on bank profits in U.S. states as an exogenous policy shock that impacts the profitability of bank lending opportunities. I find that exposed banks respond by reallocating small-business lending to otherwise unaffected states. To isolate the effect on credit supply, I compare the lending behavior, within the same county, of banks that have been impacted by a tax shock in another state to those that have not. Through an instrumental variables framework I then show that greater county exposure to banks experiencing tax shocks in other states affects aggregate local lending and, in turn, local employment and income. The effects are stronger in poorer counties, in counties with a greater share of small businesses, and in industries dependent on bank finance. Overall, these findings demonstrate that policies aimed at the banking sector in one jurisdiction can impose externalities on other regions. Critically, financial linkages between regions serve as the transmission channel for these policy externalities.

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1 Introduction

Modern banking systems are deeply interconnected across borders. Through these linkages, policies directed at banking institutions in one jurisdiction may potentially "spill over" and affect real economic activity in other regions.

For example, suppose that public policy in a particular jurisdiction negatively impacts bank profitability. A plausible hypothesis is that banks might seek to mitigate this by reallocating core activities, such as lending, to other regions in which they have a presence. Were this to occur, the local supply of credit available to non-financial firms – in jurisdictions that have not themselves implemented any policy change – would in part be determined by policy shocks emanating from elsewhere. If borrowing firms are credit-constrained, the reallocation in lending due to policy in jurisdiction A may therefore materially impact real economic outcomes, like employment and income, in jurisdiction B.

Such policy spillovers are arguably a corollary of financial integration. In particular, if individual jurisdictions compete for financial intermediation services, banking sector linkages may provide a transmission channel by which one jurisdiction's policies may potentially impose externalities on other regions.

In this paper, I present evidence of such externalities in a setting that, as I will argue, is well suited to the task: the United States. Specifically, I exploit variation in tax rates that individual states impose on locally-sourced bank profits.[1] I interpret a tax on bank profits as a particular example of a jurisdiction-specific policy aimed at the banking sector, which directly impacts the profitability of local lending opportunities.[2] Crucially, a change in the tax rate on banks in one state not only impacts the profitability of lending in that state, it also alters the *relative* appeal of lending in other states – even those that have not changed their tax rates themselves.

To examine whether such tax shocks induce a reallocation of credit across state lines, I use detailed information on the geographic distribution of bank lending activities to corporate borrowers, obtained from Community Reinvestment Act (CRA) disclosure reports. These contain information

[1] This source of variation was first used by Farre-Mensa and Ljungqvist (2014). The authors exploit the resulting supply shock to the availability of banks loans to evaluate the efficacy of commonly used measures of financial constraints.

[2] I describe how states tax banks in Section 2. In short, they are taxed in the state where their borrowers are located.

on the value of all small-business loans (those with a value under $1m) made by U.S. depository institutions over the period 1996-2011. The data are disaggregated at the bank-county-year level. That is, I observe the total amount of lending by a given bank in different counties over time.[3] Importantly, in the data loans are attributed to counties based on the location of the borrower, and not the bank branch that made the loan.

My analysis proceeds in two main steps. First, I examine whether banks that are exposed to tax shocks occurring in one state ("treated banks") respond by shifting credit supply to other states in which they have a presence and which have not undertaken a tax change. Second, I assess the impact of this credit reallocation on real economic activity (in non-tax-changing states) through an instrumental variables framework. To this end, I construct a measure of county-level exposure to treated banks and evaluate whether greater exposure affects aggregate county lending and, in turn, employment and income.

Identifying a spillover in credit supply to one state due to a tax shock occurring in another is empirically challenging. The main difficulty is that while some states change their bank tax rates, other states that do not are likely experiencing coincident fluctuations in economic conditions. Such fluctuations would affect credit demand and the credit-worthiness of local borrowers, and so alter bank incentives to lend in non-tax-changing states independent of any contemporaneous tax shock occurring elsewhere. The chief empirical challenge is therefore to control for coincident variation in local lending opportunities. Although such variation is not directly observable, the richness of the CRA data allows me to control for it at a highly granular local level by including county×year fixed effects in my regressions. Doing so filters out unobserved variation in local lending opportunities by comparing the lending behavior – within the same county and year – of banks that have been impacted by a tax shock occurring in another state to those that have not.

To understand the essence of this identification strategy, consider the following example. In 2001, Alabama increased its tax rate on bank profits from 6% to 6.5%. Compass Bank has a substantial presence in Alabama, and so I classify it as "treated." Compass Bank also has lending operations in New Mexico, a state that has not changed its bank tax rate. Sandoval County, one of the New Mexico counties in which it lends,[4] has 26 other lenders that in turn have no exposure to Alabama

[3]The data do not contain information on individual loan terms or borrower identities.

[4]This is a fairly typical county with a population of 89,908 based on the 2000 U.S. Census.

(or to any other tax-changing state). Since these lenders are exposed to the same local economic conditions in Sandoval County as Compass Bank, they plausibly share similar lending opportunities on average. By comparing the lending behavior of Compass Bank in Sandoval County to the 26 other "control" banks, I can difference away the confounding influence of unobserved variation in local lending opportunities.

Based on this empirical strategy, my first main finding is that a 100 basis point tax increase (cut) in one state results in an average increase (fall) in credit supply of 6.9% in counties located in non-tax-changing states. In dollar terms, this corresponds to a change in lending of $316,000 per county (in 2010 dollars). The effect is symmetric, such that tax increases (cuts) lead to credit flowing towards (away from) non-tax-changing states. Moreover, non-tax-changing states with relatively low bank tax rates experience larger credit inflows in response to tax increases elsewhere, while those with relatively high bank tax rates experience larger outflows in response to tax cuts.

I perform numerous tests to validate that the results are indeed due to a credit reallocation across states. First, I examine bank lending in states that have undertaken a tax change. Consistent with a reallocation of credit having taken place, here the results run in the opposite direction to those in non-tax-changing states: namely, tax increases (cuts) lead to falls (rises) in lending in the tax-changing state.[5] Second, for both tax-changing and non-tax-changing states, I test for dynamic effects. In both cases, only after a tax change has occurred is there an impact on lending. Moreover, the two opposing effects are perfectly synchronized; that is, at the exact same time that banks withdraw lending from one state, they increase lending in another. Third, my results are robust to excluding instances where bank tax changes coincide with corporate tax changes, which are likely to expose treated banks to changing credit demand conditions (Heider and Ljungqvist (2014)). Fourth, my findings hold even in "far-away" states that do not border any tax-changing states, and so are geographically remote from the original tax shock.

In addition, I present evidence that informational asymmetry within banks themselves – namely, between different levels of the decision-making hierarchy, as emphasized in Stein (2002) – affects credit reallocation decisions. Specifically, I find that credit is more likely to flow to areas where bank headquarters is likely to have an informational advantage (that is, to counties located closer

[5] Since all banks that operate in a tax-changing state are affected by the tax change, I measure the effect on lending relative to nearby states that are likely experiencing similar economic conditions on average.

to headquarters).

The finding that bank tax changes trigger a reallocation of lending to other states raises a fundamental question: is there a resulting impact on real economic outcomes, such as employment and income? For this to occur, the documented credit reallocation by individual banks would need to "aggregate up" to the county level. In this sense, greater county exposure to treated banks would induce exogenous variation in county-level lending. To the extent that borrowing firms are credit-constrained, this may in turn affect employment and income.

I examine whether this is the case through an instrumental variables framework. My first step in this analysis is to construct a measure of county exposure to treated banks to serve as an instrument for county-level lending. I measure "county exposure" as the share of total county lending attributable to treated banks, prior to them experiencing tax shocks in other states. Essentially, this measure exploits both tax shocks and geographic heterogeneity in the intensity of financial linkages within the U.S. banking system to provide an instrument for local credit supply.

To be a valid instrument, my county exposure measure needs to satisfy the exclusion restriction. That is, the *only* reason that the instrument can affect county-level employment and income is through its effect on lending. As I argue below, this is likely to be the case. In short, the instrument is a function of bank tax changes occurring in other states. Moreover, it is specifically constructed to measure the intensity of banking sector linkages that exist between tax-changing states and counties located in other states.

The first-stage results confirm a strong relation between the instrument and county-level lending: a one-standard-deviation increase (fall) in county exposure results in a 2.3% increase (fall) in lending, corresponding to an average change of $1.7m. As before, the effect is symmetric. It is also robust to excluding instances where bank tax changes coincide with corporate tax changes, and to restricting the analysis to "far-away" states (i.e., those that do not border any tax-changing states).

The second-stage results show that credit supply spillovers affect county-level economic outcomes. Employment and income increase (fall) by 0.17% and 0.24%, respectively, in response to a one-standard-deviation increase (fall) in the instrument, corresponding to a difference of 69 jobs and $4.3m in income on average. The results are robust to controlling for industry-level shocks. Consistent with the instrument operating through county-level lending, the effects are stronger where credit constraints are more likely to be binding; that is, in poorer counties and in counties where small businesses account for a greater share of total employment. The effects are also stronger for industries that are more dependent on bank finance.

Overall, my findings demonstrate that policy changes that impact bank profitability in one jurisdiction can spill over and impose externalities on other regions. Financial linkages between regions serve as the transmission channel for these policy externalities.

My analysis connects to several strands of literature in economics and finance. First, I contribute to a nascent empirical literature on the effects of bank regulation in the context of multi-jurisdictional rule-making. Recent work by Houston, Lin, and Ma (2012) finds evidence of regulatory arbitrage in international bank flows, as banks transfer funds to countries with fewer regulations. Similarly, Ongena, Popov, and Udell (2013) find that EU banks respond to stricter domestic regulation by making riskier loans abroad (specifically, in Eastern Europe), while Karolyi and Taboada (2013) argue that cross-border bank acquisitions are another outlet for risk-taking. One reading of these findings is that, in the absence of policy coordination, competing jurisdictions will implement "beggar thy neighbor" policies that may result in a harmful "race to the bottom" in regulatory standards (see e.g., Acharya, Wachtel, and Walter (2009)). My finding that one jurisdiction's policies can impose sizable externalities that affect real activity in other regions arguably lends support to this view.

My paper also relates to the literature on the bank lending channel. Prior empirical work in this area has shown that shocks to bank funding are transmitted across markets and have real economic effects (see Peek and Rosengren (1997), Peek and Rosengren (2000), Paravisini (2008), Khwaja and Mian (2008), Chava and Purnanandam (2011), Schnabl (2012), Chodorow-Reich (2012), and Gilje, Loutskina, and Strahan (2013)).[6] My study differs from these in that I do not examine the effect of shocks to bank funding availability – i.e., shocks to the liability side of the balance sheet.[7] Rather, the bank tax rate changes that I study represent shocks to bank investment opportunities. My contribution is therefore to show that banking sector linkages can amplify diverging economic outcomes

[6]Theoretical work on the bank lending channel has emphasized that for bank funding shocks to affect the real economy, financing frictions must exist both at the bank and borrower level (see Bernanke and Blinder (1988), Holmstrom and Tirole (1997), and Stein (1998)). In my setting, the absence of any bank-level frictions would imply that lending in non-tax-changing states would be determined solely by local lending opportunities. In this regard, my results reject this null hypothesis, consistent with prior empirical work on the bank lending channel.

[7]In several of the papers listed above (e.g., Peek and Rosengren (2000)), the shocks to bank health involve a depletion in bank capital positions. Since capital requirements are binding, these therefore represent shocks to bank funding liquidity.

across regions because banks reallocate lending in response to shocks to investment opportunities. In this sense, my findings accord with Kalemli-Ozcan, Papaioannou, and Peydró (2013), who argue that greater banking integration leads to less synchronized business cycles between countries, as banks exacerbate country-specific shocks by reallocating lending to other markets.[8]

My findings also relate to studies examining the internal capital allocation process within firms (empirical contributions include Lamont (1997), Shin and Stulz (1998) and Giroud and Mueller (2014) who study non-financial firms, and Houston, James, and Marcus (1997) and Campello (2002) who investigate internal capital markets within financial firms). By showing that banks reallocate lending to relatively more attractive states in response to tax shocks, my findings support the "efficient internal capital markets hypothesis" of Stein (1997) and Stein (2002).

I also contribute to the wider literature on the effects of corporate taxation (see e.g., Summers (1981), Cummins, Hassett, and Hubbard (1996), Goolsbee and Maydew (2000), Auerbach, Hines, and Slemrod (2007), Djankov, Ganser, McLiesh, Ramalho, and Shleifer (2010), Suarez Serrato and Zidar (2014), Ljungqvist and Smolyansky (2014)). By focusing on taxes imposed on banks, I bring attention to a previously overlooked consequence of jurisdictional tax competition. I show that not only do regional tax differentials influence the location of physical plants and machinery, they also affect the flow of capital within the financial system.

The remainder of this paper is organized as follows. Section 2 provides an overview of state-level taxation of banks. Section 3 describes the data. Section 4 presents the empirical methodology and results for bank credit reallocation. Section 5 examines real effects. Section 6 concludes.

2 State Taxation of Banks

In the U.S., corporate profits are taxed both at the federal level and the state level. Many states also impose separate taxes on the profits earned by banks (see Koch (2005) and Farre-Mensa and Ljungqvist (2014)).

A bank's taxable income in a given state is determined by multiplying its total income by an apportionment weight designed to reflect the share of the bank's total income that is sourced from the state. This weight is usually computed as the average of three fractions: the proportion of

[8] Morgan, Rime, and Strahan (2004) show that bank integration can lead to either a divergence or a convergence in business cycles, depending on whether shocks to banks or to firms predominate.

property, the proportion of payroll, and the proportion of receipts that are attributable to the state in question.[9]

When a bank makes a loan, interest and fee income from that loan enter into the calculation of the receipts factor. For the purposes of state taxation, the location of the loan, and hence the state to which it is attributable, is based on the location of the borrower, not on the lender's own location.[10] This institutional feature is critical to my empirical strategy. It means that a tax increase in one state reduces the after-tax profitability of lending in that state, and as a result increases the relative appeal of lending in other states that have not changed their bank tax rates.

A full list of state bank tax changes is available from Appendix B of Farre-Mensa and Ljungqvist (2014). Between 1996 and 2011 there were 49 state bank tax changes, consisting of 13 tax increases and 36 tax cuts. The average tax increase was 84 basis points, while the average tax cut was 51 basis points. Relative to the prior year's tax rates, these represent an average increase of 11.66% and an average cut of 6.43%, respectively.

What factors drive states' decisions to change their bank tax rates? This question would be particularly relevant if my primary aim was to examine outcomes in tax-changing states, as it would give insight into potential omitted variables. A key strength of my approach, however, is that I focus on spillover effects in states that have not themselves undertaken any tax change. As I discuss in Section 4.1, this extra degree of separation between the state where the policy change is implemented, and the state where the effect of the policy change is measured, is central to my identification strategy.

Still, I briefly review the findings of Farre-Mensa and Ljungqvist (2014), who investigate the determinants of state bank tax changes for the period 1990-2011.[11] Their central result is that key

[9]For example, the Multistate Tax Compact, in an effort to harmonize apportionment regulations across states, provides for such a three-factor apportionment formula. Similarly, to take the relevant law from one state as an example, in California the relevant provision appears under Code Regs. § 25137–4–2(a):

"All business income shall be apportioned to this state by multiplying such income by the apportionment percentage which is determined by adding the taxpayer's receipts factor... property factor... and payroll factor... together and dividing the sum by three".

Some states have adopted variants of this standard three-factor formula, either by double-weighting the receipts factor, or by adopting a single-factor formula based on the receipts factor alone: see Koch (2005).

[10]For example, this is the approach adopted by the Multistate Tax Compact. Again, to take the relevant law from California as an illustration, as Code Regs. § 25137–4–2(c) makes clear:

"Receipts factor includes...

(c) interest from loans secured by real property if the property is located within this state

(d) interest from loans not secured by real property if the borrower is located in this state".

[11]See Table IA.1 in Farre-Mensa and Ljungqvist (2014).

measures of state economic performance – such as state GDP growth and the state unemployment rate – do not have a significant effect on the probability that a state will change its bank tax rate. Downgrades in the state's bond rating also have no effect. On the other hand, both political and budgetary factors, as well as tax competition among neighboring states, seem to matter. Specifically, states are more likely to cut bank taxes if their budget is in surplus, if their taxes are high relative to their neighbors, and if the governor is a Republican. In contrast, they are more likely to increase bank taxes if their budget is in deficit and if their taxes are low relative to neighboring states.

Using bank tax changes across U.S. states as a laboratory to study the effects of cross-border policy spillovers is ideal for several reasons. First, the intra-U.S. setting allows me to effectively abstract from fundamental differences in institutional quality, legal systems, and levels of development that would be present in a cross-country context. Second, unlike more complex policy reforms, tax changes are – by definition – quantifiable, and therefore directly comparable across multiple treatment instances. Finally, the empirical study of banking sector policy and regulation must confront a particular form of measurement error: the uneven enforcement and application of rules that appear to be *de jure* equivalent. For example, there is significant variation across countries in what assets constitute Tier 1 capital under the Basel Accords (Gorton and Winton (2000)). Perhaps more problematically, even within the same jurisdiction, different bank regulators may implement identical rules inconsistently (Agarwal, Lucca, Seru, and Trebbi (2014)). However, in the context of the taxation of banks within the U.S., uneven enforcement is unlikely to be an issue.

3 Sample and Data

3.1 Data Sources

Data on the geographic distribution of U.S. bank lending activities for the period 1996-2011 are obtained from disclosure reports filed under the Community Reinvestment Act (CRA) of 1977. These are available from the Federal Financial Institutions Examination Council (FFIEC). The intended purpose of the CRA is encourage depository institutions to meet the credit needs of the communities in which they operate, with a traditional focus on home mortgage lending. Reforms to the regulations implementing the CRA effective from 1996 further required that banks submit disclosure statements detailing the geographic distribution of their lending activities to small businesses. It is these data that I use in my analysis.

The data contain information on the total dollar value and the number of small business loans (defined as loans under \$1m) made by each U.S. commercial bank in each county in a given year.[12] Since the data are organized at the bank-county-year level, I do not observe information requiring a finer level of disaggregation, such as information on individual loan terms or borrower characteristics. From 1996 to 2004, the data cover all banks with more than \$250m in assets. In 2005, the asset size threshold was increased to \$1bn, with a subsequent adjustment for CPI.[13]

A key feature of the data is that loans are attributed to a particular county based on the location of the borrower and not based on the location of the bank branch that granted the loan.[14] This is important because it allows me to accurately measure the volume of credit supplied to a particular county (which I then relate to real county-level economic outcomes through an instrumental variables framework). It also corresponds with how banks are taxed – that is, based on the location of the borrower (see Section 2).

I merge the CRA data with two other bank-level databases. First, I obtain information on the geographic distribution of bank deposits from the Federal Deposit Insurance Corporation's (FDIC) Summary of Deposits database, which is available from 1994. Second, I use Reports of Condition and Income data (i.e., Call Reports) for bank balance sheet information. I drop bank-years with missing information on assets or deposits.

Outcome variables measuring real economic activity are constructed from the Bureau of Labor Statistics' Quarterly Census of Employment and Wages (QCEW). The QCEW is derived from administrative records based on mandatory filings submitted quarterly by every establishment to calculate payroll taxes. It therefore represents the most comprehensive publicly available data set on private-sector employment and income. Both these variables are available at the county level and at the county-industry four-digit NAICS level. To obtain annualized measures for employment

[12] The data also contain analogous information on all loans made to small businesses with annual revenues of under \$1m. However, since my ultimate aim is to study the real effects of policy-induced credit reallocation, I focus on the former measure – that is, all loans under \$1m (irrespective of the revenues of the borrower) – in order to obtain as comprehensive a picture as possible of county-level bank lending. My baseline results are nonetheless unchanged if I restrict the analysis to small businesses with annual revenues of under \$1m.

[13] I show in Section 4.3.2 that my results are unaffected if I focus only on the pre-2005 period.

[14] See "A Guide to CRA Data Collection and Reporting," Federal Financial Institutions Examination Council, January 2001.

and income (and so match the frequency of my lending data), I take the average of the four quarterly observations during a given year. Finally, I obtain county-level population and demographic information from the Census Bureau's Population Estimates Program.

3.2 Summary Statistics

Table 1 reports summary statistics for bank-years in Panel A, county-years in Panel B, and bank-county-years in Panel C. The full sample consists of 19,022 bank-years (or 2,680 unique banks), 50,233 county-years and 738,152 bank-county-years.

In Panel A, several points merit particular mention. First, banks are highly taxed. For the average bank, the ratio of total income taxes to pre-tax income is 0.30. The high average tax rate suggests that banks are likely to be responsive to tax incentives – i.e., they will seek to minimize taxes where they can (for example, by reallocating lending). Second, small-business lending is an important economic activity for banks. The average bank makes \$189.6m worth of small-business loans per year (in 2010 dollars, which I use throughout the paper). This corresponds to 3,924 separate loans per year. Moreover, small-business lending makes up a sizable portion of total commercial lending. For the average bank, the value of small-business loans represents about 55% of their total commercial loan portfolio.[15]

Panel B describes key aspects of small-business lending at the county level. The average county receives approximately \$72m worth of small-business loans per year, corresponding to 1,488 separate loans per year. The average loan size of \$44,332 is fairly small, which is perhaps not surprising given that the CRA data conditions on loans having a value under \$1m.

Local lending markets appear to be competitive. The average county has about 22 banks actively engaged in small-business lending. The competitive nature of local lending markets might be expected given that my sample begins in 1996, by which point historical restrictions on bank branching and interstate bank ownership had been lifted (see Jayaratne and Strahan (1996) and Morgan, Rime, and Strahan (2004)).

Finally, Panel C describes the data at the finest level of disaggregation, i.e., by bank-county-

[15] A bank's commercial loan portfolio is defined as the value of all loans secured by nonfarm nonresidential properties, plus commercial and industrial loans (the latter is defined as all other business loans, whether secured or not, that are not primarily secured by nonfarm nonresidential real estate). Loans secured by residential real estate are excluded. This measure of a bank's commercial loan portfolio corresponds with the definition of small-business loans.

year. This shows that the average bank makes approximately $4.3m in small-business loans in the average county per year, corresponding to 87 separate loans per year.

4 Credit Supply Reallocation

4.1 Empirical Strategy

To investigate whether banks experiencing state-level tax shocks reallocate credit supply to other states, I estimate the following baseline regression:

$$\triangle ln(Lending)_{b,c,s,t} = \alpha_{c,s,t} + \delta \times Treatment_{b,t-1} + \gamma' \triangle X_{b,t-1} + \epsilon_{b,c,s,t} \tag{1}$$

where b indexes banks, c indexes counties (located in non-tax-changing states s, defined below), and t indexes time; $\triangle$ is the first-difference operator; $ln(Lending)_{b,c,s,t}$ is the natural log of the value of lending done by bank b in county c at time t; $\alpha_{c,s,t}$ is a set of county-year fixed effects; $Treatment_{b,t-1}$ measures the degree to which bank b is exposed to tax shocks occurring in other states in the prior year; $X_{b,t-1}$ is a vector of time-varying bank-level control variables; and $\epsilon_{b,c,s,t}$ is the error term. Standard errors are clustered at the bank level.

A bank is treated if it has exposure to a tax-changing state, which I label s'. As explained in Section 2, a bank's state tax liability is typically determined by a three-factor formula based on the proportions of property, payroll, and receipts that are attributable to a particular state. To obtain a suitable empirical proxy that reflects these three factors, I use the share of the bank's deposits that it sources from the state in question. Deposit exposure indicates the presence of branches (i.e., property), staff (i.e., payroll), and receipts (from lending and other services),[16] and so provides an appropriate measure of a bank's tax exposure to a given state. Accordingly, I measure $Treatment$ as follows:

$$Treatment_{b,t-1} = \sum_{s'} \triangle Tax_{s',t-1} \times Exposure_{b,s',t-1} \tag{2}$$

where $\triangle Tax_{s',t-1}$ is the percentage-point change in the bank tax rate in state s' in the prior year.

[16] The correlation between the proportion of deposits sourced from a state and the proportion of small-business lending attributable to that state is 0.96 (at the bank-state-year level).

$Exposure_{b,s',t-1}$ measures the proportion of bank b's total deposits that it sources from the tax-changing state s' in the year prior to the tax change.[17] To account for the possibility that a bank is subject to tax shocks in multiple states, measure (2) sums the exposure-weighted tax changes across all the tax-changing states s' to which bank b is exposed.[18]

Importantly, while bank tax changes occur in states that I label s', my aim is to measure the spillover effect on bank lending in counties located in other states that have not changed their tax rates on bank profits (labeled s in equation (1)). Accordingly, I impose the condition that to be classified as a non-tax-changing state s, and so be included in the estimation of equation (1), the state must not have changed its tax rate on bank profits for at least 3 years.

My identification strategy can be illustrated with the following example. In 2001, Alabama increased its tax rate on bank profits from 6% to 6.5%. Compass Bank has exposure to Alabama, and therefore is "treated." Compass Bank also has lending operations in New Mexico, a state that has not changed its bank tax rate according to the above criteria. For example, Sandoval County is one of the New Mexico counties in which it lends. The first element of my analysis is therefore to observe Compass Bank's change in lending to Sandoval County following the tax increase in Alabama.

Of course, Compass Bank lends in many other New Mexico counties. More generally, there is heterogeneity in the degree to which different banks lend to different counties. To remove the time-invariant level component in bank-to-county lending behavior, I estimate equation (1) in first-differenced form. This allows me to focus on the change in bank-to-county lending behavior following tax shocks occurring in other states. (In other words, first-differencing removes the bank-county fixed effect that would appear in the corresponding levels equation.)

To continue with my example, simply comparing Compass Bank's lending in Sandoval County before and after Alabama's tax increase would not be sufficient. The reason is that local economic conditions in Sandoval County may themselves have changed. Such changes in local economic conditions could, for example, impact local demand for bank loans or the general credit-worthiness of local borrowers. This would in turn affect Compass Bank's lending opportunities in Sandoval

[17]I measure *Exposure* in the year prior to the tax change to ensure that this is not itself impacted by the treatment, a convention that I adopt throughout the paper.

[18]This implicitly assumes that positive shocks can offset negative shocks. However, the results in no way hinge on this assumption. In Section 4.3.2, I show that the results are robust to only considering cases where banks are subject to a single tax shock; see Table 4, column 5.

County, and thus its lending behavior, irrespective of whatever was taking place in Alabama. The empirical challenge is that such changes in local lending opportunities are not directly observable. However, unless these are controlled for it would be impossible to convincingly attribute the change in lending behavior in Sandoval County to Alabama's tax increase.

The richness of the CRA data offers a solution. Specifically, in Sandoval County there are 26 other lenders with no exposure to Alabama (or to any other tax-changing state). Since these lenders are exposed to the same local economic conditions in Sandoval County as Compass Bank, they arguably share similar lending opportunities on average. This means that they provide an appropriate control group. By comparing the lending behavior of Compass Bank in Sandoval County to these 26 other control banks, I can therefore effectively difference away the confounding influence of otherwise unobserved variation in local lending opportunities.

In equation (1), this is generalized through the inclusion of county-year fixed effects, $\alpha_{c,s,t}$. These ensure that the change in lending of treated banks is always compared to untreated banks within the same county and year. In this way, equation (1) controls for coincident variation in local lending opportunities common to all banks operating within a particular county at a given point in time.

In my baseline specification, I also include is a vector of time-varying bank-level control variables, $X_{b,t-1}$, consisting of bank size (measured as the natural log of total assets in the prior year), bank age (which equals the natural log of one plus the number of years the bank has operated), and the natural log of total bank deposits from the prior year.

The identifying assumption that underpins my empirical strategy is that, absent the bank tax change, there would be no difference in the lending behavior of treated and control banks in counties located in non-tax-changing states. Only if this assumption is satisfied does the coefficient of interest, δ, estimate the causal effect that a tax-induced shock to profitability in one state has on the reallocation of credit supply to another. I return to the plausibility of this assumption in section 4.3.2.

4.2 Lending in Tax-Changing States

If my results using the above methodology are indeed due to a reallocation of credit, then it must be that the *opposite* effect is observed in tax-changing states. Since bank tax changes directly affect the after-tax profitability of lending, banks are expected to cut lending in states where taxes have increased and expand lending in states where taxes have fallen. Indeed, this is precisely the finding of Farre-Mensa and Ljungqvist (2014). They use Call Report data to show that changes in a state's bank tax rate affect the amount of commercial and industrial (C&I) loans made by banks that are headquartered there. Since I use different data, I verify that the same effect holds in my setting.[19]

The identification strategy that I employ for non-tax-changing states, as detailed above, is not available in states that have undertaken a tax change. The reason is that if a state changes its bank tax rate, *all* banks that lend in that state will be affected, thus leaving no within-state control group.

The next best alternative is therefore to compare bank lending with nearby states. Such states, by virtue of their proximity, are likely experiencing similar economic conditions to tax-changing states. They thus provide a plausible control group. Accordingly, I aggregate the data up to the bank-state-year level and estimate the following regression:

$$\triangle ln(Lending)_{b,s,t} = \alpha_{region,t} + \delta \times Treatment'_{b,s,t-1} + \gamma' \triangle X_{b,t-1} + \theta' \triangle Z_{s,t-1} + \epsilon_{b,s,t} \qquad (3)$$

where b indexes banks, s indexes states, and t indexes time. The regression is estimated in first-differences to remove time-invariant level heterogeneity in bank-to-state lending behavior. $ln(Lending)_{b,s,t}$ is the log of lending done by bank b in state s at time t. To control for lending opportunities common to nearby states, I include a set of region-year fixed effects, $\alpha_{region,t}$, corresponding to eight economic regions within the U.S., as defined by the Bureau of Economic Analysis.[20]

$Treatment'_{b,s,t-1}$ is the treatment measure of bank b in state s,[21] defined as follows:

$$Treatment'_{b,s,t-1} = \triangle Tax_{s,t-1} \times Exposure_{b,s,t-1} \qquad (4)$$

[19]Unlike Call Reports, the CRA lending data are disaggregated by borrower location and cover only small-business loans

[20]See http://www.bea.gov/regional/docs/regions.cfm for details.

[21]The "prime" distinguishes $Treatment'_{b,s,t-1}$from the treatment measure for non-tax-changing states in eq. (1).

where $\triangle Tax_{s,t-1}$ is the percentage-point change in the bank tax rate in state s in the prior year, and $Exposure_{b,s,t-1}$ measures the proportion of bank b's total deposits that it sources from state s in the year prior to the tax change.[22] In equation (3), bank lending is evaluated in the *same* (potentially tax-changing) state s as the treatment.

The vector of bank-level control variables, $X_{b,t-1}$, is the same as that included in equation (1). In addition, I also include a vector of state-level control variables, $Z_{s,t-1}$, to control for time-varying state economic conditions. Specifically, these include the natural log of state GDP, population, and total employment. Finally, since the data in this specification are disaggregated at the bank-state-year level, this raises to possibility of serial correlation in the error term, $\epsilon_{b,s,t}$, both within banks and within states. To account for this possibility, standard errors are two-way clustered by bank and by state.

4.2.1 Effects in Tax-Changing States

I first present the results for the effect on lending in tax-changing states, shown in Table 2. The negative coefficient estimate of -0.051 ($p = 0.022$) in column (1) shows that if a state increases (cuts) taxes, banks reduce (expand) lending there, measured relative to nearby control states where bank tax rates have remained unchanged. This mirrors the finding of Farre-Mensa and Ljungqvist (2014) and suggests that bank lending is responsive to tax-based incentives.

A concern, however, is that states' decisions to change their bank tax rates might be driven by an omitted variable which also affects bank lending opportunities. For example, states might change their bank tax rates in response to deteriorating economic conditions. Since these same economic conditions would also affect bank lending opportunities, the resulting estimate would be biased.

This is unlikely given Farre-Mensa and Ljungqvist's (2014) other finding, discussed in Section 2, that bank tax changes are unrelated to state economic performance – as measured by state GDP growth, the unemployment rate, and downgrades in the state's bond rating.

Still, in column (2), I address this specific concern in my setting by including additional lead and lag terms of the treatment variable (e.g., $Treatment'_{b,s,t+1}$ means that the treatment will occur one year from now, and so on). If deteriorating (or improving) economic conditions result in states

[22] Section 4.1 explains why this is an appropriate empirical proxy for a bank's state tax exposure.

changing their bank tax rates, then the effect of these conditions on lending would likely be apparent even prior to the tax change taking place. The results show, however, that there is no difference in bank lending in tax-changing states compared to nearby control states prior to the tax change. Only after the tax change has occurred does bank lending respond. The timing therefore suggests that the reaction is due specifically to the tax change. Moreover, the coefficient estimate of -0.053 ($p = 0.034$) on the treatment variable is virtually the same as that in column (1). There also is no evidence that this effect is subsequently reversed.

Overall, these results support the conclusion that bank tax rates have a direct impact on loan supply in tax-changing states. The next question is therefore whether banks respond by reallocating lending to other states that have not undertaken a tax change.

4.3 Evidence of Credit Supply Reallocation

4.3.1 Baseline Results

Table 3 presents the baseline results for non-tax-changing states. Column (1) shows the effect that bank tax changes have on the lending behavior of exposed banks in counties located in non-tax-changing states, measured relative to other banks that lend in those counties but have no exposure to state-level tax shocks. The treatment coefficient measures 0.216 and is highly statistically significant ($p = 0.002$). Consistent with a reallocation of credit having taken place, the positive sign of the coefficient is the opposite to that observed in tax-changing states. This suggests that banks respond to tax increases (cuts) occurring in one state by reallocating lending towards (away from) otherwise unaffected states.

To interpret the magnitude of the coefficient, recall that $Treatment$ is calculated by multiplying the bank tax change by the bank's deposit exposure to the tax-changing state (see equation (2)). Since the average bank sources deposits from 3.25 states,[23] this implies that a 100 basis point change in the bank tax rate results in a 6.6% (=0.216/3.25) reallocation of lending to counties in non-tax-changing states. In dollar terms, this corresponds to $302,000 for the average bank per county.

Column (2) includes bank-level controls (described in Section 4.1). The resulting estimate of

[23] This is conditional on having exposure to at least two states, and hence having the capacity to be treated.

0.225 ($p = 0.001$) is essentially unchanged and implies that a 100 basis point change in the bank tax rate results in a 6.9% (=0.225/3.25) credit supply reallocation to non-tax-changing counties, or $316,000 for the average bank per county. The stability of the coefficient supports the conclusion that bank tax changes are exogenous from the perspective of individual banks, i.e., they are uncorrelated with changes in bank characteristics that might otherwise affect lending behavior. For the remainder of the analysis, I continue to include bank-level controls in my regressions.

Column (3) of Table 3 examines the dynamics of the treatment effect (e.g., $Treatment_{b,t+1}$ means that the treatment will occur one year from now, and so on). This shows that the credit reallocation is perfectly synchronized with the opposing effect observed in tax-changing states (presented in Table 2). That is, at the exact same time that treated banks are withdrawing lending from one state, they are increasing lending in another. To elaborate, as is the case for tax-changing states, there is no difference between the lending behavior of treated and control banks prior to the tax change. Only after the tax change has occurred do treated banks exhibit a change in their lending behavior. Moreover, the magnitude of 0.226 ($p = 0.005$) of the estimated $Treatment_{b,t-1}$ coefficient is virtually identical to that in column (2). There is no evidence that this effect is subsequently reversed.

4.3.2 Robustness

(A) Identifying Assumption

A causal interpretation of the above result rests on the following identifying assumption: in the absence of the bank tax change, there would be no difference in the lending behavior of treated and control banks in counties located in non-tax-changing states. Given that my regressions control for coincident variation in local lending opportunities, and given the timing of the observed effect, there remains one condition under which this identifying assumption would be violated. Namely, if bank tax changes in one state systematically coincide with contemporaneous shocks to county-level lending opportunities in other states that *differentially* affect treated banks. Since such shocks would be unique to treated banks, their impact would not be absorbed by the county-year fixed effects, which control only for variation in lending opportunities common to all lenders in a given county at a given point in time. Moreover, if these shocks occur at the exact same time as the bank tax changes, their effect may not be evident beforehand – consistent with there being no pre-existing differential trends between treated and control banks.

A possible candidate for this type of shock could be other state-level policy changes that coincide with bank tax changes but that ultimately drive the observed effect. Notably, in some instances bank tax changes overlap with corporate income tax changes at the state level.[24] These corporate tax changes are likely to affect firms' demand for credit in tax-changing states.[25] The reallocation of credit by treated banks might therefore be a response to their exposure to changing credit demand conditions, rather than to shocks to their own tax rates. To rule out this possibility, in column (1) of Table 4, I exclude from my treatment measure bank tax changes that coincide with corporate tax changes. The resulting coefficient estimate of 0.171 ($p = 0.043$) is very similar to that found previously. This confirms that the observed credit reallocation is indeed a response to tax-induced shocks to bank profitability, and is not confounded by changing demand conditions unique to treated banks.

A more subtle possibility remains. Banks may attempt to circumvent the effects of tax changes by incentivizing firms to relocate their loans. This would only be possible by taking the same loan and securing it against different collateral located in another state.[26] Were this to occur, it would give the false impression that credit has been "reallocated." Although possible, this is unlikely in my setting, as my data consist of loans to small businesses which typically do not have multi-state operations. Still, one way to address this concern directly is to consider only the effect in "far-away" states – i.e., in states that do not border any tax-changing states and so are too geographically remote to plausibly be affected by this potential confound. In column (2) of Table 4, I restrict the sample to counties located in such far-away states. As is shown, the resulting coefficient estimate of 0.242 ($p = 0.007$) is little changed.

The above two results render it unlikely that my baseline findings are confounded by shocks

[24] Of the 49 bank tax changes between 1996-2011, 12 coincide with corporate income tax changes. The tax changes are always in the same direction (e.g., bank tax increases only coincide with corporate tax increases, and never with cuts).

[25] The overall effect of corporate tax changes on firms' credit demand, however, is ambiguous. For example, corporate tax increases reduce the after-tax return on corporate investment. With potentially less investment to finance, the demand for credit may fall. On the the other hand, since interest payments are tax deductible, this is counteracted by the incentive of firms to increase leverage to take advantage of debt tax shields (see Asker, Farre-Mensa, and Ljungqvist (2014) and Heider and Ljungqvist (2014)).

[26] As explained in Section 2, banks are taxed based on where their borrowers are located. In the case of a secured loan, this would be the location of the underlying collateral. Loan location in the CRA lending data is similarly based on the location of the borrower. In contrast, in the case of an unsecured loan, its location could only change if the borrower moved.

whose timing coincides perfectly with bank tax changes and which differentially impact treated banks.

(B) Miscellaneous Robustness Tests

In column (3), I test whether the 2005 discontinuity in the criteria for a bank to report under the CRA affects my results. In 2005, the asset size threshold for filing lending disclosure reports under the CRA was increased from \$250m to \$1bn, with a subsequent adjustment for inflation. In principle, this should have no impact on the results – it simply means that in 2005 a subset of smaller banks are arbitrarily dropped from the sample. Indeed, as column (3) shows, if I restrict the analysis to the pre-2005 period, the treatment coefficient of 0.210 ($p = 0.004$) is unaffected.

In column (4), I drop banks following their acquisition of another bank.[27] Many have argued that bank mergers lead to declines in small-business lending (e.g., Stein (2002)). At the very least, bank acquisitions add noise to my data, because subsequent to an acquisition the target disappears from my sample and its lending is attributed to the acquirer. This potentially affects both treated banks and banks in the control group. However, as the coefficient estimate of 0.192 ($p = 0.010$) shows, my results do not change if I restrict the sample to non-acquirers.

My treatment measure allows for a bank to potentially experience tax shocks occurring in multiple states (see equation (2)). This means that if, for example, a bank were exposed to tax increases in two separate states, both would contribute to its overall treatment measure. Taking both into account reflects the fact that the bank's incentive to reallocate lending to non-tax-changing states is greater than if only one of the tax increases were considered. On the other hand, coding the treatment variable in this way implicitly assumes that positive tax shocks offset negative tax shocks at the bank level. To test whether my results in any way depend on this assumption, in column (5) I restrict the treatment to instances where banks are subject to at most one tax change (and drop banks that are exposed to tax shocks in multiple states). The estimated coefficient of 0.192 ($p = 0.005$) shows that allowing for multiple tax shocks has no bearing on my results.

(C) Symmetric Effects

Column (6) considers the effects of negative and positive shocks separately. The results presented so far have assumed a symmetric treatment effect – i.e., that banks respond to tax increases (cuts)

[27] Specifically, I drop acquiring banks for two years: the year of the acquisition and also the following year, to allow for potentially delayed responses.

by reallocating lending towards (away from) other states where they lend. To test whether the effect is indeed symmetric, I define two new variables. $Positive\,Treatment$ is the absolute value of $Treatment$ if it is positive (meaning that treated banks are exposed to tax increases). Analogously, $NegativeTreatment$ is the absolute value of $Treatment$ if it is negative (meaning that treated banks are exposed to tax cuts). The negative coefficient of -0.190 ($p = 0.016$) for negative treatments confirms that banks respond to tax cuts by withdrawing credit supply from other states where they lend. For positive treatments, the positive coefficient of 0.422 indicates that banks respond to tax increases by reallocating lending towards non-tax-changing states. The lower significance for positive treatments ($p = 0.084$) is most likely due to the smaller number of tax increases as compared to tax cuts (13 versus 36, respectively). Importantly, the absolute magnitudes of the two coefficients are not significantly different from each other ($p = 0.377$). Overall, these results support the conclusion of a symmetric treatment effect.

(D) Tax Rates in Non-Tax-Changing States

Finally, in column (7) I consider the role of bank tax rates in non-tax-changing states. So far, my analysis has been premised on the observation that if a state changes its bank tax rate, then this alters the relative appeal of lending in all non-tax-changing states, irrespective of their tax rates. The reason for this is that banks choose where to lend based on numerous considerations, of which taxes are but one. When a state changes its bank tax rate, this therefore disrupts the prior equilibrium. So in the case of a tax increase, lending becomes relatively more attractive in all non-tax-changing states – even in those with high tax rates. Still, one might expect that following a tax increase credit is more likely to be reallocated to states that have relatively low tax rates, given that tax considerations are what trigger the reallocation. By the same logic, in the case of tax cuts, non-tax-changing states with higher tax rates should experience greater outflows of credit.

To test these predictions, for each bank I categorize the states in which it lends as either $HighTax$ (i.e., above the median bank tax rate among all states in which that bank lends that year), or $Low\,Tax$ (below the median). I then interact these indicators with the positive treatment and negative treatment variables defined earlier. The results show that tax rates in non-tax-changing states matter. Specifically, although both high-taxing and low-taxing states receive inflows of credit following a tax increase in another state, the effect is only significant for low-taxing states

($p = 0.040$). Similarly, after a tax cut, lending is withdrawn from all non-tax-changing states, but this outflow is only significant for those with high tax rates ($p = 0.003$). The effects for high-taxing versus low-taxing states (conditional on the direction of the treatment) are also significantly different from each other ($p = 0.072$).

4.3.3 Small-Business Lending, Soft Information and Bank Internal Capital Markets

My setting provides an opportunity to study the internal capital allocation process inside banks. In particular, Stein (2002) argues that the workings of a bank's internal capital markets will depend crucially on informational asymmetries within the bank.

One example of this is that bank headquarters – where high-level capital allocation decisions are made – is likely to be better informed about the investment prospects of some areas as opposed to others. In particular, headquarters may be better informed about lending opportunities in more closely located counties. The reason is that geographic proximity fosters access to information, local knowledge, and easier monitoring of subordinates (see Petersen and Rajan (2002), Mian (2006) and Giroud (2012)). This implies that in response to a tax-induced profitability shock in one state, more lending should flow to counties closer to headquarters, i.e., to those counties where headquarters has an informational advantage.

A similar prediction follows directly from the reasoning of Stein (2002). Stein argues that small-business lending – which forms the basis of the CRA data – is a quintessential example of investment based on "soft" information; namely, complex qualitative factors that are difficult to objectively verify. Decisions based on soft information necessarily involve delegation, because such information cannot be credibly communicated to those further up the hierarchy. The theory predicts that when decision-making is decentralized because information is soft, headquarters should engage in less capital reallocation across divisions. The intuition is as follows: in order to provide subordinates with sufficient ex ante incentives to acquire soft information (which headquarters cannot easily verify), they needed to be insulated from the possibility that their efforts will go to waste, which would occur if, ex post, management reallocates their capital to another division.[28]

[28] In other words, the presence of soft information adds another dimension to the traditional "winner-picking" argument formalized in Stein (1997). The latter argument, also known as the "efficient internal capital markets hypothesis," is that binding credit constraints at the firm-level will induce headquarters to reallocate scarce funds to relatively more profitable projects (irrespective of a project's absolute merits). As Stein (2002) explains, the point of departure from Stein (1997) is that "in Stein (1997), line managers are treated as passive robots who do not need

In my setting, the degree of delegation is expected to be especially pronounced in counties further from headquarters. In such counties, heightened difficulties in communication and coordination with headquarters, along with management's lack of local knowledge, would presumably increase their reliance on the decisions of their subordinates. Stein's (2002) theory implies, therefore, that lending in further away counties should be less sensitive to tax shocks occurring in other states.

To test these predictions, I calculate for each bank the distance between headquarters and each of the counties in which it lends.[29] I then interact the treatment measure with indicators for whether the county's distance from headquarters is above or below the median among all the counties in which that particular bank lends in the prior year.

As column (1) of Table 5 shows, while the treatment effect is significant for counties both close to and far away from headquarters, the response of lending in closer counties is more than double the size, at 0.317 ($p < 0.001$) compared to 0.151 ($p = 0.029$) – a difference in magnitudes that is highly significant ($p = 0.003$). The greater sensitivity of lending in closer counties supports the conclusion that non-uniformity in headquarters' knowledge about local conditions and local lending opportunities impacts where credit is reallocated. In other words, consistent with the reasoning of Stein (2002), it appears that informational asymmetry within banks themselves is an important friction in the functioning on bank internal capital markets.

5 Real Effects

5.1 Empirical Strategy

Does the observed reallocation of credit impact real economic activity? To understand how this might occur, consider, for example, a county in a non-tax-changing state that is exposed to several banks that experience tax increases in other states. We know from the prior section that the expected response of each of these treated banks is to reallocate credit supply to this

to be motivated." However, when information is soft and cannot be verified (as Stein (2002) argues is the case for small-business loans), the only available mechanism that motivates line managers to acquire that information in the first place is that they get to keep their capital allocations. This follows from the assumption that line managers value the size of their capital allocations, i.e., that they have empire building incentives.

[29] Specifically, Call Report data contain information on the zip code of bank headquarters. I then obtain county and zip code latitudes and longitudes from the Census Bureau's 2000 Gazetteer files. With these inputs, I calculate distances using the great-circle distance formula, which gives the shortest distance between two points on the surface of a sphere (i.e., the earth).

county. A natural question to ask is therefore whether the increase in lending by each individual bank "aggregates up" to the county level. If it does, and if local borrowers themselves face credit market frictions, the resulting increase in county-level lending may impact real economic outcomes like employment and income. Such a finding would demonstrate that policies directed at financial intermediaries in one jurisdiction can spill over and impose externalities on other regions. Moreover, financial linkages between jurisdictions would serve as the transmission channel for these policy externalities.

To formally investigate this intuition, I employ an instrumental variables framework. Specifically, I exploit the fact that counties in non-tax-changing states are differentially exposed to banks that experience tax shocks occurring in other states. As I argue below, variation in a county's exposure to these treated banks provides a suitable instrument for local credit supply, since it is plausibly exogenous to other determinants of county-level lending.

5.1.1 First-Stage

An instrumental variables analysis requires that a strong relation exist between a county's exposure to treated banks and the aggregate quantity of county-level lending. To examine this "first-stage," I estimate the following regression:

$$\triangle ln(Lending)_{c,s,t} = \alpha_{region,t} + \delta \times County\ Exposure_{c,s,t-1} + \gamma' \triangle X_{c,s,t} + \epsilon_{c,s,t} \quad (5)$$

where c indexes counties, s indexes non-tax-changing states, and t indexes time; $ln(Lending)_{c,s,t}$ is the natural log of the value of total small-business lending in county c at time t, scaled by county population; $\alpha_{region,t}$ is a set of region-year fixed effects; $County\ Exposure_{c,s,t-1}$ measures a county's exposure to treated banks; $X_{c,s,t}$ is a vector of time-varying county-level demographic control variables; and $\epsilon_{c,s,t}$ is the error term. Standard errors are clustered at the county level.

The regression is estimated in first-differences to remove time-invariant heterogeneity in the level of lending within a given county. Doing so allows me to focus on shocks to county-level lending that result from the county's exposure to treated banks. As before, since my aim is to understand the inter-state spillover effects of bank tax shocks, I restrict my analysis to counties located in non-tax-changing states (i.e., those that have not changed their tax rates on bank profits for at least 3 years).

An important issue is how a county's exposure to treated banks should be measured. An intuitive measure is the share of total county lending that is attributable to treated banks prior to them experiencing tax shocks occurring in other states. There are two reasons for this. The first follows from theory and economic intuition. In particular, counties where banks have a greater share of total lending are ones where banks have larger networks of established lending relationships and more developed institutional infrastructures (e.g., in the form of more branches and more loan officers). This will in turn facilitate the reallocation of lending to such counties. The second reason is mechanical. As I found in the prior section, in response to a 100 basis point bank tax change, treated banks increase their lending in non-tax-changing counties by 6.9%. It follows that the larger a given bank's share of total county lending, the greater the impact that a given percentage-point change in that bank's lending will have on the aggregate amount of county-level lending.

Accordingly, I measure a county's exposure to treated banks as follows:

$$County\ Exposure_{c,s,t-1} = \sum_{b} \omega_{b,c,s,t-1} \times Treatment_{b,t-1} \times ln(Assets_{b,t-1}) \tag{6}$$

where $\omega_{b,c,s,t-1}$ is the proportion of total small-business lending that treated bank b provides to county c (located in non-tax-changing state s) in the two-year period prior to the tax change; $Treatment_{b,t-1}$ is the same treatment measure of bank b as used previously (see equation (2)); and $ln(Assets_{b,t-1})$ is the natural log of bank b's total assets in the year prior to the tax change. *County Exposure* is therefore simply the weighted sum of all bank-level treatments to which a particular county is exposed, where weights are based on pre-treatment lending shares.[30] Since heterogeneity in exposure to treated banks forms the basis of this measure, it essentially exploits the geographic network structure of financial linkages within the U.S. banking system to provide an instrument for local credit supply. In Section 5.2.3, I return to whether *County Exposure* satisfies the requirements of a valid instrument.

A remaining issue is that, as before, observed changes in county-level lending might be due to coincident variation in local lending opportunities. This would impact bank incentives to lend to a

[30]Specifically, each bank-level treatment is scaled by the bank's share of total lending to a given county (which can be zero), and by bank size (as measured by $ln(Assets_{b,t-1})$). The latter is needed because the original treatment measure is invariant to bank size (see equations (1) and (2)); yet a county's exposure to treated banks should reflect the fact that larger banks can potentially reallocate more credit.

given county independent of their exposure to tax shocks occurring in other states. In the previous section, I included county-year fixed effects in my regressions to show that banks reallocate credit supply even controlling for all such variation in local lending opportunities. Since the analysis is now conducted at the county-year level, prefect collinearity precludes the inclusion of county-year fixed effects. Instead, I include a set of region-year fixed effects, $\alpha_{region,t}$.[31] Unlike time fixed effects, these allow for heterogeneity in macroeconomic conditions across U.S. regions and so provide for tighter identification.

Finally, I control for county-level demographic changes by including (in first-differenced form) a vector of time-varying demographic control variables, $X_{c,s,t}$. This consists of the proportion of the county's population that is Hispanic, Black, Asian, over 65 years old, or under the age of 1 (i.e., newborns).[32]

5.1.2 Second-Stage and Reduced-Form

If variation in *CountyExposure* induces exogenous variation in county-level lending, then fitted values from the first-stage regression can be used to obtain an estimate of the *causal* effect that changes in lending have on real economic activity. This therefore leads to the following "second-stage" regression:

$$\triangle ln(Y)_{c,s,t} = \alpha_{region,t} + \delta \times \triangle ln(\widehat{Lending})_{c,s,t} + \gamma' \triangle X_{c,s,t} + \epsilon_{c,s,t} \tag{7}$$

where $\triangle ln(\widehat{Lending})_{c,s,t}$ are the fitted values of the dependent variable from the first-stage regression (i.e., equation (5)); all controls, $X_{c,s,t}$, and fixed effects, $\alpha_{region,t}$, are the same as those included in the first-stage regression; and $ln(Y)_{c,s,t}$ is the natural log of a real economic variable in county c at time t – specifically, either total private-sector employment (measured as the number of jobs, full-time or part-time),[33] or total private-sector income (i.e., wages), each scaled by county population.

Likewise, the "reduced-form" relationship between a county's exposure to treated banks and real economic activity is as follows:

[31] As explained in section 4.2, these correspond to eight economic regions in the U.S., as defined by the Bureau of Economic Analysis; see http://www.bea.gov/regional/docs/regions.cfm for details.

[32] The results are unchanged if these are excluded.

[33] Information on the intensive margin of employment (i.e., hours worked) is not available at the county level.

$$\triangle ln(Y)_{c,s,t} = \alpha_{region,t} + \delta \times County\ Exposure_{c,s,t-1} + \gamma' \triangle X_{c,s,t} + \epsilon_{c,s,t} \tag{8}$$

where all variable definitions are the same as above.

5.2 Evidence of Real Effects

5.2.1 First-Stage Results

Table 6 reports the results from the first-stage regression. Column (1) shows that greater county exposure to treated banks has a highly significant effect on county-level lending; the instrument's F-stat of 10.2 exceeds the Staiger and Stock (1997) rule-of-thumb of 10. The coefficient magnitude of 0.065 ($p = 0.001$) implies that a one-standard-deviation increase (decrease) in county exposure results in a 2.3% increase (fall) in lending, corresponding to $1.7m on average. This result therefore shows that the previously documented credit reallocation by individual banks "aggregates up" to affect the total volume of lending at the county level.

The next three columns contain core robustness tests, mirroring those in Section 4.3.2 (where the analysis was performed at the bank-county-year level).

In column (2), I exclude from the construction of the county exposure measure instances where bank tax changes coincide with state corporate tax changes. The resulting coefficient estimate of 0.057 ($p = 0.017$) is very similar to that in column (1). This confirms that changes in county-level lending are due to credit reallocations induced by tax shocks to bank profitability (rather than, say, coincident corporate tax changes affecting the amount of credit demanded from treated banks).

Column (3) restricts the analysis to "far-away" states (states that do not border any tax-changing states). As explained in Section 4.3.2, banks might seek to circumvent the effects of tax changes by incentivizing firms to secure their loans against collateral located in other states. If the same loan is secured against different collateral in another state, this would give the false impression that credit has been "reallocated." Although possible, this is unlikely given that my data consist of loans to small businesses which are not expected to have multi-state operations. Such operations are especially unlikely in far-away states. Indeed, the coefficient estimate in far-away states of 0.076 ($p = 0.043$) is little different from the baseline in column (1), thus ruling out this potential confound.

Finally, in column (4) I consider the effects of negative and positive shocks separately. Specifically, I define two new variables: $PositiveCountyExposure$ is the absolute value of $CountyExposure$ if it is positive (meaning that the county is exposed banks experiencing tax increases in other states); and $Negative\ County\ Exposure$ is the absolute value of $County\ Exposure$ if it is negative (the county is exposed banks experiencing tax cuts in other states). The results support the conclusion of a symmetric effect. In particular, lending rises in counties exposed to banks experiencing tax increases in other states. (That is, these banks reallocate lending towards non-tax-changing counties.) Analogously, lending falls in counties exposed to banks experiencing tax cuts in other states. (That is, these banks reallocate lending away from non-tax-changing counties.) Both the positive and negative county exposure coefficients are significant ($p < 0.05$). Moreover, the absolute magnitudes of the two effects are not significantly different from each other ($p = 0.60$). This result is therefore consistent with my prior finding of a symmetric effect at the individual bank level.

5.2.2 Reduced-Form and I.V. Results

Table 7 presents the results from the reduced-form and instrumental variables regressions. Columns (1) and (2) consider the effects on county-level private-sector employment, while columns (3) and (4) consider the effects on county-level private-sector income (i.e., wages).

In column (1), the reduced-form coefficient estimate of 0.005 ($p = 0.011$) implies that a one-standard-deviation increase (fall) in county exposure results in a 0.17% increase (fall) in employment, or 69 jobs on average. In other words, it takes approximately $25,000 (=$1.7m/69) in extra lending to create one new job. In column (2), the instrumental variables regression estimates the elasticity of local employment with respect to local credit supply. A 1% change in lending leads to a 0.075% change in employment ($p < 0.05$).

In column (3), the reduced-form coefficient estimate of 0.007 ($p = 0.006$) implies that a one-standard-deviation increase (fall) in county exposure results in a 0.24% increase (fall) in income, or $4.3m on average. The elasticity estimate from the instrumental variables regression in column (4) shows that a 1% change in lending leads to a 0.104% change in income ($p = 0.040$).

5.2.3 Instrument Validity

To be a valid instrument, *County Exposure* must satisfy the exclusion restriction. That is, the only way *County Exposure* can affect county-level employment and income is through its effect on lending. This requirement can be broken down into two sub-conditions (see Angrist and Pischke (2009), pp. 151-3). The first sub-condition is that *County Exposure* must be orthogonal to any other determinant of county-level economic outcomes (the "exogeneity" requirement). The second sub-condition is that *County Exposure* can only affect employment and income through lending, rather than any other channel (the "uniqueness" requirement).[34]

Consider first the exogeneity requirement. Notice that *County Exposure* is a function of bank tax changes occurring in entirely separate states.[35] For the exogeneity requirement to be violated, state tax changes would have to systematically coincide with contemporaneous shocks to economic conditions in counties located in non-tax-changing states. It is unlikely, however, that state tax policies are driven by shocks to economic conditions in counties located in other states. This conclusion, in addition to being intuitive, is supported by at least three pieces of evidence.

First, as discussed in Sections 2 and 4.2.1, Farre-Mensa and Ljungqvist (2014) find that bank tax changes are unrelated to state economic performance – as measured by state GDP growth, the unemployment rate, and downgrades in the state's bond rating. Since economic conditions in the home state do not drive changes in bank tax rates, it even less plausible that economic conditions in other states will do so.

Second, the possibility that bank tax changes may coincide with shocks to economic performance in other states is more of a concern for nearby states, rather than, say, states on the other side of the country. However, as I show in Section 4.2.1, there is no difference in bank lending in tax-changing states compared to nearby states prior to the tax change taking effect. If economic conditions in nearby states led to changes in the bank tax rates, these economic conditions would likely be reflected in differences in bank lending behavior prior to treatment. The data reject this hypothesis.

Third, given the lack of pre-trends in prior result, the remaining possibility is that bank tax changes occur at the exact same time as shocks to economic conditions in nearby states. I find,

[34] Angrist and Pischke (2009) refer to the first requirement as "independence" and the second requirement as "exclusion."

[35] This can be seen by substituting equation (2) into equation (6).

however, that *County Exposure* affects lending even in "far-away" states that do not border any tax-changing states (see column (3) of Table 6). If bank tax changes were due specifically to concurrent shocks to economic conditions in nearby states, there would be no effect in far-away states. This is not the case.

These three arguments support the intuitive observation that state tax policies are unlikely to be driven economic conditions in non-tax-changing states. It is therefore likely that the exogeneity requirement is satisfied.

Turning to the uniqueness requirement, it is necessary that *CountyExposure* affect employment and income only through lending, rather than any other channel. The primary argument for this is as follows: *County Exposure* is specifically constructed to measure the intensity of banking sector linkages that exist between treated banks and counties located in non-tax-changing states. In this sense, the instrument builds on the evidence presented in the prior section that banks reallocate credit supply to non-tax-changing states in response to tax shocks occurring in other states. The added dimension is that the effect on lending at the county-level should be greater for counties in which treated banks represent a larger share of total (pre-treatment) lending. This hypothesis is confirmed by the first-stage results (see Table 6). The instrument therefore extends my prior findings by additionally exploiting geographic heterogeneity in the exposure of counties to banks that experience tax shocks in other states.

As I show below, further tests support the conclusion that the effect of *County Exposure* on employment and income operates exclusively through its effect on lending.

5.2.4 Heterogeneous Real Effects

Shocks to county-level business lending should only affect employment and income to the extent that local firms are credit-constrained. Such constraints are especially likely among smaller firms. This implies that the impact of exposure to treated banks ought to be stronger in counties where small businesses are more prevalent – as measured, for example, by the share of the total county workforce that they employ. Testing whether this is the case is therefore an important robustness check of the instrument's validity. That is, if *CountyExposure* only affects employment and income through its effect on lending, then its impact should be greater in counties where firms are smaller and so expected to be more sensitive to lending shocks.

To examine this, I obtain information on the breakdown of county employment by establishment size from the Census Bureau's County Business Patterns (CBP) database. For each county-year, I then calculate the share of total employment attributable to small establishments, i.e., those with fewer than 10 employees.[36] Following this, I classify counties as having either high or low "small establishment employment share," based on whether they are above or below the median among all counties in the year prior to the relevant tax change. To investigate potential heterogeneous effects on employment and income, I interact these indicators with *County Exposure*.[37]

Table 8 reports the results. Column (1) shows that the effect of *County Exposure* on local employment is only significant for counties in which small establishments account for a high share of total employment. The instrument's coefficient estimate in such counties is 0.009 ($p = 0.004$), compared to 0.002 ($p = 0.245$) for counties in which small establishments account for a low share of total employment. Moreover, these effects are significantly different from each other ($p = 0.043$).

As shown in column (2), a similar story emerges for the effect on income. Here, the instrument's coefficient estimate for counties in which small establishments account for a high share of total employment is 0.010 ($p = 0.006$), compared to 0.004 ($p = 0.081$) for counties in which the share is low.

Together, these findings provide corroborative evidence of the instrument's validity. Consistent with *County Exposure* only affecting local employment and income through its effect on lending, the results are stronger in counties where shocks to local credit supply should have the most impact, namely, in counties where firms are smaller, and thus more financially constrained.

In columns (3) and (4), I employ a similar test, but now sort counties based on their per capita income. This measure is useful because income and credit availability are inextricably connected: greater income means that larger debts can be serviced and that borrowers will typically be less risky. Moreover, in wealthier counties, property values are likely to be higher. The extra collateral

[36] More specifically, CBP provides information on the number of establishments broken down by size categories based on the number of employees. The establishment size categories are 1-4 employees, 5-9 employees, etc. To calculate total employment attributable to establishments with fewer than 10 employees I follow the approach used by Adelino, Schoar, and Severino (2014). That is, I first obtain employment for each establishment size category (i.e., 1-4 employees and 5-9 employees) by multiplying the number of establishments by the middle point of that category. For example, the total employment of 1-4 employee establishments is 2.5 multiplied by the number of 1-4 employee establishments. The share of total employment attributable to small establishments is thus the sum of employment in 1-4 and 5-9 employee establishments, divided by total county employment.

[37] That is, as is standard in the literature, I test for heterogeneous effects in reduced-form because interaction terms are to difficult implement within an IV setting (see e.g., Gilje, Loutskina, and Strahan (2013)).

therefore provides greater debt capacity (see for example, Adelino, Schoar, and Severino (2014)).

Accordingly, I classify counties as either "rich" or "poor," based on whether their per capita income is above or below the median among all counties in the year prior to the relevant tax change. These indicators are then interacted with $County\ Exposure$.

As column (3) shows, the instrument's effect on employment is only significant in poor counties. Specifically, the coefficient estimate in poor counties is 0.009 ($p = 0.002$) compared to 0.001 ($p = 0.483$) in rich counties – a difference in magnitudes that is significant ($p = 0.014$). The effects on income shown in column (4) reveal a similar pattern: the coefficient estimate for poor counties is 0.010 ($p = 0.011$), whereas the estimate for rich counties is 0.004 ($p = 0.094$). These results provide further support for the exclusion restriction. But they are also relevant in their own right. In particular, the fact that poorer areas are disproportionately impacted by policy shocks occurring in other jurisdictions has implications for income inequality.

5.2.5 County-Industry Level Analysis

The data contain further information on employment and income at the county-industry level. Exploiting this dimension of the data allows me to control for industry-level shocks. I do so by estimating the following regression:

$$\triangle ln(Y)_{i,c,s,t} = \alpha_{i,region,t} + \delta \times County\ Exposure_{c,s,t-1} + \gamma' \triangle X_{c,s,t} + \epsilon_{i,c,s,t} \tag{9}$$

where i indexes industries, c indexes counties, s indexes non-tax-changing states, and t indexes time; $ln(Y)_{i,c,s,t}$ is the natural log of either private-sector employment or income in industry i in county c at time t, scaled by county population; $\alpha_{i,region,t}$ is a set of industry-region-year fixed effects; the instrument, $County\ Exposure_{c,s,t-1}$, and the county demographic controls, $X_{c,s,t}$, are the same as used previously; and $\epsilon_{i,c,s,t}$ is the error term. Standard errors are clustered at the county level.

The industry data I use are disaggregated at the four-digit NAICS level. Following the literature, I exclude the financial and utilities sectors from all specifications.

In this regression, the industry-region-year fixed effects, $\alpha_{i,region,t}$, control for all coincident shocks to a given industry in a given region at a given point in time. By allowing for industry shocks to potentially vary by region, these fixed effects provide tighter identification than industry-time fixed effects alone, which assume that industry shocks are always uniform throughout the U.S.

If variation in *County Exposure* is indeed exogenous from the individual county perspective, controlling for industry-level shocks should have no impact on my results.

Additionally, the industry breakdown allows for another potential confound to be addressed. In particular, one possibility is that variation in *County Exposure* might coincide with local demand shocks that are specific to individual counties (and so not accounted for by the industry-region-year fixed effects). Although this is unlikely, I nonetheless test for this. I do so by following Mian and Sufi (2014), who exploit the fact that for industries in the "tradable" sector, demand is driven by national or regional factors, as opposed to localized expenditure shocks. In this sense, industries in the tradable sector (for example, manufacturing) fundamentally differ from both non-tradable industries (e.g., retail and restaurants), as well as the construction sector, for which demand is driven primarily by local conditions. To rule out localized demand shocks, I can therefore exclude the non-tradable and construction sectors from my regressions.[38]

Table 9 reports the results from these tests. Columns (1) and (2) show that the effect of *County Exposure* on employment is robust at the county-industry level ($p < 0.05$). Moreover, the estimated coefficient is identical whether or not the non-tradable and construction sectors are excluded from the regression, thus ruling out localized demand shocks. Columns (3) and (4) show that the same is true for income; the coefficients are both significant ($p < 0.01$) and identical.

5.2.6 Industry Bank Dependence

The county-industry level data permit another test of the instrument's validity. I can exploit the fact that not all industries are equally reliant on banks for external financing. To the extent that bank-dependent industries can be identified with reasonable accuracy, the effects on employment and income in such industries should therefore be stronger.

I test this by using two measures of an industry's bank dependence. My first measure is based on the Survey of Business Owners (SBO) Public Use Microdata Sample. The SBO consists of

[38] I obtain classifications of tradable, non-tradable and construction industries at the 4-digict NAICS level from Appendix Table 1 of Mian and Sufi (2014).

survey responses from a random sample of firms operating during 2007 with receipts of at least $1,000 provided by the IRS. The survey groups firms into two-digit NAICS industries. From this, I obtain information on the proportion of businesses per industry that rely on bank finance for either startup or expansion capital.[39] I then classify industries as having either "high" or "low" bank dependence, based on whether they are above or below the median.

My second measure is the commonly-used industry external financial-dependence index of Rajan and Zingales (1998). This measure allows for industry groupings at the finer four-digit NAICS level. To construct this measure, I follow Becker (2007). Specifically, I first obtain capital expenditures (capex) and operating cash flow for all U.S. Compustat firms during the sample period. For each firm, I then take the time-series sum of capex minus operating cash flow and divide by the time-series sum of capex. The median firm within each industry provides a measure of the industry's external financial dependence. Intuitively, industries in which the median firm's operating cash flow is insufficient to cover its investment needs are more dependent on external financing. As before, I classify industries into "high" or "low" categories based on whether they are above or below the median.

For each measure, the high and low indicators are then interacted with *County Exposure*. The results are shown in Table 10. Columns (1) and (2) consider the differential impact of industry bank dependence based on the SBO data. The results show that for both employment and income, *County Exposure* is only significant for industries with high bank dependence ($p < 0.001$). Moreover, for both employment and income the coefficient estimates of "high" versus "low" are significantly different from each other ($p < 0.05$). The results based on the Rajan and Zingales (1998) measure are similar: for both employment and income the effects are only significant for industries with high external financial dependence ($p < 0.05$). Consistent with the validity of the instrument, the impact of *County Exposure* on employment and income is therefore strongest in exactly the industries that are expected to be most reliant on banks for external financing.

[39] Adelino, Schoar, and Severino (2014) use the same data for information on the average amount of capital needed to start a firm per industry.

6 Conclusion

The past four decades have witnessed an unprecedented pace of cross-border banking sector integration. And yet, banking sector policy and regulation remains predominantly the responsibility of individual jurisdictions.

Given this environment, I investigate whether policies that impact banking sector profitability in one jurisdiction can spill over and affect real economic outcomes in other regions. To this end, I exploit changes in tax rates imposed on bank profits across U.S. states. I show that banks exposed to a tax shock in one state respond by reallocating small-business lending to other states in which they have a presence. Through an instrumental variables framework, I further show that greater county exposure to banks experiencing tax shocks in other states affects aggregate lending at the county level and, as a result, local employment and income. These findings therefore demonstrate that financial linkages provide a transmission channel by which one jurisdiction's policies may impose externalities on otherwise separate regions.

I end by noting that my findings inform the ongoing policy debate about the appropriate scope of international financial regulatory coordination. In particular, many observers have argued that, in the absence of policy coordination, competing jurisdictions will implement "beggar thy neighbor" policies that are likely to result in a harmful "race to the bottom" in regulatory standards. Yet whether such an outcome is likely in large part depends on how responsive banks are to policy differentials across jurisdictions. The potential harm that may result from jurisdictional competition is arguably limited if banking activities are "sticky" – for example, if high reallocation costs prevent banks from shifting their activities to regions with friendlier regulations. I find, however, that bank credit supply – at least within the laboratory of the U.S. – is highly sensitive to variation in policy-based incentives. Because of credit constraints at the firm-level, policy in one jurisdiction can therefore have sizable effects on real activity in other regions. To the extent that these findings can be generalized to an international context, such externalities suggest that substantial policy coordination may well be needed to prevent a regulatory race to the bottom.

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Table 1: Summary Statistics

This table presents descriptive statistics for the period 1996-2011 for bank-years in Panel A, county-years in Panel B, and bank-county-years in Panel C. "Small business lending" (i.e., the value of all business loans under $1m) and "number of small business loans" are presented in Panels A, B and C. In Panel A, "value small business loans/total commercial" is the book value of small business loans as a proportion of the bank's total commercial loan portfolio. In Panel B, "number of banks" refers to the number of distinct banks making small business loans within a county-year. All dollar values are expressed in 2010 dollars. All ratios are winsorized 0.5% in each tail.

Panel A: bank-years (N=19,022)					
			percentile		
	mean	s.d.	25th	50th	75th
Assets ($bn)	6.54	51.8	0.42	0.72	1.71
Deposits/Assets	0.79	0.10	0.74	0.81	0.86
Income Taxes/Pre Tax Income	0.30	0.13	0.28	0.33	0.36
Small Business Lending, annual ($m)	189.6	808.1	22.3	50.4	110.7
Number Small Business Loans, annual	3,924	45,069	166	383	827
Value Small Business Loans/Total Commercial	0.55	0.25	0.39	0.58	0.73

Panel B: county-years (N=50,233)					
			percentile		
	mean	s.d.	25th	50th	75th
Lending variables:					
Small Business Lending, annual ($m)	71.9	252.7	3.1	11.5	44.4
Number Small Business Loans, annual	1,488	6,112	97	289	900
Number of Banks	22.2	17.7	12	18	26
Average Loan Size ($)	44,332	25,113	26,567	40,250	57,605
Economic variables:					
Total Income, private-sector ($m)	1,765.3	7,882.1	87.2	245.4	783.7
Total Employment, private-sector (# jobs)	39,988	142,105	3,131	8,089	22,826
Per Capita Personal Income ($)	30,277	7,720	25,425	29,046	33,498
Population	93,729	299,043	11,286	25,599	66,002

Panel C: bank-county-years (N=738,152)					
			percentile		
	mean	s.d.	25th	50th	75th
Lending variables:					
Small Business Lending, annual ($1000's)	4,283	19,876	85	371	1,750
Number Small Business Loans, annual	87	577	3	10	42

Table 2: Bank Lending in Tax-Changing States

This table tests for the effect that a change in the tax rate on bank profits has on bank lending in tax-changing states. The unit of analysis is a bank-state-year. $ln(Lending)_{b,s,t}$ is the natural log of the total dollar value of small-business loans (those under \$1m) that bank b makes to state s at time t. $Treatment^{'}_{b,s,t-1}$ is defined as the percentage-point change in the tax rate on bank profits in state s in the prior year, multiplied by bank b's exposure to that state, where exposure equals the proportion of the bank's deposits that are sourced from that state in the year prior to the tax change. Column (2) contains a dynamic specification where, for example, $Treatment^{'}_{b,s,t+1}$ is the treatment measure one year prior to the tax change, and so on. Bank-level control variables are the log of total bank assets from the prior year, the log of total bank deposits from the prior year, and the log of one plus the number of years the bank has operated (i.e., bank age), while state level controls variables are the lagged values of log gross state product, employment, and population. These are omitted for brevity. All specifications include region-year fixed effects to control for unobserved time-varying shocks to macroeconomic conditions across U.S. regions, and are estimated in OLS first-differences to remove bank-state fixed effects from the levels equations. Standard errors, shown in parentheses, are two-way clustered at the bank and at the state level. *, ** and *** denotes significance at the 10%, 5% and 1% level, respectively.

	dep-var: $\triangle ln(Lending)_{b,s,t}$	
	(1)	(2)
$Treatment^{'}_{b,s,t-2}$		0.001
		(0.033)
$Treatment^{'}_{b,s,t-1}$	-0.051**	-0.053**
	(0.022)	(0.025)
$Treatment^{'}_{b,s,t}$		-0.004
		(0.018)
$Treatment^{'}_{b,s,t+1}$		0.024
		(0.021)
$Treatment^{'}_{b,s,t+2}$		0.004
		(0.016)
Region-year FE	Yes	Yes
State controls	Yes	Yes
Bank controls	Yes	Yes
Observations	20,563	20,563
Adj. R^2	0.061	0.061

Table 3: Credit Reallocation

I estimate the effect that changes in the tax rate on bank profits across U.S. states have on credit supply in otherwise unaffected states. The unit of analysis is a bank-county-year. Only counties located in states that have not experienced a tax change for at least three years are included. $ln(Lending)_{b,c,s,t}$ is the natural log of the total dollar value of small-business loans (those under \$1m) that bank b makes to county c (located in state s) at time t. $Treatment_{b,t-1}$ is defined as the percentage-point change in the tax rate on bank profits in another state in the prior year, multiplied by bank b's exposure to that state, where exposure equals the proportion of the bank's deposits that it sources from the tax-changing state in the year prior to the tax change (see equation (2)). Column (3) contains a dynamic specification where, for example, $Treatment_{b,t+1}$ is the treatment measure one year prior to the tax change, and so on. Bank-level control variables are the log of total bank assets from the prior year, the log of total bank deposits from the prior year, and the log of one plus the number of years the bank has operated (i.e., bank age). These are omitted for brevity. All specifications include county-year fixed effects to control for unobserved time-varying shocks to local economic conditions, and are estimated in OLS first-differences to remove bank-county fixed effects from the levels equations. Standard errors, shown in parentheses, are clustered at the bank level. *, ** and *** denotes significance at the 10%, 5% and 1% level, respectively.

	dep-var: $\triangle ln(Lending)_{b,c,s,t}$		
	(1)	(2)	(3)
$Treatment_{b,t-2}$			-0.078
			(0.141)
$Treatment_{b,t-1}$	0.216***	0.225***	0.226***
	(0.070)	(0.069)	(0.081)
$Treatment_{b,t}$			0.024
			(0.105)
$Treatment_{b,t+1}$			0.089
			(0.091)
$Treatment_{b,t+2}$			-0.098
			(0.130)
Bank controls	No	Yes	Yes
County-year FE	Yes	Yes	Yes
Observations	583,996	583,996	583,996
Adj. R^2	0.036	0.041	0.041

Table 4: Robustness

This table presents key robustness checks. Column (1) excludes instances where bank tax changes coincide with state corporate income tax changes. Column (2) restricts the sample to counties located in "far-away" states (those that do not border any tax-changing states). Column (3) considers only the pre-2005 period, prior to the increase asset size threshold for filing under the CRA. Column (4) excludes banks that have recently undertaken an acquisition. Column (5) excludes treated banks exposed to tax shocks in multiple states. Column (6) investigates the symmetry of treatment by coding two new variables: the absolute value of $Treatment$ if it is positive or negative. Column (7) examines the effect of bank tax rates in non-tax-changing states by interacting the positive and negative treatment variables with indicators for whether the state's tax rate is above or below the median among all states in which bank b lends. Bank-level control variables, omitted for brevity, are the same as in Table 3. All specifications include county-year fixed effects to control for unobserved time-varying shocks to local economic conditions, and are estimated in OLS first-differences to remove bank-county fixed effects from the levels equations. Standard errors, shown in parentheses, are clustered at the bank level. *, ** and *** denotes significance at the 10%, 5% and 1% level, respectively.

	dep-var: $\triangle ln(Lending)_{b,c,s,t}$						
	exclude corporate tax changes	only far-away states	year <2005	only non-acquirers	only single shocks	symmetry	high v low tax states
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
$Treatment_{b,t-1}$	0.171**	0.242***	0.210***	0.192***	0.192***		
	(0.084)	(0.089)	(0.073)	(0.075)	(0.069)		
$Positive\,Treatment$						0.442*	
						(0.256)	
$Negative\,Treatment$						-0.190**	
						(0.079)	
$Positive\,Treatment\times$							
...$High\,Tax\,State$							0.268
							(0.276)
...$Low\,Tax\,State$							0.564**
							(0.274)
$Negative\,Treatment\times$							
...$High\,Tax\,State$							-0.267***
							(0.089)
...$Low\,Tax\,State$							-0.107
							(0.108)
County-year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Bank controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	583,996	461,950	317,494	483,870	560,776	583,996	583,996
Adj. R^2	0.041	0.045	0.013	0.046	0.042	0.041	0.041
F-stat: $Positive\,Treatment = -Negative\,Treatment$						0.78	
F-stat: $High\,Tax\,State = Low\,Tax\,State$							3.25*

Table 5: Within-Bank Informational Asymmetry

This table tests for heterogeneous treatment effects based county distance from bank headquarters. As in Tables 3 and 4, the unit of analysis is a bank-county-year and only counties located in states that have not experienced a tax change for at least three years are included. In column (1), $Treatment$ is interacted with indicators for whether the county's distance from bank headquarters is above or below the median among all counties in which bank b lends in the prior year. Bank-level control variables, omitted for brevity, are the same as in Table 3. All specifications include county-year fixed effects to control for unobserved time-varying shocks to local economic conditions, and are estimated in OLS first-differences to remove bank-county fixed effects from the levels equations. Standard errors, shown in parentheses, are clustered at the bank level. *, ** and *** denotes significance at the 10%, 5% and 1% level, respectively.

	dep-var: $\triangle ln(Lending)_{b,c,s,t}$
	county distance from bank HQ (1)
$Treatment_{b,t-1} \times$ ...	
$County\ Close\ to\ Bank\ HQ$	0.317***
	(0.081)
$County\ Far\ from\ Bank\ HQ$	0.151**
	(0.069)
County-year FE	Yes
Bank controls	Yes
Observations	583,618
Adj. R^2	0.041
F-stat: $Close\ to\ HQ = Far\ from\ HQ$	9.15***

Table 6: Real Effects – First-Stage Results

This table presents the results from the "first-stage" regression of the the instrumental variables analysis; this tests for the effect that a county's exposure to treated banks has on county-level lending. The unit of analysis is a county-year and only counties located in states that have not experienced a tax change for at least three years are included. $ln(Lending)_{c,s,t}$ is the natural log of the total dollar value of small-business loans (those under $1m) made in county c (located in state s) at time t. The instrumental variable, $County\ Exposure_{c,s,t-1}$, is measured as the weighted sum of all bank-level treatments to which county c is exposed, where the weights are based on the lending shares of treated banks in county c prior to the tax change (see equation (6)). Column (1) shows the baseline specification. Column (2) excludes from the construction of the county exposure measure instances where bank tax changes coincide with state corporate income tax changes. Column (3) restricts the sample to counties located in "far-away" states (those that do not border any tax-changing states). Column (4) considers whether the effect is symmetric by coding two new variables: the absolute value *County Exposure* if it is positive or negative. County-level demographic control variables are the proportion of the county's population that is Hispanic, Black, Asian, over 65 years old, or under the age of 1 (newborns). These are omitted for brevity. All specifications include region-year fixed effects to control for unobserved time-varying shocks to macroeconomic conditions across U.S. regions, and are estimated in OLS first-differences to remove county fixed effects from the levels equations. Standard errors, shown in parentheses, are clustered at the county level. *, ** and *** denotes significance at the 10%, 5% and 1% level, respectively.

	dep-var: $\triangle ln(Lending)_{c,s,t}$			
	baseline	exclude corporate tax changes	only far-away states	symmetry
	(1)	(2)	(3)	(4)
$County\ Exposure_{c,s,t-1}$	0.065***	0.057**	0.076**	
	(0.020)	(0.024)	(0.038)	
$Positive\ County\ Exposure_{c,s,t-1}$				0.081**
				(0.038)
$Negative\ County\ Exposure_{c,s,t-1}$				-0.057**
				(0.024)
Region-year FE	Yes	Yes	Yes	Yes
Demographic controls	Yes	Yes	Yes	Yes
Observations	32,290	32,290	25,831	32,290
Adj. R^2	0.166	0.166	0.182	0.166
F-stat: $Positive\ County\ Exposure = -Negative\ County\ Exposure$				0.27

Table 7: Real Effects – Reduced-Form and I.V. Results

This table presents the results from the "reduced-form" and "second-stage" regressions of the instrumental variables analysis; these test for the effect that a county's exposure to treated banks has on county-level employment and income. The unit of analysis is a county-year and only counties located in states that have not experienced a tax change for at least three years are included. $ln(Employment)_{c,s,t}$ and $ln(Income)_{c,s,t}$ are the natural logs of total private-sector employment (i.e., the total number of jobs, full-time or part-time) and total private-sector income (i.e., wages), each scaled by county population, in county c (located in state s) at time t. The instrumental variable, $County\,Exposure_{c,s,t-1}$, is the same as in Table 6. Columns (1) and (3) show the reduced-form effect that $County\ Exposure$ has on employment and income. Columns (2) and (4) estimate the elasticities of employment and income with respect to lending, where lending is instrumented using $County\,Exposure$. County-level demographic control variables, omitted for brevity, are the same as in Table 6. All specifications include region-year fixed effects to control for unobserved time-varying shocks to macroeconomic conditions across U.S. regions, and are estimated in OLS first-differences to remove county fixed effects from the levels equations. Standard errors, shown in parentheses, are clustered at the county level. *, ** and *** denotes significance at the 10%, 5% and 1% level, respectively.

	$\triangle ln(Employment)_{c,s,t}$		$\triangle ln(Income)_{c,s,t}$	
	reduced form	I.V.	reduced form	I.V.
	(1)	(2)	(3)	(4)
$County\ Exposure_{c,s,t-1}$	0.005**		0.007***	
	(0.002)		(0.020)	
$\triangle ln(\widehat{lending})_{c,s,t}$		0.075**		0.104**
		(0.038)		(0.050)
Region-year FE	Yes	Yes	Yes	Yes
Demographic controls	Yes	Yes	Yes	Yes
Observations	32,290	32,290	32,290	32,290
Adj. R^2	0.127	0.127	0.152	0.152

Table 8: Real Effects – Heterogeneous Impact by County

This table tests for heterogeneous effects of *County Exposure* based on county characteristics. As in Tables 6 and 7, the unit of analysis is a county-year and only counties located in states that have not experienced a tax change for at least three years are included. Columns (1) and (2) investigate heterogeneous effects on employment and income based on the share of total county employment attributable to small establishments, defined as those with fewer than 10 employees. I interact *County Exposure* with indicators for whether a county's "small establishment employment share" is above or below the median among all counties in the year prior to the tax change. Columns (3) and (4) investigate heterogeneous effects on employment and income based on county per capita income. I classify counties as either "rich" or "poor" based on whether their per capita income is above or below the median among all counties in the year prior to the tax change, and then interact these indicators with *County Exposure*. County-level demographic control variables, omitted for brevity, are the same as in Table 6. All specifications include region-year fixed effects to control for unobserved time-varying shocks to macroeconomic conditions across U.S. regions, and are estimated in OLS first-differences to remove county fixed effects from the levels equations. Standard errors, shown in parentheses, are clustered at the county level. *, ** and *** denotes significance at the 10%, 5% and 1% level, respectively.

	county small establishment employment share		county income (per capita)	
	$\triangle ln(Emp.)_{c,s,t}$	$\triangle ln(Inc.)_{c,s,t}$	$\triangle ln(Emp.)_{c,s,t}$	$\triangle ln(Inc.)_{c,s,t}$
	(1)	(2)	(3)	(4)
County $Exposure_{c,s,t-1} \times ...$				
High	0.009***	0.010***		
	(0.003)	(0.004)		
Low	0.002	0.004*		
	(0.002)	(0.002)		
Rich			0.001	0.004*
			(0.002)	(0.003)
Poor			0.009***	0.010**
			(0.003)	(0.004)
Region-year FE	Yes	Yes	Yes	Yes
Demog. controls	Yes	Yes	Yes	Yes
Observations	32,288	32,288	32,280	32,280
Adj. R^2	0.127	0.153	0.127	0.152
F-stat: *High* = *Low*	4.09**	2.21		
F-stat: *Rich* = *Poor*			6.02**	1.76

Table 9: Real Effects – County-Industry Level

This table controls for industry level shocks. The unit of analysis is an industry-county-year, where industries are disaggregated at the four-digit NAICS level. Only counties located in states that have not experienced a tax change for at least three years are included. $ln(Employment)_{i,c,s,t}$ and $ln(Income)_{i,c,s,t}$ are the natural logs of total private-sector employment and income, each scaled by county population, in industry i in county c (located in state s) at time t. All specifications exclude the financial and utilities sectors. Columns (2) and (4) further exclude industries in the non-tradable and construction sectors (as defined by Mian and Sufi (2014)). County-level demographic control variables, omitted for brevity, are the same as in Table 6. All specifications include industry-region-year fixed effects to control for unobserved time-varying industry-level shocks across U.S. regions, and are estimated in OLS first-differences to remove county-industry fixed effects from the levels equations. Standard errors, shown in parentheses, are clustered at the county level. *, ** and *** denotes significance at the 10%, 5% and 1% level, respectively.

	$\triangle ln(Employment)_{i,c,s,t}$		$\triangle ln(Income)_{i,c,s,t}$	
	(1)	(2)	(3)	(4)
$County\ Exposure_{c,s,t-1}$	0.004**	0.004**	0.006***	0.006***
	(0.002)	(0.002)	(0.002)	(0.002)
Region-industry-year FE	Yes	Yes	Yes	Yes
Demographic controls	Yes	Yes	Yes	Yes
Excl. non-tradable & construction	No	Yes	No	Yes
Observations	1,591,146	987,653	1,591,146	987,653
Adj. R^2	0.049	0.044	0.053	0.047

Table 10: Real Effects – Heterogeneous by Industry Bank Dependence

This table tests for heterogeneous effects by industry, based on two measures of industry bank dependence. The unit of analysis is an industry-county-year, where industries are disaggregated at the four-digit NAICS level. Only counties located in states that have not experienced a tax change for at least three years are included. Columns (1) and (2) use data from the Survey of Business Owners. I obtain information on the proportion of businesses per industry that rely on banks for either startup or expansion capital. I then classify industries as having either high or low bank dependence based on whether they lie above or below the median. Columns (3) and (4) use the Rajan and Zingales (1998) external financial-dependence index. I classify industries as having either high or low external financial-dependence based on whether they lie above or below the median. The respective indicators are then interacted with *County Exposure*. County-level demographic control variables, omitted for brevity, are the same as in Table 6. All specifications include industry-region-year fixed effects to control for unobserved time-varying industry-level shocks across U.S. regions, and are estimated in OLS first-differences to remove county-industry fixed effects from the levels equations. Standard errors, shown in parentheses, are clustered at the county level. *, ** and *** denotes significance at the 10%, 5% and 1% level, respectively.

	industry bank dependence: small business survey data		industry financial dependence: Rajan and Zingales (1998)	
	$\triangle ln(Emp.)_{i,c,s,t}$	$\triangle ln(Inc.)_{i,c,s,t}$	$\triangle ln(Emp.)_{i,c,s,t}$	$\triangle ln(Inc.)_{i,c,s,t}$
	(1)	(2)	(3)	(4)
$County\ Exposure_{c,s,t-1} \times ...$				
High Bank Dep.	0.007***	0.009***		
	(0.002)	(0.002)		
Low Bank Dep.	-0.001	0.000		
	(0.003)	(0.003)		
High RZ			0.006**	0.008***
			(0.002)	(0.003)
Low RZ			0.002	0.002
			(0.002)	(0.003)
Region-industry-year FE	Yes	Yes	Yes	Yes
Demographic controls	Yes	Yes	Yes	Yes
Observations	1,591,146	1,591,146	1,453,353	1,453,353
Adj. R^2	0.049	0.053	0.051	0.056
F-stat: *High* = *Low*	7.26***	5.86**	1.18	2.06