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The spillover effects of bank taxes on corporate investment

DISSERTATION

submitted in partial satisfaction of the requirements
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in Management

by

Aruhn Venkat

Dissertation Committee:
Professor Terry Shevlin, Chair
Professor Siew Hong Teoh
Professor Mort Pincus

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DEDICATION

To

My sister, my parents and my dearest Yoojin

This is your achievement more than mine. One dissertation, one book, one lifetime is not enough to convey my gratitude to each of you. I'll avoid trying to do so in one page

The real trick in life is to want nothing, and to succeed in getting it.

Gregory David Roberts
Shantaram

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VITA

Aruhn V Venkat

2011	B.A. in Political Science, University of Southern California
2014	J.D. (*magna cum laude*), University of Nevada, Las Vegas
2017	M.A. in Economics, The American University
2020	Ph.D. in Management, University of California, Irvine

FIELD OF STUDY

Business taxation, corporate tax avoidance, corporate misbehavior, law and accounting

ABSTRACT OF THE DISSERTATION

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Professor Terrence Shevlin, Chair

In this study, I examine whether bank taxation "spills over" onto corporate investment. I use state bank tax rate changes as a quasi-natural experiment and measure corporate investment using the number of establishments and the number of employees of publicly traded firms in a given state in a given year. Using generalized difference-in-differences, I find evidence that bank taxes (1) reduce corporate investment in the taxing jurisdiction (direct spillover effect) and (2) increase corporate investment in non-taxing jurisdictions (indirect spillover effect). In terms of magnitude, I find that rate increases reduce employment and establishments by 8-9% and 4% respectively in the taxing jurisdiction while increasing employment and establishments by roughly 2% and 1% in non-taxing jurisdictions. Rate decreases increase employment and establishments by 2-3% in the taxing jurisdiction while decreasing employment and establishments by 3% and 1%, respectively. To provide evidence on the mechanism at play, I find results consistent with (1) bank taxes reducing lending among banks headquartered in taxing states and (2) bank taxes reducing debt financing and leverage among firms headquartered in taxing states. These results provide evidence that bank taxes affect investment by altering lending.

INTRODUCTION

In this study, I examine whether bank taxation "spills over" onto corporate investment. Bank taxes have emerged as a popular policy proposal, especially among Democrats. For example, Democratic Presidential candidates Elizabeth Warren and Bernie Sanders propose taxes or "fees" on the financial sector as part of their fiscal policy plans while President Obama proposed a tax on large banks in 2015 (American Banker 2019; Yglesias 2015). Although these proposals often garner political support, bank taxes may have negative effects on economic activity. For example, bank lending is an important source of financing for publicly traded corporations (e.g. Kalemli-Ozcan, Kamil and Villegas-Sanchez 2016). Thus, tax policy changes in the banking sector may "spill over" onto corporations by affecting the supply of credit, altering borrowers' financing choices and, ultimately, changing investment decisions. However, evidence on these effects is relatively sparse.[1]

Bank taxation reduces the after-tax profitability of banks, which may lead to less lending from local banks (e.g., Schandlbauer 2017; Smolyanksy 2019; I test this below in Section VI.A.). If bank taxation reduces the supply of local lending, firms may respond by reducing their local investment. On the other hand, taxation may encourage banks to use more debt financing by reducing the after-tax cost of debt. If banks increase their debt financing without altering their equity financing, they may increase their lending activities due to the increased levels of financing. As a result, corporate investment may rise. Thus, I first examine whether local bank taxes directly "spill over" onto local corporate investment

[1] In a recent review on bank taxation, Gawehn (2019) notes that identifying the investment and real effects of bank taxation on an economy is important because empirical evidence is currently weak or mixed.

(the direct effect).[2] Next, I examine indirect spillovers onto other, non-taxing jurisdictions. Specifically, I examine whether bank taxes increase investment in other jurisdictions in which the firm operates (the indirect effect). If firms weigh the costs and benefits of investing in particular states, higher bank taxes that reduce credit in a particular state increase the costs of investing that state. At the margin, investment in other states appears more attractive. Alternatively, if bank taxes increase the availability of credit in taxing states, firms may shift investment away from non-taxing states and into the state with more bank credit.

However, bank taxes may have little or no effect on investment for several reasons. First, publicly traded firms may not rely on local debt financing in making investment decisions. These firms have access to public sources of financing (e.g. equity or bonds) and may not require local financing (Becker, Jacob and Jacob 2013). Thus, such firms might substitute towards alternative sources of financing (e.g. equity or non-local debt) following reductions in local debt financing. Second, any changes in the credit supply may be too small to affect the investment choices of firms. For example, using similar samples, Schandlbauer (2017) documents a 4% decrease in credit supply following bank tax rate increases while Smolyansky (2019) finds that a 1% increase in bank tax rates leads banks to a 6.6% reallocation of credit. Thus, the effect of bank taxes on corporate investment is unclear, *a priori.*

[2] Subsidiaries or parents might engage in local borrowing. Prior studies document that local subsidiaries use local bank debt in their capital structures. For example, Desai, Foley and Hines (2004) argue that subsidiaries use external debt (including bank loans) less in locations where creditor rights are weak in part because local banks must expend additional monitoring resources in such locations, which raises the costs of borrowing. Dewaelhyns and van Hulle (2010) use Belgian data to document that subsidiaries use internal debt more than bank debt, but this varies in the cross-section of subsidiaries. Both studies also posit that parents might borrow locally to finance subsidiaries with internal debt.

Prior studies on bank taxes often focus on the systemic risk consequences of bank taxation. For example, de Mooij and Keen (2012) posit that high bank taxes may lead to excessive systemic risk by encouraging banks to finance with debt. In this spirit, Chaudry and Mullineux (2014) argue that income taxation may have been an indirect determinant of the 2008 financial crisis by increasing bank leverage and de Mooij, Keen and Orihara (2013) find evidence consistent with this argument. Other studies on the effects of bank income taxation focus specifically on bank leverage without reference to systemic risk (e.g., de Mooij et al. 2013, Schandlbauer 2017; Milonas 2018; Smolyanksy 2019) while others focus on lending outcomes. For example, several studies in European and international settings find evidence that bank taxation raises the costs of borrowing to customers and/or reduces banks' net profit margins (Demirgüç-Kunt and Huizinga 1999; Gaganis, Pasiouras and Tsaklanganos 2013). Schandlbauer (2017) studies the effects of state bank tax rate increases on banks' balance sheets. He finds results consistent with banks increasing their debt following rate increases and finds heterogeneity on the asset side of the balance sheet. Specifically, he documents that poorly capitalized banks reduce lending following tax increases, likely due to capital requirements constraining bank behavior. Other papers identify similar relations (e.g. de Mooij et al. 2013; Horvath 2013). Most similar to this study, Smolyansky (2019) finds that state bank tax rate changes affect small-business credit allocation (at the bank level) which results in changes to county-level unemployment levels.[3]

[3] Bank taxes should arguably affect smaller or regional banks more than large banks (e.g. "money center" banks). Large banks have more access to external capital and may even receive targeted tax breaks from local jurisdictions (Beatty, Ke and Petroni 2002; New York Business Journal 2018). However, both Schandlbauer (2017) and Smolyansky (2019) use banks of all size in their sample in providing evidence that bank taxes reduce lending, consistent with bank taxes affecting even large banks. Moreover, Smolyansky (2019) finds consistent results even when limiting his sample to only large banks.

I rely on state bank tax rate changes to provide a quasi-natural experiment to test my hypotheses.[4] My sample contains 15 bank tax increases and 35 bank tax rate decreases spanning 2000 to 2017. I use establishment data measured at the state-firm-year level from the YourEconomy Time Series to identify the geographic investment activities of publicly traded firms. My investment variables include (1) the number of establishments of a given firm in a given state in a given year and (2) the number of employees of a given firm in a given state in a given year.[5] Using generalized difference-in-differences in a pooled sample, I find that state bank tax rate increases reduce corporate investment while rate decreases increase corporate investment in the jurisdiction enacting the rate change. I also find that state bank tax rate hikes increase investment while rate decreases reduce investment in all other jurisdictions in which the firm operates except the one enacting the rate change.[6] Thus, my results are consistent with bank tax rate increases having both a direct effect and an indirect effect on corporate investment. These results are robust to inclusion of various firm-specific controls drawn from the investment model in Biddle, Hilary and Verdi (2009) as well as various state-level tax and economic variables.

To provide evidence on mechanisms, I provide evidence that bank taxes affect lending using state-level lending data from the FDIC's First Call reports and that bank tax rate increases in firms' headquarter states reduce firm-level debt financing. These last two sets

[4] Bank tax rates are often separate from corporate tax rates, but not always. Some states impose corporate taxes without collecting financial institution taxes. Details of state bank taxation are discussed in section 2.

[5] I do not distinguish between extensive and intensive margins of response. Thus, my results may be driven by new investment in a particular jurisdiction or increased investment in a jurisdiction in which a firm already has invested.

[6] In running indirect effect regressions, I omit treated observations from direct specifications and vice versa. Thus, control observations are consistent across both regressions. My identification strategy is explained further in Section 3.

of results provide evidence that bank taxes affect corporate investment because bank taxes affect the lending channel. Next, I limit my sample to only exogenous bank tax rate changes i.e. those that are *not* intended to counteract economic conditions (e.g. fiscal expansion during a depression). I use the narrative approach explained in Romer and Romer (2010) and Giroud and Rauh (2019) to identify bank tax rate changes that are exogenous to counter-cyclical fiscal policy. I also find evidence that bank taxes "spill over" more into neighboring states relative to non-neighboring jurisdictions. Next, I find results consistent with increased higher investment sensitivity among debt reliant firms and smaller firms within the same state following rate changes, thus limiting the threat of state-level factors biasing estimates. Using sales per establishment and return on assets, I find evidence that bank tax rate increases *enhance* investment efficiency while decreases have the opposite results.

My study adds to the literature identifying the effects of various forms of taxation on investment. This literature generally documents that taxation is negatively related to investment in various forms (capital expenditures, number of establishments, innovation). However, most studies in this literature generally focus on the taxation of business entities themselves (e.g. corporate taxes). A complementary and long line of studies in this literature focus on the investment effects of taxing *equity* financing. For example, Poterba and Summers (1983) find evidence that dividend taxes affect corporate investment, though other studies find that dividend taxes have no effect (e.g. Auerbach and Hassett 2003). More recently, Alstaedter, Jacob and Michaely (2017) find evidence that dividend taxes reduce investment in cash-poor firms but increase investment in cash-rich firms, consistent with the marginal source of financing for small firms being equity. By contrast, my study focuses on the effects of taxing a *debt* financing – specifically, bank debt financing – on investment

activities. My study contributes to this literature by documenting that *bank* tax rates are negatively associated with investment but also generate indirect effects into other jurisdictions. Smolyanksy (2019) finds evidence that state bank taxes affect small business lending and, as a result, county-level employment but does not document indirect effects nor study corporate investment activities. Thus, this study provides novel, policy-relevant evidence on the effects of one form of taxation.

This study also extends the literature on the consequences of bank taxation. For example, prior studies find that bank taxes alter bank balance sheets and capital ratios (e.g. Schepens 2016; Schandlbauer 2017). Andries, Gallemore and Jacob (2017) provide evidence that bank taxation can affect bank transparency. As noted above, prior studies find evidence consistent with bank taxation reducing lending and increasing both bank and systemic risk. These studies became particularly important in the wake of the financial crisis. However, these studies do not generally focus on investment effects. As capital intermediaries and liquidity providers, banks facilitate business investment (Acharya and Mora 2014). Their efficacy in lending is important to economic growth and the successful transmission of government policy (de Haan and Vlahu 2015). My study provides some evidence that bank taxes affect banks' roles in the economic system.

CHAPTER 1:

HYPOTHESIS DEVELOPMENT

Policymakers often hope to generate investment by changing business tax policy. For example, governments offer investment tax credits, R&D credits, accelerated depreciation and lower tax rates to induce investment and reverse these policies to disincentivize investment when an economy is "overheating." Numerous academic studies analyze the effects of various tax provisions on investment and business activity to provide policy-relevant evidence. For example, prior studies examine whether changes to depreciation rates affect investment (e.g., Zwick and Mahon 2016; Lester 2019) while others examine whether changes in tax payments or tax rates affect growth and investment (e.g. Green and Kerr 2019, Shevlin, Shivakumar and Urcan 2019; Giroud and Rauh 2019, Akcigit, Grigsby, Nicholas and Stantcheva 2019). From a financing perspective, numerous studies examine the effects of taxing the equity financing (e.g. dividend or capital gains taxes) channel on corporate investment (Poterba and Summers 1983, Auerbach, Hassett, Grullon, Gordon and Devereux 2006; Alstaedter, Jacob and Michaely 2017). However, few, if any, studies identify the corporate investment effects of taxing the debt financing channel. Even more specifically, prior studies do not generally focus on the effects of *bank* taxes on non-bank investment activities.

States impose taxes on banks for the same reasons they collect taxes more generally: higher government revenues. States generally tax banks either separately or as part of a larger business (e.g. corporate) tax scheme. Thus, several states impose separate financial institutions taxes which are often formalized in separate code sections from corporate tax code sections. For example, New York's financial institution tax applied to banks organized

as C-corporations and S-corporations until 2015 (New York State Department of Taxation and Finance; De, Mehran and Suher 2014). Other states tax all corporations - including banks organized as C-corporations – under a single code section. States impose various forms of taxes on banks (e.g., income taxes, bank surcharges and gross revenues taxes). I focus on taxes on bank income or franchise taxes collected on the basis of profits, but I also include taxes on net worth or capital taxes. Both S-corporation and C-corporation banks often pay these taxes. To the extent states impose different taxes on banks organized as S-corporations compared to C-corporations, I focus on C-corporation bank tax rates because the majority of banks are C-corporations.

Legally, banks owe taxes in any state jurisdiction to which they have physical or economic nexus. Banks have physical nexus to states in which they maintain a physical presence (e.g. establishments) (Serether, Eberle and Colavito Jr. 2011; Rowe 2016). Economic nexus is somewhat more ambiguous but generally arises when a bank has minimum contacts to a state (Serether et al. 2011). For example, under both the Massachusetts and Connecticut nexus requirements, banks owe nexus to a state when they solicit customers (e.g. lenders) in that state (Rowe 2016). Once nexus is established, firms must calculate the *amount* of taxes they owe to that state. Apportionment laws govern the division of taxes a bank pays to all state to which it has nexus. For banks, the location of the customer drives apportionment (Serether et al. 2011). More generally, the state in which a "preponderance of the contacts" (i.e. the state in which the loan was negotiated, signed, etc.) related to a loan arises is the state to which the lending bank will pay taxes on income from that loan (Rowe 2016). Importantly, these rules imply that borrowing firms cannot escape

bank taxation by borrowing across state lines. The out-of-state bank from which they borrow will owe taxes on that loan in the state in which the borrower is located.

Banks are subject to different regulatory scrutiny than other entities. Generally, regulation takes two forms: (1) minimum capital requirements and (2) maximum leverage restrictions (de Haan and Vlahu 2015).[7] For example, banks seeking Federal Deposit Insurance Corporation (FDIC) insurance must maintain particular levels of Tier 1 capital (i.e. less risky forms of capital such as equity) and cannot use high levels of leverage. These requirements are imposed on most banks with state or federal charters in the United States. Prior studies offer some evidence that these constraints bind financial institution behavior. For example, Schandlbauer (2017) finds that bank taxes reduce lending in banks that are poorly capitalized. He infers that this result arises because poorly capitalized banks are in danger of violating capital requirements and thus are unable to issue new loans when expected future after-tax profits fall (which would also reduce expected future equity capital).

Taxes can interact with these constraints to affect bank behavior. Taxes reduce the after-tax returns to a marginal investment. In banks, marginal investment generally takes the form of lending (usually short-term debt, see e.g. Dang, Gorton, Holmstrom and Ordonez 2017). Thus, taxation reduces the profitability of lending while also reducing a future source of capital. Several studies note that negative shocks to banks' equity reduces lending and may even lead to asset sales (e.g., Greenwood, Landier and Thesmar 2015). Consequently, taxation reduces the incentives to lend by reducing the profitability of marginal investments.

[7] While capital requirements are mandatory, some theory suggests that banks would hold equity cushions in the absence of regulation to avoid bank runs and to allow them to hold long-run loans on the asset side of their balance sheet (Hanson, Shleifer, Stein and Vishny 2015).

Moreover, banks are constrained in borrowing (or taking deposits) by capital and leverage constraints. Thus, they may eschew lending when tax rates are high. Empirically, prior studies provide evidence in support of this argument (e.g., Schandlbauer 2017, Milonas 2018; Smolyansky 2019).[8]

On the other hand, bank taxes incentivize the use of leverage in banks' capital structures. Leverage provides a tax shield that can offset the increase in tax rates (see e.g. Modigliani and Miller 1963; Graham 1996). If banks increase use of debt following a tax increase, they will have more capital available for lending *ceteris paribus*.[9] Empirically, several studies find evidence that bank taxes increase leverage following bank tax rate increases, though effects on lending are unclear. For example, Keen and de Mooij (2015) use an international sample of bank tax rate changes and find that leverage at banks increases following bank tax rate increases. Schepens (2016) uses a Belgian regulatory change and finds that banks reduce leverage in their capital structures when the tax benefits of debt decrease. Schandlbauer (2017) examines lending activities as a function of bank taxes but finds mixed results.

In light of these studies, I examine whether bank taxes affect non-bank corporate investment. The theory offered above predicts that bank taxes either increases or decreases

[8] A related literature examines the effects of taxes (among other things) on banks' decisions to increase capital. Scholes, Wilson and Wolfson (1990) find evidence that taxes, regulatory capital and profits all affect banks' decisions to realize investment gains, which are used to increase regulatory capital and reduce leverage. Moyer (1990) finds similar results, though she finds that taxes have limited effects on banks' financing decisions. Beatty, Chamberlain and Magliolo (1995) find that regulatory capital affects financing decisions, though they too find limited evidence that taxes affect financing.

[9] In particular, increased leverage may *replace* equity financing in the capital structure. If that is the case, then banks will not increase lending following bank tax rate increases because overall financing available to the bank is unchanged.

lending in taxing jurisdictions. If corporations use local banks to finance local investment, changes in lending due to taxation should ultimately lead to changes in corporate investment in the taxing jurisdiction. Prior studies provide evidence that firms borrow locally because local lenders provide cheaper loans than other lenders due to their superior local knowledge and local monitoring capabilities (see e.g., Berger, Klapper and Udell 2001, Giannetti and Laeven 2012, Nini 2004 and Winton 1999).

Moreover, borrowers are unlikely to switch to equity financing in response to changes in the bank credit supply. Evidence is consistent with bank debt comprising a substantial part of firm capital structure. Denis and Mihov (2002) find that 43% of all debt issued by public firms in their sample is bank debt. Rauh and Sufi (2010) document that bank debt is 13% of public firms' capital structure. Most recently, Gomes and Phillips (2012) document that public firms in their sample issued private debt (which includes bank loans) worth 22% of total firm value. Theoretically, firms are likely unable to *perfectly* substitute away from private debt because the many variations of financing are accompanied by their own sets of costs and benefits (e.g. agency costs; see Myers 1977, Becker, Jacob and Jacob 2013). Firms likely use private debt because the benefits of doing so outweigh the explicit costs and opportunity costs of using other financing (see e.g., Dang et al. 2017 who suggest that borrowers sort themselves into riskier and less risky borrowers and that risky borrowers use capital markets while safe borrowers use banks due to the nature of capital provision offered by these two sources). This leads to my first hypothesis (two-sided):

> ***H1***: Bank tax rate increases (decreases) increase or decrease corporate investment in the taxing jurisdiction (direct effect).

Next, I examine whether state bank taxes "spill over" investment activity into other states in which the firm operates. In theory, tax changes in a given jurisdiction "spill over" into other jurisdictions to the extent investment is mobile. For example, high taxes in California may induce firms to invest more in low-tax Nevada to the extent they can actually shift production facilities, stores, employees, etc. In general equilibrium models, the marginal, after-tax return on investment is constant across taxing jurisdictions. Thus, for example, higher bank taxes in a particular state lead to a re-allocation of investment into other states until the rates of return are equal across jurisdictions. Based on this simple theory, numerous studies in the public economics literature examine the "horizontal" externalities on investment imposed by a given state's tax policy on other states, resulting in competition among governments (e.g. Keen and Kotsogiannis 2002; Brulhart and Jametti 2006).

However, this literature does not generally examine the "horizontal" investment externalities arising from state taxation of the lending channel. Smolyansky (2019) finds some evidence that banks reallocate lending into less-taxed states, but does not examine the reallocation of corporate investment. In the business tax context, Giroud and Rauh (2019) find that corporate and personal taxes lead to the reallocation of business activity into other less taxed states. Suarez Serrato and Zidar (2016) find some evidence that state-level taxation affects the location of employees. In the state bank tax context, firms may reallocate activities in response to changes in bank taxes to the extent bank taxes affect the credit supply available in taxing jurisdictions. This reasoning leads to my second hypothesis (two-sided):

H2: Bank tax rate increases (decreases) decrease or increase corporate investment in other jurisdictions in which the firm operates (indirect effect).

Both hypotheses have substantial tension. First, corporations (especially large, publicly traded corporations) may substitute towards other forms of financing when bank taxes reduce the availability of local debt, though some evidence suggests that they do not. Thus, bank taxes may have little effect on the investment decisions of firms. Second, corporations may not be locally biased in borrowing decisions. That is, firms may not invest using local financing options. Finally, bank tax effects may be too small to ultimately affect corporate investment choices.

CHAPTER 2:

IDENTIFICATION STRATEGY

To test my hypotheses, I use a generalized difference-in-differences strategy with staggered treatment dates. The generalized difference-in-differences approach uses time and unit-specific fixed effects in lieu of a pre-post binary variable and a treatment-control binary variable as used in a traditional difference-in-differences approach. Moreover, the generalized approach is amenable to staggered event dates. Staggered event dates limit the threat of omitted variable bias arising from macroeconomic factors because macroeconomic factors likely do not coincide with each staggered event date. I rely on state bank tax rate changes to provide exogenous (in the context of my model) variation in financial institution taxes.[10] I use the following model to test my first hypothesis:

$$Investment_{ist} = \alpha + \beta_1 Increase_{ist} + \beta_2 Decrease_{ist} + \sum \beta_k \mathbf{X} + \epsilon_{ist} \quad (1)$$

Similarly, I use the following model to test my second hypothesis:

$$Investment_{i-st} = \alpha + \beta_1 Increase_{ist} + \beta_2 Decrease_{ist} + \sum \beta_k \mathbf{X} + \epsilon_{ist} \quad (2)$$

In equation (1), *Investment* is either the number of establishments of firm *i* in state *s* in year *t* or the number of employees of firm *i* in state *s* in year *t*. In equation (2), *Investment* is either the number of establishments of firm *i* in state -*s* (i.e. all other states in which firm *i* operates) in year *t* or the number of employees of firm *i* in state -*s* in year *t*.[11] Thus, the difference in these two specifications is that *Investment* is measured in the enacting state in equation (1) but in all other states in which firm *i* operates in equation (2). In both equations, *Increase* takes a value of 1 if firm *i* faced a bank tax rate increase in state *s* in year *t* and in all

[10] By exogenous, I do not mean purely random. Rather, I mean that bank tax rate changes limit the sources of endogeneity to only factors that coincide with the bank tax rate change.
[11] My main results are robust to using investment measured in *t*+1 as my dependent variable.

subsequent years (and 0 otherwise). *Decrease* takes a value of 1 if firm *i* faced a bank tax rate decrease in state *s* in year *t* and in all subsequent years (and 0 otherwise). In estimating equation (1), I omit treated observations from equation (2) from the control sample and vice versa. For example, I omit observations of a given firm's investment in non-enacting states when estimating whether a given firm's investment decreases in the state passing a bank tax rate increase. I omit these observations to avoid biasing estimates in my favor. By excluding these observations, I ensure that the control sample does not include observations affected by a bank tax rate change (e.g. due to firms shifting their investment). Thus, the control observations across both specifications are the exact same. These specifications include state and year fixed effects, consistent with the generalized difference-in-differences approach and also firm fixed effects to rule out any firm-specific time-invariant firm confounds.

I include both *Increase* and *Decrease* in all specifications to control for prior other bank tax rate changes. Specifically, the coefficient on *Increase* can be interpreted as the effect of a bank tax rate increase on investment after controlling for any prior bank tax rate decreases. **X** is a vector of controls. In baseline specifications, this set is empty because I assume bank tax rate changes are exogenous to investment. In secondary specifications, I include several firm-specific covariates and state-level economic and tax covariates.[12] All variables are defined in the next section and in Appendix A. I cluster standard errors at the state level, following the suggestions to cluster at the treatment level in Bertrand, Duflo and Mullainathan (2004).

[12] My main results are robust to incorporating the current corporate income tax rate, lagged state gross domestic product and lagged unemployment rate.

Concurrent changes in state fiscal policy pose the largest threat to my identification strategy. For example, states may offer corporations investment incentives while reducing bank tax rates to stimulate investment. I take two approaches to deal with this threat. First, I control for various state-level tax variables and concurrent state tax expense in secondary specifications. I control for the state corporate income tax rate (as a treatment variable) and the state personal tax rate to control for contemporaneous changes in these rates. State tax expense reflects average state-level tax rates but also various state-level tax incentives. Thus, state tax expense controls for changes in tax policy that reduce the current state tax expense while affecting investment. Second, I adopt a narrative approach to identifying "exogenous" tax rate changes. Following Romer and Romer (2010) and Giroud and Rauh (2019), I search for news regarding political and legislative justification for tax rate changes in the 3 years before and after a rate change using LexisNexis. I omit bank tax rate changes that were part of a tax plan intended to affect short-term growth via fiscal policy. For example, rate changes that were part of a package to stimulate economic activity due to recession concerns are omitted. Such rate changes are likely accompanied by simultaneous fiscal policy actions intended to spur investment. I use the remaining "exogenous" rate changes as my treatment variable in an additional test.

CHAPTER 3:

DATA, VARIABLES AND SAMPLE

I collect data from several sources to form my sample. I collect bank tax rate information from the Book of the States and CCH's State Tax Reporter for every year from 2000 to 2017. The Book of the States provides information on state finances along several dimensions. For example, it provides information on state revenues, tax collections, expenditures, etc. I collect financial institution tax information from the "Range of State Corporate Income Taxes" files. I cross-check these rates with financial institutions rates collected from CCH's State Tax Reporter. To the extent rates differ, I rely on the rates reported in the state's tax code. I find 15 bank tax rate increases and 35 bank tax rate decreases. These rates are primarily bank income tax rates but also include net worth taxes and capital taxes. On average, states raise bank tax rates by 2.9% in my sample and decrease state bank tax rates by 1.5%. I collect firm establishment data from the YourEconomy Time Series (YTS). YTS provides data on publicly traded firms' establishments and employees by state for all years in my sample. Finally, I use Compustat for firm-level financial statement information.[13] I omit all firm observations following a corporate headquarter location change. In secondary specifications, I omit observations with missing controls.

Both measures of investment (*ln(Emp)* and *ln(Est)*) are drawn from the YTS dataset and are measured at the firm-state-year level. Thus, these measures should be particularly sensitive to bank tax rate changes if bank tax rate changes affect investment. In secondary specifications, I incorporate several firm-specific covariates. First, I include current state tax expense scaled by lagged total assets (*State_Tax*). This measure captures the effects of

[13] I collect state gross state product and unemployment rates from the Bureau of Labor Statistics.

contemporaneous tax policy changes on firm investment decisions. I include increases and decreases in the state corporate income tax rate (*CIT_Inc* and *CIT_Dec)* to ensure that my results are not driven by concurrent corporate income tax rate changes. These variables are coded symmetrically to my variables of interest.[14] I include personal income tax rates (*PIT*) and the state's unemployment rate (*UE*) to control for local demand conditions. I include state-level GSP growth (*GSP_Growth*) to control for any economic effects that may drive bank tax rate changes and corporate investment. Next, I incorporate several variables drawn from the investment model in Biddle, Hilary and Verdi (2009).[15] I include an indicator for losses (*Loss*) because loss firms may invest less than profitable firms. I include the natural log of assets (*Size*) because larger firms may invest more. I include the market-to-book ratio (*MTB*) because growth firms may invest more. I include sales growth scaled by lagged total assets (*SalesGrowth*) for the same reason. I include Altman's Z-score (*AltmanZ*) because financially constrained firms may invest less. I include cash flow volatility measured over 3 years ($Vol(CFO)$) because volatile cash flows may deter investment (Minton and Schrand 1999). I include sales volatility calculated over 3 years ($Vol(Sales)$) because sales volatility may deter investment. I include both firm-level and industry-level K-structure (*FirmK* and *Ind_K*, respectively) as measures of market leverage at the firm and industry levels (computed as the ratio of long-term debt to the sum of long-term debt to the market value of equity). I include the ratio of cash flows from operations to sales (*CFOSale*) and the ratio of cash to property, plant and equipment (*Slack*).

[14] Results are robust to using a continuous measure of corporate income taxes instead of binary treatment variables.

[15] I am unable to control for several of the governance variables in Biddle et al. (2009) due to data limitations. However, they likely do not bias my estimates because bank taxes are unlikely to simultaneously change with firm governance.

I include these covariates to sharpen my estimates while also ruling out some correlated omitted variables. Some of these covariates are closely related to my hypothesized channel. Specifically, I hypothesize that bank taxes affect investment by affecting financing choices which may affect firm performance. Several of the variables in the Biddle et al. (2009) investment model such as k-structure, losses and growth lie along the causal channel I am interested in identifying. Therefore, I lag each of these covariates except for tax measures and state economic measures by one year to limit their effects on my proffered channel. I do not lag tax measures and state economic measures because they are intended to control for *concurrent* changes in tax policy or economic conditions which may affect tax payments (e.g. increased deductions or higher tax rates) and investment.

CHAPTER 4:

RESULTS

Descriptive statistics

I report descriptive statistics in Table 1. Average (median) *ln(Emp)* is around 1.18 (.08), which implies that the average (median) firm has 3.25 (1.08) employees per state. Average *ln(Est)* is .07 (.05) which implies that the average firm has just over 1 (1.05) establishment in each state.[16] I find that roughly 22% of my observations are treated by a bank tax rate increase (*Increase*) while 32% are treated by a bank tax rate decrease (*Decrease*). The median firm-year is not treated by either a bank tax rate increase or decrease. Mean current state tax expense (*State_Tax*) is roughly 1.6% of lagged total assets while median state tax expense is 2.2%. 21% of my sample are loss (*Loss*) firms. Average *Size* in my sample is around 7.1 while median *Size* is roughly 7.2. Average market-to-book (*MTB*) is around 3.3 while median market-to-book is 2.2. Sales growth (*SalesGrowth*) is around 9.3% on average with a median of 6.4%. Average Altman Z is roughly 1.66 while median Altman Z is 2.128. Average (median) volatility of cash flows ($Vol(CFO)$ is .334 (.361). The average (median) volatility of sales ($Vol(Sales)$) is around 4.997 (4.281). Firm K-structure (*K-structure)* averages 20% in my sample (with a median of 14%) while industry k-structure (*Ind_K*) is marginally higher at 20.5% (with a median of 19.9%). The average (median) ratio of CFO to sale (*CFOSale*) is around .078 (.089) while average (median) *Slack* is around 0.0284 (0.00).

In Table 2, I provide a list of state bank tax rate increases and decreases by year. Both increases and decreases are mostly evenly dispersed, suggesting that rate changes are not

[16] These statistics are small because firms often have 0 establishments in many states.

driven by the same macroeconomic effects. The highest number of bank tax rate changes in any year is 4 while several years in my sample do not contain any bank tax rate changes.

B. Parallel trends: Treatment dynamics

I begin by capturing the dynamics of corporate investment. Specifically, I create treatment variables that are coded to 1 in each of the 3 years before and after a bank tax increase and decrease.[17] That is, each of these variables captures investment in the *t+x* periods prior to and after treatment, where *x* ranges from -3 to 3. I then regress my measures of investment on these binary variables (after including controls, firm, state and year fixed effects) and tabulate the resulting coefficients in Table 3. Each coefficient represents the difference in investment between treated firms and control observations in a specific period relative to the treatment period.

In column (1), I report dynamic treatment effects when *ln(Emp)* is my outcome. The coefficients on $Increase_{t-3}$, $Increase_{t-2}$ and $Increase_{t-1}$ are not significant at conventional levels. Moreover, the coefficients are not monotonically increasing or decreasing, consistent with pre-treatment parallel trends. Following treatment, the coefficients on the *Increase* variables are generally negative with varying levels of significance. The coefficient on $Decrease_{t-3}$, $Decrease_{t-2}$ and $Decrease_{t-1}$ are mostly not significant, consistent with pre-treatment parallel trends. Following treatment, the coefficients related to *Decrease* are generally positive, though magnitudes vary.

In columns (2), I use *ln(Est)* as my outcome. The coefficients on pre-treatment variables are mostly not significant at conventional levels. Following a bank tax rate increase,

[17] These tests are intended to capture short-term trends in investment. Therefore, I do not include pre-post variables for the entire sample period. In my main regressions, I do not limit the sample to three years before and after a bank tax rate change.

ln(Est) drops, consistent with bank taxes reducing corporate investment. The pre-treatment coefficients on *Decrease* are not significant at conventional levels, implying pre-treatment parallel trends. Following treatment, the coefficients are positive, though significance varies. Overall, this evidence is consistent with parallel pre-treatment trends between treated and control firms.

The direct effect of bank taxes on corporate investment

I report the results of testing the *direct* effects of bank tax rate changes on investment in the taxing jurisdiction in Table 4. In columns (1) and (2), I use *ln(Emp)* as my dependent variable. In column (1), I report results using baseline specifications (i.e. sans covariates). The coefficient on *Increase* is negative and significant at the 1% level and the coefficient on *Decrease* is positive and significant at the 1% level. In column (2), I find that the coefficient on *Increase* is negative and significant at the 1% level after adding covariates while the coefficient on *Decrease* is positive and significant at the 1% level. In columns (3) and (4), I use *ln(Est)* as my dependent variable. I find that the coefficient on *Increase* is negative and significant at the 1% level while the coefficient on *Decrease* is positive and significant at the 1% level. In column (4), the coefficients retain the same signs and are significant at conventional levels. These results are consistent with my first hypothesis. Specifically, these results are consistent with bank rate changes affecting investment in the taxing jurisdiction.[18]

The indirect effect of bank taxes on corporate investment

[18] Notably, the coefficients on *CIT_Inc* and *CIT_Dec* are significant, consistent with investment decreasing incorporate income tax rates (see e.g., Giroud and Rauh 2019).

I report results of testing whether bank tax rate changes affect investment in other jurisdictions in which the firm operates in Table 5. In column (1), I report results using *ln(Emp)* as my dependent variable in my baseline specification. I find that the coefficient on *Increase* is positive and significant at the 1% level. The coefficient on *Decrease* is negative and significant at the 1% level. After adding covariates in column (2), these two coefficients retain the same sign and significance. In columns (3) and (4), I use *ln(Est)* as my dependent variable. Across both columns, I find that the coefficient on *Increase* is negative and significant at the 1% level while the coefficient on *Decrease* is positive and significant at the 1% level. Taken together, these results provide evidence that bank state taxes induce indirect effects onto other jurisdictions. Specifically, my results provide evidence that firms shift investment away from states that raise bank taxes and increase investment in states that reduce bank taxes.

Table 1. Descriptive statistics

	N	*Mean*	*p25*	*Median*	*p75*
Primary Outcomes					
ln(Emp)	649,636	1.180	0.0800	0.080	1.269
ln(Est)	649,636	0.071	0.0500	0.050	0.050
Treatment Variables					
Increase	649,636	0.219	0.000	0.000	0.000
Decrease	649,636	0.316	0.000	0.000	1.000
Covariates					
CIT_Inc	649,636	0.284	0.000	0.000	0.000
CIT_Dec	649,636	0.411	0.000	1.000	1.000
PIT	649,636	5.25	3.4	5.72	7.01
GSP_Growth	649,636	0.038	0.025	0.038	0.053
UE	649,636	6.7	5.1	6.6	8.3
State_Tax	542,554	0.016	0.008	0.022	0.030
Loss	542,554	0.205	0.000	0	0
SalesGrowth	542,554	0.093	-0.0147	0.064	0.157
AltmanZ	542,554	1.664	1.141	2.128	3.097
MTB	542,554	3.025	1.324	2.209	3.703
Size	542,554	7.177	5.964	7.252	8.573
Vol(CFO)	542,554	0.334	0.214	0.361	0.526
Vol(Sales)	542,554	4.997	2.697	4.281	6.437
K-structure	542,554	0.198	0.0241	0.140	0.300
Ind_K	542,554	0.205	0.120	0.199	0.266
CFOSale	542,554	0.078	0.0420	0.089	0.144
Slack	542,554	0.028	0.000	0.000	0.001

Notes: This table presents descriptive statistics of my sample. My sample spans 2000 to 2017. All continuous variables are winsorized at the 1st and 99th percentiles.

Table 2. Tax rate changes by year

Year	# of Increases	# of Decreases
2000	0	0
2001	0	0
2002	0	0
2003	4	4
2004	0	4
2005	1	0
2006	1	2
2007	0	0
2008	0	1
2009	2	2
2010	2	3
2011	0	2
2012	1	2
2013	1	3
2014	1	3
2015	0	4
2016	1	2
2017	1	3
2018	0	0
Total	15	35

Notes: This table presents the number of state bank tax rate increases and state bank tax rate decreases by year

Table 3. Treatment dynamics

	(1) *ln(Emp)*	(2) *ln(Est)*
$Increase_{t-3}$	-0.005	-0.006***
	(-0.30)	(-6.01)
$Increase_{t-2}$	-0.004	0.004
	(-0.22)	(0.06)
$Increase_{t-1}$	-0.002	-0.059
	(-0.57)	(-0.14)
$Increase_{t}$	-0.012***	-0.008***
	(-4.46)	(-4.25)
$Increase_{t+1}$	-0.009	-0.001
	(-1.15)	(-1.34)
$Increase_{t+2}$	-0.022***	-0.017***
	(-4.01)	(-4.78)
$Increase_{t+3}$	-0.021*	-0.014*
	(-1.86)	(-1.89)
$Decrease_{t-3}$	-0.025*	0.015
	(-1.86)	(1.19)
$Decrease_{t-2}$	-0.001	-0.001
	(-1.00)	(-1.18)
$Decrease_{t-1}$	-0.000	-0.001
	(-0.33)	(-1.18)
$Decrease_{t}$	0.012*	0.046
	(1.68)	(1.48)
$Decrease_{t+1}$	0.003*	0.014**
	(1.77)	(2.14)
$Decrease_{t+2}$	0.064***	0.034***
	(4.98)	(5.20)
$Decrease_{t+3}$	0.047***	0.039***
	(2.86)	(3.83)
Observations	153,954	153,954
Adjusted R-squared	0.116	0.141
Controls	Yes	Yes
State FE	Yes	Yes
Firm FE	Yes	Yes
Year FE	Yes	Yes

Notes: This table presents results of estimating treatment dynamics. Each *Increase* and *Decrease* variable takes a value of 1 in the period *t+x,* where *x* ranges from -3 to 3 and *t* is the treatment year, and 0 otherwise. Investment is measured using *ln(Emp)* and *ln(Est)*. *Increase* is coded to 1 if state *s* raises its bank tax rate in year *t* or a prior year and 0

otherwise. *Decrease* is coded to 1 if state *s* lowered its bank tax rate in year *t* or a prior year and 0 otherwise. All variables are defined in Appendix A. Standard errors are clustered at the state level and are robust to heteroscedasticity. Two-sided t-tests are reported in parentheses. *, ** and *** denote significance at the 10%, 5% and 1% levels.

Table 4. The *direct* effect of bank taxes on corporate investment

	Pred.	(1) *ln(Emp)*	(2) *ln(Emp)*	(3) *ln(Est)*	(4) *ln(Est)*
Increase	-/+	-0.091***	-0.078***	-0.039***	-0.041***
		(-3.67)	(-3.15)	(-5.84)	(-5.37)
Decrease	-/+	0.032***	0.024***	0.032***	0.026***
		(4.28)	(2.92)	(7.16)	(5.93)
CIT_Inc			-0.157***		-0.051***
			(-6.30)		(-3.26)
CIT_Dec			0.020		0.022
			(0.00)		(0.00)
PIT			-0.001		0.001
			(-0.36)		(0.57)
GSP_Growth			0.096		0.059
			(1.13)		(1.01)
UnempRate			-0.003		0.001
			(-1.51)		(0.32)
State_Tax			-0.081***		-0.081***
			(-6.64)		(-6.64)
Loss			-0.001		0.007***
			(-1.22)		(4.97)
Size			0.024***		0.020***
			(13.79)		(9.57)
MTB			0.000**		-0.000***
			(2.46)		(-2.76)
SalesGrowth			-0.003***		-0.005***
			(-3.26)		(-3.53)
AltmanZ			-0.001***		-0.002***
			(-10.44)		(-12.74)
Vol(CFO)			0.009***		0.031***
			(7.20)		(8.21)
Vol(Sales)			-0.000		0.003***
			(-1.05)		(11.64)
K-structure			0.004		-0.003*
			(1.48)		(-1.78)
Ind_K			0.037***		0.127***
			(3.15)		(12.83)
CFOSale			-0.019***		-0.052***
			(-8.17)		(-8.99)
Slack			0.008***		0.018***
			(7.21)		(5.10)

Observations	190,742	153,954	190,742	153,954
Adjusted R-squared	0.057	0.221	0.056	0.230
State FE	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes

Notes: This table presents the results of testing whether bank rate changes affect investment in the taxing jurisdiction using a generalized difference-in-differences approach. Investment is measured in the taxing jurisdiction using *ln(Emp)* and *ln(Est)*. *Increase* is coded to 1 if state *s* raises its bank tax rate in year *t* or a prior year and 0 otherwise. *Decrease* is coded to 1 if state *s* lowers its bank tax rate in year *t* or a prior year and 0 otherwise. All variables are defined in Appendix A. Standard errors are clustered at the state level and are robust to heteroscedasticity. Two-sided t-statistics are reported in parentheses. *, ** and *** denote significance at the 10%, 5% and 1% levels.

Table 5. The *indirect* effect of bank taxes on corporate investment

	Pred.	(1) *ln(Emp)*	(2) *ln(Emp)*	(3) *ln(Est)*	(4) *ln(Est)*
Increase	-/+	0.019***	0.021***	0.009***	0.009***
		(3.21)	(3.05)	(3.52)	(3.56)
Decrease	-/+	-0.030***	-0.032***	-0.008***	-0.015***
		(-3.42)	(-3.19)	(-4.28)	(-4.00)
CIT_Inc			0.101***		0.137***
			(3.37)		(4.53)
CIT_Dec			-0.001		-0.001
			(-0.00)		(-0.01)
PIT			-0.010		0.030*
			(-0.92)		(1.90)
GSP_Growth			-0.000		-0.001
			(-0.49)		(-0.64)
UnempRate			-0.000		-0.001
			(-0.89)		(-1.23)
State_Tax			0.311***		0.089***
			(12.40)		(9.00)
Loss			-0.002		-0.000
			(-1.57)		(-1.44)
Size			0.026***		0.004***
			(18.36)		(9.96)
MTB			0.000		0.000
			(0.06)		(1.44)
SalesGrowth			-0.017***		-0.004***
			(-8.40)		(-8.76)
AltmanZ			-0.003***		-0.001***
			(-12.88)		(-11.38)
Vol(CFO)			0.052***		0.007***
			(18.09)		(13.24)
Vol(Sales)			0.006***		0.001***
			(16.96)		(10.45)
K-structure			-0.004		-0.003***
			(-1.22)		(-5.58)
Ind_K			0.193***		0.030***
			(16.23)		(10.99)
CFOSale			-0.086***		-0.006***
			(-14.50)		(-5.93)
Slack			0.021***		0.002***
			(7.40)		(3.01)
Observations		458,894	388,600	458,894	388,600

Adjusted R-squared	0.070	0.209	0.072	0.125
State FE	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes

Notes: This table presents the results of testing whether bank rate changes affect investment in non-enacting jurisdictions in which the firm operates using a generalized difference-in-differences approach. Investment is measured using *ln(Emp)* and *ln(Est)* in all other states in which the firm operates except the taxing jurisdiction. *Increase* is coded to 1 if state *s* raises its bank tax rate in year *t* or a prior year and 0 otherwise. *Decrease* is coded to 1 if state *s* lowers its bank tax rate in year *t* or a prior year and 0 otherwise. All variables are defined in Appendix A. Standard errors are clustered at the state level and are robust to heteroscedasticity. Two-sided t-tests are reported in parentheses. *, ** and *** denote significance at the 10%, 5% and 1% levels.

CHAPTER 5:

ADDITIONAL TESTS

Bank taxes and lending

Next, I assess whether bank taxes affect lending. In my main tests, I assumed that bank taxes reduce lending and, as a result, affect corporate investment behavior. I explicitly test this assumption in this section. Prior studies find evidence that state bank tax rate changes affect lending. Schandlbauer (2017) finds evidence that taxes reduce lending among poorly capitalized banks while Smolyansky (2019) finds similar evidence but focuses on small business lending.

I collect bank lending data from the Federal Deposit Insurance Corporation's Consolidated Reports of Condition and Income (Call Reports). I collect state-level data on lending activities of all banks headquartered in a given state in a given year, as well as other aggregated bank characteristics (e.g. number of employees, income, branches, etc.). I am unable to identify the amount of lending *within* the taxing state due to data restrictions but expect that banks headquartered in a state provide a substantial amount of credit to that state. I focus on two lending outcomes: commercial lending and total loans outstanding. Commercial lending is the total dollar amount of commercial lending outstanding, net of loan loss reserves. Gross lending is the total dollar amount of loans outstanding (not netted against loan loss reserves). The former is intended to capture lending to businesses such as corporate borrowers while the latter captures all forms of lending. I take the natural log of these amounts plus one to form my dependent variables.

I include several controls. I include the natural log of the number of branches (*ln(Branches)*), employees (*ln(Employees)*) and deposits (*(ln(Deposits)*) of banks

headquartered in state *s* in year *t* to control for the size of banks. I control for the natural log of the total loan loss provisions (*(ln(Loan_loss_prov)*) of banks headquartered in the state to control for bank risk-taking. I control for individual loans (*ln(Individ_loan)*) to ensure results are not driven by general expansions of loan portfolios. I control for the net income of these banks to control for profitability differences in banks across states (*NetIncome*). I also control for other tax rates (*CIT* and *PIT*) to ensure results are not driven by concurrent changes in non-bank tax rates. To control for demand conditions, I control for state economic growth (*GSP_Growth*) and the unemployment rate (*UE*).

Results are reported in Table 6. In Column (1), I use *ln(Com_Loan)* as my dependent variable. I find that the coefficient on *Increase* is negative and significant at the 1% level. In the same column, the coefficient on *Decrease* is positive and significant at the 1% level. These results provide evidence that bank tax rate changes affect commercial lending among banks headquartered in that state. In column (2), I use *ln(Gross_Loan*) as my dependent variable. The coefficient on *Increase* is negative and significant at the 1% level while the coefficient on *Decrease* is positive and significant at the 10% level. These results reinforce the inference that bank taxes affect lending, at least among banks headquartered in taxing states.

Firm-level debt financing and leverage

Next, I provide evidence that bank taxes affect investment by altering the *use* of debt. In my main analysis, I assumed that bank taxes altered the supply and use of debt by potential borrower firms. I provided evidence that bank taxes alter the supply of debt in the prior section. Thus, in this section, I examine whether bank tax rate changes affect the use of debt, measured using the debt financing and leverage of firms.

I measure debt financing (*DebtFin*) as the sum of long-term and current debt financing activities from the statement of cash flows (Compustat items dltis and dlcch) scaled by lagged total assets. I replace missing values to 0 because missing values imply that the firm did not issue debt in that particular year. I measure leverage (*Leverage*) as the firm's long-term debt scaled by lagged total assets. I use these variables as outcomes in the same difference-in-differences specifications I employed in my main analyses. However, *Increase* (*Decrease*) is now coded to 1 if firm *i*'s headquarter state raised (lowered) its financial institution taxes in year *t* and 0 otherwise. I use Tim McDonald's headquarter data.

Results are reported in Table 7. In columns (1) and (2), I examine the effects of headquarter bank tax rate changes on debt financing (*DebtFin*). I find that the coefficients on *Increase* are negative and significant at conventional levels across both columns while the coefficient on *Decrease* is positive and significant across both columns. In columns (3) and (4), I use *Leverage* as my dependent variable. Across both columns, the coefficient on *Increase* is negative and significant at the 5% level or higher and the coefficient on *Decrease* is positive and significant at the 5% level. Taken together, these results provide evidence that firms use less debt following bank rate increases.

"Exogenous" bank tax rate changes

In this section, I replace my measure of bank rate changes with an "exogenous" subset of bank rate changes. By "exogenous," I do not mean random. Rather, I borrow the Romer and Romer (2010) definition in defining "exogenous" rate changes as those that are not accompanied by or motivated by countercyclical fiscal policy. In the same spirit, I follow

Romer and Romer (2010) in identifying exogenous tax rate changes.[19] Specifically, I search LexisNexis for news articles 3 years prior and subsequent to a tax rate change to identify rate changes that are unrelated to macroeconomic concerns. For example, an exogenous bank rate change might be driven by politics rather than the state's yearly output. In my sample, I find exogenous bank rate changes are primarily driven by (1) concerns about government revenue, (2) related to the expiration of prior rate changes and (3) political motivations, generally related to campaign promises. Several rate changes are not well-covered by media sources and I am thus unable to determine their exogeneity. I omit such rate changes. I classify 5 bank rate increases and 14 bank rate decreases as exogenous. I replace *Increase* and *Decrease* with these rate changes in my main specifications to determine whether my main results are robust to using these exogenous tax rate changes. Firms treated by *endogenous* rate changes are omitted from my sample in this test.

My results are reported in Table 8. In Panel A, I report results of using exogenous rate changes in testing for direct effects. In columns (1) and (2), I use *ln(Emp)* as my dependent variable. In column (1), the coefficient on *Exog_Inc* is negative and significant at the 1% level while the coefficient on *Exog_Dec* is positive and significant at the 1% level in my baseline specification. After adding covariates, the coefficient on *Exog_Inc* remains negative and significant at the 1% level while the coefficient on *Exog_Dec* is positive and significant at the 1% level. In columns (3) and (4), I use *ln(Est)* as my dependent variable. In column (3), the coefficient on *Exog_Inc* is negative and significant at the 1% level while the coefficient on

[19] I also follow Romer and Romer (2010) in defining "exogenous" rate changes. I do not refer to random or unanticipated changes in bank rates as exogenous. Rather, I define exogenous to mean rate changes that are unlikely correlated with other fiscal policy changes.

Exog_Dec is positive and significant at the 1% level. After adding covariates in column (4), the coefficient on *Exog_Inc* remains negative and significant at the 1% level while the coefficient on *Exog_Dec* is positive and significant at the 1% level.

In Panel B, I report results of using exogenous tax rate changes in testing for indirect effects. In columns (1) and (2), I report results when *ln(Emp)* is my dependent variable. In column (1), the coefficient on *Exog_Inc* is positive and significant at the 1% level while the coefficient on *Exog_Dec* is negative and significant at the 1% level. In columns (3) and (4), I use *ln(Est)* as my dependent variable. Across both columns, the coefficient on *Exog_Inc* is negative and significant at the 1% level while the coefficient on *Exog_Dec* is positive and significant at the 1% level. These results provide evidence that the main results of testing for indirect effects are not driven by concurrent changes in fiscal policy that may confound analyses.

Neighboring states v. non-neighboring states

Next, I examine whether indirect effects arise in neighboring jurisdictions relative to other jurisdictions. I define neighboring jurisdictions as those that share a border with the taxing jurisdiction. In the case of Alaska, I define Washington as its neighbor and in the case of Hawaii, I define California as its neighbor. Re-allocation of investment into distant jurisdictions is likely more costly due to distance relative to neighboring states. Moreover, neighboring states are likely similar in economic characteristics and thus reduces the risk of investment relative to distant jurisdictions (Mukherjee, Singh and Zaldokas 2017; Smolyansky 2019).

To test my prediction, I split the indirect effects sample into neighboring jurisdictions in which a given firm operates and other jurisdictions in which a firm operates. I then

perform the indirect effects analysis in each, respective sub-sample. Results are reported in Table 9. In Panel A, I report results in the "neighboring" state jurisdictions. I find that the coefficient on *Increase* is positive and significant at the 1% level and the coefficient on *Decrease* is negative and significant at the 1% level across all columns. In Panel B, I report results using non-neighboring states in which a given firm operates. Overall, I find weaker results in this sample. From a magnitude standpoint, the coefficients on both *Increase* and *Decrease* are generally larger in the neighboring states sample relative to the non-neighboring states sample. Thus, the results are consistent with bank tax rate changes "spilling over" more into neighboring jurisdictions compared to non-neighboring jurisdictions, likely because investment in neighboring jurisdiction is less costly and less uncertain compared to investment in distant jurisdictions.

Within-state analyses: Leverage

Next, I compare investment outcomes at firms within the same state-year, but with varying degrees of exposure to changes in the credit supply, by including $State \times Year$ fixed effects. By doing so, I ensure that state-level economic or political factors do not drive my results. In order to include $State \times Year$ fixed effects, I require different treatment variables because *Increase* and *Decrease* vary by state-year. Thus, I interact these variables with firm-level variables that are intended to capture exposure to credit supply shocks due to bank tax rate changes. First, I use firm-level leverage. Intuitively, high-leverage firms should be more exposed to a bank tax rate change compared to low-leverage firms in the same state because high leverage implies debt reliance. I omit all firm-years with less than 5 establishments in the treated state to ensure that comparison groups are sufficiently exposed to the bank rate change. I calculate the leverage ratio (*Leverage*) as the firm's ratio of long-term debt to lagged

total assets. I interact *Leverage* with my treatment variables to examine the effect of bank taxes on firms exposed to the same bank tax rate change and also include firm fixed effects. Notably, state-level variables are dropped in these regressions because they are collinear with $State \times Year$ fixed effects.

I report results in Table 10. In Panel A, I examine whether the *direct* effects of bank tax rate changes are increasing in the leverage ratio among firms in the same state-year. I find that the coefficient on *Increase*×*Leverage* is negative and significant across all specifications while the coefficient on *Decrease*×*Leverage* is positive and significant across all specifications. In Panel B, I examine whether *indirect* effects are increasing in the leverage ratio. This test essentially compares investment in non-enacting states of firms with higher leverage to investment in non-enacting states of firms with lower leverage within the same state-year. Across all four columns, the coefficients on *Increase*×*Leverage* is positive and significant while the coefficients on *Decrease*×*Leverage* is negative and significant. These results provide evidence that state-year level factors (such as political issues or economic issues) are not responsible for my main findings.

Within-state analysis: Size

Next, I interact my treatment variables with firm size. That is, I compare investment outcomes between firms in the same-state year but with different firm sizes. I expect that the effects of bank taxes are weaker for large firms because large firms are less reliant on local bank debt compared to other firms because they have access to other financing sources (Becker, Jacob and Jacob 2013).

Empirically, I expect that the coefficient on the interaction between *Large* and *Increase* is positive in direct effect specifications and negative in indirect effect specification.

I expect that the coefficient on *Large* and *Decrease* is negative in direct effect specifications and positive in indirect effect specifications. These predictions are consistent with large firms reducing investment less in taxing jurisdictions than other firms and shifting investment less into other jurisdictions in response to bank tax rate increases.

Results are reported in Table 11. In Panel A, I report estimates of direct effects specifications. The coefficient on the interaction between *Large* and *Increase* is positive and significant at the 10% level or above across all four columns. The coefficient on the interaction between *Large* and *Decrease* is positive and significant across all columns except for column (2). Generally, these results are consistent with larger firms' local investment responding less to bank tax rate changes compared to smaller firms' investment.

In Panel B, I report estimates of indirect specifications. The coefficient on the interaction between *Large* and *Increase* is negative and significant at the 5% level or higher across all columns. The coefficient on the interaction between *Large* and *Decrease* is positive and significant at the 10% level or higher across all columns. These results are consistent with larger firms shifting less investment in and out of enacting jurisdictions compared to other firms in response to bank tax rate changes. Overall, these tests provide evidence that the effects of bank taxes on corporate investment are not driven by various state-year factors such as concurrent changes in tax policy, economic conditions or political conditions.

Investment efficiency

Next, I examine whether bank taxes affect investment efficiency. The effect of bank taxes on investment efficiency is ambiguous. On the one hand, bank taxes may reduce supply of credit in the taxing jurisdiction, thus leading the firm to invest in less efficient projects in that jurisdiction. On the other hand, borrowing firms may reduce marginally profitable

projects and only undertake highly profitable, infra-marginal projects. In non-taxing jurisdictions, firms reallocating investment may face lower returns from investment but, alternatively, might be substituting into high-return jurisdictions.

I measure investment efficiency as (1) the ratio of sales to the number of establishments of a given firm in a given state in a given year (*SalesEst*) and (2) net income scaled by lagged total assets (*ROA*). The former measure captures investment efficiency for each firm-state-year observation while the latter captures the firm's overall level of investment efficiency. Both measures reflect the same intuition at different levels of observation. Specifically, if investment efficiency is higher, then the turnover/profitability of the entity as a proportion of assets is likely higher (e.g. Biddle, Hilary and Verdi 2009). In other words, each invested asset has a higher "payoff," as measured by establishment-level sales or firm-level earnings. Thus, sales or earnings over some measure of assets should rise if investment efficiency rises.

My results are reported in Table 12. In Panel A, columns (1) and (2), I find that the coefficient on *Increase* is positive and significant at the 1% level while the coefficient on *Decrease* is negative and significant at conventional levels when *SalesEst* is my dependent variable in the taxing jurisdiction. In columns (3) and (4), both coefficients are not significant at conventional levels when *SalesEst* is my dependent variable in "spillover" jurisdictions. In Panel B, columns (1) and (2), *Increase* is positive and significant at the 1% level while *Decrease* is negative and significant at the 5% level when *ROA* is my outcome. Taken together, these results are consistent with bank tax rate increases (1) increasing investment efficiency in the taxing jurisdiction, (2) having no effect on investment efficiency in "spillover"

jurisdictions and (3) increasing overall investment efficiency. Bank tax rate decreases have opposite effects.

Alternative bank tax rate measures

Next, I use alternative measures of bank taxes. In my main analysis, I used a binary variable coded to 1 for bank tax rate changes. In this section, I first use the bank tax rate change as my variable of interest (*Rate)*. This test provides evidence that the level of bank taxation (i.e. bank tax rates) are associated with corporate investment. I use an OLS specification because rates are not easily incorporated into a difference-in-differences analysis. Second, I use a continuous treatment variable in a difference-in-differences design. *Rate_Treat* is coded to the positive or negative magnitude of the most recent bank tax rate change in a given state in the year of the change and all subsequent years prior to the next rate change.

Results are reported in Table 13. In Panels A and B, I use *Rate* as my variable of interest and in Panels C and D, I use *Rate_Treat* as my variable of interest. In Panels A and C, I examine the direct effects of bank taxes on investment in the taxing jurisdiction. In Panels B and D, I examine the indirect effects. In columns (1) and (2), I use state and year fixed effects. In columns (3) and (4), I include firm fixed effects. In Panel A, the coefficient on *Rate* is negative and significant at the 1% level across all columns. In Panel B, the same coefficient is negative and significant at the 10% level or better. In Panel C, I find that the coefficients on *Rate_Treat* are negative and significant at conventional levels. Similarly, in Panel D, the same coefficients are negative and significant at conventional levels. Overall, these results complement my results and are consistent with corporate investment decreasing in the bank tax *rate*.

Table 6. Lending outcomes

	Pred.	(1) *ln(Com_Loan)*	(1) *ln(Gross_Loan)*
Increase	+/-	-0.088***	-0.118***
		(-2.87)	(-4.27)
Decrease	+/-	0.121***	0.134*
		(3.20)	(1.82)
ln(Branches)		0.845***	0.163
		(2.77)	(0.78)
ln(Deposits)		0.380	0.177***
		(1.52)	(3.17)
ln(Loan_loss_prov)		-0.029	0.090***
		(-0.46)	(3.94)
ln(Employees)		0.284	-0.060
		(1.66)	(-1.13)
ln(Pers_loan)		0.069	0.153***
		(0.92)	(5.90)
NetIncome		-0.032	0.065***
		(-0.69)	(3.24)
CIT		-0.008	-0.012
		(-0.49)	(-1.29)
PIT		0.018	0.017
		(0.93)	(1.48)
GSP_Growth		-0.344	0.001
		(-0.67)	(0.00)
UE		-0.002	-0.003
		(-0.10)	(-0.44)
Observations		900	900
Adjusted R-squared		0.974	0.996
State FE		Yes	Yes
Year FE		Yes	Yes

Notes: This table presents the results of testing whether bank rate changes affect lending at banks headquartered in the taxing jurisdiction using a generalized difference-in-differences approach. Lending is measured in the taxing jurisdiction using *ln(Com_Loan)* and *ln(Gross_Loan)*. *Increase* is coded to 1 if state *s* raises its bank tax rate in year *t* or a prior year and 0 otherwise. *Decrease* is coded to 1 if state *s* lowers its bank tax rate in year *t* or a prior year and 0 otherwise. All variables are defined in Appendix A. Standard errors are clustered at the state level and are robust to heteroscedasticity. Two-sided t-statistics are reported in parentheses. *, ** and *** denote significance at the 10%, 5% and 1% levels.

Table 7. Firm-level debt financing and leverage

	Pred.	(1) *DebtFin*	(2) *DebtFin*	(3) *Leverage*	(4) *Leverage*
Increase	-/+	-0.004***	-0.004***	-0.003*	-0.003*
		(-2.93)	(-2.89)	(-1.71)	(-1.68)
Decrease	-/+	0.003**	0.001***	0.001**	0.001**
		(2.20)	(3.38)	(2.07)	(2.15)
CIT_Inc			-0.003		-0.019**
			(-0.38)		(-2.38)
CIT_Dec			0.061***		0.033***
			(4.22)		(2.91)
PIT			-0.000		0.001
			(-0.20)		(0.18)
GSP_Growth			0.118		0.000
			(1.55)		(1.44)
UnempRate			0.000		-0.001**
			(0.18)		(-2.38)
State_Tax			-0.650***		0.019*
			(-15.18)		(1.99)
Loss			-0.029***		0.004***
			(-23.43)		(13.88)
Size			-0.014***		-0.002***
			(-20.32)		(-11.04)
MTB			0.002***		0.000***
			(19.29)		(10.09)
SalesGrowth			0.080***		0.031***
			(27.08)		(25.83)
AltmanZ			-0.005***		-0.003***
			(-8.09)		(-19.33)
Vol(CFO)			0.025***		0.052***
			(10.84)		(22.80)
Vol(Sales)			0.008***		0.001***
			(29.83)		(12.97)
Ind_K			0.065***		0.142***
			(7.51)		(91.52)
CFOSale			-0.036***		-0.004
			(-4.00)		(-0.97)
Slack			-0.093***		-0.019***
			(-20.31)		(-21.39)
Observations		542,554	542,554	542,554	542,554
Adjusted R-squared		0.009	0.051	0.009	0.051

Firm FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes

Notes: This table presents the results of testing whether bank rate changes affect firm-level debt financing and firm-level leverage. *Increase* is coded to 1 if firm *i*'s headquarter state increased its bank tax rate in year *t* or a prior year and 0 otherwise. *Decrease* is coded to 1 if firm *i*'s headquarter state decreased its bank tax rate in year *t* or a prior year and 0 otherwise. All variables are defined in Appendix A. Standard errors are clustered at the state level and are robust to heteroscedasticity. Two-sided t-tests are reported in parentheses. *, ** and *** denote significance at the 10%, 5% and 1% levels.

Table 8. *Exogenous* bank tax rate changes

Panel A: Direct Effect					
	Pred.	(1) *ln(Emp)*	(2) *ln(Emp)*	(3) *ln(Est)*	(4) *ln(Est)*
Exog_Inc	-/+	-0.035***	-0.057***	-0.012***	-0.019***
		(-6.72)	(-7.14)	(-5.94)	(-7.28)
Exog_Dec	-/+	0.037***	0.029***	0.027***	0.024***
		(3.94)	(3.16)	(3.19)	(3.16)
Observations		145,003	106,574	145,003	106,574
Adjusted R-squared		0.083	0.165	0.077	0.163
Controls		No	Yes	No	Yes
State FE		Yes	Yes	Yes	Yes
Firm FE		Yes	Yes	Yes	Yes
Year FE		Yes	Yes	Yes	Yes
Panel B: Indirect effect					
		ln(Emp)	*ln(Emp)*	*ln(Est)*	*ln(Est)*
Exog_Inc	-/+	0.002***	0.004***	0.004***	0.001***
		(4.76)	(4.65)	(6.02)	(5.73)
Exog_Dec	-/+	-0.007***	-0.0077***	-0.008***	-0.005***
		(-3.26)	(-3.42)	(-5.60)	(-4.54)
Observations		304,909	246,988	304,909	246,988
Adjusted R-squared		0.112	0.176	0.103	0.220
Controls		No	Yes	No	Yes
State FE		Yes	Yes	Yes	Yes
Firm FE		Yes	Yes	Yes	Yes
Year FE		Yes	Yes	Yes	Yes

Notes: This table presents the results of testing whether exogenous bank rate changes affect investment using a generalized difference-in-differences approach. Panel A examines the direct effects in taxing jurisdictions while Panel B examines the indirect effects into other jurisdictions. *Exog_Inc* is coded to 1 if state *s* raises its bank tax rate in year *t* or a prior year for reasons unrelated to countering economic conditions and 0 otherwise. *Exog_Dec* is coded to 1 if state *s* lowers its bank tax rate in year *t* or a prior year for reasons unrelated to countering economic conditions and 0 otherwise. All variables are defined in Appendix A. Standard errors are clustered at the state level and are robust to heteroscedasticity. Two-sided t-tests are reported in parentheses. *, ** and *** denote significance at the 10%, 5% and 1% levels.

Table 9. Indirect effect: Neighboring states v. non-neighboring states

Panel A: Neighboring states					
	Pred.	(1) *ln(Emp)*	(2) *ln(Emp)*	(3) *ln(Est)*	(4) *ln(Est)*
Increase	-/+	0.036***	0.032***	0.014***	0.012***
		(3.24)	(3.05)	(3.52)	(3.56)
Decrease	-/+	-0.060***	-0.095***	-0.031***	-0.039***
		(-3.42)	(-2.99)	(-4.00)	(-4.03)
Observations		156,525	127,998	156,525	127,998
Controls		No	Yes	No	Yes
State FE		Yes	Yes	Yes	Yes
Firm FE		Yes	Yes	Yes	Yes
Year FE		Yes	Yes	Yes	Yes
Panel B: Non-neighboring states					
		ln(Emp)	*ln(Emp)*	*ln(Est)*	*ln(Est)*
Increase	-/+	0.017***	0.019***	0.012**	0.008***
		(4.51)	(4.48)	(2.08)	(2.95)
Decrease	-/+	-0.025***	-0.039***	-0.028***	-0.033***
		(-5.23)	(-5.70)	(-6.22)	(-5.93)
Observations		302,369	260,602	302,369	260,602
Controls		No	Yes	No	Yes
State FE		Yes	Yes	Yes	Yes
Firm FE		Yes	Yes	Yes	Yes
Year FE		Yes	Yes	Yes	Yes

Notes: This table presents results of testing whether indirect bank tax effects arise in neighboring states and non-neighboring states. Neighboring states are required to share a geographic border with a taxing jurisdiction. Panel A reports results of using neighboring states while Panel B reports results using all other states. Investment is measured using *ln(Emp)* and *ln(Est)* in non-enacting jurisdictions. All variables are defined in Appendix A. Standard errors are clustered at the state level and are robust to heteroscedasticity. Two-sided t-tests are reported in parentheses. *, ** and *** denote significance at the 10%, 5% and 1% levels.

Table 10. Within-state analyses: Leverage

Panel A: Direct effect					
	Pred.	(1) *ln(Emp)*	(2) *ln(Emp)*	(3) *ln(Est)*	(4) *ln(Est)*
Increase×*Leverage*	-/+	-0.009***	-0.008***	-0.007**	-0.006**
		(-6.81)	(-6.23)	(-2.67)	(-2.14)
Decrease×*Leverage*	-/+	0.008***	0.006**	0.007***	0.006***
		(2.69)	(2.46)	(3.35)	(3.27)
Observations		190,742	153,954	190,742	153,954
Adjusted R-squared		0.269	0.275	0.254	0.273
Controls		No	Yes	No	Yes
State×Year FE		Yes	Yes	Yes	Yes
Firm FE		Yes	Yes	Yes	Yes
Panel B: Indirect effect					
		(1) *ln(Emp)*	(2) *ln(Emp)*	(3) *ln(Est)*	(4) *ln(Est)*
Increase×*Leverage*	-/+	0.002***	0.002***	0.001***	0.001***
		(7.60)	(7.48)	(6.61)	(6.82)
Decrease×*Leverage*	-/+	-0.007***	-0.007***	-0.002***	-0.002***
		(-2.84)	(-3.16)	(-4.13)	(-3.96)
Observations		458,894	388,600	458,894	388,600
Adjusted R-squared		0.269	0.275	0.254	0.273
Controls		No	Yes	No	Yes
State×Year FE		Yes	Yes	Yes	Yes
Firm FE		Yes	Yes	Yes	Yes

Notes: This table presents results of testing whether bank tax effects are decreasing in firm leverage after including state-year fixed effects. Investment is measured using *ln(Emp)* and *ln(Est)*. All variables are defined in Appendix A. Standard errors are clustered at the state level and are robust to heteroscedasticity. Two-sided t-tests are reported in parentheses. *, ** and *** denote significance at the 10%, 5% and 1% levels.

Table 11. Within-state analyses: Size

Panel A: Direct effect					
	Pred.	(1) *ln(Emp)*	(2) *ln(Emp)*	(3) *ln(Est)*	(4) *ln(Est)*
Increase×*Size*	-/+	-0.011***	-0.011***	-0.004*	-0.006*
		(-3.47)	(-3.29)	(-1.73)	(-1.83)
Decrease×*Size*	-/+	0.017***	0.018**	0.007**	0.005*
		(3.41)	(2.08)	(2.28)	(1.72)
Observations		190,742	153,954	190,742	153,954
Adjusted R-squared		0.269	0.275	0.254	0.273
Controls		No	Yes	No	Yes
State×Year FE		Yes	Yes	Yes	Yes
Firm FE		Yes	Yes	Yes	Yes
Panel A: Indirect effect					
		(1) *ln(Emp)*	(2) *ln(Emp)*	(3) *ln(Est)*	(4) *ln(Est)*
Increase×*Size*	-/+	0.004*	0.005*	0.001*	0.001**
		(1.92)	(1.83)	(1.82)	(2.41)
Decrease×*Size*	-/+	-0.002*	-0.001	-0.005**	-0.003**
		(-1.73)	(-0.89)	(-2.28)	(-2.12)
Observations		190,742	153,954	190,742	153,954
Adjusted R-squared		0.269	0.275	0.254	0.273
Controls		No	Yes	No	Yes
State×Year FE		Yes	Yes	Yes	Yes
Firm FE		Yes	Yes	Yes	Yes

Notes: This table presents results of testing whether bank tax effects are increasing in firm-level size after including state-year fixed effects. Investment is measured using *ln(Emp)* and *ln(Est)*. All variables are defined in Appendix A. Standard errors are clustered at the state level and are robust to heteroscedasticity. Two-sided t-tests are reported in parentheses. *, ** and *** denote significance at the 10%, 5% and 1% levels.

Table 12. Investment efficiency

Panel A: *SalesEst* as outcome					
	Pred.	(1) $SalesEst_s$	(2) $SalesEst_s$	(3) $SalesEst_{-s}$	(4) $SalesEst_{-s}$
Increase	?	6,397.611***	10,749.090***	5,278.064	7,194.676
		(3.67)	(3.49)	(0.57)	(1.23)
Decrease	?	-4,060.637***	-9,068.534**	9,421.816	-10,273.884
		(-2.92)	(-2.15)	(0.59)	(-0.66)
Observations		124,602	103,855	284,481	250,256
Adjusted R-squared		0.005	0.021	0.002	0.015
Controls		No	Yes	No	Yes
State FE		Yes	Yes	Yes	Yes
Firm FE		Yes	Yes	Yes	Yes
Year FE		Yes	Yes	Yes	Yes

Panel B: *ROA* as outcome			
		(1) *ROA*	(2) *ROA*
Increase	?	1.703***	1.591***
		(3.08)	(3.96)
Decrease	?	-1.345**	-1.029**
		(-2.64)	(-2.33)
Observations		542,554	542,554
Adjusted R-squared		0.001	0.003
Controls		No	Yes
State FE		Yes	Yes
Year FE		Yes	Yes

Notes: This table presents results of testing whether bank taxes affect investment efficiency in taxing jurisdictions (columns 1 and 2) and in non-enacting jurisdictions (columns 3 and 4). Investment is measured using *SalesEst* in the taxing jurisdiction in columns (1) and (2) of Panel A and in all other states in which a firm operates in columns (3) and (4). Investment is measured using *ROA* in Panel B. *Increase (Decrease)* is coded to 1 if state *s* increased (decreased) its bank tax rate in year *t* or a prior year in Panel A, and 0 otherwise. In Panel B, *Increase (Decrease)* is coded to 1 if firm *i*'s headquarter state increased (decreased) its bank tax rate in year *t* or a prior year and 0 otherwise. All variables are defined in Appendix A. Standard errors are clustered at the state level and are robust to heteroscedasticity. Two-sided t-tests are reported in parentheses. *, ** and *** denote significance at the 10%, 5% and 1% levels.

Table 13. Alternative bank tax rate measures

Panel A: Continuous *rate*: direct effect					
		(1)	(2)	(3)	(4)
	Pred.	*ln(Emp)*	*ln(Emp)*	*ln(Est)*	*ln(Est)*
Rate	+/-	-0.021***	-0.024***	-0.011***	-0.022***
		(-3.20)	(-4.61)	(-2.89)	(-3.61)
Observations		190,742	153,954	190,742	153,954
Adjusted R-squared		0.057	0.221	0.056	0.230
Controls		Yes	Yes	Yes	Yes
Firm FE		Yes	Yes	Yes	Yes
State FE		Yes	Yes	Yes	Yes
Year FE		Yes	Yes	Yes	Yes
Panel B: Continuous *rate*: indirect effect					
		(1)	(2)	(3)	(4)
	Pred.	*ln(Emp)*	*ln(Emp)*	*ln(Est)*	*ln(Est)*
Rate	+/-	-0.035***	-0.040**	-0.011*	-0.012*
		(-3.00)	(-2.44)	(-1.70)	(-1.78)
Observations		458,894	388,600	458,894	388,600
Adjusted R-squared		0.070	0.209	0.072	0.125
Controls		Yes	Yes	Yes	Yes
Firm FE		Yes	Yes	Yes	Yes
State FE		Yes	Yes	Yes	Yes
Year FE		Yes	Yes	Yes	Yes
Panel C: Continuous *treatment*: direct effect					
		(1)	(2)	(3)	(4)
	Pred.	*ln(Emp)*	*ln(Emp)*	*ln(Est)*	*ln(Est)*
Rate_treat	+/-	-0.020***	-0.028***	-0.022**	-0.025**
		(-3.33)	(-2.87)	(-2.25)	(-2.40)
Observations		190,742	153,954	190,742	153,954
Adjusted R-squared		0.057	0.221	0.056	0.230
Controls		Yes	Yes	Yes	Yes
Firm FE		Yes	Yes	Yes	Yes
State FE		Yes	Yes	Yes	Yes
Year FE		Yes	Yes	Yes	Yes
Panel D: Continuous *treatment*: indirect effect					
		(1)	(2)	(3)	(4)
	Pred.	*ln(Emp)*	*ln(Emp)*	*ln(Est)*	*ln(Est)*
Rate_treat	+/-	-0.035***	-0.040***	-0.007**	-0.009**
		(-4.00)	(-3.11)	(-2.00)	(-2.50)
Observations		458,894	388,600	458,894	388,600
Adjusted R-squared		0.070	0.209	0.072	0.125

Controls	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes

Notes: This table presents results of using alternative bank tax rate measures. Panels A and B present results using bank tax *rates*. Panels C and D present results using a continuous treatment variable in a difference in differences design. Investment is measured using *ln(Emp)* and *ln(Est)*. All variables are defined in Appendix A. Standard errors are clustered at the state level and are robust to heteroscedasticity. Two-sided t-tests are reported in parentheses. *, ** and *** denote significance at the 10%, 5% and 1% levels.

CHAPTER 6:

CONCLUSION

In this study, I examine the effects of bank taxes on non-bank corporate investment. Bank taxation is an important consideration for policymakers because banks are capital intermediaries and bank taxation may reduce lending. Moreover, prior studies document the effects of various other tax provisions (e.g. corporate income tax rates, depreciation benefits) on corporate investment activities but do not generally focus on the effects of *bank* taxes on corporate investment. Therefore, my study provides an important contribution to prior literature.

I hypothesize that bank taxes reduce corporate investment in the taxing jurisdiction and increase investment in other jurisdictions. Bank taxes reduce bank lending by reducing the after-tax profitability of marginal lending and due to regulatory constraints, such as maximum leverage ratios and capital requirements (Schandlbauer 2017; Smolyansky 2019). Therefore, bank taxes should reduce corporate investment in the taxing jurisdiction if corporations rely on local bank financing for local investment. At the same time, investment in other jurisdictions may increase because returns to investment in these jurisdictions are relatively more attractive than in taxing jurisdictions, where bank taxes reduce credit supply.

I test my hypotheses using state bank tax rate changes. State bank tax rate changes provide a quasi-experimental setting because not all states change their bank tax rates in my setting and changes are arguably exogenous to my model. Thus, my setting offers a plausibly exogenous intervention and a natural control group. I use publicly traded firms' establishment data by state to measure corporate investment as the number of

establishments and the number of employees that a firm employs in a state in a given year. I use generalized difference-in-differences as my identification strategy.

Across both baseline and secondary specifications, my results are consistent with state bank tax rate increases reducing investment and state bank tax rate decreases increasing investment. Additionally, I find that bank tax rate hikes increase investment in non-enacting jurisdictions while decreases reduce investment in these jurisdictions. These results are consistent with bank taxes generating direct effects in enacting jurisdictions and indirect effects into non-enacting jurisdictions.

I perform several additional tests. First, I provide evidence that bank taxes affect lending. Second, I find that bank tax rate changes affect debt financing and leverage. Next, I rely on the narrative approach of Romer and Romer (2010) to identify "exogenous" bank tax rate changes. Specifically, I identify bank tax rate changes that are unrelated to concurrent economic conditions based on news articles from LexisNexis searches. My main results are robust to using this smaller set of bank tax rate changes. Next, I find evidence that indirect effects are stronger in neighboring states relative to other states. I find that investment efficiency rises following bank tax rate increases and falls following bank tax rate decreases. In within-state-year analyses, I find that the both the direct and indirect effects are increasing in leverage and decreasing in size.

My study contributes to the literature identifying the effects of taxation. My study extends this literature by providing evidence of the effects of bank taxes on the location of corporate investment. Bank taxes are an important policy consideration because banks are capital intermediaries. Moreover, prior studies have focused on business taxes or taxes on

the equity financing channel. I extend this literature by focusing on the consequences of taxing the *debt* financing channel.

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Appendix A. Variable definitions

Outcomes	
ln(Emp)	The natural log of 1 + firm *i*'s number of employees in state *s* in year *t* (YourEconomy Time Series)
ln(Est)	The natural log of firm *i*'s number of establishments in state *s* in year *t* (YourEconomy Time Series)
DebtFin	The sum of firm *i*'s long-term and current debt financing scaled in year *t* by lagged total assets
Leverage	Firm *i*'s long-term debt scaled by lagged total assets in year *t* (Compustat Annual)
SalesEst	Firm *i*'s total sales in state *s* in year *t* scaled by firm *i*'s total establishments in state *s* in year *t* (YourEconomy Time Series)
ROA	Firm *i*'s pre-tax income plus interest expense scaled by lagged total assets in year *t* (Compustat Annual)
ln(Com_Loans)	The natural log of 1 + the dollar amount of commercial loans of banks headquartered in state *s* in year *t* (FDIC)
ln(Gross_Loans)	The natural log of 1 + the dollar amount of loans of banks headquartered in state *s* in year *t* (FDIC)
Treatment Variables	
Increase	Takes a value of 1 if state *s* increased bank tax rates in year *t* or a prior year (Book of the States; CCH)
Decrease	Takes a value of 1 if state *s* decreased bank tax rates in year *t* or a prior year (Book of the States; CCH)
Exog_Inc	Takes a value of 1 if state *s* increased bank tax rates in year *t* or a prior year for reasons unrelated to local fiscal policy (LexisNexis)
Exog_Dec	Takes a value of 1 if state *s* decreased bank tax rates in year *t* or a prior year for reasons unrelated to local fiscal policy (LexisNexis)
Rate	Equal to the bank tax rate in state *s* in year *t* (Book of the States; CCH)
Covariates	
CIT_Inc	Takes a value of 1 if state *s* increased corporate income tax rates in year *t* or a prior year (Book of the States)
CIT_Dec	Takes a value of 1 if state *s* decreased corporate income tax rates in year *t* or a prior year (Book of the States)
PIT	Personal income tax rate of state *s* in year *t* (Book of the States)
GSP_Growth	The difference between the gross state product of state *s* in year *t* and the gross state product of state *s* in year *t*-1, scaled by gross state product of state *s* in year *t*-1 (Bureaue of Labor Statistics)
UE	Unemployment rate of state *s* in year *t* (Bureau of Labor Statistics)
State_Tax	Firm *i*'s current state tax expense in year *t* scaled by lagged total assets (Compustat Annual)
Loss	Coded to 1 if firm *i* reported a loss in year *t* and 0 otherwise (Compustat Annual)
SalesGrowth	The difference in firm *i*'s sales from *t* to *t-1*, scaled by firm *i*'s sales in *t-1.*

Size	The natural log of firm *i*'s total assets in year *t-1* (Compustat Annual)
MTB	The ratio of firm *i's* market value of total assets to book value of total in year *t-1* (Compustat Annual)
Vol(CFO)	The standard deviation of firm *i*'s cash flows from operations measured from *t*-4 to *t*-1 (Compustat Annual)
Vol(Sales)	The standard deviation of firm *i*'s sales measured from *t*-4 to *t*-1 (Compustat Annual)
AltmanZ	Calculated as 1.2*(current assets – current liabilities)/total assets +1.4*(retained earnings/total assets) + 3.3*(net income + interest + total tax expense)/total assets + 0.6*(shares outstanding*closing price)/total liabilities _ 0.999*(sales/total assets) (Compustat Annual)
Vol(CFO)	The volatility of firm *i*'s cash flows from operations measured over *t*-4 to *t*-1 (Compustat Annual)
K-structure	The ratio of firm *i's* long-term debt to the sum of long-term debt and the market value of equity in year *t-1* (Compustat Annual)
Ind_K	The average K-structure in each 3-digit SIC industry in year *t-1* (Compustat Annual)
CFOSale	The ratio of firm *i*'s sales to cash flows from operations in year *t-1* (Compustat Annual)
Slack	The ratio of firm *i*'s cash to property, plant and equipment in year *t*-1 (Compustat Annual)
ln(Branches)	The natural log of 1 + the number of branches of banks headquartered in state *s* in year *t* (FDIC)
ln(Deposits)	The natural log of 1 + the dollar amount of deposits of banks headquartered in state *s* in year *t* (FDIC)
ln(Loss_Prov)	The natural log of 1 + the dollar amount of loan loss provisions of banks headquartered in state *s* in year *t* (FDIC)
ln(Employees)	The natural log of 1 + the number of employees of banks headquartered in state *s* in year *t* (FDIC)
ln(Pers_loan)	The natural log of 1 + the dollar amount of personal loans of banks headquartered in state *s* in year *t* (FDIC)
NetIncome	The net income of all banks headquartered in state *s* in year *t*, scaled by total deposits