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Essays on Municipal Finance and its Real Effects

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Essays on Municipal Finance and its Real Effects

Abstract

This dissertation examines three research questions on municipal finance and its real effects. First, I study whether one municipality's bankruptcy exposes other local governments to the economic costs of financial contagion.[1] Utilizing bankruptcies that are unrelated to the economic trend, I show that other non-bankrupt municipalities issue less debt following the bankruptcy. Examining the economic consequences, I find that local governments with a high fraction of maturing debt decrease their expenditures and display a decline in tradable employment. These results suggest that bankruptcy as a resolution mechanism can deteriorate the development of other municipalities that rely on debt financing. Second, I examine firms' investment response to the supply of private activity bonds (PABs). Exploiting a legal reform that introduces variation in PAB supply across states, I document that PAB supply has a stimulating effect on corporate investment. Although PABs subsidize capital over labor, my results do not support input factor substitution, as I find a positive effect on employment. I exploit the random outcome of a lottery-based PAB distribution mechanism to show that states' project selection does not drive the results. Third, I examine the use and the corporate real effects of tax-subsidized green bonds, using a large sample of pollution control bonds. I find that the issuance of tax-subsidized green bonds increases when conventional bond market yields are high. Examining the period of high interest rates in the early 1980s, I show that firms using such bonds display an increase in output after issuance. For corporate issuers, this output effect highlights a potential positive economic aspect correlated with undertaking tax-subsidized green investments.

[1] In this dissertation, I use the term "I" in the introduction and conclusion. It does not necessarily refer to me directly since the first essay is based on joint work with my co-author.

Thesis advisor: Christoph Kaserer Lisa Knauer

Aufsätze zu den Realeffekten von Municipal Finance

Abstract

Diese Dissertation untersucht drei Forschungsfragen zu den Realeffekten von Municipal Finance. Zuerst untersuche ich, ob der Konkurs einer Kommune aufgrund von finanziellen Ansteckungseffekten wirtschaftliche Kosten für andere Kommunen verursacht. Anhand konjunkturunabhängiger Konkurse zeige ich, dass andere Kommunen nach einem Konkurs weniger Fremdkapital emittieren. Hinsichtlich der wirtschaftlichen Auswirkungen zeige ich, dass nicht insolvente Kommunen mit einem hohen Anteil fällig werdender Schulden ihre Ausgaben reduzieren und einen Beschäftigungsrückgang im Sektor der handelbaren Güter verzeichnen. Diese Ergebnisse legen nahe, dass Konkurse als Abwicklungsmechanismus die Entwicklung anderer Kommunen, die auf Fremdkapitalfinanzierung angewiesen sind, beeinträchtigen können. Zweitens untersuche ich, wie Unternehmensinvestitionen auf das Angebot an Private Activity Bonds (PABs) reagieren. Unter Ausnutzung einer Gesetzesreform, die zu einer Variation im Anleiheangebot von PABs zwischen den US-Bundesstaaten führt, zeige ich, dass das PAB-Angebot eine stimulierende Wirkung auf Unternehmensinvestitionen hat. Obwohl PABs Kapital relativ zum Faktor Arbeit subventionieren, sprechen meine Ergebnisse nicht für eine Substitution von Inputfaktoren, da ich einen positiven Beschäftigungseffekt feststelle. Anhand eines lotteriebasierten Verteilungsmechanismus für PABs zeige ich auf, dass die Projektauswahl der Staaten die Ergebnisse nicht beeinflusst. Drittens untersuche ich die Verwendung und die Effekte von steuerlich subventionierten grünen Unternehmensanleihen auf Basis eines umfassenden Datensatzes zu Pollution Control Bonds. Dabei zeigt sich, dass die Nutzung von steuerlich subventionierten grünen Anleihen zunimmt, wenn die Renditen auf dem konventionellen Anleihemarkt hoch sind. Für die Hochzinsphase Anfang der 1980er Jahre zeige ich, dass Unternehmen, die solche Anleihen emittieren, anschließend ein höheres Umsatzwachstum aufweisen. Für Emittenten ist dieser Umsatzeffekt ein möglicher positiver wirtschaftlicher Aspekt, der mit steuerlich geförderten grünen Investitionen korreliert.

Overview

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0

Introduction

Over the past decades, the municipal bond market has grown to an overall large market, with about USD 4.05 trillion outstanding as of 2023 (Board of Governors of the Federal Reserve System, 2023).[1] An important characteristic of the municipal bond market is that yields are lower than those of similarly rated corporate bonds due to the federal income tax exemption (Cestau et al., 2019).[2] Primarily, state and local governments use municipal bonds to provide essential public services and infrastructure to local communities (see, e.g., Cestau et al., 2019; Internal Revenue Service, 2019). Besides, state and local governments can use private activity bonds to provide private sector firms access to the municipal bond market (see, e.g., Driessen, 2022), aiming to foster local economic develop-

[1] For comparison, about USD 400 billion in municipal bonds were outstanding in 1980, according to the Board of Governors of the Federal Reserve System (2016). Note that the data on municipal securities in the Federal Reserve Board's Financial accounts has been subject to a data revision (see, e.g., Bagley, Vieira and Hamlin, 2022, for a summary of the revision).

[2] Many municipal bonds are also exempt from state taxes. Babina et al. (2021) provide a detailed overview of state taxation of municipal bonds.

ment. Private activity bonds for all types of non-governmental entities currently account for about one quarter of the issuance volume in the municipal bond market (Internal Revenue Service, 2019).

As Rossi and Yun (2024) note, there has been relatively little economic analysis of municipal financing to date. However, a growing body of literature is addressing this economically relevant topic and studies how municipal financing affects the real economy (e.g., Adelino, Cunha and Ferreira, 2017; Dagostino, 2022; Rossi and Yun, 2024).

This dissertation consists of three essays that examine research questions on municipal finance and its real effects. The first essay analyzes whether one municipality's bankruptcy exposes other local governments to economic costs of financial contagion, exploiting bankruptcies that are unrelated to the economic trend. The second essay investigates how corporate investment and employment respond to the supply of private activity bonds. It therefore utilizes a legal reform and a bond distribution lottery. The third essay studies the use and the corporate real effects of tax-subsidized green bonds, using a large sample of pollution control bonds.

0.1 RESEARCH QUESTIONS

Each of the three essays utilizes a specific empirical strategy and dataset to examine the respective research question. I outline the three research questions and the empirical strategies in the following subsections.

0.1.1 MUNICIPAL BANKRUPTCY AND THE ECONOMIC COSTS OF FINANCIAL CONTAGION

In light of the recent municipal bankruptcies of Central Falls (RI, 2011), Jefferson County (AL, 2011), and Detroit (MI, 2013), state officials expressed concerns that Chapter 9 bankruptcy filings could limit the credit market access for other

local governments. Empirically, Gao, Lee and Murphy (2019) analyze a large sample of municipal bond defaults and find evidence for within-state financial contagion on the secondary market if these states allow for unconditional access to Chapter 9. However, the economic effects of financial contagion on other local governments remain unclear: Do bankruptcies impede other local governments' access to the credit market? And if so, how does this affect the non-bankrupt municipalities' ability to provide infrastructure and services?

The empirical challenge in studying these questions is to disentangle the economic costs of financial contagion from the state-wide economic trend, given that municipal bankruptcies are more prevalent in times of weak economic conditions. I use a narrative approach as established by Romer and Romer (1989, 2004, 2010) and screen historical records of municipal bankruptcies to identify idiosyncratic filings. I identify 16 idiosyncratic bankruptcies due to financial speculation, legal judgments, loss from failed public projects, and other financial mismanagement that were filed between 1982 and 2011. A prominent example is Orange County (CA, 1994) which filed for bankruptcy due to a loss of $1.7 billion from financial speculation.

I start by using a cross-border setting to examine the effect of municipal bankruptcies on the credit market access of other local governments. I thereby compare the debt issuance of local governments located in bordering counties in the year after one state is exposed to a bankruptcy filing. I find that a municipal bankruptcy lowers the probability of other local governments issuing debt by 6.3% in the year after the filing. This result suggests that a Chapter 9 bankruptcy limits the access to the credit market for other non-bankrupt local governments in that state.

Then, I investigate how the restricted credit market access after a bankruptcy affects other local governments' expenditures and thus exposes them to the economic costs of financial contagion. I thereby follow the idea of Almeida et al.

(2012) and exploit ex-ante variation in the maturity of long-term debt across local governments in the bankruptcy states. In particular, I examine whether non-bankrupt local governments with a high fraction of maturing long-term debt at the time of the bankruptcy adjust their expenditures. I find that a one standard deviation increase in the fraction of maturing long-term debt decreases local governments' expenditures by about 2.2%. The negative expenditure effect materializes two years after the bankruptcy filing and remains constant in the third year. When examining which areas of spending are most affected, I find that it is primarily capital outlay expenditures that are reduced, while the negative effect on current expenditures is less pronounced.

Finally, I analyze how the negative externalities are transmitted to the private sector. Using county-level data, I find that local government's fraction of maturing long-term debt has a significant negative effect on employment in the tradable sector, but no effect on employment in the non-tradable and services sectors. Consistent with the employment effect, I also find a negative effect on the number of tradable establishments, while I find no effect on non-tradable and services sector establishments. Thus, the private sector externality seems to be concentrated in the tradable sector.

0.1.2 PRIVATE ACTIVITY BONDS AS INVESTMENT SUBSIDY: EVIDENCE FROM THE 1986 CAP ON BOND VOLUMES

Historically, yields on tax-exempt municipal bonds have been considerably lower than yields on comparable taxable corporate bonds (Council of Economic Advisers, 2023). U.S. state and local governments can grant private sector firms access to the tax-exempt municipal bond market through issuing private activity bonds (PABs). In total, PAB issuance for all types of non-governmental beneficiaries currently accounts for about one quarter of the entire tax-exempt bond market (Internal Revenue Service, 2019). Yet, the corporate response to PAB funding

remains largely unexplored. I examine how the supply of private activity bonds affects firm investment and employment.

The analysis of firm responses to PAB supply is empirically challenging for three main reasons. First, PAB issuance may be concentrated on regions with favorable local investment opportunities. Second, even within regions characterized by similar investment opportunities, firms' demand for PAB funding is endogenous. Third, states have discretionary power in the allocation of PABs, which adds another dimension of selection complexity to PAB funding.

To identify the effect of PAB supply on firm investment and employment, I exploit new state-level volume limits for PABs introduced with the 1986 Tax Reform Act. Because of the underlying distribution formula for these limits, the tax reform creates plausibly exogenous variation in PAB supply on a per cap level: relatively larger states are restricted to 50 USD of PABs per person, while smaller states can supply relatively higher per cap volumes. A state is thereby categorized as small if it has a population of less than 3 million, which is the case for 23 states during the post-reform period that is analyzed. I utilize this state-level variation in per cap PAB supply after the 1986 Tax Reform in a difference-in-differences framework.

Given that firms' demand for private activity bonds is endogenous, I approximate the effect of PAB financing by analyzing two samples of firms: First, I adapt the idea of Bonfim, Custódio and Raposo (2023) and compare firms that are eligible for PAB funding, providing an intention-to-treat effect of the program. Focusing on PAB eligible firms located in state border counties also allows me to control for common economic trends and investment opportunities in border regions. Second, I use a sample of firms that receive PAB funding before and after the tax reform.

Using the two firm samples in the difference-in-differences framework, I find that higher per cap PAB supply has a significant positive effect on investment of

PAB eligible and PAB beneficiary firms. For the sample of PAB eligible firms, increasing per cap PAB supply by 50 USD is associated with an increase of the capex-to-assets ratio by 10.5%. While this positive investment response to PAB funding could lead to increased hiring of labor, it could also have a negative effect on employment if firms substitute labor with tax-subsidized capital. Examining how PAB supply affects firm employment, I find that employment significantly increases with PAB supply for both samples of firms. Hence, I do not find any evidence for an input factor substitution.

Finally, I rule out that states' project selection is driving the results. I utilize PAB program data from the State of Texas, which employs a lottery system to allocate its PAB volume among funding applicants. Leveraging a sample of PAB applicant firms for the program years 1996 to 2001 of which only a subset obtains PAB funding randomly by lot, I document a sizeable positive investment effect of receiving PAB funding through the lottery. This result supports my previous finding that PAB supply has a stimulative effect on corporate investment.

0.1.3 TAX-SUBSIDIZED GREEN BONDS AND THEIR REAL EFFECTS

The empirical literature examining corporate green bonds finds that the premium at issue compared to conventional corporate bonds is at best economically small (Flammer (2021), for example, finds no evidence of a premium at issue, and Caramichael and Rapp (2024) show that the premium ranges from three to eight basis points). From a corporate financing perspective, these results indicate that green bonds so far do not substantially reduce the financing costs of green projects (Daubanes, Mitali and Rochet, 2021; Flammer, 2021). At the same time, different types of subsidies are politically discussed as fiscal policy tools to increase the growth of the green bond market (Chiang, 2017; Climate Bonds Initiative, 2024b). Among these potential tools, tax incentives are one approach that can be used to lower green bonds' financing costs for corporate

issuers (Climate Bonds Initiative, 2024c).

If corporate green bonds are tax-subsidized, providing a source of low-cost financing for green projects, what types of companies are making use of such a financing instrument? What conventional bond market conditions spur the use of these bonds? Finally, what are the real effects associated with the use of tax-subsidized green bonds? To explore these questions, I utilize a comparatively large sample of tax-exempt pollution control bonds (PCBs). This sample comprises 3,200 PCBs issued between 1980 and 2013 to finance pollution abatement facilities of U.S. public firms.

The sample of pollution control bonds is well suited for the analysis of tax-subsidized green bonds for three reasons: First, due to their issuance on the tax-exempt municipal bond market, PCBs have significantly lower financing costs than conventional corporate bonds. Second, with the proceeds of PCBs being used to finance pollution abatement facilities, this financing purpose would typically allow them to be labeled as green under current standards. Third, by combining bond-level data with firm-level financial data, I overcome the measurement challenge often associated with local business incentive programs.

I first present several facts about the corporate use of tax-subsidized pollution control bonds. Adapting the approach of Chodorow-Reich et al. (2022) to explore the use of PCBs across the firm-size distribution, I document that pollution control bonds are primarily used by large firms, even within industries. Furthermore, I show that firms in the manufacturing and utility industries are the main users of this financing tool, and that the use of PCBs peaked in the first half of the 1980s.

Then, I analyze whether PCB issuance is correlated with financing costs on the conventional corporate bond market. I document that the issuance of new money PCBs is positively correlated with conventional bond market yields. For the sample of PCB issuing firms, I find that a one percentage point increase in

the conventional bond yield is associated with a 4.1% increase in the probability of issuing a new money PCB.

Finally, I examine corporate real effects that correlate with the issuance of PCBs during the high-interest rate period of the early 1980s. As PCB issuers are not randomly assigned, I apply the approach of Flammer (2021) and compare PCB issuers that tap the PCB market between 1980 and 1984 with a matched control group. I show that firms using PCBs display a relative increase in sales growth after issuance. While this result does not imply a causal relation, it suggests that the use of a low-cost source of financing for pollution abatement investments can be correlated with potential positive economic aspects for the issuers.

0.2 CONTRIBUTIONS

The three essays in this dissertation contribute to multiple strands of the literature. I briefly summarize the main contribution of each of the essays.

In the *first* essay, I show that a municipal bankruptcy limits the debt market access for other non-bankrupt local governments and imposes economic costs on local governments with immediate refinancing needs. By identifying the economic costs of financial contagion for local governments based on idiosyncratic municipal bankruptcies, I contribute to the literature stream that examines municipal credit market frictions and their effects on local governments (e.g., Adelino, Cunha and Ferreira, 2017; Dagostino, 2022; Gao, Lee and Murphy, 2019). Furthermore, I add to the literature on spillover effects of bankruptcies. Much work has been devoted to the spillover effects of corporate bankruptcies (e.g., Benmelech and Bergman, 2011; Benmelech et al., 2019; Bernstein et al., 2019). I provide novel evidence on the spillover effects of municipal bankruptcies. Finally, the essay contributes to the literature that analyzes and discusses bankruptcies as the resolution mechanism for municipalities (e.g., Rossi and Yun, 2024; Skeel Jr., 2013). My findings provide empirical evidence

that bankruptcy as a resolution mechanism may deteriorate the development of other municipalities that rely on debt financing when bankruptcy events occur.

In the *second* essay, I show that the supply of private activity bonds has a stimulating effect on corporate investment and is associated with a scale effect for employment. I complement the growing literature stream on municipal finance and its real effects, which largely focuses on conventional municipal bond supply and its effect on the local economy (e.g., Adelino, Cunha and Ferreira, 2017; Agrawal and Kim, 2022; Dagostino, 2022). Different to these papers, I study the role of PAB supply and provide evidence on its stimulating impact on private sector investment. By conducting a micro-econometric assessment of the PAB program, I further contribute to the literature that analyzes government incentive programs for private sector investment and employment (e.g., Criscuolo et al., 2019; Hyman et al., 2023). My findings also add to the policy debate on private activity bonds (e.g., Congressional Budget Office and Joint Committee on Taxation, 2009; Osterberg, 1991; Zimmerman, 1989, 1990). From a public sector perspective, the debate over PABs focuses primarily on the loss of federal revenue. I present novel insights on the micro-level by directly investigating real effects for firms.

In the *third* essay, I use a sample of pollution control bonds and show that the use of such tax-subsidized green bonds correlates with financing costs on the conventional bond market, and that firms that issue such bonds display an increase in sales growth after their issuance. By providing insights on a comparably large sample of pollution control bonds that could plausibly be labeled green under current standards, I add to the growing literature stream that examines green bonds (e.g., Baker et al., 2022; Caramichael and Rapp, 2024; Flammer, 2021), which are a relatively new financial instrument. Given that corporate green bonds so far do not significantly reduce the financing costs of green projects (Daubanes, Mitali and Rochet, 2021; Flammer, 2021), I pro-

vide insights on the use of a low-cost source of financing for green projects by studying tax-subsidized green bonds. Finally, analyzing the use and real effects of pollution control bonds as a long-standing subsidy tool also adds to the literature stream that studies fiscal policies targeting the private sector to improve environmental outcomes (e.g., Timilsina, 2022; Williams III, 2016), and the respective policy discussion (e.g., Delgado-Téllez, Ferdinandusse and Nerlich, 2022; Eurostat, 2015).

0.3 OUTLINE

The remainder of this dissertation is structured as follows. In Chapter 1, I analyze whether the bankruptcy of a municipality exposes other local governments to economic costs of financial contagion. In Chapter 2, I investigate firms' investment response to the supply of private activity bonds. In Chapter 3, I study the use and the corporate real effects of tax-subsidized green bonds. Finally, in Chapter 4, I provide a brief summary of the main results and highlight their contributions and implications.

"Access to the credit markets (...) is extremely important, and given the size of Rhode Island, we did not want Central Falls to have some kind of contagion impact on our other communities." – Rosemary Booth Gallogly, director of the Rhode Island Department of Revenue, quoted in Russ (2012).

1

Municipal Bankruptcy and the Economic Costs of Financial Contagion

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Abstract:

This paper examines whether one municipality's bankruptcy exposes other local governments to economic costs of financial contagion. To disentangle the bankruptcy's effect from the general economic trend, we identify idiosyncratic bankruptcies using a narrative approach. We show that non-bankrupt municipalities issue less debt following the bankruptcy. To identify the economic consequences of the limited credit market access, we exploit ex-ante heterogeneity in local governments' maturity of long-term debt. We find that high fractions of maturing debt lead to lower government spending, as well as to lower tradable employment. Overall, our results suggest that bankruptcy as resolution mechanism deteriorates the development of other municipalities that rely on debt financing.

1.1 INTRODUCTION

In response to the recent municipal bankruptcies of Central Falls (RI, 2011), Jefferson County (AL, 2011), and Detroit (MI, 2013), state officials expressed concerns that filings for bankruptcy under Chapter 9 may restrict other local governments' access to credit markets. Indeed, the pricing of default risk in municipal bond markets is high, as it accounts for about 75% of the spreads (Schwert, 2017). Studying a large sample of municipal bond defaults, Gao, Lee and Murphy (2019) find evidence for secondary market contagion effects from defaults to other bonds in that state (if it allows for unconditional access to Chapter 9). However, the economic effects of financial contagion on other local governments remain unclear. In this paper, we examine how one municipality's bankruptcy affects the debt issuance by other non-bankrupt local governments, their public expenditures, and finally, transmits to the private sector.

The empirical challenge is to disentangle the economic costs of financial contagion from the state-wide economic trend as municipal bankruptcies are more prevalent in times of weak economic conditions. Therefore, we use narrative records—bankruptcy filings, news coverage, and statements by local officials—to identify bankruptcies that are unrelated to the economic trend. Searching for evidence of financial speculation, legal judgments, loss from failed public projects, and other financial mismanagement, we screen the records of 52 city and county bankruptcies between 1980 and 2015. We identify 16 idiosyncratic bankruptcies. A prominent example is Orange County (CA, 1994) which filed for bankruptcy due to a loss of $1.7 billion from financial speculation.

We first examine whether a municipal bankruptcy affects other local governments' access to credit markets. We therefore use a cross-border setting in which we compare the debt issuance of municipalities located in bordering counties in the year after one state is exposed to a bankruptcy filing. A munic-

ipal bankruptcy lowers the probability of other local governments issuing debt by 6.3% in the year after the filing. Further, the amount of debt issued by local governments located in the state in which the bankruptcy occurs decreases by about 46.5%. These results support the interpretation that bankruptcy under Chapter 9 limits the credit market access for other non-bankrupt local governments in that state.

Next, we investigate how the restricted access to credit markets affects other local governments' expenditures. To pin down the real effects, we apply the empirical strategy of Almeida et al. (2012) and exploit ex-ante variation in the maturity structure of municipalities' debt. We examine whether non-bankrupt local governments with a high fraction of maturing long-term debt at the time of the bankruptcy must adjust their public expenditures. This identification strategy suits well for local governments as the average municipal bond has a time to maturity of ten years (see, e.g., Cornaggia, Hund and Nguyen, 2023). Thus, our measure for municipal refinancing needs in the year following the bankruptcy is pre-determined by contracting decisions that were made a decade ago.

We find significant negative effects on the expenditures of local governments with high refinancing needs. A one standard deviation increase in the fraction of maturing long-term debt decreases local governments' expenditures by about 2.2%. At the county level, a one standard deviation increase leads to a decrease in expenditures of about 3.9%. The negative externality on expenditures manifests two years after the bankruptcy filing and remains constant in the third year. These results support the interpretation that the limited access to credit markets following a municipal bankruptcy adversely affects other local governments' budgets in subsequent years.

A potential concern in our analysis is that the results capture unobserved municipal differences that correlate with our measure of refinancing needs rather

than the limited credit market access due to the bankruptcy. We utilize two strategies to address this concern. The first strategy is a range of placebo tests that assume a fictional bankruptcy to take place in the years before the actual bankruptcy event. We find no significant effect of refinancing needs on local governments' expenditures. The second strategy rules out that local governments with high refinancing needs geographically cluster around the bankrupt municipality. Our results remain unchanged if we exclude local governments within a radius of 40, 60, and 80 miles around the bankrupt municipality.

Having established that a municipal bankruptcy imposes negative externalities on other non-bankrupt municipalities' expenditures, it is important to understand which areas of spending are most affected by the budget cuts. We find that the negative expenditure effect is more pronounced for capital outlay expenditures than for current expenditures.

Finally, we analyze how the negative externalities transmit to non-bankrupt counties' private sector employment. We find a significant negative effect on employment in the tradable sector, whereas we do not find an effect on the non-tradable and services sector. A one standard deviation increase in the fraction of maturing long-term debt decreases tradable employment by about 2.7% in the three years following the bankruptcy.

In this paper, we show that a municipal bankruptcy limits the debt market access for other non-bankrupt local governments and imposes substantial economic costs on local governments with immediate refinancing needs. To the best of our knowledge, our study is the first to identify the economic costs of financial contagion for local governments by exploiting idiosyncratic municipal bankruptcies.

We contribute to different strands of the literature. First, we contribute to the literature on financial distress and its impact on municipalities as well as to the broader stream on municipal credit market frictions and their effects. Gao,

Lee and Murphy (2019) show that a state policy allowing financially distressed municipalities to unconditionally file for Chapter 9 leads to higher borrowing costs for the local governments located in these states. Adelino, Cunha and Ferreira (2017) document that an extended municipal debt capacity following a rating upgrade has a positive effect on local governments' expenditures. Further, Dagostino (2022) documents a positive effect on debt issuance and employment when bank credit rationing for municipalities is relaxed. We highlight that a municipal bankruptcy limits the debt market access for other non-bankrupt local governments, and show that local governments with high refinancing needs reduce their expenditures after the bankruptcy filing.

Second, we complement the literature on spillover effects of bankruptcies. Corporate bankruptcies negatively affect the local economy through lower foot traffic and knowledge spillovers (Bernstein et al., 2019) as well as weakened agglomeration effects (Benmelech et al., 2019). Aside from the local economy, Benmelech and Bergman (2011) show that corporate bankruptcies adversely affect competitors by reducing collateral values. We extend this literature by showing that a municipal bankruptcy imposes economic costs on other local governments through financial contagion. Several studies also show financial contagion effects for corporate bankruptcies (e.g., Addoum et al., 2020; Hertzel and Officer, 2012; Jorion and Zhang, 2007; Lang and Stulz, 1992).

Finally, our findings add to the literature which analyzes and discusses implications of the legal framework for municipal bankruptcies. Rossi and Yun (2024) show that the state-level adoption of municipal bankruptcy law decreases financing costs for municipalities and positively impacts their amount of financing. In the event of a bankruptcy filing, several authors point to the risk of contagion (e.g., Gillette, 2012; Schragger, 2012; Skeel Jr., 2013) when discussing the current bankruptcy regime. Our study provides empirical evidence that bankruptcy as a resolution mechanism may deteriorate the development of other municipalities

that rely on debt financing when bankruptcy events occur.

The remainder of the paper is structured as follows. Section 1.2 describes the financial contagion effect and illustrates the sample of idiosyncratic municipal bankruptcies. Section 1.3 describes the data. Section 1.4 presents empirical results for the effect of bankruptcies on other local governments' credit market access. Section 1.5 examines externalities of the bankruptcy on other local governments' expenditures. Section 1.6 examines externalities on private sector employment. Section 1.7 concludes.

1.2 USING IDIOSYNCRATIC MUNICIPAL BANKRUPTCIES TO STUDY ECONOMIC COSTS OF FINANCIAL CONTAGION

1.2.1 MUNICIPAL BANKRUPTCIES AND FINANCIAL CONTAGION

For financially distressed municipalities, Chapter 9 of the U.S. Bankruptcy code contains the provision to file for bankruptcy. Generally, the purpose of Chapter 9 is to provide municipalities filing for bankruptcy with the possibility to adjust their debt. Unlike other Chapters of the U.S. Bankruptcy Code, Chapter 9 does not include a provision for liquidation of assets, and cases may not be converted to other Chapters (Federal Judicial Center, 2017a). Chapter 9 policies vary between states. Spiotto (2012) provides an overview: 12 states specifically authorize Chapter 9 bankruptcies, while 12 other states conditionally authorize Chapter 9 bankruptcies. 3 states have limited authorization, and 2 states prohibit Chapter 9 bankruptcy filings. The remaining states (21) appear to be unclear regarding Chapter 9, or have no specific authorization provision.

The majority of municipal bankruptcies are filed in consequence of weak economic conditions (e.g., Detroit, MI, in 2013). Therefore, the empirical challenge when analyzing the economic costs of financial contagion is to disentangle the bankruptcy's effect from state-wide economic trends. To address this challenge, we exploit idiosyncratic bankruptcy filings. We define two criteria for a

bankruptcy filing to be idiosyncratic: (i) The main driver for the bankruptcy filing must not be an economic downturn. (ii) The filing is surprising or provides novel information to the debt market.

A very prominent example of such an idiosyncratic bankruptcy filing is Orange County (CA), which filed for bankruptcy in 1994 due to a loss of $1.7 billion from financial derivatives. Following this bankruptcy filing, The New York Times (see, e.g., Hofmeister, 1994; Wayne, 1994) documents the turmoil on the Californian municipal bond market and investor concerns about whether there are other counties with similar risk of investment losses. This anecdotal example illustrates how, through updated investor beliefs, an idiosyncratic bankruptcy filing of one municipality may impose negative externalities on other non-bankrupt municipalities through tightening of the debt market.

In general, municipal bond markets may react particularly sensitive to an idiosyncratic bankruptcy due to a high share of retail investors (see, e.g., Cestau et al., 2019), who are often over-proportionally invested in bonds of their home state (see, e.g., Babina et al., 2021). Schwert (2017) also shows that the default risk premium for municipal bonds is very high.

Gao, Lee and Murphy (2019) provide empirical evidence for a contagion effect on the secondary municipal bond market by studying a comprehensive sample of municipal bond defaults. They document a contagion effect on other municipal bonds within the same state if a state grants unconditional access to Chapter 9 bankruptcy filings. Further, they show that this contagion effect persists for one year. Several other studies (e.g., Gospodinov, Robertson and Tkac, 2014; Halstead, Hegde and Schmid Klein, 2004; Kidwell and Trzcinka, 1982; Yang, 2019a) conduct single event studies, and find mixed results. Not all of the events used by these single event studies satisfy our definition of an idiosyncratic bankruptcy event. We argue that idiosyncratic bankruptcy events are suited to estimate the economic costs of financial contagion.

1.2.2 IDENTIFYING IDIOSYNCRATIC BANKRUPTCIES USING A NARRATIVE APPROACH

To identify idiosyncratic bankruptcy filings, we follow the narrative approach established by Romer and Romer (1989, 2004, 2010). We base our narrative analysis on a comprehensive list of municipal bankruptcy filings of county and city governments, which we collect from two main sources. First, we make use of the broad case list as provided by Feldstein and Fabozzi (2008), of which we extract bankruptcy filings from 1980 to 2007. Second, we add bankruptcy filings for the period 2008 to 2015 by using the bankruptcy filing case list from the Federal Judicial Center (2017b). We complement the obtained case list by singular other cases that we collect from an overview provided by the Tax Foundation. For the period between 1980 and 2015, our case list comprises a total of 52 bankruptcy filings of city and county governments.

For each of the 52 bankruptcy filings, we collect historical records which we screen for descriptions of the filing reason. The historical records thereby include documents from the following four sources: (i) If available, we extract news coverage from the New York Times as a major nationwide newspaper. (ii) Second, we attempt to collect articles published by The Bond Buyer as an important newspaper focusing on the municipal bond market. (iii) As the third source, we make use of articles available in local newspapers that generally report on the area of the bankrupt municipality. We access those articles mainly via Nexis Uni. (iv) Finally, if available, we extract disclosure statements of the bankrupt municipalities filed with the respective bankruptcy courts. To do so, we make use of Bloomberg BNA. An exemplary narrative analysis for Orange County's bankruptcy filing is presented in Appendix A.

Overall, we identify four filings reasons that we classify as idiosyncratic: financial speculation, other financial mismanagement, legal judgment, and failed public project. To cleanly estimate the economic costs of financial contagion

caused by a municipal bankruptcy, we impose the following additional three restrictions: (i) We exclude idiosyncratic bankruptcy filings if in the same state and same year, another municipality filed for bankruptcy due to economic reasons. (ii) We exclude idiosyncratic bankruptcies which are dismissed within 6 months after the filing. We argue that these filings do not represent new information for the debt market and most likely do not lead to an update of belief of investors (see Section 1.2.1). (iii) For our analyses of the economic costs, we analyze an event window of plus/minus three years around the respective filing. If event windows of idiosyncratic bankruptcy filings overlap, we only consider the larger filing.

Table 1.1
Municipal bankruptcy filings due to idiosyncratic reasons

This table presents 16 bankruptcy filings by cities and counties due to idiosyncratic reasons. To identify bankruptcy filings that are not related to the general economic trend but occur due to idiosyncratic reasons, we apply a narrative approach (e.g., Romer and Romer, 1989, 2004, 2010). We screen historical records (e.g., news coverage by the New York Times or court dockets) for the reasons underlying the bankruptcy filings. For further details on the narrative approach, please refer to Appendix A.

State	Bankrupt municipality	Filing year	Filing reason (main)
Total	16	-	-
Alabama	Greene County	1996	financial mismanagement
Alabama	Jefferson County	2011	financial / public project
Arizona	City of South Tucson	1983	legal judgement
Arkansas	Town of Ozan	1995	legal judgement
California	Orange County	1994	financial speculation
California	City of Desert Hot Springs	2001	legal judgement
Illinois	Village of Alorton	2005	legal judgement
Mississippi	City of Mound Bayou	1987	legal judgement
Missouri	City of Wellston	1984	legal judgement
Missouri	City of Kinloch	1994	financial mismanagement
Missouri	City of Reeds Spring	2002	legal judgement
Oklahoma	City of Wapanucka	1982	legal judgement
Oklahoma	Town of Muldrow	2005	legal judgement
Tennessee	City of Copperhill	1988	public project, legal/dispute
Texas	City of Kendleton	2001	legal judgement
Washington	City of North Bonneville	1991	legal judgement

This leaves us with sixteen bankruptcy cases filed by three county and thirteen city governments in eleven distinct U.S. states during the period 1982 to 2011. Table 1.1 presents an overview of the final municipal bankruptcy event sample.

1.3 DATA

1.3.1 LOCAL GOVERNMENT FINANCIALS AND OTHER CHARACTERISTICS

We collect data on local governments' debt and expenditure characteristics as well as other financial data from the U.S. Bureau of the Census' Annual Survey of State and Local Government Finances. This survey comprehensively captures detailed historical data on all types of governments' revenues, expenditures, debt, and assets on the state and local government level. A full survey of local governments is conducted every five years, and a sub-sample is surveyed in all other years. Survey years end on June 30 each year.

We consider county, city, township and special district governments for our analyses. We do not include school districts in our sample. As noted by Yang (2019b), several states with Chapter 9 provisions prohibit school districts from filing for bankruptcy. Besides, numerous states had or have credit support or bond guarantee programs in place, supporting the debt funding activities of school districts (see, e.g., Bland and Yu, 1988; Cirrotti, 2013; Hsueh and Kidwell, 1988). To block any potential direct economic effect of the bankrupt municipality on other local governments, we exclude local governments located in counties in which a city or county government filed for bankruptcy from the sample. We also exclude local governments in counties adjacent to the bankruptcy locations. Data on county adjacency are obtained from the U.S. Bureau of the Census.

We require availability of local government financial data for three years before and after the bankruptcy filing. For the county-level analyses, we aggregate financial data by adding up the respective figures of all local governments located in a county. Appendix A.1 provides a detailed description of the financial data included in our analyses. All continuous variables are winsorized at the 1^{st} and 99^{th} percentiles. For the variable intergovernmental revenue, which we include as control variable in several specifications, we impute missing data with state-

year average values. We use the county-level house price index as an additional control variable in several analyses. Data are collected from the Federal Housing Finance Agency. Missing datapoints are imputed with the state-level house price index for the respective year.

1.3.2 COUNTY EMPLOYMENT AND ESTABLISHMENT CHARACTERISTICS

Information on employment and the number of establishments by county and industry is gathered from the U.S. Bureau of the Census' County Business Pattern (CBP) annual series. The data series contains information on U.S. counties' employment and establishment during the week of March 12 for each year. We follow Bernstein et al. (2019) and aggregate the data by three industry sectors: non-tradable, services, and tradable. Non-tradable sector employment includes the NAICS sectors for retail trade as well as accommodation and food services. The services sector comprises the NAICS groups as defined by the Census and includes professional, scientific, and technical services, educational services, and health care and social assistance, amongst others. The tradable sector includes the remaining NAICS sectors, mainly consisting of manufacturing.[1] Data availability and disclosure in the CBP series varies by industry. For each county, we require availability of employment data across all sectors from three years before to three years after the bankruptcy filing. When matching the CBP data to the event period, we require a time lag of 6 months or more between the first CBP series measurement date and the date of the respective bankruptcy filing. All further measurement points result accordingly. The continuous variables are winsorized at the 1^{st} and 99^{th} percentiles.

[1]Industry classification schemes changed from SIC to NAICS in 1998. In the employment data aggregation process, we therefore map earlier data series to corresponding two-digit NAICS industries.

1.3.3 DESCRIPTIVE STATISTICS

Table 1.2, Panel A, presents summary statistics on local government financials for the 1,579 local governments included in our main sample. The sample consists of 13,027 local government-years across the 16 idiosyncratic municipal bankruptcy events, covering the period from 1979 to 2015.[2] The average (median) local government in our sample has expenditures of $107.6 million ($18.3 million). The average (median) maturing debt fraction is 11% (5%).

Panel B presents summary statistics for government financials aggregated on the county level. The sample comprises 4,773 county-years. The average (median) county aggregate comprises 3 (2) local governments. Finally, summary statistics on county business characteristics are presented in Panel C. The average (median) county in our sample has 11,191 (2,140) employees in the tradable sector. Besides, 9,971 (2,054) employees in a county are allocated to the non-tradable sector, and 21,832 (2,391) to the services sector.

Table 1.3 further presents descriptive statistics for the sample years before the bankruptcy filing (i.e., for the years t=-3 to t=-1). Comparing local governments and counties which display high and low fractions of maturing debt in the year after the bankruptcy, entities with above-median maturing debt have higher amounts of debt outstanding, have higher expenditures as well as a higher number of employees and establishments across all industry sectors, amongst others. We follow the approach of Adelino, Cunha and Ferreira (2017) and calculate this difference when taking into account state-year as well as local government or county size quintile-year fixed effects.[3] For Panel A, we further adjust the differences for government type fixed effects. Subsequent to these adjustments, the differences in financial as well as county business characteristics

[2]Four states in our sample are exposed to more than one idiosyncratic bankruptcy during the sample period. Depending on data availability, local governments can therefore be included in the analysis around several bankruptcy events.

[3]Since population data are not available on the local government level, we use the local government's or county's amount of revenue as a measure for size.

between local governments and counties with high and low fractions of maturing debt are statistically insignificant.

Table 1.2
Descriptive statistics

This table presents descriptive statistics for the baseline sample. This sample consists of 13,027 local government-years and covers 399 distinct counties, 579 cities, 70 townships, and 531 special districts in 11 states. Local governments' financials are adjusted for changes in the consumer price index based on the data provided by the U.S. Bureau of Labor Statistics and are expressed in 2015 $k. A detailed description of all variables can be found in Appendix A.1.

	N	Mean	SD	25^{th}	50^{th}	75^{th}
Panel A: Local government financials						
Maturing debt	13,027	0.11	0.18	0.00	0.05	0.11
Outstanding debt amount	13,026	94670.66	397377.27	346.72	6416.5	36320.54
Maturing debt amount	13,024	9897.16	48348.88	0.00	377.86	2631.34
Issuer dummy	12,978	0.30	0.46	0.00	0.00	1.00
Issued debt amount	12,978	14445.82	74065.79	0.00	0.00	681.59
Total expenditures	13,027	107596.23	336700.86	4252.38	18309.21	64504.72
Current expenditures	13,015	92279.07	295965.00	3609.06	15928.67	55289.55
Capital outlay exp.	13,015	15253.62	55125.64	100.22	1404.05	7480.55
Panel B: County-level government financials						
$\text{Maturing debt}_{\text{mean}}$	4,773	0.11	0.15	0.03	0.07	0.13
Government entities	4,773	2.73	2.83	1.00	2.00	3.00
Outstanding debt amount	4,773	308,641.54	1,662,795.79	2,342.86	12,941.54	74,964.57
Maturing debt amount	4,773	31,741.77	176,078.97	137.43	838.16	6,535.79
Issued debt amount	4,755	46,276.41	253,200.88	0.00	0.00	6,654.72
Total expenditures	4,773	315,587.48	1,216,436.15	11,756.84	33,950.36	140,177.52
Current expenditures	4,773	268,519.06	989,602.54	10,637.15	30,273.20	123,285.26
Capital outlay exp.	4,773	46,524.96	230,278.66	602.73	3,199.22	14,108.85
Panel C: County business pattern						
Total employment	4,584	43,144.19	141,969.52	2,450.50	6,538.00	25,087.00
Tradable employment	4,584	11,190.63	34,392.76	581.00	2,139.50	7,278.50
Non-tradable employment	4,584	9,970.54	26,799.06	777.50	2,054.00	7,356.00
Services employment	4,584	21,832.22	82,147.64	820.00	2,390.50	10,159.00
Total establishment	4,584	2,939.36	7,919.04	350.00	722.00	2,013.00
Tradable establishment	4,584	673.46	1,694.41	90.00	181.00	488.00
Non-tradable establishment	4,584	708.24	1,663.49	113.00	220.00	589.00
Services establishment	4,584	1,543.33	4,563.75	143.00	318.00	984.00

Table 1.3

Pre-bankruptcy characteristics of local governments with high and low fractions of maturing debt

This table presents the mean pre-bankruptcy characteristics of local governments with a fraction of maturing long-term debt below or equal to the median and those with a fraction greater than the median in the year following the bankruptcy. Difference is calculated as the difference between the mean values of local governments with a high fraction and those with a low fraction. Difference adjusted takes into account state-by-year fixed effects as well as size quintile-by-year fixed effects when calculating the difference in the mean values between the two groups. Moreover, in Panel A, we further adjust the differences for government type fixed effects. The pre-bankruptcy period spans the three years before a bankruptcy filing. Local governments' financials are adjusted for changes in the consumer price index and are expressed in 2015 $k. ***, ** and * indicate significance at the 1%-, 5%- and 10%-levels, respectively. A detailed description of all variables can be found in Appendix A.1.

	Maturing debt $> p50$		Maturing debt $\leq p50$			
	N	Mean	N	Mean	Diff.	Diff. adj.
Panel A: Pre-bankruptcy local government financials						
Outstanding debt amount	2,790	120,778.16	2,793	58,660.71	62,117.46***	-2,456.36
Maturing debt amount	2,790	13,244.57	2,793	5,723.62	7,520.95***	1,326.23
Issuer dummy	2,790	0.41	2,793	0.19	0.21***	0.08***
Issued debt amount	2,790	17,932.51	2,793	9,567.52	8,364.98***	-1,454.12
Total expenditures	2,790	133,305.27	2,793	68,536.48	64,768.79***	4,616.72
Current expenditures	2,790	113,674.87	2,793	60,008.02	53,666.85***	3,641.99
Capital outlay exp.	2,790	19,726.57	2,793	8,509.23	11,217.34***	1,091.11
Panel B: Pre-bankruptcy county-level government financials						
Government entities	1,022	3.15	1,023	2.31	0.84***	0.28**
Outstanding debt amount	1,022	459,837.10	1,023	128,047.87	331,789.23***	11,411.33
Maturing debt amount	1,022	47,585.28	1,023	14,593.10	32,992.18***	-147.20
Issued debt amount	1,022	66,785.50	1,023	20,854.02	45,931.48**	-956.88
Total expenditures	1,022	430,980.29	1,023	161,020.48	269,959.82***	31,027.75
Current expenditures	1,022	362,288.75	1,023	142,205.25	220,083.50***	25,007.14
Capital outlay exp.	1,022	67,774.74	1,023	18,965.36	48,809.38***	5,452.86
Panel C: Pre-bankruptcy county business pattern						
Total employment	983	61,287.45	981	22,407.50	38,879.95***	4,479.23
Tradable employment	983	16,289.52	981	5,942.07	10,347.45***	1,894.68
Non-tradable employment	983	13,402.66	981	5,785.48	7,617.17***	763.36
Services employment	983	31,284.59	981	10,618.65	20,665.93***	1,801.62
Total establishment	983	3,944.61	981	1,774.71	2,169.90***	219.81
Tradable establishment	983	905.16	981	422.83	482.32***	65.48
Non-tradable establishment	983	928.01	981	463.11	464.90***	45.93
Services establishment	983	2,081.73	981	880.20	1,201.54***	105.11

1.4 MUNICIPAL BANKRUPTCIES AND THE CREDIT MARKET ACCESS OF OTHER LOCAL GOVERNMENTS

We first examine whether a municipal bankruptcy affects other local governments' access to the credit market. We therefore compare the credit market access of local governments located in neighboring states in the year after one state is exposed to a bankruptcy filing. To mitigate potential concerns that state-level differences in economic development distort our results, we additionally conduct our analyses on a restricted sample which only includes local governments located in state border counties.[4] We employ two different measures for local governments' credit market access. Our first measure is the issuer dummy, which is a dummy that equals one if a local government issues long-term debt and zero otherwise. Secondly, we use the issued debt amount, which is the natural logarithm of the amount of long-term debt issued by a local government plus one.

When analyzing the credit market effect, we draw on the findings of Gao, Lee and Murphy (2019) and select the year after the bankruptcy filing as our period of interest. Accordingly, we define *Financial contagion window* as a dummy which equals one in the year after the bankruptcy and zero otherwise. Further, we define *Bankruptcy state* as a dummy that equals one for the states that are exposed to a bankruptcy and zero for their neighboring states in which no bankruptcy occurs. We analyze a window of plus/minus three years around the bankruptcy filings. The regressions are estimated on the local government-year level and are based on the following specification:

$$\begin{aligned} \textit{Issuer dummy}_{i,t} = \alpha + \beta \textit{Financial contagion window}_{b,t} \cdot \textit{Bankruptcy state}_s \\ + \gamma \textit{Financial contagion window}_{b,t} + \delta \textit{Bankruptcy state}_s + \varphi_{i,b} + \chi_{b,t} + \varepsilon_{i,t} \end{aligned} \tag{1.1}$$

[4] An illustration of the border county sample is presented in Appendix Figure A.1.

where i denotes a local government, t a fiscal year, s a state, and b a municipal bankruptcy filing. $\varphi_{i,b}$ are local government-by-bankruptcy event fixed effects, and $\chi_{b,t}$ bankruptcy event-by-year fixed effects. We include state GDP per cap as a control variable in all credit market access regressions. Local government-event fixed effects and event-year fixed effects absorb both the *Financial contagion window* and the *Bankruptcy state* dummy variables in the empirical estimation. The variable of interest is the coefficient on the interaction term between the Financial contagion window dummy and the Bankruptcy state dummy (*Financial contagion window x Bankruptcy state*). Standard errors are clustered at the state-by-bankruptcy-event level to correct for serial correlation of the error term within states and events.

Table 1.4

Municipal bankruptcies and local governments' credit market access

The dependent variables are indicated in each column. Issuer dummy is a dummy that equals one if a local government issues long-term debt and zero otherwise. Issued debt amount is the natural logarithm of the amount of long-term debt that a local government issues plus one. Financial contagion window is a dummy that equals one in the year following the bankruptcy and zero otherwise. Bankruptcy state is a dummy that equals one for the states with a municipal bankruptcy and zero for their neighboring states without a bankruptcy. The regressions are estimated on the local government-year level. In column (1) and (3), the sample consists of all local governments in bankruptcy states and their neighboring states. In column (2) and (4), we restrict the sample to local governments in bordering counties. The bordering county sample is illustrated in Appendix Figure A.1. The sample period ranges from three years before to three years after the bankruptcy filing. T-statistics based on Huber/White robust standard errors clustered by state and bankruptcy filing are presented in parentheses. ***, ** and * indicate significance at the 1%-, 5%-and 10%-levels, respectively. A detailed description of all variables can be found in Appendix A.1.

	(1)	(2)	(3)	(4)
	Issuer dummy		Issued debt amount	
	All	Border	All	Border
Fin. contagion window x Bankruptcy state	-0.040*	-0.063**	-0.281**	-0.465**
	(-1.990)	(-2.172)	(-2.240)	(-2.031)
State GDP per cap	Yes	Yes	Yes	Yes
Local government x Event FE	Yes	Yes	Yes	Yes
Event x Year FE	Yes	Yes	Yes	Yes
Cluster	State-Event	State-Event	State-Event	State-Event
Number of observations	30,961	4,340	30,961	4,340
Adjusted R^2	0.411	0.422	0.498	0.507

The effect of a municipal bankruptcy on other local governments' credit market access is presented in Table 1.4. In column (1) and (2), the dependent variable is the issuer dummy. For local governments located in bankruptcy states, the likelihood of issuing debt in the year following the bankruptcy filing is 4.0% lower compared to local governments in neighboring states. The linear probability model for bordering counties shows that a municipal bankruptcy lowers the probability of other local governments issuing debt by 6.3% in the year after the filing. In column (3) and (4), we present results for changes in the debt issue amount. The coefficient of the interaction term indicates that local governments in bankruptcy states decrease the amount of debt issued by 28.1% relative to local governments in neighboring states. When only comparing local governments in bordering counties, the relative difference in debt issuance is more pronounced, with the point estimate being equal to a decrease by 46.5%.

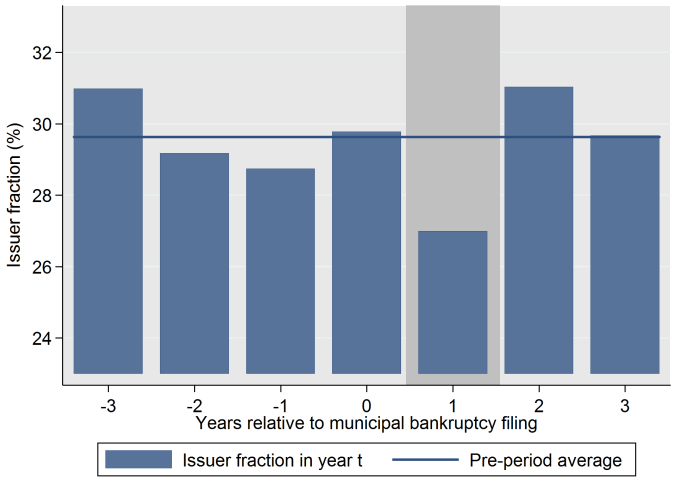
In Figure 1.1, we illustrate the time dynamics of the bankruptcy's effect on the credit market access of other local governments. Subfigure (a) shows the fraction of local governments issuing long-term debt in the bankruptcy state over the event window from three years before to three years after the bankruptcy. The fraction of debt issuing local governments is consistent in the years prior to the bankruptcy. In the year following the municipal bankruptcy filing, the fraction of local governments that issue debt decreases compared to the pre-bankruptcy period. Two and three years after the bankruptcy, the issuer fraction reverts to a similar level as before the bankruptcy.

Using the issuer dummy as the dependent variable, Subfigure (b) shows the coefficient estimates and 90% confidence intervals for the regression on event time dummies interacted with a dummy that equals one if the local government is located in the state of the bankruptcy, and zero otherwise. The sample is restricted to local governments in bordering counties of bankruptcy states and their neighboring states. We do not observe differences between the local gov-

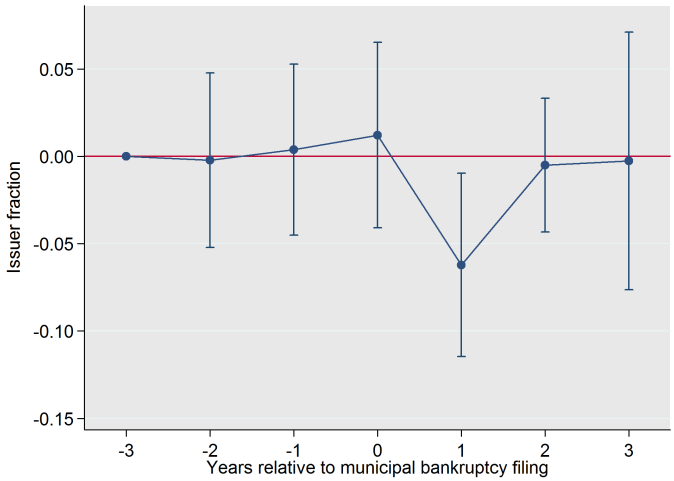
Figure 1.1
Municipal bankruptcies and local governments' credit market access

This figure illustrates the debt issuance of local governments from three years before to three years after the bankruptcies. Subfigure (a) shows the fraction of local governments issuing long-term debt in the bankruptcy state over the event window. Subfigure (b) presents the coefficient estimates and 90% confidence intervals for regressing the issuer dummy on event time dummies interacted with a dummy that equals one if the local government is located in the state of the bankruptcy, and zero otherwise. Issuer dummy equals one if the local government issues long-term debt, and zero otherwise. The specification follows equation 1.1, including local government-event and event-year fixed effects. The sample includes only local governments in bordering counties of bankruptcy states and their neighboring states. Subfigure (c) shows the mean values of the long-term debt issued by local governments in the bankruptcy state over the event window. Subfigure (d) presents coefficient estimates and 90% confidence intervals for event time dummies interacted with a bankruptcy state dummy, with the dependent variable being the logarithm of the debt issuance amount plus one. Other than that, the regression specification is identical to Subfigure (b). A detailed description of all variables can be found in Appendix A.1.

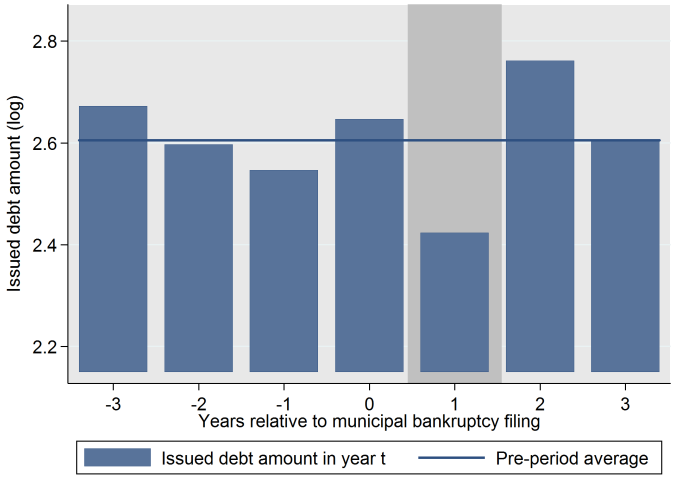
(a) Issuer fraction in bankruptcy states



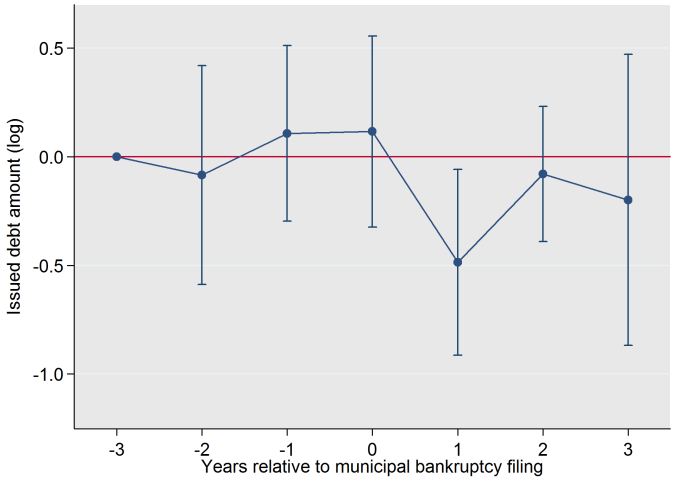
(b) Regression coefficient for issuer dummy in bankruptcy states versus neighbor states



(c) Debt issue amount in bankruptcy states



(d) Regression coefficient for debt issue amount in bankruptcy states versus neighbor states



ernments in bankruptcy states and neighboring states prior to the bankruptcy as well as in the filing year. In the year after the bankruptcy, local governments located in bankruptcy states have a substantially lower probability of issuing debt compared to local governments in neighboring states. In year two and three after the filing, we again do not observe any statistical difference in the debt-issuing probability. We conclude that our results are in line with a temporarily restricted credit market access for other local governments in bankruptcy states after a bankruptcy filing occurs.

Subfigure (c) shows the mean values of the long-term debt issued by local governments in the bankruptcy state over the event window. Subfigure (d) presents regression coefficients and 90% confidence intervals for event time dummies interacted with the bankruptcy state dummy, using the logarithm of the debt issue amount as the dependent variable and again focusing on the subset of local governments located in bordering regions. In the year after the bankruptcy filing, both figures show a relative decrease in the debt issue amount for local governments in bankruptcy states. Overall, our results indicate that municipal bankruptcy filings limit the credit market access of other local governments located in the same state.

1.5 MUNICIPAL BANKRUPTCIES AND THEIR ECONOMIC COSTS FOR OTHER LOCAL GOVERNMENTS

1.5.1 EXTERNALITIES ON LOCAL GOVERNMENTS' EXPENDITURES

We analyze how the restricted credit market access in the year after a bankruptcy, documented in Section 1.4, affects the expenditures of other local governments located in the bankruptcy states. We therefore follow the empirical strategy of Almeida et al. (2012) and exploit ex-ante variation in the maturity of long-term debt across local governments in the year after the bankruptcy filing. This fraction of maturing long-term debt is the outcome of contracting decisions that

municipalities made several years ago. Hence, a local government's fraction of maturing long-term debt is likely exogenous to the bankruptcy filing of another municipality.

We analyze a window of plus/minus three years around the bankruptcy filings. The sample includes local governments located in the bankruptcy states. We conduct our analyses both on the local government and on the county level. In our baseline specification, the dependent variable is the natural logarithm of a local government's expenditures plus one. We estimate the following regression:

$$\begin{aligned} \textit{Total expenditures}_{i,t} = &\alpha + \beta \textit{Post}_{s,t} \cdot \textit{Maturing debt}_{i,b} + \gamma \textit{Post}_{s,t} \\ &+ \delta \textit{Maturing debt}_{i,b} + \varphi_{i,b} + \chi_{s,t} + \varepsilon_{i,t} \end{aligned} \tag{1.2}$$

where i denotes a local government, t a fiscal year, s a state, and b a municipal bankruptcy filing. $\varphi_{i,b}$ are local government-by-bankruptcy-event fixed effects, and $\chi_{s,t}$ state-by-year fixed effects. We further include local government type-by-post dummy fixed effects in all regressions on the local government level. $\textit{Post}_{s,t}$ is a dummy that equals one in the three years following the bankruptcy and zero otherwise. $\textit{Maturing debt}_{i,b}$ is the fraction of maturing long-term debt in the year following the municipal bankruptcy.[5]

Local government-event fixed effects and state-year fixed effects absorb both the *Post* dummy and the *Maturing debt* variable in the empirical estimation.

[5] As noted in Section 1.3.1, we collect data on local governments' debt from the U.S. Bureau of the Census' Annual Survey of State and Local Government Finances. The survey year of the census ends on June 30 and includes financial data for municipalities whose fiscal year ends between July 1 of the previous year and June 30 of the survey year. Our continuous treatment variable is a local government's fraction of maturing long-term debt of the fiscal year whose course is mainly within the debt market contagion period following the bankruptcy filing of another municipality. For the measurement date of maturing debt as a flow variable, this requires a period of 6 months or more between the fiscal year end and the bankruptcy filing. To identify the measurement—refinancing—year, we therefore proceed in two steps: (i) For each state, we collect the month in which the fiscal year of its municipalities ends. If no such prevalent month exists, i.e., for the States of Illinois and Missouri, we select June as the survey year ends in June. (ii) We calculate the time period between the fiscal year end and the bankruptcy date. (iii) We define the refinancing year as the fiscal year which ends at least 6 months after the bankruptcy.

The variable of interest is the coefficient on the interaction term between the post dummy and the maturing debt fraction (*Post x Maturing debt*). Standard errors are clustered at the county-by-bankruptcy-event level to correct for serial correlation of the error term within counties and events. For the county-level analyses, we aggregate local governments' expenditures by county and year and use county fixed effects instead of local government fixed effects in our regressions. As the treatment variable, we use Maturing $\text{debt}_{\text{mean}}$, which we calculate as the county-level mean fraction of maturing long-term debt in the year following the bankruptcy.

We illustrate the geographic distribution of Maturing $\text{debt}_{\text{mean}}$ for three exemplary bankruptcy filings in Figure 1.2. We do not observe any systematic pattern or geographical clustering of counties based on the average fraction of maturing debt. Rather, they are geographically widely distributed.

The effect of municipal bankruptcies on other local governments' expenditures is presented in Table 1.5. In Panel A, we show results for the local government level. Column (1) presents the result for our baseline specification. Since the regression includes local government-event fixed effects, we estimate within-local government changes of expenditures for entities with higher fractions of maturing long-term debt relative to changes for entities with lower fractions. The coefficient on the interaction term *Maturing debt x Post* is -0.129 and indicates that for the average local government, total expenditures decrease by about 1.4% after the bankruptcy filing of another municipality.[6] A one standard deviation increase in the maturing debt fraction leads to a decrease in total expenditures by about 2.3%.

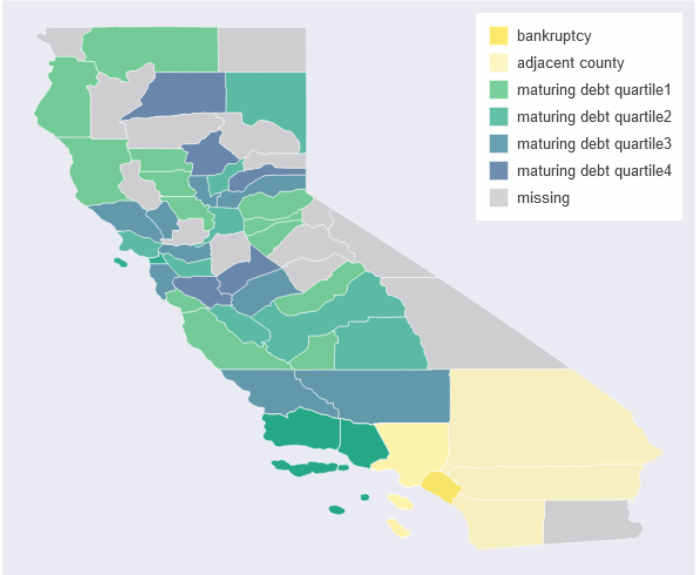
In column (2), we further include the lagged house price index as well as intergovernmental revenue as control variables. We obtain a point estimate of -0.130, similar to the one for our baseline specification. In column (3), we add

[6] As presented in Table 1.2, the average fraction of maturing debt is 0.11.

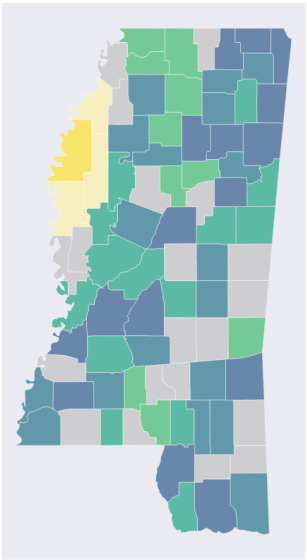
Figure 1.2
Maturing long-term debt: geographic distribution

This figure illustrates the geographic distribution of counties' mean fraction of maturing long-term debt for the municipal bankruptcy filings of Orange County (a), the City of Mound Bayou (b), and the City of Reeds Springs (c). We further highlight the county of the local government filing for bankruptcy (■) and counties adjacent to the bankrupt local government (■). Local governments in the county of the bankruptcy and in adjacent counties are excluded from the sample. Counties marked in gray are either not included in the sample as data are not available or are excluded from the sample as any other city or county government that filed for bankruptcy—irrespective of the filing reason—is located there.

(a) Orange County, CA, 1994



(b) City of Mound Bayou, MS, 1987



(c) City of Reeds Springs, MO, 2002

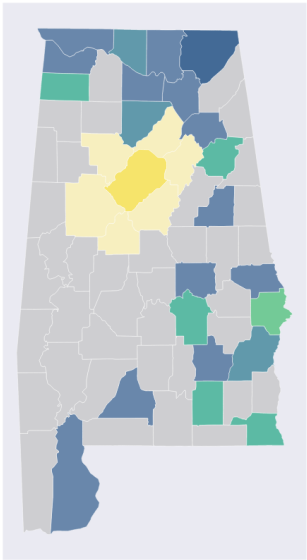


Table 1.5

Municipal bankruptcies and externalities on local governments' expenditures

The dependent variable is the natural logarithm of total expenditures. Maturing debt is the fraction of maturing long-term debt in the year following the municipal bankruptcy. Maturing $\text{debt}_{\text{mean}}$ is the county-level mean fraction of maturing long-term debt in the year following the municipal bankruptcy. Post is a dummy that equals one in the three years following the bankruptcy and zero otherwise. The regressions are estimated on the local government-year level in Panel A, and on the county-year level in Panel B. The sample period ranges from three years before to three years after the bankruptcy filing. T-statistics based on Huber/White robust standard errors clustered by county and bankruptcy filing are presented in parentheses. ***, ** and * indicate significance at the 1%-, 5%- and 10%-levels, respectively. A detailed description of all variables can be found in Appendix A.1.

	(1)	(2)	(3)
	Log(Total expenditures)		
Panel A: Local government level			
Maturing debt x Post	-0.129***	-0.130***	-0.120**
	(-2.587)	(-2.660)	(-2.313)
House price index		0.000**	0.000**
		(2.460)	(2.462)
Intergovernmental revenue		0.023***	0.025***
		(3.744)	(4.064)
Local government x Event FE	Yes	Yes	Yes
State x Year FE	Yes	Yes	Yes
Government type x Post FE	Yes	Yes	Yes
Size group x Year FE	No	No	Yes
Cluster	County-Event	County-Event	County-Event
Number of observations	13,027	13,027	13,027
Adjusted R^2 (within)	0.169	0.174	0.181
Panel B: County level			
$\text{Maturing debt}_{\text{mean}}$ x Post	-0.231***	-0.237***	-0.258***
	(-2.952)	(-3.227)	(-3.545)
House price index		0.001	0.001
		(1.335)	(0.923)
Intergovernmental revenue		0.022	0.024
		(1.233)	(1.243)
County x Event FE	Yes	Yes	Yes
State x Year FE	Yes	Yes	Yes
Size group x Year FE	No	No	Yes
Cluster	County-Event	County-Event	County-Event
Number of observations	4,773	4,773	4,773
Adjusted R^2 (within)	0.198	0.206	0.213

local government size quintile-year fixed effects to mitigate concerns that our results are driven by a potential size-specific development of local governments over time. The coefficient on the interaction term is -0.120 and thus still very close to the baseline estimate.

In Panel B, we present results on the county level. All point estimates for the interaction term *Maturing debt$_{mean}$ x Post* are negative and statistically significant. A one standard deviation increase in Maturing debt$_{\text{mean}}$ leads to a county-level decrease in aggregate expenditures by 3.5% to 3.9%.

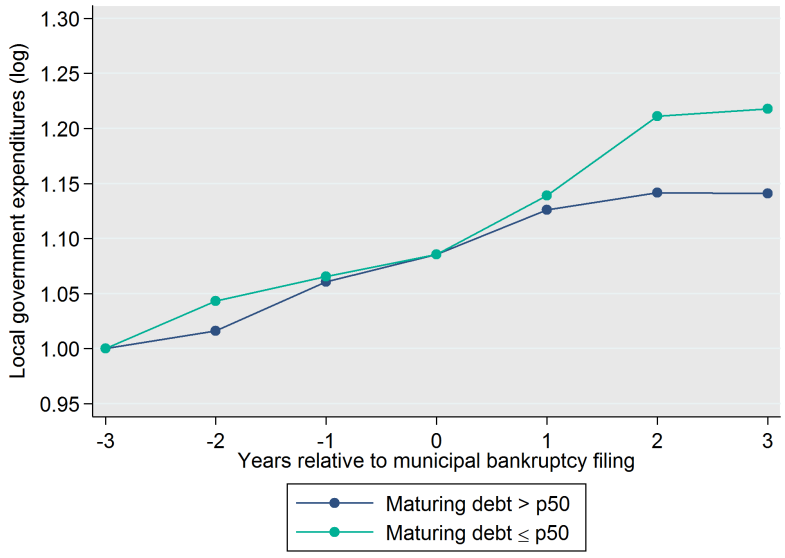
We illustrate the time dynamics of the bankruptcy's effect on other local governments' expenditures in Figure 1.3. In particular, we document the development of local governments' expenditures from three years before the bankruptcy to three years thereafter. Subfigure (a) shows the mean value of total expenditures for local governments with a fraction of maturing long-term debt greater than the size-group median and those with a fraction below or equal to the size-group median, with maturing debt measured in the year following the bankruptcy. Both groups follow similar trends prior to the bankruptcy filing. In the years following the municipal bankruptcy, local governments with a relatively higher fraction of maturing long-term debt display a considerably lower increase in expenditures than local governments with a lower fraction of maturing debt.

Subfigure (b) shows the coefficient estimates and the 90% confidence intervals on the fraction of maturing long-term debt interacted with event time dummies. The regression specification follows equation 1.2 and includes all control variables and fixed effects as in Table 1.5, Panel A, column (3). In line with the raw data, we do not observe differences in the development of local governments with higher and lower fractions of maturing debt prior to the bankruptcy filing. The negative externality on expenditures manifests two years after the bankruptcy filing and remains constant in the third year.

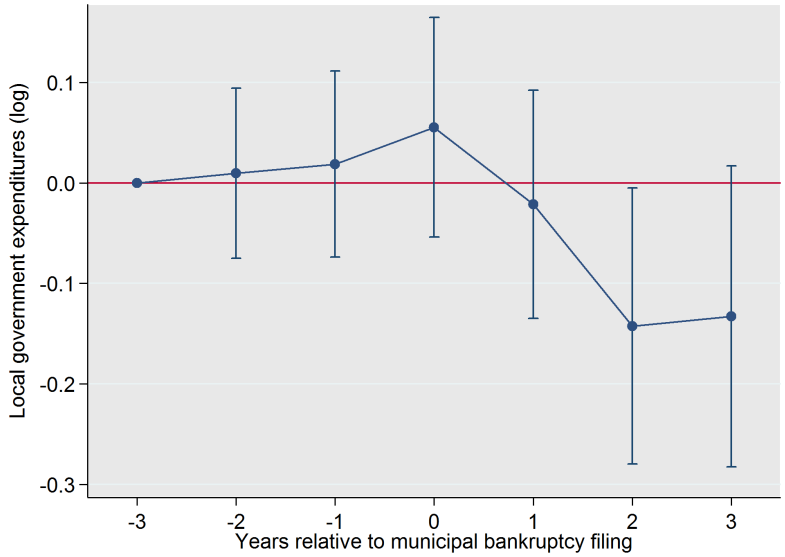
Figure 1.3
Municipal bankruptcies and externalities on local governments' expenditures

This figure illustrates the development of local government expenditures from three years before the bankruptcy to three years thereafter. Subfigure (a) shows the mean value of total expenditures for local governments with a fraction of maturing long-term debt greater to the size-group median (●) and those with a fraction below or equal to the size-group median in the year following the bankruptcy (●). Subfigure (b) shows the coefficient estimates and the 90% confidence intervals on the fraction of maturing long-term debt (in the year following the bankruptcy) interacted with event time dummies. The regression specification follows Table 1.5, Panel A, column (3). A detailed description of all variables can be found in Appendix A.1.

(a) Mean values of total expenditures



(b) Regression coefficients on maturing debt



Overall, our results indicate that the restricted credit market access after a municipal bankruptcy negatively affects other local governments' expenditures and therefore exposes these local governments to economic costs of financial contagion.

1.5.2 ROBUSTNESS TESTS

We conduct several robustness and placebo tests to address potential concerns that our results capture unobserved differences of local governments that correlate with our measure of maturing debt.

In the first set of robustness tests, we rule out that local governments with high fractions of maturing debt geographically cluster around the bankrupt municipality and might therefore be exposed to a direct demand effect from the bankruptcy location. We exclude local governments located in counties within 40, 60, and 80 miles distance to the bankrupt municipality from our sample and repeat our analysis for the expenditure effect. Table 1.6, Panel A, presents the results. The coefficient on the interaction term *Maturing debt x Post* is negative for all three restricted samples. Compared to our baseline estimates, the negative effect on expenditures is slightly less pronounced. A one standard deviation increase in maturing debt corresponds to a decrease in expenditures of 1.7% to 2.2%.

Secondly, we conduct a range of placebo tests. Instead of conducting our analyses around the actual bankruptcy filings, we apply our regression framework to unaffected non-shock periods. In particular, we use three, two, and one year before the actual bankruptcy filing as fictional bankruptcy years. If our effect is only driven by the general refinancing activity of local governments and unconnected to contagion effects on the municipal debt market, we should find a similar expenditure effect in these analyses. Results are presented in Panel B of Table 1.6. For none of the three placebo tests, we obtain a statistically sig-

nificant result. Overall, the robustness tests support our previous finding that municipal bankruptcy filings expose other local governments to the economic costs of financial contagion.

Table 1.6
Robustness and placebo tests

The dependent variable is the natural logarithm of total expenditures. Maturing debt is the fraction of maturing long-term debt in the year following the municipal bankruptcy. Post is a dummy that equals one in the three years following the municipal bankruptcy and zero otherwise. The regressions are estimated on the local government-year level. Controls comprise the house price index and intergovernmental revenues. In Panel A, we exclude local governments in counties within a certain distance to the county of the bankrupt municipality. In Panel B, we use the three, two, and one year before the actual bankruptcy as placebo tests. T-statistics based on Huber/White robust standard errors clustered by county and bankruptcy filing are presented in parentheses. ***, ** and * indicate significance at the 1%-, 5%- and 10%-levels, respectively. A detailed description of all variables can be found in Appendix A.1.

	(1)	(2)	(3)
	Log(Total expenditures)		
Panel A: Distance to bankruptcy location			
	> 40 mi	> 60 mi	> 80 mi
Maturing debt x Post	-0.121**	-0.119**	-0.097*
	(-2.315)	(-2.262)	(-1.724)
Controls	Yes	Yes	Yes
Local government x Event FE	Yes	Yes	Yes
State x Year FE	Yes	Yes	Yes
Government type x Post FE	Yes	Yes	Yes
Size group x Year FE	Yes	Yes	Yes
Cluster	County-Event	County-Event	County-Event
Number of observations	12,894	12,495	11,634
Adjusted R^2 (within)	0.180	0.182	0.173
Panel B: Placebo tests in the years before the bankruptcy			
	t−3	t−2	t−1
Maturing debt x Post	-0.022	-0.018	-0.033
	(-0.466)	(-0.244)	(-0.697)
Controls	Yes	Yes	Yes
Local government x Event FE	Yes	Yes	Yes
State x Year FE	Yes	Yes	Yes
Government type x Post FE	Yes	Yes	Yes
Size group x Year FE	Yes	Yes	Yes
Cluster	County-Event	County-Event	County-Event
Number of observations	12,194	13,027	12,369
Adjusted R^2 (within)	0.132	0.0979	0.160

1.5.3 EXTERNALITIES ON LOCAL GOVERNMENTS' EXPENDITURES BY PURPOSE

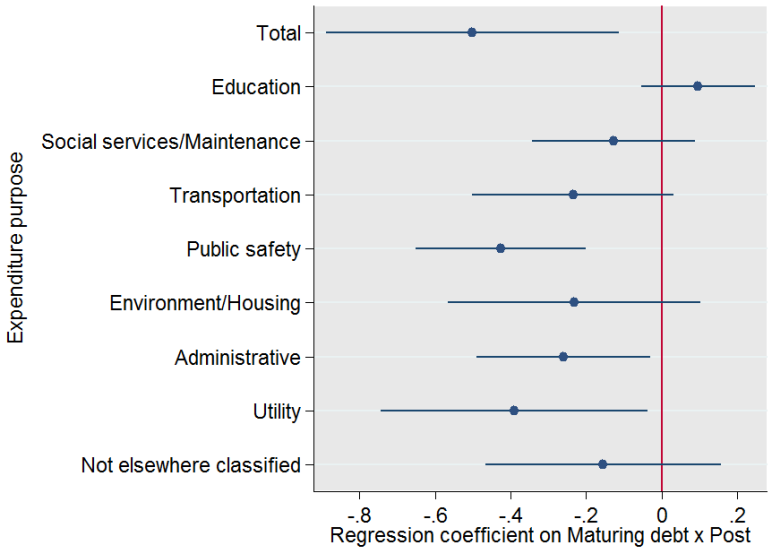
To provide further insights on the economic costs of financial contagion, we investigate the expenditure adjustments of other local governments by expenditure purpose. We therefore aggregate local governments' expenditures by function (i.e., purpose) and employ these expenditures as dependent variables in our regression framework as specified in equation 1.2. We display the regression coefficients for our variable of interest—the interaction term of Maturing debt and Post—and their 90% confidence intervals in Figure 1.4. For the estimation, we follow the specification as displayed in Table 1.5, Panel A, column (3), except for the different dependent variables. In Subfigure (a), the dependent variables are the natural logarithm of capital outlay expenditures for the purposes indicated on the y-axis. In Subfigure (b), the dependent variables are the natural logarithm of current expenditures for the purposes indicated on the y-axis.

The key finding is that the relative cut in expenditures is more pronounced for capital outlay expenditures than it is for current expenditures. In particular, a one standard deviation increase in Maturing debt decreases capital outlay expenditures by about 9.0%, while it decreases current expenditures by about 1.9%. Next, we analyze changes by expenditure purpose within these two general expenditure categories. We document a relative decrease in capital outlay expenditures along various expenditure purposes, including a relative decrease in capital outlays for transportation, public safety, and utility. When analyzing effects on current expenditures by purpose, our results only point towards a relative adjustment in expenditures for social services and maintenance, as well as for expenditures not elsewhere classified. Overall, our point estimates for the expenditure cuts following a municipal bankruptcy are economically relevant and thus provide evidence that municipal bankruptcies are costly for other local governments that rely on debt financing.

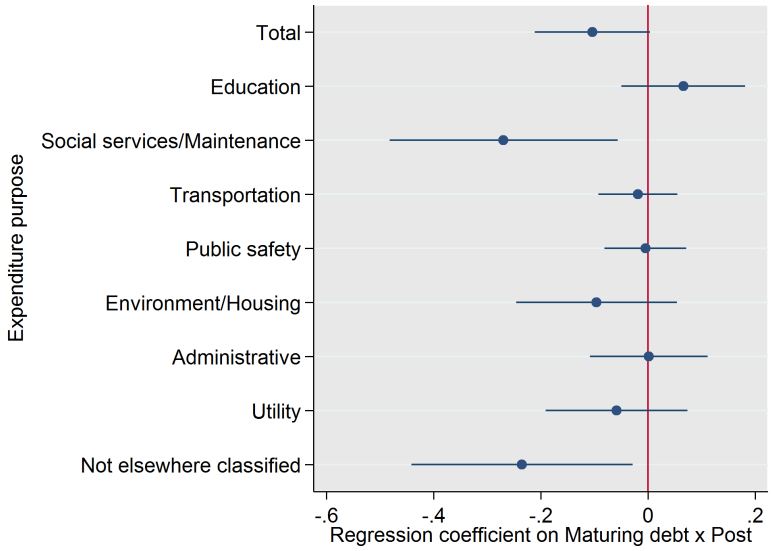
Figure 1.4
Municipal bankruptcies and externalities on local governments' expenditures by purpose

This figure illustrates regression coefficients on the interaction of Maturing debt and Post and their 90% confidence interval. Maturing debt is the fraction of maturing long-term debt in the year following the municipal bankruptcy. Post is a dummy that equals one in the three years following the bankruptcy and zero otherwise. In Subfigure (a), the dependent variables are the natural logarithm of capital outlay expenditures for the purposes indicated on the y-axis. In Subfigure (b), the dependent variables are the natural logarithm of current expenditures for the purposes indicated on the y-axis. The regressions are estimated on the local government-year level and, except for the different dependent variable, follow the specification as in Table 1.5, Panel A, column (3). A detailed description of all variables can be found in Appendix A.1.

(a) Capital outlay expenditures by purpose



(b) Current expenditures by purpose



1.6 MUNICIPAL BANKRUPTCIES AND EXTERNALITIES ON EMPLOYMENT

As a final test for the economic costs of financial contagion following a bankruptcy, we analyze externalities on the private sector. Since private sector data are only available at the county level, the dependent variables in these regressions are the natural logarithm of a county's number of employees or establishments in the indicated industry sector, respectively. All regressions follow the specification as presented in equation 1.2, using the county-level mean fraction of maturing long-term debt in the year following the municipal bankruptcy as the treatment variable (Maturing $\text{debt}_{\text{mean}}$).

Results for the effect on employment are presented in Table 1.7, Panel A. We only obtain a statistically significant point estimate for the interaction term *Maturing* $debt_{mean}$ *x Post* when using tradable employment as the outcome variable. The coefficient denotes that for a one standard deviation increase in Maturing $\text{debt}_{\text{mean}}$, tradable employment decreases by about 2.7%, which in turn corresponds to about 297 tradable sector employees for the average county.

Panel B shows results for the effect on the number of establishments across industry sectors. Consistent with the employment effect, the point estimate for tradable establishment as the dependent variable is negative and statistically significant. For the services and non-tradable sector, the coefficient on the interaction term is not statistically significant or close to significance.

We conclude that the externality on the private sector seems to be concentrated on the tradable sector. Taking into account that the expenditure effect is more pronounced for capital outlay expenditures (see Section 1.5.3), and further assuming that such expenditures are more relevant for the tradable sector, we consider the employment result to be in line with our previous findings.

Overall, our results support the view that the negative externalities of municipal bankruptcies also transmit to the private sector.

Table 1.7
Municipal bankruptcies and externalities on other counties' private sector employment

The dependent variables in Panel A are the natural logarithm of the number of employees in the indicated industry sector. In Panel B, the dependent variables are the natural logarithm of the number of establishments in the indicated industry sector. Maturing $debt_{mean}$ is the county-level mean fraction of maturing long-term debt in the year following the municipal bankruptcy. Post is a dummy that equals one in the three years following the bankruptcy and zero otherwise. The regressions are estimated at the county-year level. Controls comprise the house price index and intergovernmental revenues. T-statistics based on Huber/White robust standard errors clustered by county and bankruptcy filing are presented in parentheses. ***, ** and * indicate significance at the 1%-, 5%- and 10%-levels, respectively. A detailed description of all variables can be found in Appendix A.1.

	(1)	(2)	(3)	(4)
	Total	Tradable	Non-Tradable	Services
Panel A: Effect on employment				
Maturing $debt_{mean}$ x Post	-0.041	-0.177**	0.011	-0.005
	(-1.231)	(-2.096)	(0.408)	(-0.101)
Controls	Yes	Yes	Yes	Yes
County x Event FE	Yes	Yes	Yes	Yes
State x Year FE	Yes	Yes	Yes	Yes
Size group x Year FE	Yes	Yes	Yes	Yes
Cluster	County-Event	County-Event	County-Event	County-Event
Number of observations	4,584	4,584	4,584	4,584
Adjusted R^2 (within)	0.358	0.136	0.280	0.295
Panel B: Effect on establishments				
Maturing $debt_{mean}$ x Post	0.003	-0.042*	0.019	0.013
	(0.221)	(-1.711)	(1.008)	(0.829)
Controls	Yes	Yes	Yes	Yes
County x Event FE	Yes	Yes	Yes	Yes
State x Year FE	Yes	Yes	Yes	Yes
Size group x Year FE	Yes	Yes	Yes	Yes
Cluster	County-Event	County-Event	County-Event	County-Event
Number of observations	4,584	4,584	4,584	4,584
Adjusted R^2 (within)	0.649	0.442	0.356	0.704

1.7 CONCLUSION

Local governments are currently facing numerous challenges that place a financial burden on them. Among others, public pension liabilities are high (see, e.g., Novy-Marx and Rauh, 2011a,b, 2014), and climate change is adversely affecting municipal financing costs (see, e.g., Goldsmith-Pinkham et al., 2023; Painter, 2020). Consequently, municipal bond analysts consider liquidity as well as potential defaults of municipalities as central issues of today's municipal bond market (see., e.g., Kozlik, 2020). The disrupting potential of municipal bankruptcies has raised political awareness. However, the economic effects of financial contagion on other local governments remain unclear.

In this paper, we analyze how one municipal bankruptcy exposes other non-bankrupt local governments to the economic costs of financial contagion. For this, we make use of idiosyncratic bankruptcy filings to isolate their externalities from the state-wide economic trend. We find that a municipal bankruptcy lowers the probability of other local governments issuing debt in the year after the filing. We then investigate how the restricted credit market access after a bankruptcy affects other local governments' expenditures as well as private sector employment. To do so, we follow the idea of Almeida et al. (2012) and exploit ex-ante variation in the maturity of long-term debt across local governments. We document a decrease in expenditures after the filing occurs. This effect is mainly driven by a decrease in capital outlay expenditures. Further, we find evidence for a decrease in tradable employment.

Overall, our results indicate that bankruptcies can have persistent negative economic consequences for other local governments that rely on debt financing. This highlights the importance of functioning municipal credit markets on a state level since even temporary market disruptions may lead to cuts in local public infrastructure and service provision.

2

Private Activity Bonds as Investment Subsidy: Evidence from the 1986 Cap on Bond Volumes

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Abstract:

I examine firms' investment response to the supply of private activity bonds (PABs) – a subsidy tool granting corporate beneficiaries access to the tax-exempt municipal bond market. I leverage the variation in PAB supply limits across states introduced by the 1986 Tax Reform. By documenting a significant positive investment effect, I show that higher PAB supply stimulates firm investment. Although PABs subsidize capital over labor, my results do not support input factor substitution, as I find a positive effect on employment. I exploit the random outcome of a lottery-based PAB distribution mechanism to show that states' project selection does not drive the results.

2.1 INTRODUCTION

Historically, yields on tax-exempt municipal bonds have been about 20 percent lower than yields on comparable corporate bonds (Council of Economic Advisers, 2023).[1] State and local governments can issue tax-exempt private activity bonds (PABs) for the benefit of corporations to lower the capital costs for private sector projects that may contribute to local economic development. With total PAB issuance currently accounting for about one quarter of the entire tax-exempt market (Internal Revenue Service, 2019),[2] this tax-subsidy is often called into question from the political side (see, e.g., Pierog, 2017, for a recent debate). The main concern centers on potential federal tax revenue losses, questioning whether the subsidized investment projects would have been realized even without the provision of PAB funding (Congressional Budget Office, 2018). At the same time, current industrial policies emphasize the importance of mobilizing private sector investments (see, e.g., Boushey, 2023). Despite its potential to enhance the political discourse, the corporate response to PAB funding remains largely unexplored.

In this paper, I examine how corporate investment and employment respond to the supply of PAB funding.[3] PABs, which are only secured by the benefiting corporate entity, could stimulate firm investment by enabling projects that might not be realized, or at least not to the same extent, without the capital subsidy. The subsidy might have no investment effect if PABs are merely used to substitute for conventional financing (Mulcahy and Guszkowski, 1974; Zimmerman, 1989). Regarding the impact of PAB funding on employment, the subsidy to capital as an input factor could spark two contradictory effects (Moore and

[1] The figure is calculated based on the average yield difference between high-grade municipal bonds and Aaa-rated corporate bonds over the period from 1980 to 2010, using data reported in Council of Economic Advisers (2023), Table B–42, Bond yields and interest rates, 1952–2022.

[2] The reported PAB issuance share thereby includes projects for all types of nongovernmental beneficiaries such as non-profit organizations besides private businesses.

[3] PABs are also referred to as conduit bonds. I use the term private activity bonds throughout this paper. Further, taxable PABs exist. I only look at qualified PABs that are tax-exempt.

Squires, 1988): If PABs lead to an increase in firm investment and output in the first place, PABs could also induce a rise in firm employment, hence generating a "scale effect". However, PABs could also lead to a substitution effect of input factors and thus reduce employment.

Analyzing corporate responses to PAB supply is empirically challenging for three major reasons. First, PAB issuance may primarily focus on regions with favorable local investment opportunities. Second, even within regions characterized by similar investment opportunities, firms' demand for PAB funding is endogenous, so that firms may request PAB funding on the basis of various observable and unobservable factors. Third, states have discretionary power in the allocation of PABs, adding another dimension of selection complexity to the analysis of PAB funding.

To identify the effect of PAB supply on firm investment and employment, I exploit new state-level volume caps for PABs introduced as part of the 1986 Tax Reform Act. These limits to the supply of PABs were intended to combat the previous growth in PAB issuance (Livingston, 1989), as PABs accounted for more than half of total issuance in the tax-exempt bond market in 1982 (Zimmerman, 1990). The 1986 Tax Reform Act imposed a population-dependent limit to a state's aggregate PAB supply volume: From 1988 to 2000, each state was allowed to issue the greater of i) $150 million in baseline PAB volume and ii) $50 per cap multiplied by the state's population figure (H.R.3838 - Tax Reform Act of 1986, 1986). This PAB allocation formula generates plausibly exogenous variation in PAB supply on a *per cap* level: Because of the population-based kink in the formula, states with smaller population figures can supply relatively higher per capita volumes of PABs to firms, whereas larger states with a population of 3 million or more are restricted to the maximum of 50 USD PAB supply per cap. 23 states can provide more than 50 USD of PABs per cap.

I exploit the state-level variation in per cap PAB supply after the 1986 Tax

Reform in a difference-in-differences framework. For some of my analyses, I use a sample of state border counties. This allows me to control for common economic trends and investment opportunities in border regions before and after the 1986 Tax Reform, thereby mitigating concerns that these trends drive my results (see, e.g., Gustafson and Kotter, 2023). To account for the endogenous demand of firms for PAB funding, I analyze two samples of firms: First, I adapt the idea of Bonfim, Custódio and Raposo (2023) and compare firms that are eligible for PAB funding. Second, I use a sample of firms that receive PAB funding before and after the tax reform. In additional analyses, I leverage results from a lottery-based—therefore random—PAB distribution mechanism, implemented to allocate a state's volume cap among firms seeking PAB funding. This allows me to isolate firm responses to PAB supply from potential distortions due to states' project selection.

I start by showing that higher state-level caps for PABs lead to higher PAB supply and, consequently, to higher PAB allocation to firms after the 1986 Tax Reform Act. I do this in two steps. First, I focus on local governments as PAB issuers and examine changes to PAB issuance when different state-level per cap PAB supply limits apply. I show that a one standard deviation increase in the per cap PAB supply limit corresponds to a relative increase in PAB issuance at the county level by about 17.9%. Second, I document that firms that benefit from PAB funding before and after the reform receive considerably higher PAB allocations after the introduction of new volume caps if they are located in states with higher per cap PAB supply.

With the per cap PAB supply shock in hand, I turn to the corporate perspective and examine firms' investment response to PAB supply. I therefore leverage two firm samples. First, I compare PAB eligible firms headquartered in border counties of states that have different per cap PAB supply limits. Conducting the analyses at state borders allows for estimating differences in firms' invest-

ment responses while taking into account time-varying changes in border areas' economic conditions, particularly regarding local investment opportunities. The use of eligible firms helps to address the endogeneity inherent in firms' request for PAB funding, thus providing an intention-to-treat effect of the program (see also Bonfim, Custódio and Raposo, 2023). To define eligibility, I map the definition of PAB types by the Internal Revenue Code's (IRC) sections 141 and 146 to five broad beneficiary industry groups: manufacturing, transportation and utilities, mining and construction, real estate, and higher education.

I find a statistically significant and positive effect of PAB supply on the investment of eligible firms: Increasing the per cap PAB supply by 50 USD—equivalent to roughly one standard deviation—is associated with an increase of the capex-to-assets ratio by 10.5%. The higher PAB supply capability takes time to fully materialize in the investment response. The positive effect on firm investment starts in 1987, the year after the tax reform, and steadily increases until the end of the four-year horizon that I analyze.

Second, I analyze how firms with persistent demand for PAB funding respond to differences in PAB supply. To investigate the investment effect for recurring PAB program beneficiaries, I employ a sample of firms that received PAB funding both before and after the tax reform. My estimates show that PAB beneficiaries have an economically large investment response to PAB funding: An additional 25 USD in per cap PAB supply increases investment by 12.3%. Taken together, these results provide evidence that the supply of tax-subsidized private activity bonds stimulates firm investment.

While the documented positive investment response to PABs could lead to increased hiring of labor, it could also have a negative effect on employment if firms substitute labor with tax-subsidized capital. I test how PAB supply affects the employment of PAB eligible firms and PAB beneficiary firms. I find evidence for a scale effect of PAB supply on employment: Employment

significantly increases with PAB supply for both samples of firms. Even after controlling for common economic trends at state borders, an additional 50 USD in per cap PAB supply is associated with an increase in employment of PAB-eligible firms by about 4.9%. Together, these findings show that subsidizing capital relative to labor through PABs is not associated with an input factor substitution.

A potential threat to my identification is yet that PAB allocation committees in states with higher and lower per cap PAB supply systematically pick projects with better and worse investment prospects, respectively. To rule out that states' project selection is the main driver of my results, I utilize PAB program data from the State of Texas, which employs a lottery system to allocate its PAB volume among PAB funding applicants. The Texas PAB allocation program is also relevant due to its economic size, as the state had the second largest PAB volume among all states during the lottery period I analyze (Texas Bond Review Board, 1997).

I leverage a sample of PAB applicant firms for the program years 1996 to 2001, of which only a subset received PAB funding randomly by lot. I find a sizeable positive investment effect of receiving PAB funding through the lottery. This strongly supports my previous finding that PAB financing directly stimulates corporate investment. For firm employment, while all point estimates on receiving PAB funding are positive, they are statistically not significant. Hence, while not providing evidence for a scale effect of PAB supply, these estimates align with the earlier finding that PAB funding is not associated with a substitution of input factors.

My paper relates to several strands of the literature. First, I contribute to the growing literature stream on municipal finance and its real effects. Specifically, Adelino, Cunha and Ferreira (2017), Agrawal and Kim (2022), Amornsiripanitch (2022), and Dagostino (2022), among others, provide insights on the impact of

conventional municipal bond supply on the local economy. The real effects arise from adjustments to municipal expenditures in response to shocks on the municipal bond market. Different to these papers, I examine a change in the supply of private activity bonds, which directly expose the private sector to the market for tax-exempt financing. Rossi and Yun (2024) examine the use of conduit financing in the regulatory context of the introduction of Chapter 9 bankruptcy. My focus is on the role of PAB supply, and I provide novel evidence on its stimulating impact on private sector investment.

Second, I contribute to the literature that examines governmental incentive programs and subsidies for private sector investment and employment. Much work has been devoted to the assessment of place-based policies (see Kline and Moretti, 2014, for an overview). Juhász, Lane and Rodrik (2023) provide an overview on industrial policies, highlighting the more favorable assessment of these programs in more recent research that pays close attention to identification methods. In this regard, Criscuolo et al. (2019) conduct a micro-econometric assessment of an investment subsidy scheme for firms in the United Kingdom and find a comparably large effect of the assessed program on employment. Hyman et al. (2023) examine the effects of a hiring subsidy program in California and also document a relatively large effect on employment growth. By showing that the PAB program has a significant positive effect on beneficiaries, my findings align well with more contemporary assessments of different governmental incentives.

Third, my work relates to the broader literature on financing of corporate investment. In particular, prior literature studies credit supply related to conventional external financing and its impact on corporate investment (e.g., Alfaro, García-Santana and Moral-Benito, 2021; Lemmon and Roberts, 2010; Zarutskie, 2006). I provide novel evidence on the corporate reaction to the supply of tax-subsidized external financing in the form of private activity bonds.

Fourth, my findings contribute to the literature that studies the 1986 Tax Reform Act and its effects on firms. Auerbach and Slemrod (1997) provide a comprehensive overview of provisions that directly targeted firms. I provide insights on a corporate effect of the tax reform that works indirectly through novel provisions for the private activity bond market.[4]

Finally, my findings contribute to the policy debate on private activity bonds (e.g., Congressional Budget Office and Joint Committee on Taxation, 2009; Osterberg, 1991; Zimmerman, 1989, 1990). The debate on PABs focuses primarily on federal revenue losses and thus takes on a public sector perspective. I present novel insights on the micro-level by directly investigating real effects for firms.

The remainder of the paper is structured as follows. Section 2.2 provides background information on state-level PAB caps. Section 2.3 describes the data. Section 2.4 presents results for the effect of PAB supply on PAB issuance. Section 2.5 presents results for the effect of PAB supply on firm investment, and Section 2.6 for the effect on firm employment. In Section 2.7, I examine the corporate real effects of the Texas PAB lottery program. Section 2.8 concludes.

2.2 STATE-LEVEL CAPS FOR PRIVATE ACTIVITY BOND SUPPLY

2.2.1 INTRODUCTION OF PAB VOLUME CAPS

The 1984 Deficit Reduction Act introduced the first state-level caps for PABs to address the booming issuance of these bonds in the years beforehand (see, e.g., Driessen, 2022). These initial caps were set at the greater of i) $150 multiplied by the state population, and ii) a $200 million baseline volume. However, for instance as noted by The Bond Buyer (1984), these initial volume caps did not impose a major restriction for states regarding their PAB issuance capability.

[4]Zimmerman (1990) and Whitaker (2014) provide insights on how the private activity bond caps mandated by the tax reform affected private activity bond issuance. However, they do not provide insights on corporate real effects and, consequently, the potential benefits of the PAB program.

Likewise, Livingston (1989) indicates that the initial caps aimed to align PAB issuance with pre-year levels rather than to actively restrict it.

With the 1986 Tax Reform Act, new restrictive state limits for issuing tax-exempt PABs were introduced. In contrast to the caps imposed in 1984, these new caps were reported as a sharp reduction in the volume of PABs that states could issue (see, e.g., Kawecki, 2002). Taking California as an example, the newly enacted cap denoted a 50% reduction from the state's 1985 limit (Business Wire, 1986). Figure 2.1, Subfigure (a) provides an overview of the cap over time.

For the transition year 1987, each state was allowed to issue the greater of i) \$250 million in baseline PAB volume and ii) \$75 multiplied by the state's population. According to IRC Section 141, a bond issue is generally classified as a PAB if more than 10% of its proceeds are used by a nongovernmental entity, and this nongovernmental entity also directly or indirectly secures at least 10% of the bond's principal or interest.[5] From 1988 through 2000, the PAB cap remained constant, and each state could supply the greater of i) \$150 million in baseline PAB volume and ii) \$50 multiplied by the state's population figure.[6]

$$PAB\ volume_{s,1988} = max \begin{cases} \$150m \\ \$50 \cdot\ population_s \end{cases} \tag{2.1}$$

The two-part formula in the allocation schedule disproportionately favors smaller states (Livingston, 1989): States with lower population figures possess relatively larger per cap PAB volumes. I illustrate this property in Subfigure (b) of Figure 2.1. In particular, for the year 1988 onward, states with populations below 3 million had relatively higher per cap volumes, with values ranging up to about \$322 for the least populous state. On the contrary, states with a

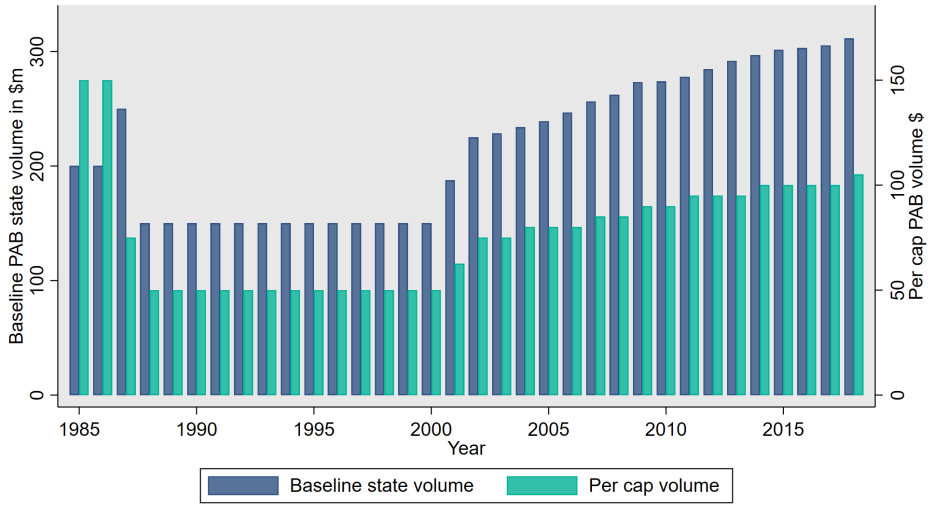
[5]These tests are referred to as private business use test, and private security or payment test. See IRC Section 141 for further details.

[6]See https://www.congress.gov/bill/99th-congress/house-bill/3838/text for details on the 1986 Tax Reform.

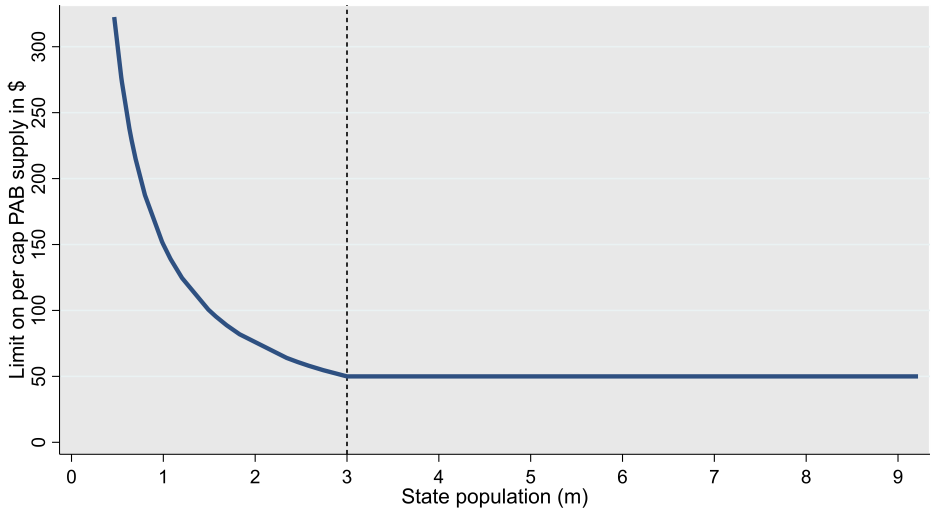
Figure 2.1
The 1986 Tax Reform Act and state-level PAB volume caps

This figure illustrates properties of the state-level cap on PABs, which is set as the higher value between a baseline PAB state volume, and a per cap volume multiplied by the state's population. Subfigure (a) shows the development of the baseline PAB state volume (■) as well as the allocation schedule's per cap volume (■) over the period from 1985 to 2018. Data comes from the Internal Revenue Code (IRC) Section 146 and the Internal Revenue Service's (IRS) statistics of income bulletin. Subfigure (b) illustrates the limit on per cap PAB supply for the period 1988 to 2000 depending on a state's population figure (—). Per cap PAB supply is calculated as a state's total PAB cap divided by the respective state's population figure. States with a population below 3 million can supply comparatively higher per cap PAB volumes.

(a) PAB volume caps over time



(b) State population and limit on per cap PAB supply for 1988 to 2000



population of 3 million and above could only distribute \$50 in PAB volume per person:[7]

$$\textit{Per cap PAB supply}_{s,1988} = \begin{cases} \$150m \div \textit{population}_s, \ \textit{if population}_s < 3m \\ \$50, \ \textit{if population}_s \geq 3m \end{cases} \tag{2.2}$$

2.2.2 CHARACTERISTICS OF PAB BENEFICIARY FIRMS

To provide a comprehensive background for the assessment of the PAB program, I compare firm characteristics of PAB beneficiary firms relative to their industry peers, i.e., relative to PAB eligible firms that do not receive a PAB allocation. Therefore, I use Compustat-matched data from SDC Platinum for the period from 1981 to 1990. 1981 is the first year of comprehensive PAB coverage in SDC, and 1990 marks the end of the sample period in later analyses. Manufacturing firms (about 49% of deals) and firms in the transportation and public utilities industry (38%) are the industry groups that receive the highest shares of PAB bonds matchable to Compustat firms.

In Figure 2.2, I display the mean values and 90% confidence intervals for the difference between PAB beneficiary firms and their industry peers. Focusing on the simple difference in means shown in Subfigure (a), PAB recipients are significantly larger as measured by the logarithm of employment.[8] I also find that PAB beneficiaries are more profitable as measured by a higher return on assets. While PABs themselves constitute a source of debt financing, PAB beneficiary firms are also generally more leveraged. The group of PAB beneficiary firms

[7] I focus on the per cap supply denoted by the more restrictive 1988 allocation schedule, which is also binding for the majority of the post-reform period of interest and even thereafter. In my analyses, I consider the 1987 PAB limit in robustness tests, which leads to similar results.

[8] PAB recipients are also larger in terms of total assets, which for displayability reasons is not captured in Figure 2.2.

shows higher firm investment as measured by the natural logarithm of capex divided by assets. However, PAB beneficiaries have lower average PPE growth rates and relatively lower market-to-book values than their industry peers. Finally, PAB beneficiary firms have significantly higher payout ratios than their peers.

As the differences in firm characteristics displayed in Subfigure (a) may to some extent be driven by the size difference between PAB beneficiaries and non-beneficiaries, I additionally show differences in means after controlling for firm-size quintile-by-industry fixed effects. Results are presented in Subfigure (b). Even after controlling for these differences, PAB beneficiaries are characterized by larger employment, higher profitability, and higher investment than their industry peers.

In line with this, Giloth (1991) finds that PAB beneficiaries are larger than non-recipients within a sample of PAB beneficiaries in Chicago. The Michigan Economic Development Corporation (2021) specifically mentions that its PAB program focuses on profitable firms. Several features of the PAB program could cause the selection effect. In this regard, two noteworthy provisions include (i) a restriction on the intended use of tax-exempt PABs, and (ii) a limit on PAB issuance costs. IRC Section 147(c) only allows for a maximum of 25% of the bond proceeds to be spent on the acquisition of land.[9] Besides, PABs usually cannot be used to finance working capital or inventory (see, e.g., Mississippi Business Finance Corporation, 2006; Utah Department of Workforce Services Housing & Community Development Division, 2023). Consequently, firms need to use other funds for these purposes. Further, as denoted by IRC Section 147, PABs may use only two percent of the bond proceeds for issuance costs.

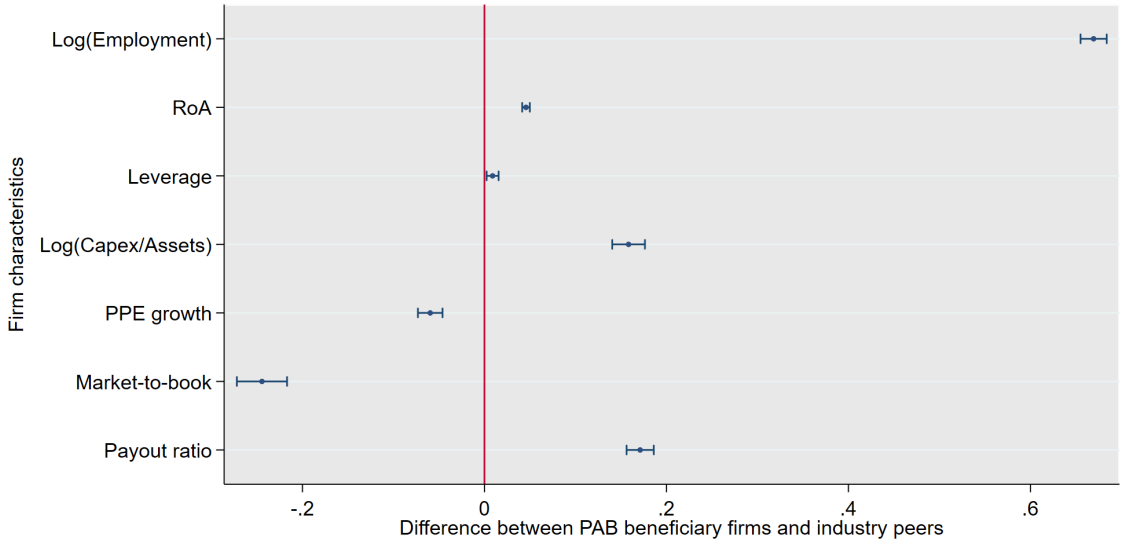
Overall, the PAB program seems to favor firms that are relatively larger, more profitable, and that display higher investment.

[9]The restriction on land acquisition expenditures might also be a reason why PABs are primarily used to expand existing facilities (Thompson, 1968).

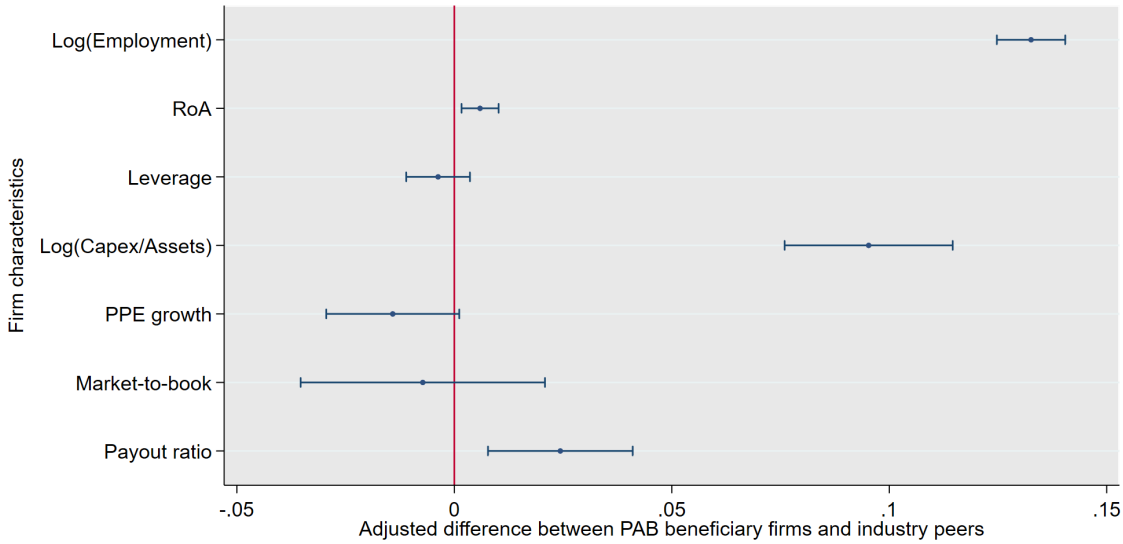
Figure 2.2
Characteristics of PAB beneficiary firms

This figure illustrates the difference in means of firm characteristics for firms that receive any PAB allocation between 1981 and 1990 compared to their industry peers without PAB allocation, according to data from SDC Platinum. The bars mark the 90% confidence intervals. The comparison period is from 1981 to 1990. Subfigure (a) shows the simple difference in means. In Subfigure (b), the adjusted difference controls for firm-size quintile-by-industry fixed effects when calculating the mean difference. Appendix B.1 provides a detailed description of all variables.

(a) Difference between PAB beneficiaries and industry peers



(b) Size group-by-industry adjusted difference between PAB beneficiaries and non-beneficiaries



2.3 DATA AND DESCRIPTIVE STATISTICS

2.3.1 DATA SOURCES

To calculate the per cap PAB supply limit for each state, I collect information on the annual allocation schedule from IRC Section 146 and the Internal Revenue Service's (IRS) statistics of income bulletin. State and county population data are obtained from the U.S. Bureau of the Census Population and Housing Unit Estimates series.

Company financial data and headquarter locations come from the Compustat database. All continuous financial variables are winsorized at the 1st and 99th percentiles. Firms in state border counties are identified using the U.S. Bureau of the Census' County Adjacency File.

Based on the historic SIC code in Compustat, I map firms to broad PAB eligibility categories. I use the 1987 historic SIC code if available, and current SIC codes otherwise. The types of bonds that qualify for tax-exempt issuance and are subject to the PAB volume cap are determined by IRC Sections 141 and 146. As noted by Whitaker (2011), this limits the use of PABs to projects from five categories: "industrial development, utilities, mortgage revenue bonds, multifamily housing bonds, and student loan bonds". I map these use cases to SIC industry groups. I then review the SIC codes of actual PAB beneficiaries and compare them to the industry-based eligibility definition.[10] The following industries are defined as eligible for PAB financing: manufacturing (SIC2 20–39), transportation and utilities (SIC2 40–49), mining and construction (SIC2 10–17), real estate (SIC2 65), and higher education industry (SIC2 82).[11]

To construct a sample of PAB issuing counties, I use local government PAB issuance data from the U.S. Bureau of the Census's Annual Survey of State

[10]PAB beneficiaries are identified by hand-matching PAB deal data from SDC Platinum to Compustat firms, which allows me to observe the (historic) beneficiary SIC codes.

[11]The vast majority of Compustat-linked PAB deals in SDC are issued for the benefit of firms in the manufacturing as well as the transportation and utilities industry group.

and Local Government Finances. I aggregate this data on the county level to identify counties with any PAB issuance in the 10-year period prior to the 1986 Tax Reform. I also employ this PAB issuance data for county-level tests on PAB issuance trends.

I identify PAB beneficiary firms using data from the Municipal New Issues Database in SDC Platinum.[12] The deal data is structured around the conduit issuers. I hand-match PAB beneficiary names to Compustat using (historic) company names. I only use new money PAB deals, as current refunding deals are not subject to the PAB caps (see IRC Section 146).

Data on the Texas PAB lottery comes from the Texas Bond Review Board (BRB). The BRB annually provides information on PAB requests through the lottery, as well as on lottery outcomes. Data from 2001 onward is available from the BRB's website. Data from 1996 to 2000 is directly obtained from the BRB. I keep two types of lottery attempts: i) those that have been allocated an amount, i.e., lottery winning attempts, and ii) those that are in line for funding, i.e., unsuccessful lottery losing attempts. I hand-match beneficiary names of projects to Compustat and aggregate lottery attempts on the firm-lottery program year level. Appendix B provides further information on the Texas PAB lottery.

2.3.2 DESCRIPTIVE STATISTICS

I use three samples of firms for the analyses. The sample of PAB eligible firms headquartered in border counties covers 682 firms in 38 states, of which 126 firms are located in 14 states with a PAB cap larger than 50 USD. The sample of PAB beneficiary firms comprises 140 firms, of which 14 are located in a state that can supply more than 50 USD in PABs per cap. Finally, the lottery sample consists of 29 firm lottery attempts. 16 applicants receive some PAB allocation,

[12] 1977 is the first year for which a limited set of PAB deals is available. More comprehensive coverage begins in 1981. After the 1986 Tax Reform, Compustat firms account for about 25% of the PAB deals in SDC, but they account for an aggregate bond volume share of about 50%. See Appendix Figure B.1 for an overview of data coverage.

i.e., *Lottery win dummy* takes a value equal to one, and 7 applicants lose all lottery attempts. Table 2.1 shows summary statistics for PAB issuance, as well as for the three firm-level samples used in the empirical analyses. Appendix B.1 provides a detailed description of all variables.

Table 2.1
Descriptive statistics

This table presents descriptive statistics for PAB issuance and the three firm samples employed in the analyses. Panel A shows descriptive statistics for PAB issuance at the county level and PAB allocation at the firm level. Panel B shows descriptive statistics for the sample of PAB eligible firms in border counties and consists of 682 firms located across 38 states. For Panel C, the sample comprises 140 firms in 34 states that receive any PAB allocation in the four years after the 1986 Tax Reform. Panel D shows pre-lottery-year descriptives for 29 lottery attempts between 1996 and 2001. Appendix B.1 provides a detailed description of all variables.

	N	Mean	SD	10^{th}	50^{th}	90^{th}
Panel A: PAB issuance						
PAB issuance $volume_{county}$ ($m)	7,150	11.02	48.53	0.00	0.00	20.00
Log (PAB issuance $volume_{county}$)	7,150	2.88	4.28	0.00	0.00	9.90
Per cap PAB supply	7,150	61.38	39.81	50.00	50.00	64.03
Firm-level PAB volume ($m)	363	56.85	82.88	3.30	25.00	141.00
Log (Firm-level PAB volume)	363	3.24	1.32	1.46	3.26	4.96
Panel B: PAB eligible firms in border counties						
Per cap PAB supply	4,097	66.68	46.48	50.00	50.00	139.53
Per cap PAB $supply_{1987}$	4,097	105.47	81.41	75.00	75.00	244.29
Capex/Assets	4,097	0.09	0.11	0.02	0.06	0.18
Log (Capex/Assets)	4,097	-2.86	1.04	-4.17	-2.76	-1.72
Employment (k)	3,948	6.16	14.23	0.05	1.06	16.05
Log (Employment)	3,946	1.10	1.12	0.05	0.72	2.84
Lag of Size	4,097	4.48	2.35	1.35	4.51	7.59
Lag of RoA	4,079	0.00	0.23	-0.12	0.05	0.12
Panel C: PAB beneficiary firms post-1986						
Per cap PAB supply	1,022	55.75	24.02	50.00	50.00	50.00
Capex/Assets	1,022	0.09	0.06	0.03	0.08	0.15
Log (Capex/Assets)	1,022	-2.59	0.65	-3.40	-2.54	-1.88
Employment (k)	993	18.39	49.65	0.46	3.90	40.20
Log (Employment)	993	1.86	1.27	0.38	1.59	3.72
Lag of Size	1,022	6.55	2.09	3.81	6.76	9.12
Lag of RoA	1,022	0.05	0.05	0.01	0.05	0.10
Panel D: Firms in Texas PAB lottery						
Lottery win dummy	29	0.55	0.51	0.00	1.00	1.00
Lottery-allocated bond volume ($m)	29	10.96	11.48	0.00	7.50	25.00
$Capex/Assets_{Pre\text{-}lottery\ year}$	29	0.08	0.06	0.02	0.07	0.12
$Log\ (Capex/Assets)_{Pre\text{-}lottery\ year}$	29	-2.75	0.65	-3.77	-2.68	-2.25
$Employment\ (k)_{Pre\text{-}lottery\ year}$	28	25.40	25.78	1.40	23.69	59.57
$Log\ (Employment)_{Pre\text{-}lottery\ year}$	28	2.73	1.20	0.88	3.21	4.10
$Size_{Pre\text{-}lottery\ year}$	29	8.71	1.73	6.40	9.16	10.53
$RoA_{Pre\text{-}lottery\ year}$	29	0.07	0.05	0.00	0.07	0.12

For firms that receive PAB funding in a given program year, the average volume allocated is USD 56.85m (median: USD 25m).[13] The average (median) capex-to-assets ratio is 9% (6%) for PAB eligible firms, and 9% (8%) for PAB beneficiaries.

Table 2.2
Pre-tax reform and pre-lottery characteristics for firms in treatment and control groups

This table presents the mean pre-period characteristics as well as differences in the mean values for firms in the treatment and control groups as indicated in the respective Panel. For Panel A and B, the pre-tax reform period is from 1983 to 1986. For Panel C, the mean values correspond to the pre-lottery year. Difference adjusted controls for industry-by-year fixed effects (Panel A) or industry-by-post$_{1986}$ dummy fixed effects (Panel B) when calculating the mean difference. For the difference calculation in Panels A and B, standard errors are clustered at the state level. ***, ** and * indicate significance at the 1%-, 5%- and 10%-levels, respectively. Appendix B.1 provides a detailed description of all variables.

	N	Mean	N	Mean	Diff.	Diff. adj.
Panel A: PAB eligible firms	**Per cap PAB > 50**		**Per cap PAB = 50**			
Log (Capex/Assets)	379	-2.65	1,625	-2.81	0.16**	0.01
Log (Employment)	358	1.11	1,570	1.12	-0.01	-0.00
Lag of Size	379	4.65	1,625	4.31	0.34	-0.02
Lag of RoA	377	-0.01	1,623	0.01	-0.02	-0.02
Lag of Leverage	379	0.40	1,624	0.37	0.03	-0.02
Lag of PPE growth	349	0.15	1,507	0.22	-0.07	-0.04
County population (k)	379	386.04	1,625	961.13	-575.09**	n/a
Panel B: PAB beneficiaries	**Per cap PAB > 50**		**Per cap PAB = 50**			
Log (Capex/Assets)	44	-2.65	437	-2.54	-0.11	-0.18
Log (Employment)	44	1.46	422	1.93	-0.47	-0.64*
Lag of Size	44	6.38	437	6.53	-0.16	-0.76
Lag of RoA	44	0.05	437	0.05	-0.00	0.00
Lag of Leverage	44	0.48	437	0.41	0.07	0.06
Lag of PPE growth	41	0.12	429	0.11	0.01	0.02
Panel C: Texas PAB lottery	**Lottery winning firm**		**Lottery losing firm**			
Log (Capex/Assets)	16	-2.8	13	-2.6	-0.21	n/a
Log (Employment)	15	2.76	13	2.7	0.06	n/a
Size	16	8.74	13	8.68	0.06	n/a
RoA	16	0.07	13	0.06	0.00	n/a
Leverage	16	0.41	13	0.51	-0.10**	n/a
PPE growth	16	0.08	13	0.02	0.06	n/a

Table 2.2 presents pre-tax reform and pre-lottery summary statistics for firms in the respective treatment and control groups. The pre-tax reform period covered in Panel A and B is from 1983 to 1986. Both PAB eligible and PAB

[13] As noted by the U.S. Bureau of the Census (2006), the item public debt for private purposes in the Census has historically been subject to some reporting difficulties. Therefore, the displayed volumes for county-level issuance might rather represent a lower bound of the actual volumes.

beneficiary firms have broadly similar financial characteristics in states with higher and lower per cap PAB supply. One notable exception is the logarithm of capex divided by assets in the sample of eligible firms. I additionally calculate an adjusted difference in means that controls for industry-by-year fixed effects (Panel A), or industry-by-post-1986 dummy fixed effects (Panel B). The adjusted mean difference for eligible firms' logarithm of capex divided by assets is close to zero, and statistically not significant.

Panel A further shows that PAB eligible firms in states with higher PAB supply are on average located in counties that are less populated. If investment opportunities are positively correlated with county size and thus would be better in states with low PAB supply, the distribution of county population should rather bias me against finding an effect.

2.4 PAB VOLUME CAPS AND THEIR IMPACT ON PAB ISSUANCE

I first examine the effect of the state-level per cap PAB supply limits on PAB issuance after the 1986 Tax Reform. Although my primary focus is on the corporate real effects of the PAB volume caps, it is critical to establish that the corresponding per cap PAB supply limits are a constraining factor. The empirical prediction is that states with higher per cap PAB supply limits should be less affected by the new volume caps and thus should show comparably higher PAB issuance after the tax reform.

I examine the effect of PAB volume caps on aggregate county-level PAB issuance, as well as on PAB volumes allocated to firms. My treatment measure *Per cap PAB supply* is the per person PAB amount in USD that a state can supply according to the 1988 allocation schedule. Because of the kink in the per cap PAB allocation schedule (illustrated in Figure 2.1, Subfigure b), the per cap PAB supply is larger than 50 USD for states with a population of less than 3 million, and is limited to 50 USD for states with a population of 3 million and more.

I run difference-in-differences regressions at the county and the firm level. I analyze a window of plus/minus four years around the introduction of new PAB volume caps.[14] At the county level, the regression specification is as follows:

$$\begin{aligned} \textit{PAB issuance volume}_{c,t} = \alpha + \beta \textit{Post } 1986_t \cdot \textit{Per cap PAB supply}_s \\ + \gamma \textit{Post } 1986_t + \delta \textit{Per cap PAB supply}_s + \varphi_c + \xi_t + \varepsilon_{i,t} \end{aligned} \tag{2.3}$$

whereby c denotes a county, t a fiscal year, and s a state. φ_c are county fixed effects, and ξ_t are year fixed effects. *Post* 1986 equals one in the four years after the introduction of new PAB volume caps, i.e., for the years 1987 to 1990. The sample period is from 1983 to 1990.

The variable of interest is the coefficient on the interaction term between the Post-dummy and the per cap PAB supply limit. I include the county-level house price index as a control variable in all regressions.[15] I add county size-decile-by-year fixed effects in some specifications. For the sample of counties at state borders, I include state border pair-by-year fixed effects. Standard errors are clustered at the state level. In the empirical estimation, the variables *Post* 1986_t and *Per cap PAB supply*$_s$ are absorbed by year and county fixed effects, respectively. For the firm-level regressions, I conduct the analyses within beneficiary firm instead of within county. I control for the lag of firm size and the lag of return on assets in these regressions.

Table 2.3 presents the results for the effect of per cap PAB supply on actual PAB issuance. The main finding is that higher limits to the per cap PAB supply on the state level lead to higher issuance at the county level, as well as to higher PAB volumes for firms. These findings are consistent with my prediction that the per cap supply limits for PABs effectively restrict beneficiaries' access to the

[14] The sample period begins in 1983 to ensure that it predates the introduction of any PAB volume cap. See Section 2.2.1 for details.

[15] Data are obtained from the Federal Housing Finance Agency, and imputed with state-level values if missing.

tax-exempt bond market.

Table 2.3
Per cap PAB supply limits and PAB issuance after the 1986 Tax Reform

In Panel A, the dependent variable is the natural logarithm of the PAB issuance volume at the county level, and the sample consists of counties in which any local government issued at least one PAB in the ten years before the 1986 Tax Reform. In Panel B, the dependent variable is the natural logarithm of PAB volume that a firm received in a given year. Firm controls include lag of size and lag of return on assets. Per cap PAB supply is the limit to the per capita amount of private activity bonds in USD that a state may distribute for calendar years 1988 onward. Pre and Post beneficiaries are firms that receive a PAB allocation in both the four year tax reform pre-period and the four year post-period. Post is a dummy that equals one from 1987 to 1990, and zero otherwise. The sample period is from 1983 to 1990. T-statistics based on Huber/White robust standard errors clustered by state are presented in parentheses. ***, ** and * indicate significance at the 1%-, 5%- and 10%-levels, respectively. Appendix B.1 provides a detailed description of all variables.

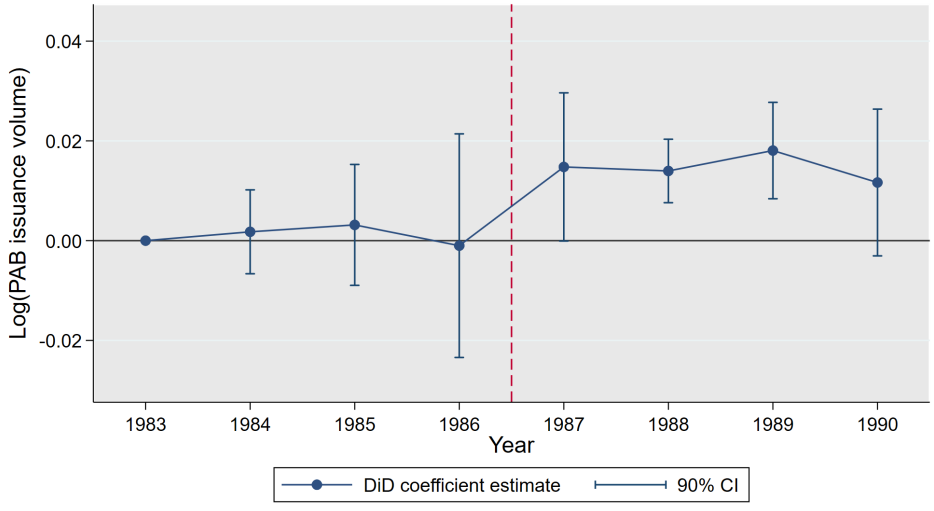
	(1)	(2)	(3)	(4)
Panel A: County-level PAB issuance volume				
	Log(PAB issuance volume) for			
	PAB issuing counties		PAB issuing border counties	
Per cap PAB supply x Post-1986	0.0048*	0.0045*	0.0120***	0.0137***
	(1.811)	(1.895)	(3.039)	(3.534)
House price index control	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes
Year FE	Yes	No	Yes	No
County size decile x Year FE	No	Yes	No	Yes
State border pair x Year FE	No	No	Yes	Yes
Number of observations	7,150	7,142	2,295	2,295
Adjusted R^2	0.376	0.376	0.375	0.373
Panel B: Firm-level PAB allocation volume				
	Log(Firm-level PAB volume) for			
	All beneficiaries		Pre and Post_{1986} beneficiaries	
Per cap PAB supply x Post-1986	0.0092***	0.0095**	0.0090***	0.0097***
	(3.554)	(2.680)	(3.227)	(2.908)
Firm controls	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Industry x Post_{1986} FE	No	Yes	No	Yes
Number of observations	363	357	218	207
Adjusted R^2	0.617	0.580	0.582	0.520

Panel A presents results for the county level. The dependent variable is the natural logarithm of the county-level sum of PAB issuance across all local governments in the county. In columns (1) and (2), the sample consists of

counties that issued any PAB in the ten years before the tax reform. The coefficient on the interaction term *Per cap PAB supply x Post 1986* is positive and statistically significant, indicating that a one standard deviation increase in the per cap PAB supply (39.81 USD) is associated with a relative increase in PAB issuance at the county-level of about 19.1% (column 1) or 17.9% (column 2), respectively. In columns (3) and (4), I limit the sample to border counties and include state border pair-year fixed effects in the regressions. The point estimates for the effect of per cap PAB supply on PAB issuance are larger in magnitude for this sample.

Figure 2.3
Per cap PAB supply and PAB issuance after the 1986 Tax Reform

This figure illustrates PAB issuance trends around the 1986 Tax Reform Act. The dependent variable is the natural logarithm of the aggregate PAB issuance volume at the county level. The figure shows the coefficient estimates and 90% confidence interval on the Per cap PAB supply measure interacted with year dummies. Per cap PAB supply is the limit to the per capita amount of private activity bonds in USD that a state may distribute for calendar years 1988 onward. The regression specification corresponds to column (4) in Table 2.3, top panel. The effective date for the PAB volume caps mandated by the 1986 Tax Reform Act is shown in red (- -). Appendix B.1 provides a detailed description of all variables.



I illustrate the time dynamics of the effect of per cap PAB supply on county level PAB issuance in Figure 2.3. The figure shows the coefficient estimates and the 90% confidence interval on the Per cap PAB supply measure interacted with

year dummies over the event period. The regression specification follows the one in Table 2.3, Panel A, column (4). Until 1986, I do not observe statistically significant differences in the development of PAB issuance for counties depending on the respective per cap PAB supply limit. Beginning with 1987, a relatively higher per cap PAB supply has a positive effect on county level PAB issuance. The effect reaches its maximum in 1989 and slightly declines thereafter.

In Panel B of Table 2.3, I present results for the effect of per cap PAB supply on the natural logarithm of firm-level PAB volume. Across all specifications, higher per cap PAB supply has a positive and statistically significant effect on PAB volume after the tax reform. Focusing on the effect within beneficiaries (column 4), i.e., at the intensive margin, a one standard deviation increase in per cap PAB supply, equivalent to 24.02 USD for the beneficiary sample, is associated with a relative increase in the allocated PAB volume by 23.3%. This corresponds to an additional USD 13.2m for the average PAB allocation.[16] In Appendix Figure B.2, I additionally show that the relative increase in firm PAB volumes begins after the tax reform.

2.5 PRIVATE ACTIVITY BOND SUPPLY AND FIRM INVESTMENT

2.5.1 PAB SUPPLY AND INVESTMENT OF PAB ELIGIBLE FIRMS

I now examine the effect of PAB supply on firm investment, given the demonstrated positive effect of higher per cap PAB limits on PAB supply after the 1986 Tax Reform. To investigate the investment response of firms, I first analyze firms eligible for PAB financing. These analyses yield an intention-to-treat effect of PAB supply, addressing that the demand for PAB funding is endogenous.

The sample for these analyses consists of PAB eligible firms located in border counties, which allows me to additionally control for the overall local economic

[16] As all analyses on the firm-level are conducted within firm, looking at all beneficiaries in columns (1) and (2) instead of at pre- and post-beneficiaries leads to very similar results.

trend as well as the development of investment opportunities in these regions. Furthermore, broadly following the approach of Adelino, Cunha and Ferreira (2017), my sample comprises firms located in counties with any PAB issuance in the ten years before the 1986 Tax Reform.

The dependent variable, firm investment, is calculated as the natural logarithm of capital expenditures divided by the beginning of period total assets. I run the following difference-in-differences regression at state borders:

$$\begin{aligned} Investment_{i,t} = \alpha + \beta Post\ 1986_t \cdot Per\ cap\ PAB\ supply_s + \gamma Post\ 1986_t \\ + \delta Per\ cap\ PAB\ supply_s + \varphi_i + \xi_t + \chi_{b,p} + \varepsilon_{i,t} \end{aligned} \tag{2.4}$$

whereby i denotes a firm eligible for PAB financing, t a fiscal year, and s a state. φ_i are firm fixed effects, ξ_t are year fixed effects, and $\chi_{b,p}$ are border region-by-post-dummy fixed effects. *Per cap PAB supply* is the limit to the per capita amount of private activity bonds in USD that a state may distribute per calendar year, according to the allocation schedule valid for 1988 to 2000. The sample period is from 1983 to 1990. The variable of interest is the coefficient on the interaction term between the post-dummy and Per cap PAB supply. As before, standard errors are clustered at the state level.

As all specifications include firm fixed effects, the estimates correspond to within-firm changes in firm investment for firms located in states with higher per cap PAB supply relative to firms in states with lower supply. I include lagged firm size measured as the natural logarithm of total assets as a control variable in all regressions. I also use the lag of return on assets as additional control. Finally, to capture potentially different investment trends of industries over time, I include industry-by-year fixed effects in further analyses.

2.5.1.1 BASELINE RESULTS

Table 2.4 presents the results for the effect of PAB supply on investment of PAB eligible firms. The main finding is that a higher per cap supply of PABs positively affects firm investment.

Table 2.4
Per cap PAB supply and firm investment after the 1986 Tax Reform

The sample consists of PAB eligible firms located in border counties in which any local government issued at least one PAB in the ten years before the 1986 Tax Reform. The dependent variable is the natural logarithm of capital expenditures divided by the beginning of period total assets. Per cap PAB supply is the limit to the per capita amount of private activity bonds in USD that a state may distribute for calendar years 1988 onward. Post is a dummy that equals one from 1987 to 1990, and zero otherwise. The sample period is from 1983 to 1990. T-statistics based on Huber/White robust standard errors clustered by state are presented in parentheses. ***, ** and * indicate significance at the 1%-, 5%- and 10%-levels, respectively. Appendix B.1 provides a detailed description of all variables.

	(1)	(2)	(3)
	Log(Capex/Assets)		
Per cap PAB supply x Post-1986	0.0018***	0.0017***	0.0021***
	(5.089)	(4.977)	(3.359)
Lag of Size	-0.4144***	-0.4788***	-0.4808***
	(-6.299)	(-7.621)	(-9.973)
Lag of RoA		1.1047***	1.0546***
		(7.294)	(7.291)
Firm FE	Yes	Yes	Yes
Year FE	Yes	Yes	No
State border pair x $Post_{1986}$ FE	Yes	Yes	Yes
Industry x Year FE	No	No	Yes
Number of observations	4,094	4,073	4,059
Adjusted R^2	0.488	0.507	0.525

The point estimate for the interaction term *Per cap PAB supply x Post 1986* ranges from 0.0018 to 0.0021 and is statistically significant in all specifications. This indicates that after the 1986 Tax Reform, a one standard deviation increase in per cap PAB supply, equivalent to 46.48 USD, leads to an increase in the capex-to-assets ratio of eligible firms of about 8.37% to 9.76%. This corresponds to a relative increase in the capex-to-assets ratio of 0.75 to 0.88 percentage points for the average PAB eligible firm in the sample. This intention-to-treat effect of PAB supply may capture both direct effects for firms that receive PAB funding,

and any potential spillover effects.[17]

The time dynamics of the effect of per cap PAB supply on firm investment are illustrated in Figure 2.4. Subfigure (a) shows the development of average investment for firms in states with the baseline PAB supply of 50 USD per cap, as well as for firms located in states with higher per cap PAB supply. Until 1986, both firm groups display a similar investment trend. In 1987, firms in states with a per cap PAB supply of more than 50 USD show a slight increase in firm investment, while firms in states limited to the baseline 50 USD per cap PAB supply continue the downward investment trend. In the mean plot, the investment gap increases until 1988 and remains constant thereafter.

Subfigure (b) shows the coefficient estimates and the 90% confidence interval on the Per cap PAB supply measure interacted with year dummies over the event period. The regression specification follows equation 2.4, and includes the full set of control variables as in Table 2.4, column (3). I do not observe statistically significant differences in the development of firm investment for firms located in states with higher and lower per cap PAB supply until 1986. The positive effect of higher per cap PAB supply on firm investments starts in 1987, the first year after the 1986 Tax Reform, and continues to rise in magnitude over the remainder of the post-period. However, the point estimates are statistically significant only for 1989 and 1990.

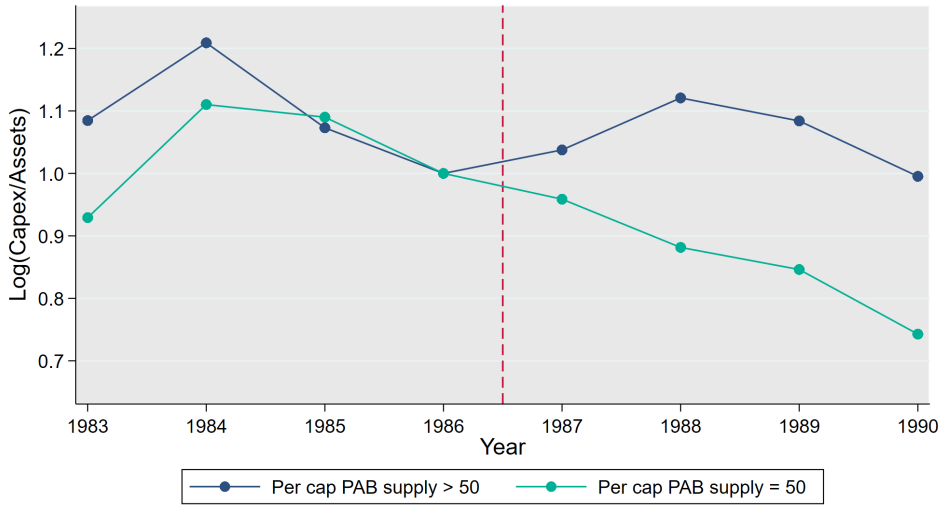
Overall, my results are consistent with a stimulating effect of PAB supply on firm investment.

[17]Spillover effects could be both positive or negative: On the one hand, a direct stimulating effect of PABs could spur investment of local non-recipients (see, e.g., Dougal, Parsons and Titman, 2015). On the other hand, PABs might give beneficiaries a competitive advantage over other firms (Moore and Squires, 1988), which could dampen non-beneficiaries investment behavior.

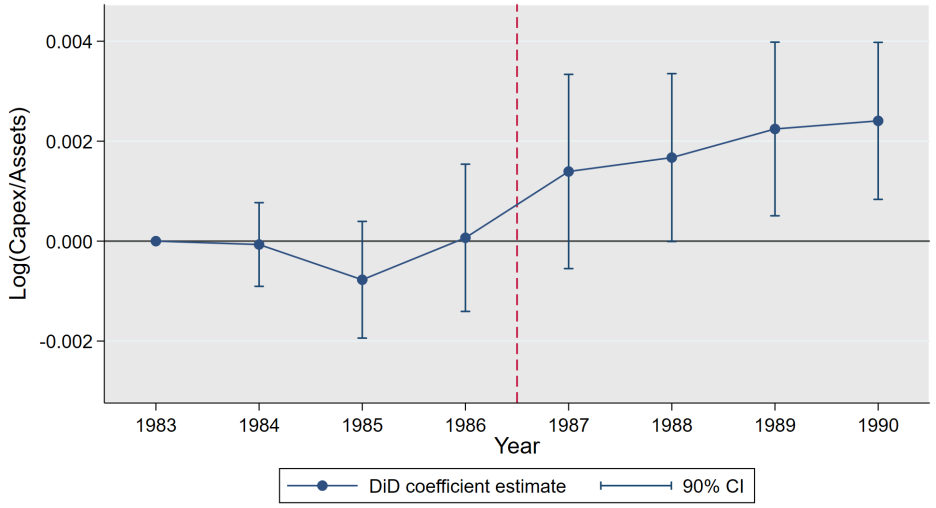
Figure 2.4
Per cap PAB supply and firm investment after the 1986 Tax Reform

This figure illustrates the development of firm investment for PAB eligible firms located in border counties around the 1986 Tax Reform Act. Per cap PAB supply is the limit to the per capita amount of private activity bonds in USD that a state may distribute for calendar years 1988 onward. Subfigure (a) shows the development of average firm investment in states with more than 50 USD per cap PAB supply (●) compared to firm investment in states restricted to 50 USD per cap PAB supply (●) over the period from 1983 to 1990. The effective date for the PAB volume caps mandated by the 1986 Tax Reform Act is shown in red (- -). For both firm groups, the level of firm investment is set to one in the fiscal year 1986. Firm investment is defined as the natural logarithm of capital expenditures divided by the beginning of period total assets. Subfigure (b) shows the coefficient estimates and the 90% confidence interval on the Per cap PAB supply measure interacted with year dummies over the event period. The regression specification follows column (3) in Table 2.4. Appendix B.1 provides a detailed description of all variables.

(a) Mean values of firm investment



(b) Difference in firm investment



2.5.1.2 PLACEBO AND ROBUSTNESS TESTS

Appendix Figure B.3 presents results for a placebo test that uses the PAB volume caps set by the 1984 Deficit Reduction Act to calculate states' per cap PAB supply. These initial caps did not restrict the supply of PABs, but merely aimed to maintain it at the level of previous years (Livingston, 1989; The Bond Buyer, 1984).[18] I adjust the specification as described by equation 2.4 and analyze the effect of the 1985 placebo per cap PAB supply limit on firm investment over the period 1981 to 1988. As shown in Appendix Figure B.3, a higher per cap PAB supply limit implied by the 1984 Deficit Reduction Act has no differential effect on firm investment overall. In 1985, as the only exception, higher PAB supply limits even have a negative effect on firm investment.

I conduct a series of robustness tests to support my baseline finding that higher per cap PAB supply positively affects firm investment. The results are presented in Appendix Table B.2 and Appendix Table B.3. In the first test, I show that the investment effect is robust to using the 1987 transition year per cap PAB supply limit as the treatment measure. Compared to the per cap PAB supply limit in place from 1988 to 2000, the 1987 per cap PAB limit is higher for all states, but especially so for the least populous states.[19] In terms of magnitude, a one standard deviation increase in per cap PAB supply according to the 1987 formula—equivalent to 81.41 USD—is associated with an increase in the capex-to-assets ratio of about 9.77% for the most restrictive specification. This effect is similar to my baseline estimate.

Second, I show that the effect of PAB supply on firm investment is independent

[18]Section 2.2.1 provides further information on the volume caps associated with the 1984 Deficit Reduction Act.

[19]The relative decrease between the 1987 and the 1988 per cap supply is not the same for all states for two reasons: (i) states with populations less than 3.3 million have higher per cap PAB limits in 1987, while states with less than 3 million do so according to the 1988 formula, and (ii) because of (minor) changes in state populations. The relative reduction in PAB supply for 1988 compared to 1987 ranges from about 33% to about 43%, and is most pronounced for the least populous states.

of the definition of the sample of PAB issuing counties.[20] As an additional placebo test, I analyze firms located in counties with no PAB issuance in the ten years prior to the tax reform. Column (3) of Appendix Table B.2, bottom panel, presents the results. If the availability of PAB funding positively affects investment, I should find no effect of per cap PAB supply on firm investment for these non-issuing counties. Consistent with this, the point estimate is statistically not significant.

Finally, I use three alternative measures for firm investment, as several common measures exist (see Bai et al., 2022, for an overview): the natural logarithm of capital expenditures (Appendix Table B.3, column 1), capex divided by the beginning of period total assets (column 2), and the growth rate for net property, plant, and equipment (column 3). Consistent with my baseline results, I find a positive effect of per cap PAB supply on firm investment when using these alternative investment measures, which is statistically significant for the logarithm of capex and the capex-to-assets ratio, but not for PPE growth.

2.5.2 PAB SUPPLY AND INVESTMENT OF PAB BENEFICIARY FIRMS

How do firms that receive PAB funding respond to the supply of PABs? To examine the investment effect for PAB beneficiaries, I focus on a sample of firms that receive PAB funding both before and after the tax reform, i.e., on firms with a likely persistent demand for PABs.

I define post-reform PAB beneficiaries as firms that receive any PAB allocation in the four years after the tax reform, and consider a narrower definition in robustness tests. Pre- and post-reform beneficiaries are firms that receive any PAB allocation in the four years after the reform, and any allocation in the four-year window before.

[20] My baseline specification considers firms that are located in counties with any PAB issuance in the ten years before the 1986 Tax Reform Act.

I run the following difference-in-differences regression:

$$\begin{aligned} Investment_{i,t} = \alpha + \beta Post\ 1986_t \cdot Per\ cap\ PAB\ supply_s + \gamma Post\ 1986_t \\ + \delta Per\ cap\ PAB\ supply_s + \varphi_i + \xi_{t,a} + \varepsilon_{i,t} \end{aligned} \tag{2.5}$$

whereby i denotes a PAB beneficiary firm, t a fiscal year, and s a state. φ_i are firm fixed effects, and $\xi_{t,a}$ corresponds to year-by-PAB allocation dummy fixed effects.[21] I add industry-by-post-dummy fixed effects to control for industry-specific trends around the tax reform.[22] The sample period is from 1983 to 1990. I include lagged firm size and lagged return on assets as control variables in all regressions. Standard errors are clustered at the state level.

The design compares firms that all receive some PAB allocation after the reform (or before and after the reform in my preferred specification), but are located in states with different per cap PAB supply due to the new PAB volume caps. I show that post-reform beneficiaries in states with higher and lower per cap PAB supply are similar in terms of observable characteristics prior to the reform. Table 2.2, Panel B presents differences in means for a range of financial characteristics. Firms in states with more than 50 USD per cap supply and firms in states limited to 50 USD per cap supply are similar in terms of investment, employment, and profitability, amongst others. Firm size is a notable exception, as firms in states limited to 50 USD per cap PAB supply are slightly larger than firms in states with a higher PAB supply.

2.5.2.1 BASELINE RESULTS

Table 2.5 presents the results for the effect of PAB supply on investment of PAB beneficiary firms. The main finding is that higher per cap supply of PABs has a

[21] PAB allocation dummy takes a value of one if the firm receives any PAB funding allocation in the respective year, and zero otherwise.

[22] I use industry-by-post dummy fixed effects instead of industry-by-year fixed effects as in previous regressions to avoid singleton observations.

positive and sizable effect on investment of firms that are PAB beneficiaries. For firms that receive any PAB allocation in the four years following the 1986 Tax Reform, displayed in the first two columns of the table, increasing per cap PAB supply by 24.02 USD—equivalent to one standard deviation—is associated with an increase in the capex-to-assets ratio by 7.21%, or by 8.41% when controlling for industry-specific trends before and after the tax reform. For beneficiaries that receive a PAB allocation both after and before the tax reform, shown in columns (3) and (4) of the table, the point estimates are more pronounced compared to the post-beneficiary sample: An additional 24.02 USD in per cap PAB supply increases the capex-to-assets ratio by 9.85% in the baseline specification, and by 11.77% when industry-by-post-dummy fixed effects are included. For the average PAB beneficiary firm, the latter two estimates correspond to an increase in the capex-to-assets ratio by 0.89 or 1.06 percentage points, respectively.

Table 2.5
Per cap PAB supply and investment of PAB beneficiary firms after the 1986 Tax Reform

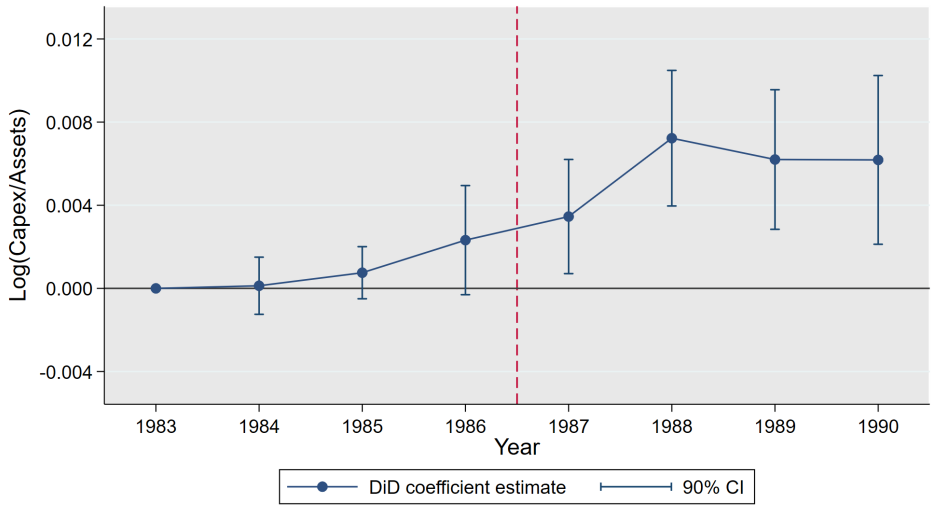
The dependent variable is the natural logarithm of capital expenditures divided by the beginning of period total assets. In columns (1) and (2), the sample consists of firms that receive any PAB allocation in the four years after the 1986 Tax Reform. Columns (3) and (4) additionally require any PAB issuance in the four years before the reform. Firm controls include lag of size and lag of return on assets. Per cap PAB supply is the limit to the per capita amount of private activity bonds in USD that a state may distribute for calendar years 1988 onward. Post is a dummy that equals one from 1987 to 1990, and zero otherwise. The sample period is from 1983 to 1990. T-statistics based on Huber/White robust standard errors clustered by state are presented in parentheses. ***, ** and * indicate significance at the 1%-, 5%- and 10%-levels, respectively. Appendix B.1 provides a detailed description of all variables.

	(1)	(2)	(3)	(4)
	Log(Capex/Assets) for			
	$Post_{1986}$ beneficiaries		Pre and $Post_{1986}$ benefic.	
Per cap PAB supply x Post-1986	0.0030***	0.0035**	0.0041**	0.0049***
	(3.507)	(2.142)	(2.752)	(3.021)
Firm controls	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
PAB allocation dummy x Year FE	Yes	Yes	Yes	Yes
Industry x $Post_{1986}$ FE	No	Yes	No	Yes
Number of observations	1,022	1,021	524	523
Adjusted R^2	0.549	0.567	0.601	0.627

Figure 2.5 shows time dynamics for the effect of per cap PAB supply on investment of beneficiary firms, utilizing the most restrictive specification as presented in Table 2.5, column (4). Before the 1986 Tax Reform, I do not observe a significant difference in beneficiary firms' investment depending on the per cap supply of PABs. Starting with 1987, a relatively higher per cap PAB supply has a statistically significant positive effect on firm investment. The effect reaches its maximum in 1988. In 1989 and 1990, the effect remains large and positive, but slightly smaller than for 1988.

Figure 2.5
Per cap PAB supply and investment of PAB beneficiary firms after the 1986 Tax Reform

This figure illustrates the development of firm investment, calculated as the natural logarithm of capital expenditures divided by the beginning of period total assets, for firms that benefit from any PAB issuance on their behalf in both the four year pre- and the four year post-period of the 1986 Tax Reform. It shows coefficient estimates and the 90% confidence interval on the Per cap PAB supply measure interacted with year dummies over the event period. Per cap PAB supply is the limit to the per capita amount of private activity bonds in USD that a state may distribute for calendar years 1988 onward. The regression specification follows column (4) in Table 2.5. The effective date for the PAB volume caps mandated by the 1986 Tax Reform Act is shown in red (- -). Appendix B.1 provides a detailed description of all variables.



As firms that receive a PAB allocation before and after the tax reform likely have a persistent demand for PAB funding, these firms may in turn also be most affected by changes in PAB supply. Thus, I consider these estimates to rather

represent an upper bound of the direct effect of PAB supply. The significant positive results with respect to PAB beneficiaries support my previous finding that PAB supply has a stimulative effect on corporate investment.

Taken together, I find a sizeable positive effect of PAB supply on investment for both PAB eligible firms and PAB beneficiary firms. I suggest that the relatively large magnitude of the effect may be influenced by specific features of the PAB program. Notably, there are several restrictions on the designated use of tax-exempt PABs. According to IRC Section 147(*c*), only 25% of PAB bond proceeds can be used for the acquisition of land. In addition, PABs usually cannot be used to finance working capital or inventory (see, e.g., Mississippi Business Finance Corporation, 2006; Utah Department of Workforce Services Housing & Community Development Division, 2023). Consequently, firms must use other funds for these purposes, likely resulting in an overall project size that exceeds the PAB funding amount. Consistent with this, Business Wire (1998) reports that the PAB beneficiary firm Pure H20 Bio-Technologies Inc. uses equity financing for working capital alongside the PAB funds. Similarly, the company Gevo, Inc. (2023) indicates plans to use PABs as part of a larger financing package for a new project.

2.5.2.2 ROBUSTNESS TESTS

In Appendix Table B.4 and Appendix Table B.5, I present robustness tests for the positive effect of PAB supply on investment of PAB beneficiary firms. First, I show that my result is robust to a narrower definition of post-reform beneficiaries. In particular, I define firms as post-reform beneficiaries if they receive any PAB allocation in 1987 or 1988. The estimates, reported in Appendix Table B.4, are slightly more pronounced compared to my baseline results. Second, I estimate the investment effect over an extended post-reform window of seven years instead of four years. Consistent with the post-reform time dynamics illus-

trated in Figure 2.5, I find that the investment effect is larger when examining an extended time period (Appendix Table B.5).

2.6 PRIVATE ACTIVITY BOND SUPPLY AND FIRM EMPLOYMENT

While the established positive investment response to PABs could lead to an increase in the hiring of labor and thus generate a "scale effect", it could also have a negative impact on employment if firms substitute labor with tax-subsidized capital (Moore and Squires, 1988). I test how PAB supply affects the employment of PAB eligible firms and PAB beneficiary firms. Therefore, I use the natural logarithm of firm employment as the dependent variable in the regression specification for PAB eligible firms, outlined by equation 2.4, and for PAB beneficiary firms, outlined by equation 2.5. To account for potential data errors, I exclude firms that report zero employment at any point in time during the event period.

Table 2.6 and Table 2.7 present the results. The main finding is that higher per cap supply of PABs has a positive effect on employment of PAB eligible and PAB beneficiary firms.

For the sample of PAB eligible firms analyzed in Table 2.6, the point estimate for the interaction term *Per cap PAB supply x Post-1986* is positive and statistically significant in all specifications (with the smallest t-statistic being 6.252). In terms of magnitude, after the 1986 Tax Reform, a one standard deviation increase in per cap PAB supply, equivalent to 46.48 USD, leads to a 4.83% increase in employment of PAB eligible firms (column 1). For the most restrictive specification (column 3), a one standard deviation increase in PAB supply is associated with an increase in employment by 4.56%. As the sample comprises PAB eligible firms, these estimates can be interpreted as an intention-to-treat effect of PAB supply on employment.

Table 2.6
Per cap PAB supply and firm employment after the 1986 Tax Reform

The sample consists of PAB eligible firms located in border counties in which any local government issued at least one PAB in the ten years before the 1986 Tax Reform. The dependent variable is the natural logarithm of firm employment. Per cap PAB supply is the limit to the per capita amount of private activity bonds in USD that a state may distribute for calendar years 1988 onward. Post is a dummy that equals one from 1987 to 1990, and zero otherwise. The sample period is from 1983 to 1990. T-statistics based on Huber/White robust standard errors clustered by state are presented in parentheses. ***, ** and * indicate significance at the 1%-, 5%- and 10%-levels, respectively. Appendix B.1 provides a detailed description of all variables.

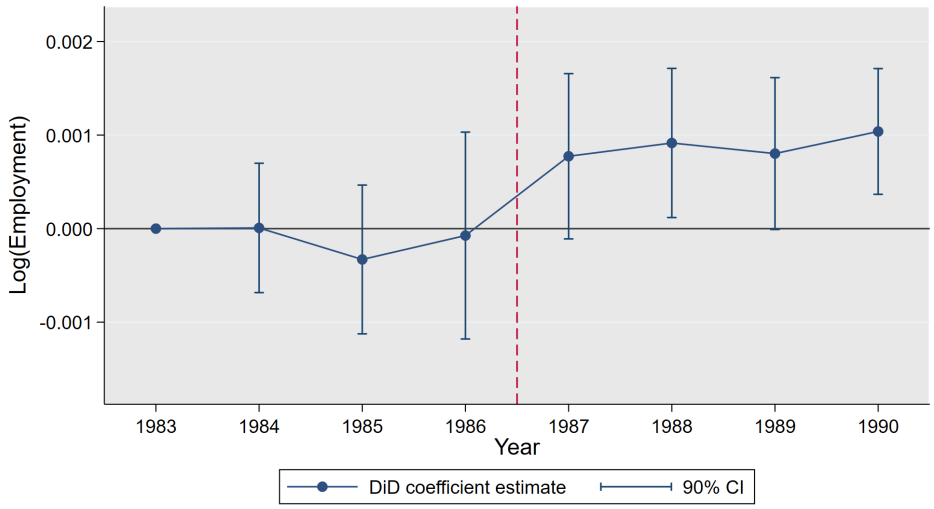
	(1)	(2)	(3)
	Log(Employment)		
Per cap PAB supply x Post-1986	0.00104***	0.00103***	0.00098***
	(9.806)	(9.779)	(6.252)
Lag of Size	0.16262***	0.16609***	0.16914***
	(7.249)	(6.997)	(6.632)
Lag of RoA		0.01730	0.01950
		(0.657)	(0.696)
Firm FE	Yes	Yes	Yes
Year FE	Yes	Yes	No
State border pair x $Post_{1986}$ FE	Yes	Yes	Yes
Industry x Year FE	No	Yes	Yes
Number of observations	4,067	4,051	4,027
Adjusted R^2 (within)	0.157	0.161	0.169

Figure 2.6 illustrates the time dynamics of the effect of per cap PAB supply on employment of eligible firms. The regression specification corresponds to column (3) of Table 2.6, but uses an interaction of Per cap PAB supply with year dummies instead of the post-dummy. Prior to the 1986 Tax Reform, I do not observe a significant difference in firm employment depending on the per cap supply of PABs. Starting in 1987, firm employment relatively rises with higher per cap PAB supply, but the effect only becomes statistically significant as of 1988. The effect reaches its maximum in 1990.

In Table 2.7, I present results for the effect of per cap PAB supply on employment of PAB beneficiary firms. For firms that receive any PAB allocation in the four years after the 1986 Tax Reform, displayed in the first two columns of the table, increasing per cap PAB supply by 24.02 USD—equivalent to one standard deviation—is associated with a 2.11% increase in employment, or a

Figure 2.6
Per cap PAB supply and firm employment after the 1986 Tax Reform

This figure illustrates the development of firm employment, calculated as the natural logarithm of the number of employees, for PAB eligible firms located in border counties. It shows coefficient estimates and the 90% confidence interval on the Per cap PAB supply measure interacted with year dummies over the event period. Per cap PAB supply is the limit to the per capita amount of private activity bonds in USD that a state may distribute for calendar years 1988 onward. The regression specification follows column (3) in Table 2.6. The effective date for the PAB volume caps mandated by the 1986 Tax Reform Act is shown in red (- -). Appendix B.1 provides a detailed description of all variables.



1.95% increase when controlling for industry-specific trends before and after the tax reform. For beneficiaries that receive a PAB allocation both after and before the tax reform, shown in columns (3) and (4) of the table, the point estimates are more pronounced compared to the post-beneficiary sample: An additional 24.02 USD in per cap PAB supply increases employment by 3.12% in the baseline specification, and by 2.98% when industry-by-post-dummy fixed effects are included.

Taken together, my results show that in addition to the stimulative effect on firm investment, higher supply of PABs also has a positive effect on firm employment. Hence, my findings do not support the notion of input factor substitution with respect to PAB funding after the 1986 Tax Reform.

Anecdotally, the Washington State Department of Commerce (2018) reports that its PAB allocation for small issue bonds as well as exempt facility bonds

created 191 new jobs and retained 150 jobs in 2016 and 2017. Some states explicitly require job creation or retention when allocating PAB funding. The State of Pennsylvania, for instance, requires the net creation of at least one job, or retention of one full-time job, for every 50,000 USD in PAB volume allocated (see 12 Pa. Code Chapter 61.3). Thus, these specific features of the PAB program could be a reason for the scale effect on employment that I document.

Table 2.7
Per cap PAB supply and employment of PAB beneficiary firms after the 1986 Tax Reform

The dependent variable is the natural logarithm of firm employment. In columns (1) and (2), the sample consists of firms that receive any PAB allocation in the four years after the 1986 Tax Reform. Columns (3) and (4) additionally require any PAB issuance in the four years before the reform. Firm controls include lag of size and lag of return on assets. Per cap PAB supply is the limit to the per capita amount of private activity bonds in USD that a state may distribute for calendar years 1988 onward. Post is a dummy that equals one from 1987 to 1990, and zero otherwise. The sample period is from 1983 to 1990 T-statistics based on Huber/White robust standard errors clustered by state are presented in parentheses. ***, ** and * indicate significance at the 1%-, 5%- and 10%-levels, respectively. Appendix B.1 provides a detailed description of all variables.

	(1)	(2)	(3)	(4)
	Log(Employment) for			
	$Post_{1986}$ beneficiaries		Pre and $Post_{1986}$ benefic.	
Per cap supply x Post-1986	0.00088**	0.00081**	0.00130**	0.00124**
	(2.043)	(2.107)	(2.606)	(2.554)
Firm controls	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
PAB allocation dummy x Year FE	Yes	Yes	Yes	Yes
Industry x $Post_{1986}$ FE	No	Yes	No	Yes
Number of observations	993	992	512	511
Adjusted R^2 (within)	0.291	0.269	0.165	0.160

2.7 REAL EFFECTS OF THE TEXAS PAB LOTTERY

So far, I have disregarded that states have discretion in allocating their PAB volume caps to projects and thus to firms. If states with higher per cap PAB supply limits systematically select projects with higher investment and employment potential than states with lower supply limits, this would bias my results. To strengthen my identification, I use PAB program data from the State of Texas,

which uses a lottery system to allocate its PAB volume among PAB funding applicants (see Appendix B for more details).[23] This allows me to compare real effects for firms that all apply for PAB allocation, but only a random subset of firms, determined by lottery, actually receives PAB funding through the program.

As the PAB lottery data is on the project level, I aggregate the lottery outcomes at the firm-year level. Accordingly, I define a *Lottery winning firm* as a lottery-participating firm that wins any lottery (project) attempt in the respective program year. A *Lottery losing firm* is a lottery-participating firm that does not receive any PAB allocation in the respective program year. I leverage a sample of PAB applicant firms for the program years 1996 to 2001, for which I observe both at least one lottery winning and one lottery losing firm per program year. To reduce any potential bias due to the staggered nature of the lottery data, I only consider lottery winning firms in the earliest program year with a successful lottery attempt. Lottery losing firms never receive any PAB allocation over the entire 1996 to 2001 period, and are included in the sample in all years in which they apply for an allocation.

I estimate the effect of receiving PAB funding through the Texas PAB lottery as follows:

$$\Delta Outcome_{i,t} = \alpha + \beta Lottery\ win\ dummy_i + \xi_{lottery\ \ program\ \ year} + \varepsilon_{i,t} \tag{2.6}$$

whereby $\Delta Outcome_{i,t}$ is the change in firm investment or employment between year t and the pre-lottery year (year 0). $Lottery\ win\ dummy_i$ is a dummy variable equal to one if a lottery-participating firm wins any lottery (project) attempt in the respective program year, and zero otherwise. I use the natural logarithm of the bond volume allocated through the lottery as an alternative treatment

[23]The Texas PAB allocation program is also relevant due to its economic size, as Texas had the second largest PAB cap among all states during the lottery period that I analyze (Texas Bond Review Board, 1997).

measure. $\xi_{lottery\ program\ year}$ are lottery program year fixed effects. I control for firm size measured in the year before the lottery in all regressions.

I show that lottery winning and losing firms are similar in terms of observable characteristics prior to their respective lottery attempts. Table 2.2, Panel C, provides an overview. In particular, lottery winning and losing firms have similar size, return on assets, and PPE growth prior to the lottery attempt and also do not differ in the levels of the outcome variables. Winning firms appear to be less leveraged than losing firms before the lottery.

Table 2.8

The Texas PAB lottery and firm investment

The dependent variable is the difference in firm investment over the indicated time window relative to the pre-lottery year (year 0). Firm investment is calculated as the natural logarithm of capital expenditures divided by the beginning of period total assets. The sample consists of firms that participate in the Texas PAB lottery between 1996 and 2001. Lottery win dummy is a dummy equal to one if the firm wins any of its lottery attempts in the respective program year, and zero otherwise. Log(lottery-allocated bond volume) is the natural logarithm of the total bond amount in USD won in a lottery year plus one, and is zero for lottery losing firms. T-statistics based on Huber/White robust standard errors are presented in parentheses. ***, ** and * indicate significance at the 1%-, 5%- and 10%-levels, respectively. Appendix B.1 provides a detailed description of all variables.

	(1)	(2)	(3)	(4)	(5)	(6)
	Change in Log(Capex/Assets) over					
	1 year	2 years	3 years	1 year	2 years	3 years
Panel A: Lottery winning firms						
Lottery win dummy	0.138	0.336**	0.320*	0.165	0.345*	0.391*
	(1.137)	(2.226)	(1.933)	(1.051)	(1.756)	(2.019)
$Size_{Pre\text{-}lottery\ year}$	-0.193***	-0.201***	-0.158***	-0.201***	-0.209***	-0.179***
	(-4.102)	(-4.431)	(-4.215)	(-4.371)	(-4.093)	(-3.829)
Lottery program year FE	No	No	No	Yes	Yes	Yes
Number of observations	29	25	24	29	25	24
Adjusted R^2	0.488	0.498	0.363	0.459	0.381	0.279
Panel B: Lottery-allocated bond volume						
Log(Lottery-allocated bond vol.)	0.009	0.021**	0.020*	0.011	0.021*	0.024*
	(1.192)	(2.298)	(2.011)	(1.119)	(1.813)	(2.108)
$Size_{Pre\text{-}lottery\ year}$	-0.193***	-0.201***	-0.158***	-0.201***	-0.209***	-0.179***
	(-4.133)	(-4.492)	(-4.284)	(-4.449)	(-4.181)	(-3.935)
Lottery program year FE	No	No	No	Yes	Yes	Yes
Number of observations	29	25	24	29	25	24
Adjusted R^2	0.491	0.504	0.372	0.465	0.389	0.294

Table 2.8 presents the results for the effect of PAB allocation through the

lottery on firm investment. The main finding is that receiving PAB funding through the lottery has a positive effect on investment. For Panel A, I use Lottery win dummy as the treatment measure. The preferred specification is reported in columns (4) to (6) and compares lottery winning and losing firms that apply for PAB funding in the same program year. When looking at the one-year change in firm investment in column (4), the coefficient for the lottery win dummy is positive, but statistically not significant (t-statistic: 1.051). I observe a statistically significant relative increase in firm investment for Lottery winning firms over a two-year window (t-statistic: 1.756) and a three-year window (t-statistic: 2.019). The magnitude of the effect implies an increase in the capex-to-assets ratio for lottery winning firms relative to losing firms of 34.5% over two years, and 39.1% over three years, respectively. In Panel B of Table 2.8, I use the natural logarithm of the lottery-allocated bond volume as the treatment measure. I find that allocated bond volume is associated with a statistically significant relative increase in firm investment over the two- and three-year windows. Overall, these results support my previous finding that PAB supply has a stimulative effect on corporate investment.

Table 2.9 presents the results for the effect of PAB allocation through the lottery on firm employment. While the coefficient on Lottery win dummy and on the logarithm of lottery-allocated bond volume is positive in all specifications, it is not statistically significant. Despite the statistically non-significant point estimates, the time pattern indicates an increase in the size of the coefficient from the one-year to the three-year window. For my preferred specification that includes lottery program year fixed effects, using the lottery win dummy as the treatment measure, I obtain a point estimate of 0.185, with a t-statistic of 1.464 (column 6, top panel). While these estimates do not provide evidence for the scale effect of PAB supply, they align with the earlier finding that PAB funding is not associated with a substitution of input factors.

Table 2.9
The Texas PAB lottery and firm employment

The dependent variable is the difference in firm employment over the indicated time window relative to the pre-lottery year (year 0). Firm employment is calculated as its natural logarithm. The sample consists of firms that participate in the Texas PAB lottery between 1996 and 2001. Lottery win dummy is a dummy equal to one if the firm wins any of its lottery attempts in the respective program year, and zero otherwise. Log(lottery-allocated bond volume) is the natural logarithm of the total bond amount in USD won in a lottery year plus one, and is zero for lottery losing firms. T-statistics based on Huber/White robust standard errors are presented in parentheses. ***, ** and * indicate significance at the 1%-, 5%- and 10%-levels, respectively. Appendix B.1 provides a detailed description of all variables.

	(1)	(2)	(3)	(4)	(5)	(6)
	Change in Log(Employment) over					
	1 year	2 years	3 years	1 year	2 years	3 years
Panel A: Lottery winning firms						
Lottery win dummy	0.066	0.083	0.144	0.085	0.114	0.185
	(1.329)	(1.241)	(1.453)	(1.490)	(1.258)	(1.464)
$\text{Size}_{\text{Pre-lottery year}}$	-0.002	-0.019	-0.039	-0.007	-0.023	-0.042
	(-0.241)	(-0.953)	(-1.043)	(-0.581)	(-1.263)	(-1.212)
Lottery program year FE	No	No	No	Yes	Yes	Yes
Number of observations	29	25	25	28	24	24
Adjusted R^2 (within)	-0.00164	0.0152	0.0530	0.0216	0.0477	0.0643
Panel B: Lottery-allocated bond volume						
Log(Lottery-allocated bond vol.)	0.004	0.005	0.009	0.005	0.007	0.011
	(1.318)	(1.272)	(1.440)	(1.497)	(1.292)	(1.439)
$\text{Size}_{\text{Pre-lottery year}}$	-0.002	-0.019	-0.039	-0.007	-0.023	-0.042
	(-0.235)	(-0.936)	(-1.035)	(-0.583)	(-1.246)	(-1.202)
Lottery program year FE	No	No	No	Yes	Yes	Yes
Number of observations	29	25	25	28	24	24
Adjusted R^2 (within)	-0.00143	0.0205	0.0524	0.0250	0.0576	0.0629

Taken together, the results from examining the Texas PAB lottery strongly support my previous finding that PAB financing stimulates corporate investment and rule out that states' project selection is the main driver of this effect.

2.8 CONCLUSION

This paper examines the effect of private activity bond supply on firm investment and employment. PABs provide firms with access to the tax-exempt bond market, making them a cost-attractive source of financing compared to conventional methods.

For identification, I exploit the introduction of new state-level caps for PABs as mandated by the 1986 Tax Reform Act. Due to the newly enacted allocation formula, less populous states can supply relatively higher per capita volumes of PABs to firms, while larger states are bound to a more restrictive baseline per cap PAB volume. I leverage this variation in per cap PAB supply limits in a difference-in-differences framework. I document that higher per cap PAB supply after the tax reform is positively associated with investment of PAB eligible and PAB beneficiary firms.

I then investigate how the limit on per cap PAB supply affects firm employment. I find that after the 1986 Tax Reform, relatively higher PAB supply is positively associated with employment. This finding suggests that despite the subsidy of capital relative to labor, PAB funding is not linked to an input factor substitution.

To address the potential concern that states' discretion in allocating PAB funding drives my results, I leverage data from the Texas PAB lottery – the PAB distribution mechanism for the State of Texas. By comparing firms that randomly win and lose their requests for PAB allocation in the PAB lottery, I confirm my previous finding that PAB supply positively affects firm investment.

The stimulating effect on PAB beneficiaries, as documented in this paper, is a necessary prerequisite for the PAB program to enhance local economic development. While the positive investment response of beneficiaries may spur investment of local non-recipients, it could also have competitive effects. My findings motivate further research on how PAB funding, through stimulating investment and employment of program beneficiaries, affects local economic development in the aggregate.

"… A spokesman for IBM said the company was using tax-exempt pollution control bonds for the first time because of the 'sustained high cost of borrowing'. 'With rates what they are, this method of financing was found to be more attractive than in the past,' the spokesman said." – Kreps (1981), in: The Bond Buyer.

3

Tax-Subsidized Green Bonds and their Real Effects

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Abstract:

This paper studies the use and the corporate real effects of green bonds that are tax-subsidized and thus a source of low-cost financing. Using a large, novel sample of pollution control bonds that finance pollution abatement facilities of U.S. public firms, I document that tax-subsidized green bonds are disproportionately used by large firms, even within industries. Issuance of tax-subsidized green bonds increases when conventional bond market yields are high. Examining the period of high interest rates in the early 1980s, I show that firms using such bonds display an increase in output after issuance. From the perspective of the corporate issuers, this output effect highlights a potential positive economic aspect correlated with undertaking tax-subsidized green investments.

3.1 INTRODUCTION

In the wake of the recent growth of the corporate green bond market, considerable attention has been paid to whether these bonds are issued at a premium compared to conventional corporate bonds. Building on the relatively short history of this financing instrument,[1] the empirical literature examining corporate green bonds finds that the premium at issue is at best economically small (for example, Flammer (2021) finds no evidence of a premium at issue, and Caramichael and Rapp (2024) show that the premium ranges from three to eight basis points).[2] From a corporate financing perspective, these results are particularly interesting as they indicate that green bonds so far do not substantially lower the financing costs of green projects (Daubanes, Mitali and Rochet, 2021; Flammer, 2021).

At the same time, the current voluntary development of the (corporate) green bond market is unlikely sufficient to meet the capital needs to address today's enormous environmental challenges (Migliorelli and Dessertine, 2019). Subsidies are generally discussed as a fiscal policy means to increase the growth of the green bond market (Chiang, 2017; Climate Bonds Initiative, 2024b). In this regard, tax incentives are one approach that can be used to reduce financing costs for corporate green bond issuers (Climate Bonds Initiative, 2024c).

If corporate green bonds are tax-subsidized and thus a source of low-cost financing for green projects, what types of companies are using such a financing tool? What conventional bond market conditions spur their use? And what are the real effects associated with the use of tax-subsidized green bonds? I use a large sample of tax-exempt pollution control bonds (PCBs) to explore these questions. This sample covers 3,200 PCBs issued between 1980 and 2013 to

[1]The first labeled corporate green bond was issued in November 2013 (see, e.g., Climate Bonds Initiative, 2024a).

[2]Similarly, studying green municipal bonds, Larcker and Watts (2020) find no evidence for a premium at the issue in the municipal bond market, and Baker et al. (2022) document that the premium is economically small.

finance pollution abatement facilities of U.S. public firms.

Three characteristics make pollution control bonds particularly well suited for the analysis of tax-subsidized green bonds: (i) Qualified PCBs have significantly lower financing costs than conventional corporate bonds as they are issued on the tax-exempt municipal bond market (see, e.g., California Pollution Control Financing Authority, 2024b). Over the 1980 to 2013 period, the yield spread between high-rated taxable corporate bonds and high-rated municipal bonds was on average about 1.59 percentage points, with particularly high absolute spreads in the early 1980s (see Council of Economic Advisers, 2023, Table B–42, for the data underlying the calculation).[3] (ii) Because the proceeds of PCBs are invested in the construction, acquisition, or installation of pollution abatement facilities (see, e.g., California Pollution Control Financing Authority, 2024b; Golemon and Burgin, 1983; McBride and Dreifus, 1983), the bonds' underlying financing purpose would typically allow them to be labeled as green under current standards (California Pollution Control Financing Authority, 2024a).[4] (iii) The dataset enables the analysis of tax-subsidized green bonds from a corporate perspective by combining bond-level data from SDC Platinum with company-level financial data from Compustat, overcoming the measurement challenges inherent in the analysis of many local corporate incentive programs (Slattery and Zidar, 2020).[5]

I start by documenting several stylized facts of firms that issue PCBs, thus using a source of low-cost financing for pollution control projects.[6] I adapt the approach of Chodorow-Reich et al. (2022) to explore the use of PCBs across

[3]For 1981, Merrill Lynch for example estimated that utilities could issue Aa-rated PCBs at a yield of about 11.5%, while conventional financing rates would have been about 1.5 times that level (The Bond Buyer, 1981).

[4]In this regard, the International Capital Market Association (2021) lists pollution prevention and control as one of the eligible categories for green projects. Still, to date, PCBs are often not labeled as green bonds (Baker et al., 2022).

[5]Another feature of PCBs is that, from a corporate perspective, PCBs are largely comparable to conventional debt financing setups, despite the municipal conduit in between (Kutak and Wagner, 1974-1975).

[6]I use the term "issuer" to refer to the firm that uses the bond proceeds and solely secures the PCB bond issue, even though the bond is technically issued by a municipal conduit.

the firm-size distribution and show that pollution control bonds are disproportionately used by large firms, even within industries. I further show that PCBs are primarily used by firms in the manufacturing and utility industries, which together account for about 97% of the issuance volume in the sample. Looking at public firms' PCB issuance over time, the use of PCBs peaked in the first half of the 1980s, with a total issuance volume of about USD 20b between 1980 and 1984.

I then turn to the market conditions that might spur the use of PCBs. In particular, I ask whether PCB issuance is correlated with financing costs on the conventional corporate bond market. Anecdotal examples suggest that tax-subsidized PCB financing may be considered especially during periods of high conventional financing costs (see, e.g., Kreps, 1981, reporting in The Bond Buyer on a PCB issuance by IBM in 1981). Using both quarterly aggregate and firm-level PCB issuance data in linear regressions, I find that PCB issuance is positively correlated with Moody's yield for Aaa- and Baa-rated corporate bonds. For the firm-level sample of PCB issuers, a one percentage point increase in the conventional Aaa-rated bond yield is associated with a 4.1% increase in the probability of issuing a new money PCB.[7] Additionally, I show that the issuance of refunding PCBs is negatively correlated with conventional bond market yields.

Motivated by the documented positive correlation between conventional bond yields and new money PCB issuance, I finally analyze whether the issuance of tax-subsidized green bonds during the period of high interest rates between 1980 and 1984 is associated with post-issuance real effects for these firms. In the absence of (quasi-)random assignment of firms as PCB issuers, I apply the approach of Flammer (2021) and compare PCB issuers that tap the PCB market between 1980 and 1984 with a matched control group. I therefore require the

[7]The municipal bond market distinguishes between new money bonds, which finance new projects, and refunding bonds, respectively (see, e.g., United States Government Accountability Office, 2008).

control firms to operate in the same industry and be headquartered in the same state as the respective PCB issuer. I then select the nearest neighbor for each issuer, using size, return on assets, the capex-to-assets ratio, the property, plant, and equipment-to-assets ratio, and sales growth as matching variables. The sample comprises 149 PCB issuers and 149 matched control firms.

Using the matched sample in a difference-in-differences regression framework, I find that PCB issuance is positively associated with post-issue sales growth: PCB issuers that tap the PCB market during the high-interest rate period from 1980 to 1984 display a relative increase in sales growth by 2.4% to 3.0% over the three-year window after the issuance.[8] This finding is robust to the use of an alternative control group that additionally requires PCB issuers and control firms to be in the same size quintile. Overall, while this result does not imply a causal relation, it suggests that the use of a low-cost source of financing for pollution abatement investments is correlated with potential positive economic aspects for the issuers.

Finally, I use the matched sample to examine whether the issuance of pollution control bonds is associated with a change in capital investment or R&D investment. While I find no statistically significant association of PCB issuance with capital investment, I do find that PCB issuance is positively associated with R&D investment after the bond issuance.

This paper relates to several strands of the literature. First, it contributes to the growing literature that examines green bonds as financial instruments.[9] Within this strand, both municipal green bonds (e.g., Baker et al., 2022; Karpf and Mandel, 2018; Larcker and Watts, 2020, among others) and corporate green bonds (e.g., Caramichael and Rapp, 2024; Dang, Wang and Wang, 2022; Flam-

[8]This result differs from what Di Giuli and Kostovetsky (2014) document on the relation of corporate social responsibility (CSR) and revenue growth. In particular, they find that an increase in CSR is negatively related to future revenue growth. While CSR is a voluntary initiative and may be perceived as costly from the firm's perspective, PCBs are essentially a subsidy for a firm's pollution abatement investment.

[9]Cortellini and Panetta (2021) provide a broad overview of the green bond literature.

mer, 2021; Tang and Zhang, 2020, among others) are examined from different points of view. As (non-subsidized) corporate green bonds are a relatively new financial instrument, I add to this literature by providing novel insights on a comparably large U.S. sample of tax-subsidized "corporate" green bonds, covering more than 30 years of issuance: corporate-backed pollution control bonds issued through municipal conduits to finance pollution abatement facilities of U.S. public firms.[10]

Second, I relate to the literature that studies the real effects of green financial instruments. Flammer (2021) shows that corporate green bond issuers subsequently improve their environmental performance. For banks that use green bonds, Fatica, Panzica and Rancan (2021) document a reduction in lending to carbon-intensive sectors. Flammer (2021) and Tang and Zhang (2020) document ownership changes after green bond issuance. I focus on sales growth and corporate investment. In particular, I show that the issuance of pollution control bonds is positively associated with sales growth and R&D investment.[11]

Third, I broadly relate to the literature on financing conditions and green investments as well as environmental outcomes. Among others, Bartram, Hou and Kim (2022) and Xu and Kim (2022) show that financial constraints lead to an increase in firm emissions. De Haas et al. (2024) document, among other findings, that credit constraints reduce investment in green technologies, while these constraints play a minor role with respect to investment in pollution control. Hartzmark and Shue (2023) show that increasing financing costs for brown firms makes them (much) more brown, while reducing the financing costs for green firms only leads to minor additional environmental improvements. Accetturo et al. (2024) show that the supply of credit increases the likelihood

[10]Previous literature has studied pollution control bonds issued by utility firms: Khanchel, Lassoued and Bargaoui (2023) study PCBs of energy utility firms and environmental performance measures. Varma and Szewczyk (1990) study the sale of PCBs by public utilities and share price reactions.

[11]Wang, Liu and Wang (2022) specifically examine green innovation in the context of green bond issuance in China and document a positive correlation with bond issuance.

that firms undertake green investments, with the effect being amplified when government subsidies are available. I document that the use of tax-subsidized pollution control bonds increases with conventional bond market yields.

Finally, my work relates to the stream of literature that examines fiscal policies directed at the private sector and aimed at improving environmental outcomes. In a survey on climate finance conducted by Stroebel and Wurgler (2021), the surveyed researchers and practitioners rank research on such government incentives as the most important research topic among a list of thirteen topics provided. There is also considerable political interest in the impact of such fiscal measures (see, e.g. Delgado-Téllez, Ferdinandusse and Nerlich, 2022; Eurostat, 2015, for the European Union). Much work has been devoted to the study of carbon taxes. Timilsina (2022) provides a comprehensive overview. Williams III (2016) examines environmental taxes more broadly. On the other hand, various types of corporate subsidies are also commonly employed fiscal instruments to incentivize sustainable investment (Columbia Center on Sustainable Investment, 2022; Lovei, 1995). I provide comprehensive insights on the use and real effects of PCBs as a long-standing subsidy tool.

The remainder of the paper is structured as follows. Section 3.2 describes the data. Section 3.3 presents stylized facts for the use of pollution control bonds. Section 3.4 presents results for the correlation between conventional bond yields and PCB issuance, and Section 3.5 presents results for real effects associated with PCB issuance. Section 3.6 concludes.

3.2 DATA AND DESCRIPTIVE STATISTICS

3.2.1 DATA SOURCES

I make use of two main data sources to construct a sample of U.S. public firms that issue pollution control bonds. First, I obtain data on pollution control bonds from the Municipal New Issues Database in SDC Platinum. Second, I

obtain firm financial data from Compustat.

Within SDC Platinum, PCBs are identified based on the bond's main use of proceeds classified as "pollution control". I exclude bonds that are marked as general obligation bonds since pollution control bonds should be in the structure of revenue bonds (see, e.g., Friedman, 1976). I also exclude taxable bonds because it is not obvious that they reduce financing costs relative to conventional financing sources. In addition, I remove bonds that are neither classified as new money nor as refunding bonds. I hand-match the names of the corporate backers of these bonds—the firms that use the bond proceeds and secure the issue—to Compustat based on (historic) company names. For one part of my analyses, I aggregate the bond-level data at the calendar year-quarter level.

The sample period starts in 1980, as this is the first year for which SDC Platinum comprehensively covers pollution control bonds. Refunding bonds are included from 1983 onwards, in line with their comprehensive data availability in SDC Platinum.[12] I choose 2013 as the final year of the tax-subsidized green bond sample because the first labeled corporate green bond was issued in November 2013 (see, e.g., Climate Bonds Initiative, 2024a).

For the firm-level analyses, I aggregate PCB issuance data at the firm-year level. I drop firm years with missing information on lagged total assets, lagged return on assets, and lagged leverage. I also exclude financial firms (SIC codes 6000–6999) because new money bonds are only allocated to this industry in four firm years. All continuous financial variables are winsorized at the 1st and 99th percentiles.

For the group of non-PCB issuing firms in Compustat, I additionally make the following adjustments: I broadly follow Flammer (2021) and exclude non-PCB issuers that never report any debt during the sample period, as PCBs are a source of external financing. Further, I exclude firms that report total assets

[12] A few refunding bonds are reported in SDC Platinum for 1982, but these bonds are not included in the sample.

of zero at any point in time during the sample period. Following Almeida and Campello (2007), I exclude firms with a market-to-book ratio greater than 10. Finally, I drop non-PCB issuers in industries, measured at the two-digit SIC level, without any pollution control bond issuance.

For the conventional bond market yield, I use Moody's yield for Aaa-rated corporate bonds, which I obtain from the Federal Reserve Bank of St. Louis economic data repository. Finally, I also obtain information on U.S. GDP per cap as well as recession indicators from the Federal Reserve Bank of St. Louis economic data repository.

3.2.2 DESCRIPTIVE STATISTICS

Summary statistics are displayed in Table 3.1. The initial bond-level sample contains 3,200 PCBs issued between 1980 and 2013. The total issuance volume over this period is about USD 136b in nominal terms, and USD 220b when adjusted for inflation and expressed in 2013 USD.[13] Of these bonds, 1,693 are new money bonds that fund new pollution control facilities.

The firm-level sample captures 313 unique PCB issuers and 846 firm years with a new money PCB issuance. Overall, the dataset on PCBs can be considered large compared to datasets used in previous analyses of the non-subsidized U.S. corporate green bond market (see, e.g., Baker et al. (2022), as well as the information on the U.S. within the international sample used by Flammer (2021)), but the latter span a shorter time period compared to my PCB sample.

[13] I use the Consumer Price Index provided by the U.S. Bureau of Labor Statistics to adjust the issuance volume for inflation whenever I consider the full sample period between 1980 and 2013.

Table 3.1
Descriptive statistics

This table presents descriptive statistics at the bond-, firm-, and quarterly aggregate level, based on pollution control bond issues between 1980 and 2013 by firms covered in Compustat. Panel A shows descriptive statistics for PCB issuance at the bond deal level for new money and refunding bonds. Panel B shows descriptive statistics for PCB issuance at the calendar-quarter level. The sample covered in Panel C comprises 313 firms with any PCB issuance during the sample period from 1980 to 2013. Panel D comprises PCB issuers that tap the PCB market during the high-interest rate period between 1980 and 1984 and their matched control firms. Appendix C.1 provides a detailed description of all variables.

	N	Mean	SD	25^{th}	50^{th}	75^{th}
Panel A: Bond deal-level characteristics						
Bond deal volume$_{\text{new money}}$ (2013$m)	1,693	64.42	84.20	15.24	34.55	79.57
Rated dummy$_{\text{new money}}$	1,693	0.76	0.43	1.00	1.00	1.00
Callable dummy$_{\text{new money}}$	1,578	0.84	0.36	1.00	1.00	1.00
Maturity$_{\text{new money}}$ (years)	1,687	26.13	12.82	20.00	30.00	30.00
Bond deal volume$_{\text{refunding}}$ (2013$m)	1,507	73.64	81.93	18.71	48.21	99.79
Panel B: Quarterly aggregate sample						
PCB number$_{\text{new money}}$	136	12.45	15.75	4.00	8.00	14.00
Aggregate PCB volume$_{\text{new money}}$ (2013$m)	136	801.93	1339.87	156.72	355.06	844.50
PCB number$_{\text{refunding}}$	124	12.15	9.41	6.00	10.00	16.00
Aggregate PCB volume$_{\text{refunding}}$ (2013$m)	124	894.96	704.43	335.64	698.07	1,341.19
Conventional bond yield$_{\text{q, avg}}$	136	7.88	2.76	5.62	7.40	9.40
Recession dummy$_{\text{q}}$	136	0.13	0.34	0.00	0.00	0.00
Panel C: Firm-level sample of pollution control bond issuers						
PCB dummy$_{\text{new money}}$	7,849	0.11	0.31	0.00	0.00	0.00
PCB volume$_{\text{new money}}$ (2013$m)	846	108.65	177.87	26.04	57.24	125.65
Log(PCB volume$_{\text{new money}}$)	7,849	0.44	1.31	0.00	0.00	0.00
Lag of Size	7,849	8.14	1.42	7.36	8.19	9.16
Lag of RoA	7,849	0.13	0.06	0.09	0.12	0.15
Lag of Leverage	7,849	0.50	0.26	0.39	0.51	0.59
Conventional bond yield$_{\text{y, avg}}$	7,849	8.21	2.71	5.67	7.59	9.38
Recession dummy$_{\text{y}}$	7,849	0.24	0.43	0.00	0.00	0.00
PCB dummy$_{\text{refunding}}$	7,045	0.10	0.30	0.00	0.00	0.00
PCB volume$_{\text{refunding}}$ (2013$m)	682	146.18	217.91	33.78	79.41	159.50
Log(PCB volume$_{\text{refunding}}$)	7,045	0.42	1.33	0.00	0.00	0.00
Panel D: PCB issuers and matched control firms						
PCB issuer$_{\text{new money}}$	2,068	0.50	0.50	0.00	0.00	1.00
PCB volume$_{\text{new money}}$($m, not inflation-adjusted)	2,068	18.19	31.99	0.00	0.00	22.25
Log(PCB volume$_{\text{new money}}$)	2,068	1.58	1.73	0.00	0.00	3.15
Sales growth	2,068	0.09	0.12	0.03	0.10	0.16
Capex/Assets	2,065	0.10	0.05	0.07	0.09	0.13
R&D/Assets$_{\text{imputed}}$	2,068	0.01	0.02	0.00	0.00	0.00
R&D/Assets	541	0.03	0.03	0.01	0.02	0.04

3.3 CORPORATE UTILIZATION OF POLLUTION CONTROL BONDS

Following Flammer (2021), I first present several stylized facts on the use of tax-subsidized PCBs in my sample, highlighting similarities and differences with the use of non-subsidized corporate green bonds as described in the literature. As discussed by Lovei (1995), firms generally weigh the costs and benefits of pollution abatement investments, thereby also taking into account subsidies (see Lovei, 1995, for a detailed introduction on such investment decisions).[14]

3.3.1 UTILIZATION BY YEAR

Figure 3.1 shows the development of pollution control bond issuance over time. For new money bonds, 1984 is the year with the highest issuance in terms of both the number of bonds (198) and the total volume of bonds ($7.6b). As noted by Ryan (1985), the 1984 issuance volume also marks an "all-time high" relative to all previous years, and thus relative to the years before the sample period.[15] Overall, the early sample years between 1980 and 1985 may be described as the boom period for pollution control bond issuance.[16] Some early sample years also show high growth rates in issuance: PCB issuance grew by about 78% between 1980 and 1981, and by about 168% between 1983 and 1984. The development of PCB issuance in the first half of the 1980s is thus roughly comparable to the strong growth in (non-subsidized) green bond issuance between 2013 and 2018, as illustrated by Flammer (2021).

[14] The investment in pollution control has often been described as non-productive (see, e.g., Chell, 1982; Ray, 1975).

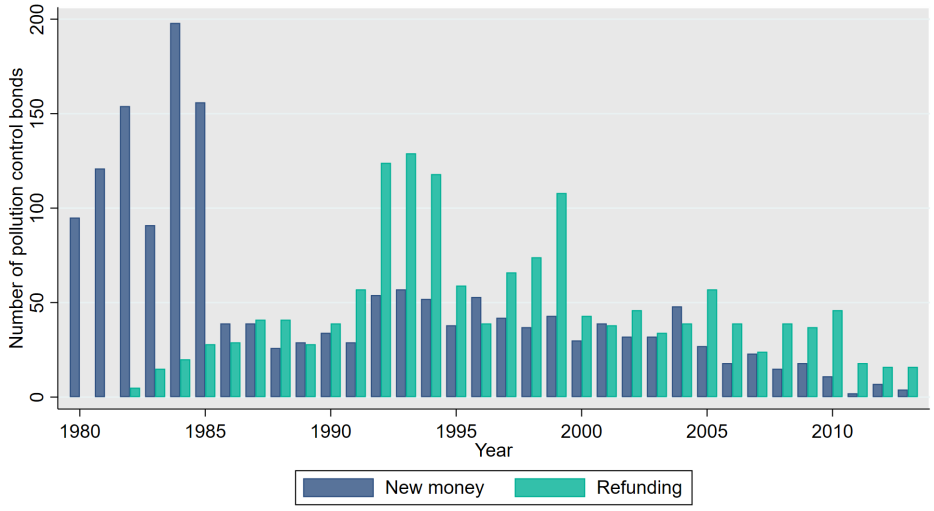
[15] In 1984, PCB issuance by all types of private sector firms also accounted for about 10% of the total issuance volume in the long-term municipal bond market (Ryan, 1985).

[16] In Appendix Figure C.1, I additionally show the development of PCB issuance volumes adjusted for inflation. This further highlights the relative issuance boom in the early 1980s.

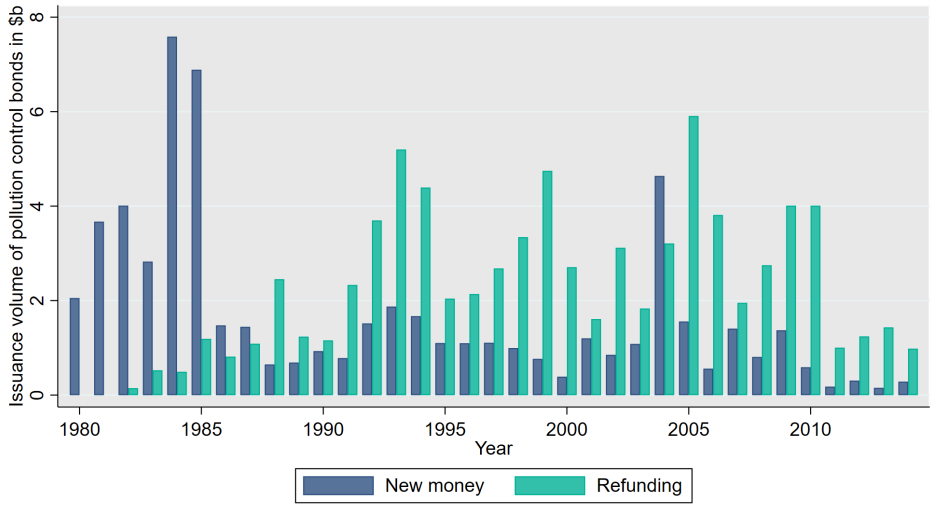
Figure 3.1
Issuance of tax-subsidized pollution control bonds over time

This figure illustrates the issuance of pollution control bonds by firms covered in Compustat over time. I separately show the issuance of new money bonds (■) and refunding bonds (■) per calendar year. Subfigure (a) shows the development of the number of bonds over the sample period from 1980 to 2013. Subfigure (b) shows the development of the total annual issuance volume.

(a) Number of pollution control bonds over time



(b) Volume of pollution control bonds over time



Since 1986, the issuance of new money PCBs has been at a much lower level than in the first half of the 1980s. This is due to regulatory reasons, as the 1986 Tax Reform Act generally restricted the issuance of municipal conduit bonds for private sector entities (see, e.g., Zimmerman, 1990, for details on the impact of the tax reform). The restrictions imposed by the 1986 Tax Reform do not affect the issuance of refunding bonds (IRC Section 146), so that 1993 represents the year with the highest refunding activity during the sample period (129 bonds, with a total issuance volume of $5.2 billion).

3.3.2 UTILIZATION BY INDUSTRY

Figure 3.2 shows total pollution control bond issuance by industry division, whereby industry divisions are determined using two-digit SIC codes. PCB issuers come primarily from two industry divisions: firms in the transportation and public utilities industry (two-digit SIC 40-49) are the major users (53% of bonds and 75% of bond volume), followed by firms in the manufacturing industry (two-digit SIC 20-39; 41% of bonds and 22% of bond volume).[17]

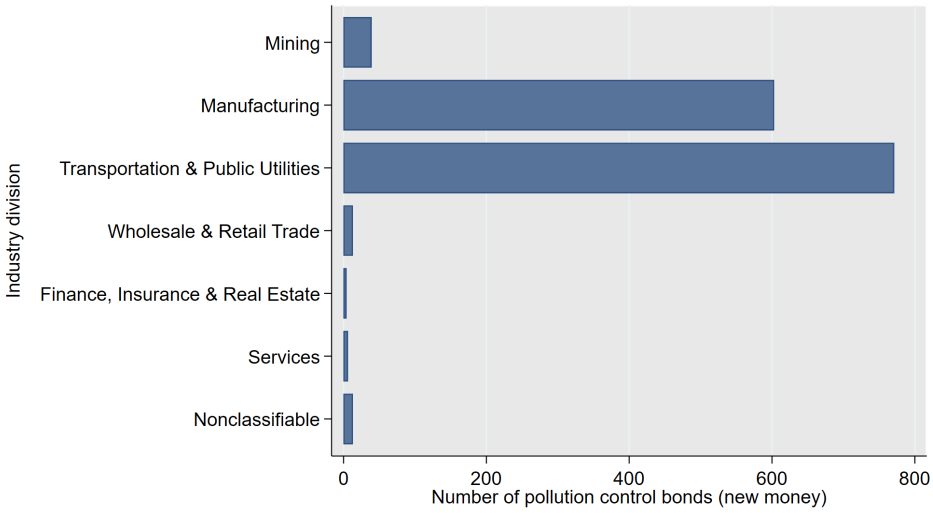
There are two notable differences in the industry distribution of PCBs compared to non-subsidized green bonds. First, PCBs are essentially only used by non-financial firms, while non-subsidized green bonds are also widely used by firms in the financial services sector (about 50% of the issuance volume according to figures provided by Flammer, 2021). The application scope of the two types of bonds differs. While there are several eligible purposes for the use of green bonds (see, e.g., International Capital Market Association, 2021), PCBs are designated for investments in (industrial) pollution abatement facilities (see, e.g., Golemon and Burgin, 1983). Second, in relative terms, manufacturing firms are more strongly represented in the PCB issuer sample than in the sample of non-subsidized green bonds used by Flammer (2021).

[17]Within the division transportation and utilities, almost all issuance is from firms in the electric, gas, & sanitary services industry (two-digit SIC code 49).

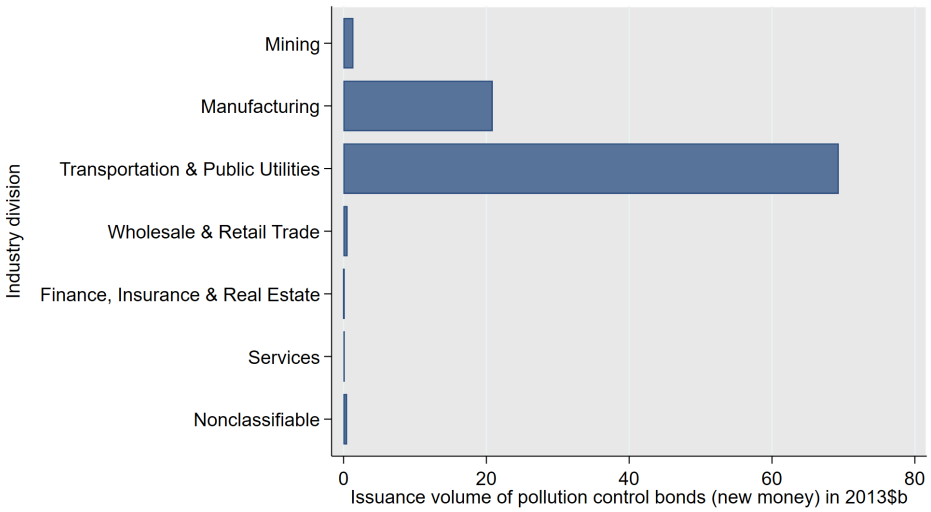
Figure 3.2
Issuance of tax-subsidized pollution control bonds by industry division

This figure illustrates the issuance of pollution control bonds at the industry division level (based on two-digit SIC codes). The sample consists of all new money PCBs issued between 1980 and 2013 by firms covered in Compustat. Subfigure (a) shows the total number of bonds by industry division. Subfigure (b) shows the total issuance volume of bonds by industry division, adjusted for inflation and expressed in 2013 USDb.

(a) Number of pollution control bonds by industry divison



(b) Issuance volume of pollution control bonds by industry division

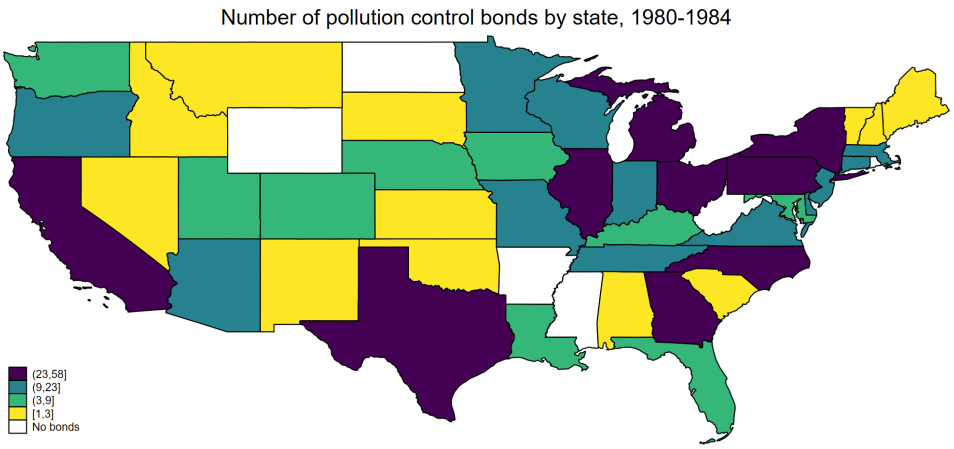


3.3.3 UTILIZATION BY STATE

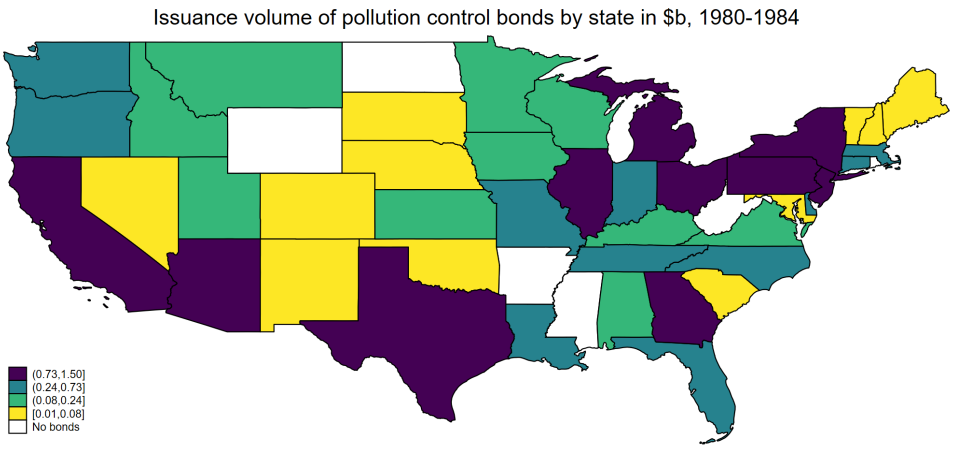
Figure 3.3
Issuance of tax-subsidized pollution control bonds by state between 1980 and 1984

This figure illustrates the issuance of pollution control bonds by state, based on the location of the issuing firms' headquarters. The sample consists of new money PCBs issued by Compustat firms between 1980 and 1984. States marked in white have no PCB issues between 1980 and 1984. States marked in yellow (■) are in the bottom quartile in terms of total state-level PCB issuance. States marked in green (■) are in the second quartile regarding total PCB issuance. States marked in blue are in the third quartile (■), while states marked in violet (■) are in the top quartile of states in terms of total PCB issuance. Subfigure (a) shows the map for the total number of bonds by state. Subfigure (b) shows the map for the total issuance volume of bonds by state (not adjusted for inflation).

(a) Number of pollution control bonds by state



(b) Issuance volume of pollution control bonds by state



For the pollution control bond dataset, Figure 3.3 provides a more detailed overview of the utilization by U.S. state. I focus on the boom period of PCB issuance between 1980 and 1984, which is the basis for additional analyses later, and which captures the use of this financing tool prior to regulatory restrictions on issuance later in the sample period.[18] For the state-level plots, I assign pollution control bonds to states based on the headquarters of the issuing firms. During the boom period of PCB issuance, Compustat firms used this type of financing in 43 states. In six states, the total issuance volume exceeded USD 1b between 1980 and 1984: Ohio is the state with the highest usage ($1.50b), followed by Texas ($1.50b), Illinois ($1.43b), California ($1.28b), New York ($1.21b), and Georgia ($1.01b).

3.3.4 FIRM SIZE AND THE USE OF POLLUTION CONTROL BONDS

The U.S. Environmental Protection Agency (1980) points out that PCBs are particularly used by large firms. I analyze how firm size correlates with PCB issuance in my sample. I therefore broadly follow the approach of Chodorow-Reich et al. (2022) and present PCB issuance statistics across the firm-size distribution. Table 3.2 shows the use of PCBs by firm size quintile, both for size quintiles across industries (Panel A) and for size quintiles within two-digit SIC code industry (Panel B). The key finding is that firm size is positively correlated with PCB issuance.

For Panel A, I divide the sample of Compustat firms into size quintiles based on a firm's average size over the sample period from 1980 to 2013. There are almost no firm years with PCB issuance in the two smallest size quintiles. For the remaining size quintiles, the summary statistics show a strong increase in PCB use with firm size. The largest size quintile captures the vast majority of PCB issuance years.

[18] Appendix Figure C.2 also illustrates issuance by state over the main sample period from 1980 to 2013.

In Panel B, I divide firms into size quintiles by industry. Even after accounting for potential firm-size differences across industries, PCB issuance increases with firm size. Considering the cost-benefit framework of Lovei (1995), large firms are likely to benefit the most from pollution control bonds as they generally derive greater benefit from pollution abatement investments. For the plant level, Becker and Henderson (2000) for instance note that the enforcement and severity of air quality regulations rise with plant size.

The fact that firm size correlates with tax-subsidized green bond issuance is also a common feature with the non-subsidized corporate green bond market: Flammer (2021) shows that green bond issuers are larger than other bond issuers.

Table 3.2
Firm size and pollution control bond issuance

This table presents summary statistics of new money PCB issuance by firm size group. In Panel A, I divide the sample of Compustat firms into five size quintiles based on a firm's average size over the sample period from 1980 to 2013. Size quintile 1 captures the group of smallest firms, while quintile 5 captures the group of largest firms. In Panel B, I split firms into size quintiles by industry. 1 refers to the smallest firms within a two-digit SIC industry, and 5 refers to the largest firms, respectively. Size is measured as the natural logarithm of total assets one plus. The table further reports the average firm size within a size quintile, the number of firm years with a PCB issuance, and the average PCB volume issued in those firm years. Appendix C.1 provides a detailed description of all variables.

	# Firm years	Total assets (mean, 2013$m)	# PCB issuance firm years	PCB volume (mean, 2013$m)
Panel A: Size quintile				
1	29,862	11.94	1	11.43
2	29,960	56.58	1	6.97
3	29,980	193.43	10	19.58
4	29,953	801.95	43	32.16
5	29,954	8,447.74	791	114.19
Panel B: Size-by-industry quintile				
1	27,834	28.53	8	20.1
2	27,803	170.35	18	34.29
3	27,846	495.07	126	73.73
4	27,869	1,432.50	170	95.23
5	27,361	7,640.86	524	125.31

3.3.5 OTHER FIRM CHARACTERISTICS AND THE USE OF POLLUTION CONTROL BONDS

I also test for the correlation between PCB bond issuance and other firm characteristics, controlling for firm size groups. The regression specification is as follows:

$$Characteristic_{i,t-1} = \alpha + \beta PCB\ dummy_{i,t} + \varphi_{size,i} + \chi_{j,t} + \psi_{s,t} + \varepsilon_{i,t} \tag{3.1}$$

whereby $Characteristic_{i,t-1}$ are a range of firm characteristics measured in the year before the PCB issuance. $PCB\ dummy_{i,t}$ is a dummy variable equal to one if the respective firm issues a new money PCB in year t, and zero otherwise. Since I am interested in ex-ante differences in firm characteristics, I only include PCB issuing firms in the years before the bond issuance. $\chi_{j,t}$ are industry-year fixed effects, and $\psi_{s,t}$ denote state-year fixed effects. I add size-by-industry quintile fixed effects ($\varphi_{size,i}$) in regressions that capture firms of all size quintiles. Standard errors are clustered at the firm level.

Table 3.3 presents results for the regression of firm characteristics on the PCB dummy.[19] The sample in Panel A comprises firms in all size-by-industry quintiles. In Panel B, the sample is restricted to firms in the largest size-by-industry quintile. Even when comparing PCB issuers to non-issuers in the same size quintile, I find that PCB issuers are larger in terms of total assets and employment. They also have more tangible assets and display a slightly lower sales growth. Yet they have similar leverage and market-to-book ratios.

[19]For completeness, Appendix Figure C.3 illustrates the unadjusted difference in means for the same set of firm characteristics.

Table 3.3
Characteristics of corporate pollution control bond issuers

This table presents the results for the regression of firm characteristics on a dummy for PCB issuance. The regression specification follows equation 3.1. The characteristic variable (dependent variable) is indicated in each column and is measured in t-1. All regressions include industry-year and state-year fixed effects. PCB $\text{dummy}_{\text{new money}}$ is a dummy variable that equals one if the respective firm issues a new money pollution control bond in year t, and zero otherwise. In Panel A, the sample comprises firms in all size-by-industry quintiles. These regressions additionally control for size-by-industry quintile fixed effects. In Panel B, the sample is restricted to firms in the largest size-by-industry quintile. T-statistics based on Huber/White robust standard errors clustered by firm are presented in parentheses. ***, ** and * indicate significance at the 1%-, 5%- and 10%-levels, respectively. Appendix C.1 provides a detailed description of all variables.

	(1) Log(Assets)	(2) Log(Emp)	(3) PPE/Assets	(4) RoA	(5) Market-to-Book	(6) Payout ratio	(7) Leverage	(8) Sales growth	(9) Cash/Assets	(10) Capex/Assets
Panel A: Firms in all size-by-industry quintiles										
PCB $\text{dummy}_{\text{new money}}$	0.621***	0.534***	0.108***	-0.084***	-0.061	0.042	-0.007	-0.038***	-0.004	0.006
	(5.911)	(6.637)	(9.512)	(-8.080)	(-1.053)	(1.198)	(-0.527)	(-6.362)	(-0.267)	(1.585)
Size-by-ind. quintile FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry x Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State x Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	125,770	115,545	116,161	125,787	104,964	116,704	125,787	115,852	116,230	115,119
Adjusted R^2	0.806	0.620	0.410	0.236	0.166	0.0479	0.0480	0.0671	0.139	0.171
Panel B: Firms in largest size-by-industry quintile										
PCB $\text{dummy}_{\text{new money}}$	0.943***	0.630***	0.077***	0.008	-0.007	0.196***	-0.023	-0.022**	-0.027**	0.013**
	(6.183)	(4.884)	(3.882)	(1.339)	(-0.099)	(3.600)	(-1.189)	(-2.002)	(-2.065)	(2.366)
Industry x Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State x Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	22,169	20,796	20,889	22,169	18,551	20,670	22,169	22,099	20,908	20,759
Adjusted R^2	0.414	0.252	0.517	0.0690	0.169	0.0183	0.0917	0.141	0.143	0.258

3.4 CONVENTIONAL CORPORATE BOND YIELDS AND POLLUTION CONTROL BOND ISSUANCE

In this section, I examine the relation between conventional bond market financing costs and the issuance of tax-subsidized pollution control bonds. I explore how conventional yields are correlated with PCB issuance at both the quarterly aggregate and the firm level. PCBs relatively reduce financing costs for pollution abatement investments compared to financing via conventional debt markets. Intuitively, this relative cost advantage should thus be particularly attractive to firms during periods characterized by high conventional financing costs. Consistent with this, Kreps (1981) anecdotally reports in The Bond Buyer that IBM made its first PCB issuance in the high-interest rate environment of 1981, quoting an IBM spokesman who states "sustained high cost of borrowing" as the underlying reason. Thus, the empirical prediction is that new money PCB issuance should be positively associated with conventional bond market yields. For refunding bonds, comparably lower interest rates should, on average, be associated with more refunding activity, so that the correlation between conventional bond yields and refunding bond issuance is likely to be negative.

3.4.1 CONVENTIONAL BOND YIELDS AND AGGREGATE PCB ISSUANCE

I test for the correlation between conventional bond yields and aggregate pollution control bond issuance at the calendar-quarter level by running the following regression:

$$PCB\ issuance_q = \alpha + \beta Conventional\ bond\ yield_{q,avg} + \gamma GDP\ per\ cap_{q,lag} + \varepsilon_q \quad (3.2)$$

whereby $PCB\ issuance_q$ is the natural logarithm of the total number of PCBs issued in the respective quarter plus one, or the total volume of PCBs plus one. I construct these measures separately for new money and refunding bonds.

Conventional bond yield$_{q,avg}$ is the quarterly average of Moody's yield for Aaa-rated corporate bonds as obtained from the Federal Reserve Bank of St. Louis. I control for lagged GDP per cap in all specifications. I also include a recession indicator and calendar-quarter number fixed effects in some specifications. Standard errors are clustered by calendar-quarter number.

Table 3.4
Conventional bond yields and aggregate pollution control bond issuance

The table reports the results of regressing measures of pollution control bond issuance on the average quarterly conventional bond market yield. All regressions are at the calendar-quarter level and follow the specification of equation 3.2. Conventional bond yield is the quarterly average of Moody's yield for Aaa-rated corporate bonds in percent, obtained from the Federal Reserve Bank of St. Louis. In Panel A, the dependent variables are the natural logarithm of the quarterly number of new money PCBs plus one (columns 1 and 2), and the natural logarithm of the quarterly total volume of new money PCBs plus one (columns 3 and 4), respectively. The sample period is from 1980 to 2013. Panel B uses the same measures of pollution control bond issuance for refunding bonds. For Panel B, the sample period is from 1983 to 2013. T-statistics based on Huber/White robust standard errors clustered by calendar-quarter number are presented in parentheses. ***, ** and * indicate significance at the 1%-, 5%- and 10%-levels, respectively. Appendix C.1 provides a detailed description of all variables.

	(1)	(2)	(3)	(4)
Panel A: Issuance of new money pollution control bonds				
	Log(Bond number$_{\text{new money}}$)		Log(Aggregate volume$_{\text{new money}}$)	
Conventional bond yield$_{\text{q, avg}}$	0.170**	0.165**	0.266***	0.249***
	(4.475)	(5.752)	(9.683)	(7.723)
Calendar-quarter nr. FE	No	Yes	No	Yes
Lag of GDP per cap	Yes	Yes	Yes	Yes
Recession indicator	No	Yes	No	Yes
Number of observations	136	136	136	136
Adjusted R^2	0.425	0.539	0.391	0.414
Panel B: Issuance of refunding pollution control bonds				
	Log(Bond number$_{\text{refunding}}$)		Log(Aggregate volume$_{\text{refunding}}$)	
Conventional bond yield$_{\text{q, avg}}$	-0.294***	-0.336***	-0.261**	-0.315*
	(-6.550)	(-7.494)	(-4.066)	(-3.177)
Calendar-quarter nr. FE	No	Yes	No	Yes
Lag of GDP per cap	Yes	Yes	Yes	Yes
Recession indicator	No	Yes	No	Yes
Number of observations	124	124	124	124
Adjusted R^2	0.102	0.168	0.0240	0.101

Table 3.4 reports the results of regressing measures of PCB issuance on the average quarterly conventional bond market yield. The table establishes two

main results. First, there is a positive correlation between conventional bond yields and new money PCB issuance. Second, there is a negative correlation between conventional bond yields and refunding PCB issuance.

Panel A presents the results for new money PCB issuance and shows that both the number of PCBs (columns 1 and 2) and the volume of PCBs (columns 3 and 4) are positively correlated with conventional bond market yields. In Panel B, I present results for the issuance of refunding bonds. Across all specifications, the conventional bond market yield is negatively correlated with measures of PCB refunding issuance.

For robustness, I examine the correlation of the quarterly average bond market yield for Baa-rated bonds with measures of PCB issuance. The results are reported in Appendix Table C.2 and support my baseline finding that there is a positive association between conventional bond yields and new money PCB issuance, and a negative association for refunding PCB issuance.

3.4.2 CONVENTIONAL BOND YIELDS AND FIRM-LEVEL PCB ISSUANCE

Next, I examine the correlation between conventional bond yields and firm-level measures of pollution control bond issuance. I thereby focus on the sample of firms with at least one PCB issuance between 1980 and 2013. All analyses include firm fixed effects so that the correlation estimates capture within-firm changes in PCB issuance relative to differences in conventional bond yields. All estimations include an indicator variable for recessions as a control variable for the macroeconomic condition. I also include lagged firm size, lagged return on assets, and lagged leverage as firm-level control variables in some of the specifications.

Table 3.5 reports the results of regressing firm-level measures of pollution control bond issuance on the average annual conventional bond market yield. The table establishes two main findings. First, the correlation of the conventional

bond yield with firm-level new money PCB issuance is positive. Second, the respective correlation with refunding bond issuance is negative. The firm-level results are thus consistent with the previous findings for aggregate issuance.

Table 3.5
Conventional bond yields and firm-level pollution control bond issuance

The table reports the results of regressing firm-level measures of pollution control bond issuance on the average annual conventional bond market yield. The sample includes Compustat firms with at least one PCB issuance during the sample period from 1980 to 2013. Conventional bond yield is the quarterly average of Moody's yield for Aaa-rated corporate bonds in percent, obtained from the Federal Reserve Bank of St. Louis. In Panel A, the dependent variables are the dummy for PCB issuance (columns 1 and 2), and the natural logarithm of the annual new money PCB volume plus one (columns 3 and 4), respectively. Panel B uses the same firm-level measures of pollution control bond issuance for refunding bonds. For Panel B, the sample period is from 1983 to 2013. Firm controls include lag of size, lag of return on assets, and lag of leverage. T-statistics based on Huber/White robust standard errors clustered by firm are presented in parentheses. ***, ** and * indicate significance at the 1%-, 5%- and 10%-levels, respectively. Appendix C.1 provides a detailed description of all variables.

	(1)	(2)	(3)	(4)
Panel A: Firm-level issuance of new money pollution control bonds				
	PCB $\text{dummy}_{\text{new money}}$		$\text{Log(PCB volume}_{\text{new money}}\text{)}$	
Conventional bond $\text{yield}_{\text{y,avg}}$	0.030***	0.041***	0.136***	0.187***
	(12.619)	(13.294)	(12.044)	(12.837)
Recession dummy	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
Firm controls	No	Yes	No	Yes
Number of observations	7,849	7,849	7,849	7,849
Adjusted R^2	0.137	0.146	0.149	0.160
Panel B: Firm-level issuance of refunding pollution control bonds				
	PCB $\text{dummy}_{\text{refunding}}$		$\text{Log(PCB volume}_{\text{refunding}}\text{)}$	
Conventional bond $\text{yield}_{\text{y,avg}}$	-0.004**	-0.005**	-0.021***	-0.025***
	(-2.242)	(-2.072)	(-3.172)	(-2.705)
Recession dummy	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
Firm controls	No	Yes	No	Yes
Number of observations	7,045	7,045	7,045	7,045
Adjusted R^2	0.0734	0.0732	0.0868	0.0867

Panel A shows firm-level results for new money bonds. In columns (1) and (2), the dependent variable is the dummy for PCB issuance. In both specifications, I find a positive and statistically significant correlation with conventional bond

yields. Focusing on column (2), a one percentage point increase in the conventional bond yield is associated with a 4.1% increase in the probability of issuing a new money PCB. In columns (3) and (4), the measure for firm-level PCB issuance is the natural logarithm of the PCB volume. The point estimates indicate that within-firm, issuance volumes are positively correlated with conventional bond yields. Panel B shows firm-level results for refunding bonds. Across all specifications, there is a negative correlation between refunding issuance measures and conventional bond yields. Overall, the results are consistent with the view that general financing conditions are correlated with the use of pollution control bonds.

3.5 POLLUTION CONTROL BOND ISSUANCE AND ITS REAL EFFECTS

Motivated by the documented positive correlation between conventional bond yields and PCB issuance, I analyze whether issuing tax-subsidized green bonds during a period of high interest rates is associated with real effects for the corporate issuers. I examine the association of PCB issuance with sales growth and corporate investment in capital and R&D, respectively. To study the real effects of PCB issuance, I use a difference-in-differences framework. As the period of interest, I use the high-interest rate period from 1980 to 1984. This period is well suited for my analyses for at least two reasons: (i) Because PCB issuance generally grew strongly during these years, many firms probably used PCBs for the first time.[20] (ii) This period is marked by particularly high conventional bond market yields within the 1980 to 2013 time window (see, e.g., Council of Economic Advisers, 2023, Table B–42).

Ideally, one would randomly have some firms issue PCBs in this high-interest rate period, and others not. I am not aware of such an experimental setting, so it is endogenous which firms are PCB issuers in the difference-in-differences

[20] IBM's first PCB issuance in 1981 is a good example. See Kreps (1981) for details.

framework. Therefore, I proceed as follows: I define treated firms as those that tap the PCB bond market during the period from 1980 to 1984. Then, to construct a counterfactual for these firms, I follow the approach of Flammer (2021) and use a nearest neighbor matching strategy, additionally requiring exact matching of some characteristics.

3.5.1 MATCHING AND DIFFERENCE-IN-DIFFERENCES SPECIFICATION

Since I examine firms that tap the PCB market between 1980 to 1984, I use the firm characteristics for 1979 as the basis for matching. While the year of issuance varies within the treatment group, this ensures that the matching characteristics predate both the boom period of PCB issuance and the high-interest rate period itself. For the matching process, I start with the group of non-PCB issuing firms in Compustat that are used as a broad comparison group for the analysis of issuers' firm characteristics (see Section 3.3).[21] These firms never issued a pollution control bond until 2013.

Starting with this sample of potential control firms, I take a two-step approach. In the first step, I impose additional data availability requirements. I remove firm years with missing information on sales growth, which is the main dependent variable in the real effects analyses. Furthermore, I require data availability for the firm in at least half of the years between 1977 and 1987.[22] In the second step, I select the variables for the matching process and conduct the matching. The selection of variables is guided by the stylized facts on the use of PCBs presented in Section 3.3. In particular, I require exact matching of the two-digit SIC code and the headquarter state. This ensures that PCBs and their matched control firms are exposed to the same industry and state-level economic conditions as well as regulatory environments (Flammer, 2021). In a robustness test, I also

[21] I already impose several restrictions on this group of firms as described in Section 3.2.1.

[22] Because of the different issue years considered, this is ultimately the time frame from which the financial data for the sample is derived.

require exact matching of the size-by-industry quintile. The nearest neighbor for each PCB issuer is then determined based on the Mahalanobis distance, which is calculated using the following five matching variables: size, return on assets, the capex-to-assets ratio, the property, plant, and equipment-to-assets ratio, and sales growth.

The matched sample comprises 149 unique new money PCB issuers and 149 control firms.[23] If a firm issues multiple bonds between 1980 and 1984, I consider only the first issue year as the treatment year. Appendix Table C.3 shows pre-issuance descriptive statistics for the group of PCB issuers and the matched control firms and compares the similarity of the two groups. There is no pre-issuance difference with respect to the outcome variables sales growth and R&D-to-assets. For capital investment, PCB issuers display a slightly higher capex-to-assets ratio. In addition, PCB issuers are larger than the control group in terms of assets and employment.[24]

For the matched sample, I consider a window of plus/minus three years around the year of PCB issuance (henceforth year 0) and run the following difference-in-differences regressions:

$$\begin{aligned} Y_{i,t} = \alpha + \beta Post\ issuance_t \cdot PCB\ issuer_{new\ \ money,i} + \gamma Post\ issuance_t \\ + \delta PCB\ issuer_{new\ \ money,i} + \xi_i + \chi_t + \psi_y + \varepsilon_{i,t} \end{aligned} \tag{3.3}$$

whereby i denotes a firm, t are event time-years around the issuance, and y is the issuance year. The outcome variable Y is either sales growth, the capex-to-assets ratio, or the R&D-to-assets ratio. *Post issuance* equals one in the three years following the PCB issuance (i.e., from year 1 to year 3). *PCB issuer* is a treatment dummy set to one if the respective firm issues a PCB bond between 1980 and 1984. I control for the lag of size, lag of return on assets, and lag of

[23] There are 189 unique PCB issuers between 1980 and 1984. However, due to the exact matching requirement for industry and state, not all of them can be matched to a control firm.

[24] Among other control variables, I control for the lag of size in all regressions.

leverage in all regressions. In some specifications, I control for state-by-issue year and industry-by-year fixed effects. Standard errors are clustered at the firm level. The variable of interest is the coefficient on the interaction term *Post issuance x PCB issuer*. In the empirical estimation, the variables *Post issance* and *PCB issuer* are absorbed by event time and firm fixed effects, respectively.

3.5.2 POLLUTION CONTROL BOND ISSUANCE AND CORPORATE SALES GROWTH

Table 3.6 presents the results of difference-in-differences regressions for the association between PCB issuance and sales growth. The key finding is that PCB issuance is positively associated with sales growth after the bond issuance. Across all specifications, the point estimate for the interaction term *PCB issuer x Post issuance* is positive and statistically significant, ranging from 0.024 to 0.030. These estimates indicate that PCB issuers display a relative increase in sales growth by 2.4% to 3.0% over the three-year post-issuance window. With an average sales growth rate in the sample of 9%, this implies a relative increase by 0.22 to 0.27 percentage points for the average firm.

Figure 3.4 illustrates the development of sales growth over the event window. Subfigure (a) shows the development of the average sales growth for PCB issuers and the matched control group from three years before to three years after the PCB issuance year (year 0). Both groups show a similar trend in sales growth up to the year of issuance.[25] After the issue year, sales growth rates continue to decline for both groups, but the decline is relatively smaller for the group of PCB issuers. Subfigure (b) shows the coefficient estimates and the 90% confidence interval on the PCB issuer dummy interacted with event time dummies. The regression specification follows column (3) in Table 3.6, which

[25]Note that sales growth is used as a matching variable when constructing the control group. The similar pre-issuance trend is thus consistent with a balancing of this characteristic in the matching process.

includes state-issue year and industry-year fixed effects beyond the baseline specification outlined in equation 3.3. There is no difference in sales growth until the PCB issuance year. The relative increase in sales growth starts in the year after the bond issue and remains almost constant until the end of the three-year post-issuance window that is examined.

Table 3.6
Pollution control bond issuance and firms' sales growth

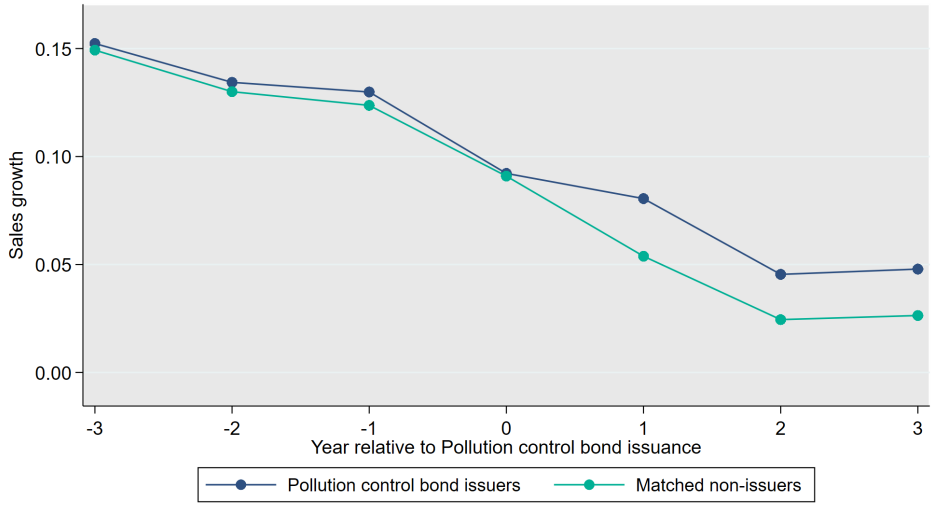
The table reports the results of difference-in-differences regressions for the association between PCB issuance and sales growth. The specification follows equation 3.3 and compares PCB issuers that tap the PCB market between 1980 and 1984 with a matched control group. The dependent variable sales growth is measured as the difference in log(sales) between year t and year t-1. Post issuance is a dummy that equals one in the three years following the firm's first PCB issuance. PCB issuer$_{\text{new money}}$ is a dummy that equals one if the firm issues any PCB between 1980 and 1984. The sample period is from three years before the PCB issuance to three years thereafter. T-statistics based on Huber/White robust standard errors clustered by firm are presented in parentheses. ***, ** and * indicate significance at the 1%-, 5%- and 10%-levels, respectively. Appendix C.1 provides a detailed description of all variables.

	(1)	(2)	(3)
	Sales growth		
PCB issuer$_{\text{new money}}$ x Post issuance	0.024**	0.030***	0.028***
	(2.436)	(2.893)	(3.257)
Lag of Size	-0.105***	-0.113***	-0.095***
	(-4.693)	(-4.881)	(-4.308)
Lag of RoA	-0.237**	-0.185	-0.271**
	(-2.012)	(-1.492)	(-1.985)
Lag of Leverage	0.085	0.106*	0.063
	(1.494)	(1.834)	(1.061)
Firm FE	Yes	Yes	Yes
Event time FE	Yes	Yes	Yes
Issue year FE	Yes	No	No
State x Issue year FE	No	Yes	Yes
Industry x Year FE	No	No	Yes
Number of observations	2,068	1,921	1,907
Adjusted R^2	0.203	0.161	0.391

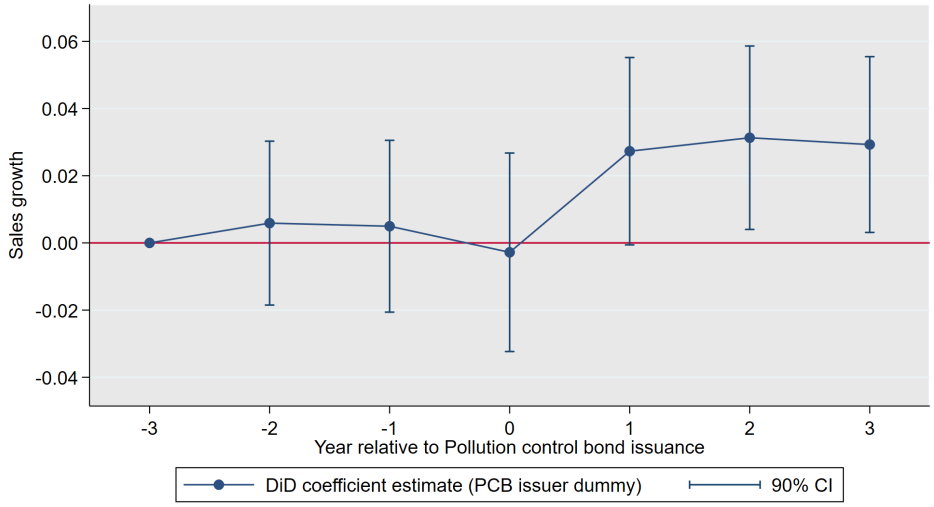
Figure 3.4
Pollution control bond issuance and firms' sales growth

This figure illustrates the development of sales growth for firms that issue pollution control bonds between 1980 and 1984, and a matched control group. The matching process is described in Section 3.5.1. Sales growth is measured as the difference in log(sales) between year t and year t-1. Subfigure (a) shows the development of average sales growth for firms issuing pollution control bonds (●) compared to the matched control group (●) over a seven year window from three years before to three years after the year of PCB issuance (year 0). Subfigure (b) shows the coefficient estimates and the 90% confidence interval on the PCB issuer dummy interacted with event time dummies. The regression specification follows column (3) in Table 3.6. Appendix C.1 provides a detailed description of all variables.

(a) Mean values of sales growth



(b) Difference in sales growth



Overall, PCB issuance appears to be positively associated with sales growth after a bond issuance during the high-interest rate period of the early 1980s. This suggests that using a low-cost source of financing for green investments can be associated with positive economic aspects for the respective firms. Note that these estimates are based on a sample of PCB issuers and a matched control group. While the matching process increases the comparability between PCB issuers and non-issuers and the groups show similar pre-issuance sales growth trends, the matching process cannot account for the selection effect associated with PCB issuance. Thus, the results do not imply a causal relation.

3.5.3 VOLUME OF POLLUTION CONTROL BONDS AND CORPORATE SALES GROWTH

To capture how the actual PCB issuance volume is associated with sales growth, I use the logarithm of the PCB issuance volume instead of the PCB issuer dummy as the issuance measure in the difference-in-differences framework. Table 3.7 presents the results. The main finding is that the PCB issuance volume is positively associated with sales growth after the bond issuance, supporting the baseline results using the PCB issuer dummy. In particular, the point estimate is positive and statistically significant in all specifications, even after controlling for industry-year trends and potential shocks at the state-issuance year level in the most restrictive specification (column 3). Appendix Figure C.4 illustrates the time dynamics for the latter specification. Again, I observe no difference in sales growth until the year of the bond issue (year 0). The relative increase in sales growth for PCB issuers begins in the year after the issuance and reaches its maximum at the end of the three-year post-issuance window that is examined.

Table 3.7
Pollution control bond issuance and firms' sales growth: effect of issuance volume

The table reports the results of difference-in-differences regressions for the association between PCB issuance volume and sales growth. The specification follows equation 3.3 but uses PCB volume as the treatment measure instead of the PCB issuer dummy. PCB volume is measured as the natural logarithm of the volume of the first PCB issuance between 1980 and 1984 plus one, and is zero for the matched control group. The dependent variable sales growth is the difference in log(sales) between year t and year t-1. Post issuance is a dummy that equals one in the three years following the firm's first PCB issuance. Firm controls are lag of size, lag of return on assets, and lag of leverage. The sample period is from three years before the PCB issuance to three years thereafter. T-statistics based on Huber/White robust standard errors clustered by firm are presented in parentheses. ***, ** and * indicate significance at the 1%-, 5%- and 10%-levels, respectively. Appendix C.1 provides a detailed description of all variables.

	(1)	(2)	(3)
	Sales growth		
Log(PCB volume$_{\text{new money}}$) x Post issuance	0.008***	0.010***	0.009***
	(2.819)	(3.269)	(3.722)
Firm controls	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes
Event time FE	Yes	Yes	Yes
Issue year FE	Yes	No	No
State x Issue year FE	No	Yes	Yes
Industry x Year FE	No	No	Yes
Number of observations	2,068	1,921	1,907
Adjusted R^2	0.204	0.162	0.392

3.5.4 ROBUSTNESS TESTS

I conduct a number of robustness tests to support my baseline finding that PCB issuance during the high-interest rate period of the early 1980s is positively associated with post-issuance sales growth. Appendix Table C.4 and Appendix Table C.5 present the results.

In the first test, I show that the positive association between PCB issuance and sales growth is robust to considering only PCB issuers that tap the PCB market between 1980 and 1982 and their matched control firms. While conventional bond market yields were still high in absolute terms in 1983 and 1984, they were somewhat lower compared to the 1981 peak for Moody's Aaa-rated bonds and the 1982 peak for Baa-rated bonds, respectively (see Council of Economic

Advisers, 2023, Table B-42, for the corresponding data).[26] The issue year restriction reduces the sample to 228 firms, with 114 PCB issuers. In all specifications, I obtain positive and statistically significant point estimates for the interaction term *PCB issuer x Post issuance* (Panel A) or *PCB volume x Post issuance* (Panel B), except for the baseline specification using the PCB issuer dummy (Panel A, column 1). Focusing on Panel A, the point estimates imply a relative increase in sales growth by 1.8% to 2.3% for PCB issuers, which is slightly less pronounced compared to the baseline estimates presented in Table 3.6.

In the second test, I show that the positive association between PCB issuance and sales growth is robust to a more restrictive construction of the control group, which also requires an exact matching of the size-by-industry quintile. Since such an exact size group match is not available for all PCB issuing firms captured in the baseline sample, the sample is reduced to 188 firms with 94 PCB issuers. For this matched sample, Appendix Table C.3, Panel B, presents the pre-issuance characteristics and between-group differences. Compared to the baseline matched sample, the additional exact matching on the size-by-industry quintile reduces the mean difference for the logarithm of total assets and employment, although the differences remain statistically significant. In all regressions, the point estimate obtained for the interaction term between the respective measure for PCB issuance and the Post issuance dummy is positive and statistically significant, even when size quintile-by-year fixed effects that control for the development of the different firm size groups over time are included (column 4).

Overall, the results of the robustness tests support the baseline finding that firms that issue PCBs during the high-interest rate period of the early 1980s display an increase in output after the bond issue.

[26] November 1982 also marks the end of the 1981 to 1982 recession period (see, e.g., Sablik, 2013).

3.5.5 POLLUTION CONTROL BOND ISSUANCE AND CORPORATE INVESTMENT

The proceeds of pollution control bonds are used for investment in pollution abatement facilities. Is pollution control bond issuance also associated with a change in capital investment or R&D investment? I use the difference-in-differences framework outlined in Section 3.5.1 to examine this relation. Capital investment is measured as capex divided by the beginning of period total assets, and R&D investment is measured as R&D expenses divided by the beginning of period total assets. I further consider $\text{R\&D investment}_{\text{imputed}}$ as a measure for which I replace missing values with zero.[27]

Table 3.8 presents the results of difference-in-differences regressions for the association between PCB issuance and corporate investment. There are two key findings. First, I find no statistically significant association of PCB issuance with capital investment. Second, I find that PCB issuance is positively associated with R&D investment post-issuance. Focusing on the result for R&D investment at the intensive margin as displayed in Panel A, column (3), the point estimate for the interaction term *PCB issuer x Post issuance* equals 0.0034 and is statistically significant. This indicates that PCB issuers display a relative increase in the R&D-to-assets ratio of 0.34 percentage points over the three-year window after the bond issuance. For the interaction term *PCB volume x Post issuance* (Panel B), the point estimate in column (3) is positive, but not statistically significant (t-stat: 1.4284).

To summarize, PCB issuance during the high-interest rate period from 1980 to 1984 appears to be positively associated with R&D investment, while I don't find any statistically significant association with capital investment. As outlined before, note that these estimates do not imply a causal relation.

[27]Due to restricted data availability for R&D and the correspondingly smaller sample size, I use industry division instead of two-digit SIC codes to construct industry-year fixed effects for all investment regressions.

Table 3.8
Pollution control bond issuance and corporate investment

The table reports the results of difference-in-differences regressions for the association between PCB issuance and capital investment (column 1), zero-imputed R&D investment (2), and R&D investment (3). Capital investment is the capex-to-assets ratio, and R&D investment is the R&D-to-assets ratio. For column 2, missing values are replaced by zero. Other than that, the specification follows equation 3.3 and compares PCB issuers that tap the PCB market between 1980 and 1984 with a matched control group. Post issuance is a dummy that equals one in the three years following the firm's first PCB issuance. PCB issuer$_{\text{new money}}$ is a dummy that equals one if the firm issues any PCB between 1980 and 1984. Log(PCB volume$_{\text{new money}}$) is the natural logarithm of the volume of the first PCB issuance between 1980 and 1984 plus one, and is zero for the matched control group. Firm controls are lag of size, lag of return on assets, and lag of leverage. The sample period is from three years before the PCB issuance to three years thereafter. T-statistics based on Huber/White robust standard errors clustered by firm are presented in parentheses. ***, ** and * indicate significance at the 1%-, 5%- and 10%-levels, respectively. Appendix C.1 provides a detailed description of all variables.

	(1)	(2)	(3)
	Capex/Assets	R&D/Assets$_{\text{imputed}}$	R&D/Assets
Panel A: PCB issuer dummy			
PCB issuer$_{\text{new money}}$ x Post issuance	-0.0069	0.0011**	0.0034*
	(-1.3200)	(2.5027)	(1.7676)
Firm controls	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes
Event time FE	Yes	Yes	Yes
State x Issue year FE	Yes	Yes	Yes
Industry division x Year FE	Yes	Yes	Yes
Number of observations	1,918	1,921	472
Adjusted R^2	0.498	0.976	0.963
Adjusted R^2 (within)	0.0823	0.0554	0.127
Panel B: PCB volume			
Log(PCB volume$_{\text{new money}}$) x Post issuance	-0.0014	0.0003**	0.0009
	(-0.9903)	(2.3507)	(1.4284)
Firm controls	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes
Event time FE	Yes	Yes	Yes
State x Issue year FE	Yes	Yes	Yes
Industry division x Year FE	Yes	Yes	Yes
Number of observations	1,918	1,921	472
Adjusted R^2	0.497	0.975	0.963
Adjusted R^2 (within)	0.0812	0.0509	0.118

3.6 CONCLUSION

This paper examines the use and the corporate real effects of green bonds that are tax-subsidized and thus a source of low-cost financing for green projects. It therefore introduces a comparably large sample of tax-exempt pollution control bonds issued between 1980 and 2013 which finance pollution control facilities of U.S. public firms.

I first present several facts about firms' use of tax-subsidized PCBs. I show that these bonds are disproportionately used by large firms, even within industries. Further, I document that PCBs are primarily used by firms in the manufacturing and utility industries. Regarding the use of PCBs over time, bond issuance peaked in the first half of the 1980s.

Then, I document that PCB issuance is correlated with the cost of conventional bond financing: New money bond issuance, which finances new projects, is positively correlated with conventional bond yields, while refunding bond issuance shows a negative correlation.

Finally, I analyze how the issuance of PCBs is associated with sales growth. Comparing firms that issue PCBs during the period of high interest rates between 1980 and 1984 with a matched control group, I find that firms using PCBs display a relative increase in sales growth after issuance. From the perspective of corporate issuers, this output effect highlights a potential positive economic aspect associated with undertaking tax-subsidized green investments.

While the focus of this paper is on the financing cost reduction aspect of PCBs, a natural follow-up question concerns the overall environmental impact of these bonds. My findings thus motivate further research on the overall environmental benefit of such bonds and, more broadly, on the role that tax-subsidized green bonds could play as part of an environmental policy mix.

4

Conclusion

The three essays in this dissertation examine research questions on municipal finance and its real effects. In the first essay, I study whether one municipality's bankruptcy exposes other local governments to the economic costs of financial contagion. To do so, I exploit bankruptcies that are unrelated to the economic trend. In the second essay, I investigate how corporate investment and employment respond to the supply of private activity bonds. I therefore utilize a legal reform and a bond distribution lottery. In the third essay, I examine the use and the corporate real effects of tax-subsidized green bonds, using a large sample of pollution control bonds. This chapter briefly summarizes the main results of the three essays and highlights their contributions and implications.

In the *first* essay, I analyze how one municipal bankruptcy filing exposes other non-bankrupt local governments to the economic costs of financial contagion. To isolate the externalities from the state-wide economic trend, I use idiosyncratic bankruptcy filings, which I identify based on a narrative approach. The sam-

ple comprises 16 idiosyncratic bankruptcies due to financial speculation, legal judgments, loss from failed public projects, and other financial mismanagement.

I first show that a municipal bankruptcy filing affects the credit market access of other non-bankrupt local governments in the state. Other local governments have a 6.3% lower probability of issuing debt in the year after the bankruptcy filing. Exploiting ex-ante heterogeneity in local governments' maturity of long-term debt, I document that local governments with a high fraction of maturing debt reduce their expenditures after the filing occurs. The negative effect on expenditures manifests two years after the bankruptcy and remains constant in the third year, and is mainly driven by a decrease in capital outlay expenditures. Finally, when analyzing how the negative externalities are transmitted to the private sector, I document a negative effect on tradable employment.

Overall, the findings of the first essay indicate that bankruptcies can have persistent negative economic consequences for other local governments that rely on debt financing. This emphasizes the importance of functioning municipal credit markets at the state level, as even temporary market disruptions may lead to a decrease in the provision of local public infrastructure and services.

The first essay relates to multiple strands of the literature. First, I contribute to the literature stream that examines municipal credit market frictions and their effects on local governments (e.g., Adelino, Cunha and Ferreira, 2017; Dagostino, 2022; Gao, Lee and Murphy, 2019), by showing that the necessity to tap the credit market shortly after a bankruptcy filing exposes other local governments to economic costs of financial contagion. Second, I add to the literature on spillover effects of bankruptcies (e.g., Benmelech and Bergman, 2011; Benmelech et al., 2019; Bernstein et al., 2019), by providing novel evidence on spillover effects of municipal bankruptcies. Third, the essay relates to the literature that analyzes and discusses bankruptcy as the resolution mechanism for municipalities (e.g., Rossi and Yun, 2024; Skeel Jr., 2013), by empirically showing that bankruptcy

filings may deteriorate the development of other municipalities that rely on debt financing.

In the *second* essay, I analyze firms' investment and employment response to the supply of private activity bonds. PABs are a subsidy tool that state and local governments can use to provide corporate beneficiaries access to the tax-exempt municipal bond market. To identify the effect of PAB supply on firm investment end employment, I utilize two settings. The first setting exploits variation in PAB supply across states, introduced by the 1986 Tax Reform Act. The second setting exploits a bond distribution lottery and thus random variation in PAB supply within one state.

I show that higher per cap PAB supply after the tax reform is positively associated with investment of PAB eligible and PAB beneficiary firms. For the sample of firms eligible for PAB financing, an additional 50 USD of per cap PAB supply leads to a 10.5% increase of the capex-to-assets ratio. With respect to the effect of PAB supply on employment, I find that subsidizing capital relative to labor through PABs is not associated with an input factor substitution. On the contrary, I find that employment significantly increases with PAB supply for both eligible and beneficiary firms, consistent with a scale effect of PAB supply on employment. Exploiting the random outcome of the lottery-based PAB distribution mechanism for the State of Texas for the program years 1996 to 2001, I document a sizeable positive investment effect of receiving PAB funding through the lottery. This finding highlights that states' project selection does not drive the results.

Overall, my results provide novel evidence on the response of firms to tax-exempt bond financing, highlighting the potentially stimulative role of tax-subsidized debt for private sector investment. Further, despite the relative subsidy for capital over labor as an input factor, my findings suggest that PAB supply does not lead to a substitution effect. Given that the corporate response

to PAB funding has been largely unexplored, my firm-level findings may provide novel insights in the context of the policy debate on private activity bonds (see, e.g., Pierog, 2017, for a recent debate), which primarily takes on a federal revenue perspective.

The findings of the second essay relate to multiple strands of the literature. First, I add to the growing literature stream on municipal finance and its real effects (e.g., Adelino, Cunha and Ferreira, 2017; Dagostino, 2022; Rossi and Yun, 2024), by focusing on the role of PAB supply and by providing novel evidence on its stimulating impact on private sector investment. Second, I contribute to the literature that analyzes government incentive programs for private sector investment and employment (e.g., Criscuolo et al., 2019; Hyman et al., 2023), by conducting a micro-econometric assessment of the PAB program. Third, the essay also relates to the broader literature on credit supply and corporate investment (e.g., Alfaro, García-Santana and Moral-Benito, 2021; Lemmon and Roberts, 2010; Zarutskie, 2006), by providing evidence on the corporate response to the supply of PABs as a source of tax-subsidized external financing. Fourth, I contribute to the literature that examines the 1986 Tax Reform Act and its impact on firms (e.g., Auerbach and Slemrod, 1997), by showing that the restrictions on private activity bonds affect firms.

In the *third* essay, I examine the use and the corporate real effects of green bonds that are tax-subsidized and thus a source of low-cost green financing. To do so, I introduce a comparably large sample of tax-exempt pollution control bonds that finance pollution abatement facilities of U.S. public firms. This sample is well suited for the analysis of tax-subsidized green bonds for three reasons: First, since PCBs are issued on the tax-exempt municipal bond market, these bonds have significantly lower financing costs than conventional corporate bonds. Second, because the proceeds of PCBs are used to finance pollution abatement facilities, this financing purpose would typically allow them to be

labeled as green under current standards. Third, my dataset allows me to examine the use of PCBs from a firm perspective by combining bond-level data with firm-level financial data, overcoming measurement challenges often present in the context of local business incentive programs.

The sample covers more than 3,000 PCBs issued between 1980 and 2013. I first present several facts about firms' use of tax-subsidized PCBs. I show that PCBs are used primarily by large firms, by large firms within industries, by firms in the manufacturing and utilities industries, and that their use peaked in the first half of the 1980s. I then document that new money PCB issuance is positively correlated with financing costs on the conventional bond market. For the sample of PCB issuing firms, I find that a one percentage point increase in the conventional bond yield is associated with a 4.1% increase in the probability of issuing a new money PCB. Examining PCB issuers that tap the PCB market during the period of high interest rates between 1980 and 1984, I show that firms using such bonds display a relative increase in sales growth after issuance.

Overall, the results provide novel insights on the use of PCBs as a fiscal policy tool that lowers the financing costs for green investment. In particular, I document that conventional bond market conditions can fuel their use, as the issuance of tax-subsidized green bonds increases when conventional bond market yields are high. From the perspective of the corporate issuers, the documented positive correlation between PCB issuance and sales growth suggests that the use of such a low-cost source of financing for pollution abatement investment can be correlated with positive economic aspects.

The third essay contributes to multiple strands of the literature. First, I contribute to the growing literature stream that examines green bonds as a relatively new financial instrument (e.g., Baker et al., 2022; Caramichael and Rapp, 2024; Flammer, 2021), by providing novel insights on a comparably large sample of tax-subsidized green bonds, covering more than 30 years of issuance.

Second, I relate to the literature that studies the real effects of green financial instruments (e.g., Fatica, Panzica and Rancan, 2021; Flammer, 2021; Tang and Zhang, 2020), by showing that PCB issuance is positively correlated with sales growth and R&D investment. Third, the essay broadly relates to the literature on financing conditions and green investments as well as environmental outcomes (e.g., Accetturo et al., 2024; Bartram, Hou and Kim, 2022; De Haas et al., 2024), by documenting that the use of tax-subsidized PCBs increases with conventional bond market yields. Finally, I add to the literature stream that studies fiscal policies targeting the private sector to improve environmental outcomes (e.g., Timilsina, 2022; Williams III, 2016), and the respective policy discussion (e.g., Delgado-Téllez, Ferdinandusse and Nerlich, 2022; Eurostat, 2015). I provide insights on the use and real effects of PCBs as a long-standing subsidy tool.

The findings of the three essays on municipal finance and its real effects presented in this dissertation motivate further research. The first essay shows that municipal bankruptcies can expose other local governments with immediate refinancing needs to the economic costs of financial contagion. The analysis is based on states in which bankruptcy filings occur, but state policies regarding Chapter 9 bankruptcy vary considerably, and Chapter 9 bankruptcy is not authorized in all states. The results motivate further research on the broader welfare implications of bankruptcy filings, also in comparison to other resolution mechanisms. In the second essay, I show that the supply of private activity bonds has a stimulating effect on the investment of beneficiaries, as well as a positive effect on firm employment. While the positive investment response for beneficiaries may spur investment of local non-recipients, it could also have competitive effects. My findings motivate further research on the impact of PAB funding on local economic development in the aggregate. The third essay provides insights on the use of tax-subsidized green bonds, drawing on the aspect of reduced financing costs associated with them. A relevant follow-up question

relates to the direct and aggregate environmental impact of these bonds. My findings motivate further research on the overall environmental benefits of pollution control bonds and, more broadly, on the role that tax-subsidized green bonds could play as part of an environmental policy mix.

A

Chapter 1

Table A.1
Variable definitions

Variable	Description
Main variables	
Post	Dummy which equals one in the three years following the municipal bankruptcy filing. Source: Own calculation.
Financial contagion window	Dummy which equals one in the year following the bankruptcy and zero otherwise. Source: Own calculation.
Bankruptcy state	Dummy which equals one for states that exhibit a bankruptcy and zero for their neighboring states without a bankruptcy. Source: Own calculation.
Issuer dummy	Dummy which equals one if the local government issues long-term debt (item: ltdissallother) and zero otherwise. Source: GovFin, own calculation.
Issued debt amount	Natural logarithm of the amount of long-term debt issued plus one (item: ltdissallother, $k). Source: GovFin.
Maturing debt	A local government's share of maturing debt as a fraction of its beginning of period outstanding debt in the year following a municipal bankruptcy filing. The fraction of maturing long-term debt is calculated as: ltdretallother/(ltdoutallother + ltdretallother- ltdissallother). The fraction is set to zero if the beginning of period outstanding debt equals zero. Source: GovFin, own calculation.
Maturing $debt_{mean}$	County-level mean of Maturing debt of the local governments located in the county. Source: GovFin, own calculation.
Total expenditures	Natural logarithm of a local government's expenditures plus one (item: totalexpenditure, $k). At the county level, total expenditures is the sum of the local governments' expenditures located in the county. Source: GovFin.
Current expenditures	Natural logarithm of a local government's current expenditures plus one (item: totalcurrentexpend, $k). Source: GovFin.
Capital outlay expenditures	Natural logarithm of a local government's capital outlay expenditures plus one (item: totalcapitaloutlays, $k). Source: GovFin.
County business characteristics	
Total employment	Natural logarithm of the number of employees in all industry sectors plus one. Source: CBP.
Industry employment	Natural logarithm of the number of employees for a denoted industry sector plus one. Source: CBP.
Total establishment	Natural logarithm of the number of establishments in all industry sectors plus one. Source: CBP.
Industry establishment	Natural logarithm of the number of establishments for a denoted industry sector plus one. Source: CBP.
Other variables	
State GDP per cap	State-level gross domestic product divided by the state population. Source: Bureau of Economic Analysis, U.S. Bureau of the Census' Population and Housing Unit Estimates.

Continued on next page

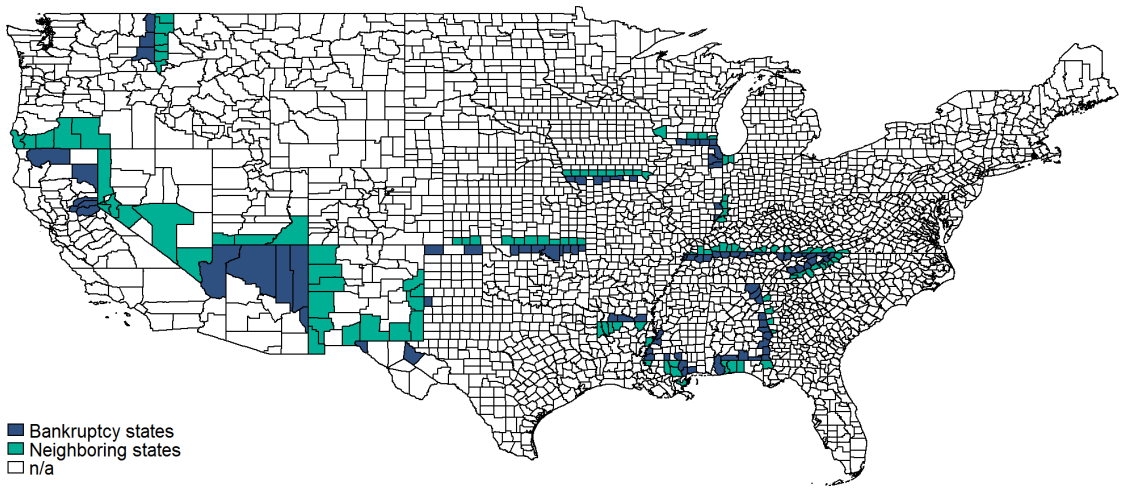
Appendix A.1 continued

Variable	**Description**
House price index	House price index for the respective county-year. Missing data points are imputed with the state-level house price index. Source: Federal Housing Finance Agency.
Intergovernmental revenue	Natural logarithm of a local government's or county's intergovernmental revenues (item: totaligrevenue, $k). Missing data are imputed with state-year average values. Source: GovFin.

CBP stands for the U.S. Census Bureau's County Business Pattern. *GovFin* stands for the U.S. Census Bureau's Annual Survey of State and Local Government Finance.

Figure A.1
Geography of bordering counties in bankruptcy states and neighboring states

This figure illustrates the geographic distribution of bordering counties in bankruptcy states (■) as well as bordering counties in neighboring states (■) for which debt issuance data on the local government-level is available throughout the event period, which runs from three years before the bankruptcy filing to three years thereafter. Counties marked in white are not included in the state border county sample.



NARRATIVE ANALYSIS OF MUNICIPAL BANKRUPTCY FILINGS

Historical records for the narrative approach include documents from the following four sources: (i) If available, we extract news coverage from the New York Times as a major nationwide newspaper. (ii) Second, we attempt to collect articles published by The Bond Buyer as an important newspaper focusing on the municipal bond market. (iii) As the third source, we make use of articles available in local newspapers which generally report on the area of the bankrupt municipality. We access those articles mainly via Nexis Uni. (iv) Finally, we extract disclosure statements of the bankrupt municipalities filed with the respective bankruptcy courts, if available. To do so, we make use of Bloomberg BNA.

The bankruptcy filing of Orange County, CA, in 1994

Short description

Orange County filed for bankruptcy on December 6, 1994, a few days after the information became public that the county's investment pool had incurred massive losses, later estimated at about $1.7 billion. The investment pool's loss was ascribed to the risky investment strategy the fund pursued.

Historical records

1. Norris (1994) in The New York Times describes the reason related to the filing as follows: "*Orange County, a suburban area south of Los Angeles that is more than twice the size of Long Island's Nassau County, filed for bankruptcy late Tuesday, after heavy borrowing and risky investments in its investment pool turned into big losers as market interest rates rose.*"

2. O'Donnell (1994) in The Bond Buyer further describes the surprising filing from the perspective of a municipal bond trader. In particular, the newspaper notes: "*The bankruptcy is particularly unsettling because it happened to Orange County, one of the richest counties in the nation, the trader said. "It's kind of like: Wait a minute. We look at our triple B hospitals, but we didn't think we had to worry about double-A Orange County, Calif.," he said. Another trader agreed, saying the county's predicament is surprising because of the level of sophistication there.*"

3. Local newspapers like the Orange County Register (Pasco, Knap and Kalfus, 1994) describe the filing reason similarly to The New York Times reporting: "*Orange County filed for bankruptcy Tuesday after failing to prevent a billion-dollar default in the county investment pool. The crisis surfaced publicly five days earlier when grim-faced county officials announced that the $8 billion pool managed by county Treasurer Robert L. Citron had lost about $1.5 billion of its value because of risky investments and increased interest rates.*"

4. Newspapers also cover the disruption of the municipal credit market subsequent to Orange County's bankruptcy filing. Austin Tobin, for instance, is quoted by Norris (1994) in The New York Times, saying that "*There is no market in California issues. […] There probably won't be one for a couple of days.*" Similarly, Richard Lehmann is quoted by Petruno (1994) in The Los Angeles Times, indicating that "*This is going to shake confidence in the entire bond market*".

B

Chapter 2

Table B.1
Variable definitions

Variable	Description
State-level caps for PABs	
Post-1986	Dummy which equals one in the four years following the 1986 Tax Reform Act, i.e., from 1987 to 1990.
Per cap PAB supply	Limit to the per capita amount of private activity bonds in USD that a state may distribute for calendar years 1988 to 2000. Source: Own calculation based on data from IRC Section 146, IRS and U.S. Bureau of the Census Population and Housing Unit Estimates.
PAB issuing county	County with any PAB issuance by a local government during the period from 1976 to 1985. Source: U.S. Census Bureau's Annual Survey of State and Local Government Finance.
PAB issuance	
Log (PAB issuance $\text{volume}_{\text{county}}$)	Natural logarithm of one plus the county-level sum of PAB issuance volumes by all local governments in the respective county. Source: U.S. Census Bureau's Annual Survey of State and Local Government Finance.
Log (Firm-level PAB volume)	Natural logarithm of one plus a firm's aggregate PAB volume for the respective year. Source: SDC Platinum.
Post_{1986} beneficiary firm	Firm for which any PAB is issued during the period 1987 to 1990. Source: SDC Platinum.
Pre and Post_{1986} beneficiary firm	Firm for which any PAB is issued during the period 1987 to 1990 and during the period 1983 to 1986. Source: SDC Platinum.
PAB allocation dummy	Dummy which equals one if the firm receives any PAB allocation in the respective year, and zero otherwise. Source: SDC Platinum.
PAB allocation dummy x Year FE	PAB allocation dummy interacted with year fixed effects. Source: SDC Platinum.
House price index	County-level house price index. Missing data points are imputed with the state-level house price index. Source: Federal Housing Finance Agency.
Texas PAB lottery	
Lottery win dummy	Dummy equal to one if a firm that participates in the Texas PAB lottery wins any of its lottery (project) attempts in the respective program year, and zero otherwise. Source: Own calculation based on data from the Texas Bond Review Board.
Log (Lottery-allocated bond volume)	Natural logarithm of one plus the total PAB volume allocated to a firm in the Texas PAB lottery in a program year. Source: Own calculation based on data from the Texas Bond Review Board.
Firm characteristics	
Log (Capex / Assets)	Natural logarithm of capex (item: capx) divided by beginning of period total assets (item: at). Source: Compustat.
Log (Employment)	Natural logarithm of one plus the number of employees (item: emp). Source: Compustat.

Continued on next page

Appendix B.1 continued

Variable	**Description**
Lag of Size	Natural logarithm of one plus total assets (item: at), lagged by one period. Source: Compustat.
Lag of RoA	Net income (item: ni) divided by total assets (item: at), lagged by one period. Source: Compustat.
Lag of Leverage	Long term debt (item: dltt) plus debt in current liabilities (item: dlc) divided by book value of common equity (item: ceq) plus long-term debt and debt in current liabilities, lagged by one period. Source. Compustat.
Log (Capex)	Natural logarithm of capex (item: capx). Source: Compustat.
Capex/Assets	Capex (item: capx) divided by the beginning of period total assets (item: at). Source: Compustat.
PPE growth	Difference between Property, plant, and equipment (item: ppent) of the current year and the pre-year, divided by the pre-year value. Source: Compustat.
Market-to-book	Total assets (item: at) minus book value of common equity (item: ceq) plus market value of common equity (items: prcc_f multiplied by csho), divided by total assets (item: at). Source: Compustat.
Payout ratio	Sum of dividends on preferred stock (item: dvp), dividends on common stock (item: dvc), and purchase of common and preferred stock (item: prstkc), divided by income before extraordinary items (item: ib). Source: Compustat.
Industry x Year FE	Historic SIC-level-2 industry group interacted with year fixed effect. Source: Compustat.
Industry x Post_{1986} FE	Historic SIC-level-2 industry group interacted with the Post-1986 dummy. Source: Compustat.

Table B.2
Per cap PAB supply and firm investment: PAB supply measure and county issuer sample definition

The dependent variable is the natural logarithm of capital expenditures divided by the beginning of period total assets. In Panel A, the setup is as in Table 2.4, but Per cap PAB supply_{1987} is the limit to the per capita amount of private activity bonds in USD that a state may distribute for the transition year 1987. In Panel B, the setup is as in Table 2.4, but alternative definitions for the sample of PAB issuing counties are used. Firm controls are lag of size and lag of return on assets. Post is a dummy that equals one from 1987 to 1990, and zero otherwise. The sample period is from 1983 to 1990. T-statistics based on Huber/White robust standard errors clustered by state are presented in parentheses. ***, ** and * indicate significance at the 1%-, 5%- and 10%-levels, respectively. Appendix B.1 provides a detailed description of all variables.

	(1)	(2)	(3)
	Log(Capex/Assets)		
Panel A: PAB supply for transition year 1987			
Per cap PAB supply_{1987} x Post-1986	0.0011***	0.0010***	0.0012***
	(4.883)	(4.773)	(3.300)
Lag of Size	-0.4144***	-0.4788***	-0.4807***
	(-6.297)	(-7.619)	(-9.972)
Lag of RoA		1.1046***	1.0545***
		(7.294)	(7.291)
Firm FE	Yes	Yes	Yes
Year FE	Yes	Yes	No
State border pair x Post FE	Yes	Yes	Yes
Industry x Year FE	No	No	Yes
Number of observations	4,094	4,073	4,059
Adjusted R^2	0.488	0.507	0.525
Panel B: PAB issuing county definition			
	Any issuance 1983-1986	Any iss. 1983-1986 and 1987-1990	Non-issuer 1976-1985
Per cap PAB supply x Post-1986	0.0022***	0.0027***	0.0047
	(3.640)	(4.490)	(0.415)
Firm controls	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes
State border pair x Post FE	Yes	Yes	Yes
Industry x Year FE	Yes	Yes	Yes
Number of observations	3,487	2,766	2,590
Adjusted R^2	0.521	0.518	0.483

Table B.3
Per cap PAB supply and firm investment: other firm investment measures

The dependent variable is indicated in each column and represents an alternative firm investment measure. Apart from that, the setup is as in Table 2.4. The sample consists of firms headquartered in bordering counties in which any local government issued at least one PAB in the ten years before the 1986 Tax Reform. Per cap PAB supply is the limit to the per capita amount of private activity bonds in USD that a state may distribute for calendar years 1988 onward. Firm controls are lag of size and lag of return on assets. Post is a dummy that equals one from 1987 to 1990, and zero otherwise. The sample period is from 1983 to 1990. T-statistics based on Huber/White robust standard errors clustered by state are presented in parentheses. ***, ** and * indicate significance at the 1%-, 5%- and 10%-levels, respectively. Appendix B.1 provides a detailed description of all variables.

	(1)	(2)	(3)
	Log (Capex)	Capex/Assets	PPE growth
Per cap PAB supply x Post-1986	0.0024***	0.0002**	0.0004
	(4.837)	(2.246)	(1.014)
Firm controls	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes
State border pair x Post FE	Yes	Yes	Yes
Industry x Year FE	Yes	Yes	Yes
Number of observations	4,157	4,157	4,211
Adjusted R^2	0.954	0.380	0.107

Table B.4

Per cap PAB supply and investment of PAB beneficiary firms after the 1986 Tax Reform: beneficiary firms in 1987 and 1988

The table repeats Table 2.5, but defines post-reform beneficiary firms as those that receive any PAB in the two years after the reform instead of in the four years after the reform. Hence, in columns (1) and (2), the sample consists of firms that receive any PAB in the two years after the 1986 Tax Reform. Columns (3) and (4) additionally require any PAB issuance in the four years before the tax reform. The dependent variable is the natural logarithm of capital expenditures divided by the beginning of period total assets. Per cap PAB supply is the limit to the per capita amount of private activity bonds in USD that a state may distribute for calendar years 1988 onward. Post is a dummy that equals one from 1987 to 1990, and zero otherwise. T-statistics based on Huber/White robust standard errors clustered by state are presented in parentheses. ***, ** and * indicate significance at the 1%-, 5%- and 10%-levels, respectively. Appendix B.1 provides a detailed description of all variables.

	(1)	(2)	(3)	(4)
	Log(Capex/Assets) for			
	$Post_{1986}$ beneficiaries		Pre and $Post_{1986}$ benefic.	
Per cap PAB supply x Post-1986	0.0038***	0.0044*	0.0048**	0.0064***
	(3.489)	(1.905)	(2.690)	(3.405)
Firm controls	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
PAB allocation dummy x Year FE	Yes	Yes	Yes	Yes
Industry x $Post_{1986}$ FE	No	Yes	No	Yes
Number of observations	527	527	358	358
Adjusted R^2	0.539	0.568	0.576	0.606

Table B.5

Per cap PAB supply and investment of PAB beneficiary firms after the 1986 Tax Reform: extended post-reform period

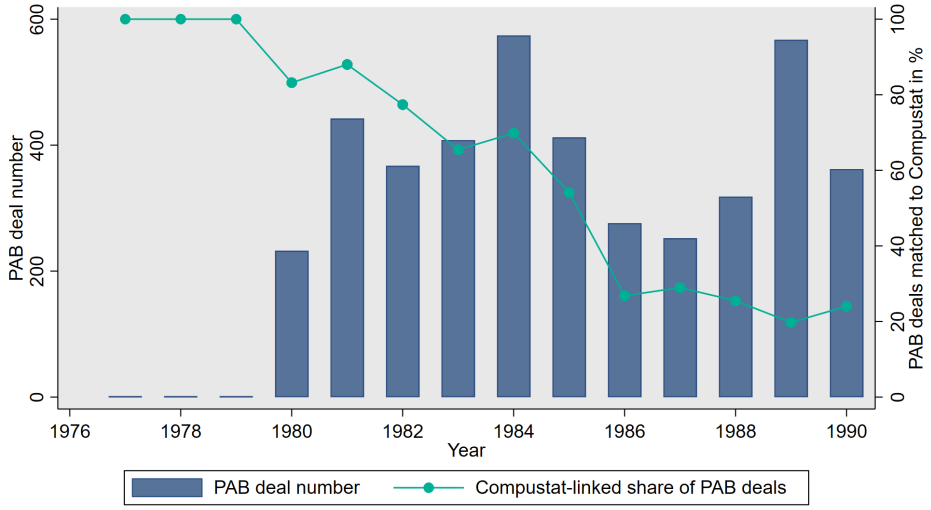
The table repeats Table 2.5, but uses an extended post-reform window of seven years instead of four years. Hence, the sample period is from 1983 to 1993. Post is a dummy that equals one from 1987 to 1993, and zero otherwise. In columns (1) and (2), the sample consists of firms that receive any PAB in the four years after the 1986 Tax Reform. Columns (3) and (4) additionally require any PAB issuance in the four years before the reform. The dependent variable is the natural logarithm of capital expenditures divided by the beginning of period total assets. Per cap PAB supply is the limit to the per capita amount of private activity bonds in USD that a state may distribute for calendar years 1988 onward. T-statistics based on Huber/White robust standard errors clustered by state are presented in parentheses. ***, ** and * indicate significance at the 1%-, 5%- and 10%-levels, respectively. Appendix B.1 provides a detailed description of all variables.

	(1)	(2)	(3)	(4)
	Log(Capex/Assets) for			
	$Post_{1986}$ beneficiaries		Pre and $Post_{1986}$ benefic.	
Per cap PAB supply x Post-1986	0.0038***	0.0044**	0.0053***	0.0058***
	(3.779)	(2.564)	(3.292)	(2.955)
Firm controls	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
PAB allocation dummy x Year FE	Yes	Yes	Yes	Yes
Industry x $Post_{1986}$ FE	No	Yes	No	Yes
Number of observations	1,438	1,437	714	714
Adjusted R^2	0.579	0.594	0.611	0.629

Figure B.1
Compustat-linked PAB beneficiary firms in the SDC database

This figure illustrates the proportion of new money PABs obtained from SDC Platinum that can be linked to Compustat within the period from 1977 to 1990. Further, it shows SDC's overall coverage of new money PAB deals during this period. Subfigure (a) illustrates properties for the number of deals, while Subfigure (b) captures deal volumes.

(a) PAB deals and Compustat PAB beneficiaries



(b) PAB deal volume and Compustat PAB beneficiaries

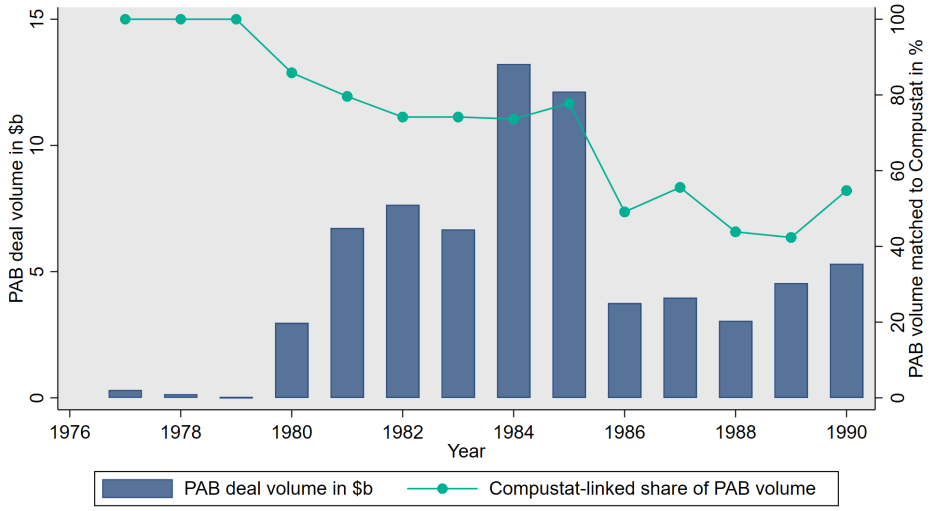


Figure B.2
Per cap PAB supply and firm-level PAB volumes after the 1986 Tax Reform

This figure illustrates trends in firm-level PAB allocation around the 1986 Tax Reform Act. The dependent variable is the natural logarithm of the aggregate PAB allocation volume at the firm level. The figure shows the coefficient estimates and 90% confidence interval on the Per cap PAB supply measure interacted with year dummies. Per cap PAB supply is the limit to the per capita amount of private activity bonds in USD that a state may distribute for calendar years 1988 onward. The regression specification corresponds to column (4) in Table 2.3, bottom panel. The effective date for the PAB volume caps mandated by the 1986 Tax Reform Act is shown in red (- -). Appendix B.1 provides a detailed description of all variables.

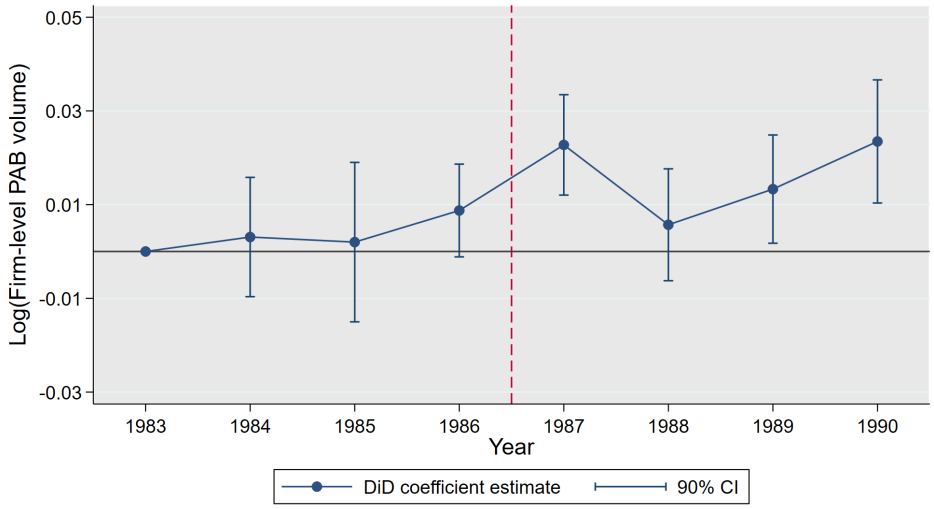
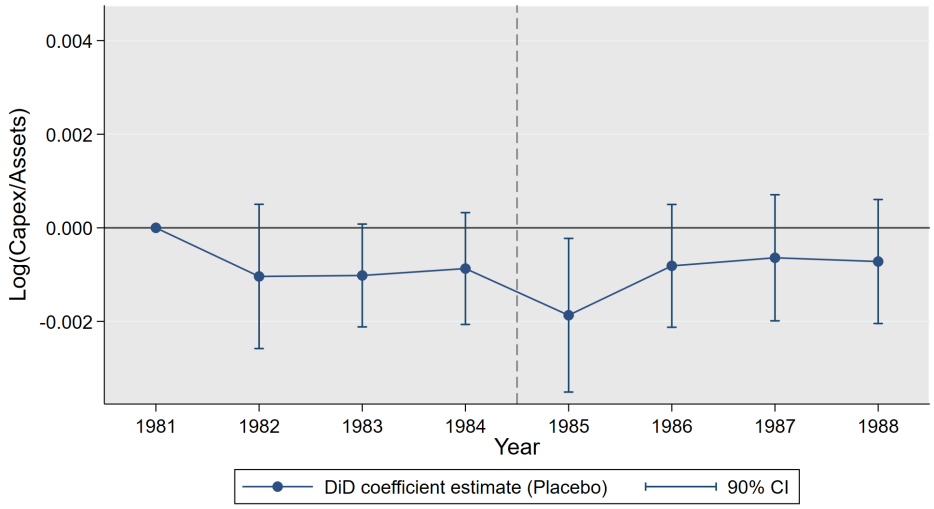


Figure B.3

Placebo test: Per cap PAB supply and firm investment after the 1984 Deficit Reduction Act

This figure shows the results of a placebo test of PAB supply on investment of PAB eligible firms. It illustrates the development of firm investment for firms located in border counties around the 1984 Deficit Reduction Act, using the thereby mandated, non-restrictive PAB volume caps to calculate per cap PAB supply. Section 2.2.1 provides background information on the 1984 caps on PABs. The figure shows coefficient estimates and the 90% confidence interval on the Per cap PAB supply_{1985} measure interacted with year dummies over the placebo event period from 1981 to 1988. Except for these differences, the regression specification follows column (3) in Table 2.4. The effective date for the PAB volume caps mandated by the 1984 Deficit Reduction Act is shown in gray (- -). Appendix B.1 provides a detailed description of all variables.



ALLOCATION OF PRIVATE ACTIVITY BONDS IN TEXAS

As put forward in IRC Section 146, states have the power to set up their own allocation scheme for PABs within their volume limits (see also Internal Revenue Service, 2019), or they can make use of the proposed scheme as described in the respective Section. The latter denotes that 50% of a state's ceiling should be allocated to state issuers, and the remaining 50% to local issuers based on relative local population figures. Zimmerman (1990) provides a comprehensive overview of both the state agencies responsible for the cap allocation and of the allocation priorities within states as of 1989. Allocation priorities vary in terms of the distribution among state and local governments as conduit issuers, and with respect to industry and project types.

For the State of Texas, the Texas Bond Review Board (BRB) administers the private activity bond allocation program since 1992 (Texas Bond Review Board, 2023b). Chapter 1372 of the Texas Government Code is the relevant legal basis. The state volume is allocated to several subceilings for an initial period of about 8 months. The focus of my analyses is on the impact of PABs on corporate beneficiaries from the Compustat universe. Therefore, the relevant sub-ceilings are the one for qualified small issue bonds (formerly industrial development bonds, IDB) and empowerment zone bonds, and the subceiling for any other issues, under which exempt facility bonds fall (Texas Bond Review Board, 1996). The reserved percentage share for the subceilings varies over time. For 1996, the category covering any other issues received 42% of the total volume, and small issue bonds received 7.5% (Texas Bond Review Board, 1996). Under the current version of Chapter 1372, the ceiling for any other issues comprises 29.5%, and small issues get a reservation amount of 2%.[1]

Within any of the subceilings, the priority of PAB projects is generally determined based on a lottery. Applicants, therefore, must submit an allocation request before the respective lottery application deadline, which currently is on October 20 before the respective PAB program year starts (see Texas Bond Review Board, 2023a, for a detailed timeline of the lottery program and further details on the distribution mechanism).

The Texas PAB lottery results provide information on the status of the application request, the conduit issuer, and the project, amongst others. Due to the common over-subscription of the program, I can leverage the data from the Texas PAB lottery to compare investment among a set of firms that applied for PAB allocation, but only a sub-set receives an allocation.

[1] As noted by the Texas Bond Review Board (1997), the 29.5% limit for the subceiling "all other issues" category was introduced with calendar year 1998.

Chapter 3

Table C.1
Variable definitions

Variable	Description
Measures for pollution control bond issuance	
Bond number$_{\text{new money}}$	Sum of new money PCBs issued during the indicated time period. Source: SDC Platinum.
Aggregate volume$_{\text{new money}}$	Sum of new money PCB issuance volume during the indicated time period. Source: SDC Platinum.
PCB dummy$_{\text{new money}}$	Dummy which equals one if the firm issues a PCB new money bond in the respective year. Source: SDC Platinum.
PCB volume$_{\text{new money}}$	Firm-level issuance volume of new money PCBs. Source: SDC Platinum.
PCB issuer$_{\text{new money}}$	Dummy which equals one if the firm issues any new money PCB between 1980 and 1984. Source: SDC Platinum.
Bond number$_{\text{refunding}}$	Sum of refunding PCBs issued during the indicated time period. Source: SDC Platinum.
Aggregate volume$_{\text{refunding}}$	Sum of refunding PCB issuance volume during the indicated time period. Source: SDC Platinum.
PCB dummy$_{\text{refunding}}$	Dummy which equals one if the firm issues a PCB refunding bond in the respective year. Source: SDC Platinum.
PCB volume$_{\text{refunding}}$	Firm-level issuance volume of refunding PCBs. Source: SDC Platinum.
Issue year	Year of the respective PCB bond issuance. Source: SDC Platinum.
State x Issue year FE	Year of the respective PCB bond issuance interacted with state fixed effects. Source: SDC Platinum, Compustat.
Post issuance	Dummy which equals one in the three years following the issuance of a PCB.
Conventional bond yield measures and control variables	
Conventional bond yield$_{\text{avg}}$	Annual or quarterly average of Moody's yield for Aaa-rated corporate bonds in percent. Source: Federal Reserve Bank of St. Louis.
Conventional BAA-rated bond yield$_{\text{avg}}$	Annual or quarterly average of Moody's yield for Baa-rated corporate bonds in percent. Source: Federal Reserve Bank of St. Louis.
Lag of GDP per cap	Quarterly real gross domestic product per capita, measured in chained 2012 dollars, lagged by one quarter. Source: Federal Reserve Bank of St. Louis.
Recession indicator$_{\text{q}}$	Dummy equal to one if the quarterly NBER-based recession indicator for the U.S. equals one. Source: Federal Reserve Bank of St. Louis.
Recession indicator$_{\text{y}}$	Dummy equal to one if the NBER-based recession indicator for the U.S. equals one in any of the respective year's quarters. Source: Federal Reserve Bank of St. Louis.
Calendar-quarter nr.	Numeric indicator for the respective calendar-year quarter.
Main firm characteristics	
Lag of Size	Natural logarithm of one plus total assets (item: at), lagged by one period. Source: Compustat.

Continued on next page

Appendix C.1 continued

Variable	**Description**
Lag of RoA	Operating income before depreciation (item: oibdp) divided by total assets (item: at), lagged by one period. Source: Compustat.
Lag of Leverage	Long term debt (item: dltt) plus debt in current liabilities (item: dlc) divided by book value of common equity (item: ceq) plus long-term debt and debt in current liabilities, lagged by one period. Source: Compustat.
Sales growth	Difference between log(sales) in year t and log(sales) in year t-1 (sales refers to item: sale). Source: Compustat.
Size quintile	Size group number based on a firm's average size over the sample period from 1980 to 2013. Size quintile 1 captures the group of smallest firms, while quintile 5 captures the group of largest firms. Source: Compustat.
Size-by-industry quintile	Size group number by two-digit SIC code industry, based on a firm's average size over the sample period from 1980 to 2013. 1 refers to the smallest firms within a two-digit SIC industry, and 5 refers to the largest firms. Source: Compustat.
Additional firm characteristics	
Capex/Assets	Capex (item: capx) divided by the beginning of period total assets (item: at). Source: Compustat.
R&D/Assets$_{\text{imputed}}$	R&D expenses (item: xrd) divided by the beginning of period total assets (item: at). Missing values are set to zero. Source: Compustat.
R&D/Assets	R&D expenses (item: xrd) divided by the beginning of period total assets (item: at). Source: Compustat.
Log(Employment)	Natural logarithm of one plus the number of employees (item: emp). Source: Compustat.
Cash/Assets	Cash and short-term investments (item: ceq) divided by the beginning of period total assets (item: at). Source: Compustat.
PPE/Assets	Property, plant, and equipment (item: ppent) divided by the beginning of period total assets (item: at). Source: Compustat
Market-to-book	Total assets (item: at) minus book value of common equity (item: ceq) plus market value of common equity (items: prcc_f multiplied by csho), divided by total assets (item: at). Source: Compustat.
Payout ratio	Sum of dividends on preferred stock (item: dvp), dividends on common stock (item: dvc), and purchase of common and preferred stock (item: prstkc), divided by income before extraordinary items (item: ib). Source: Compustat.
Industry x Year FE	Two-digit SIC industry code interacted with year fixed effects. Source: Compustat.

Table C.2

Conventional Baa-rated bond yields and pollution control bond issuance

The table reports the results of regressing measures of pollution control bond issuance on the average quarterly conventional bond market yield for Moody's Baa-rated corporate bonds. All regressions are at the calendar-quarter level and follow the specification of equation 3.2. In Panel A, the dependent variables are the natural logarithm of the quarterly number of new money PCBs plus one (columns 1 and 2), and the natural logarithm of the quarterly total volume of new money PCBs plus one (columns 3 and 4), respectively. The sample period is from 1980 to 2013. Panel B uses the same measures of pollution control bond issuance for refunding bonds. For Panel B, the sample period is from 1983 to 2013. T-statistics based on Huber/White robust standard errors clustered by calendar-quarter number are presented in parentheses. ***, ** and * indicate significance at the 1%-, 5%- and 10%-levels, respectively. Appendix C.1 provides a detailed description of all variables.

	(1)	(2)	(3)	(4)
Panel A: Issuance of new money pollution control bond				
	Log(Bond number$_{new}$)		Log(Aggregate volume$_{new}$)	
Conventional BAA-rated bond yield$_{q,\ avg}$	0.112*	0.109**	0.207***	0.201**
	(3.127)	(4.042)	(9.055)	(5.676)
Calendar-quarter nr. FE	No	Yes	No	Yes
Lag of GDP per cap	Yes	Yes	Yes	Yes
Recession indicator	No	Yes	No	Yes
Number of observations	136	136	136	136
Adjusted R^2	0.412	0.525	0.390	0.411
Panel B: Issuance of refunding pollution control bonds				
	Log(Bond number$_{ref}$)		Log(Aggregate volume$_{ref}$)	
Conventional BAA-rated bond yield$_{q,\ avg}$	-0.277***	-0.350***	-0.243**	-0.319**
	(-7.466)	(-8.325)	(-4.912)	(-3.387)
Calendar-quarter nr. FE	No	Yes	No	Yes
Lag of GDP per cap	Yes	Yes	Yes	Yes
Recession indicator	No	Yes	No	Yes
Number of observations	124	124	124	124
Adjusted R^2	0.137	0.230	0.0346	0.122

Table C.3
Matching of PCB issuers to other non-PCB issuing Compustat firms

This table presents the mean pre-period characteristics and differences in means for firms that issue a pollution control bond between 1980 and 1984, and a matched control group. All figures are calculated after the matching is conducted. The pre-period corresponds to the three years before the respective year of the PCB issue. The matching process is described in Section 3.5.1. In Panel A, the comparison is displayed for the main control group. Panel B captures an alternative control group that additionally requires exact matching based on size-by-industry quintile. ***, ** and * indicate significance at the 1%-, 5%- and 10%-levels, respectively. Appendix C.1 provides a detailed description of all variables.

	PCB issuing firms		**Matched control firms**			
	N	Mean	N	Mean	Diff.	T-stat
Panel A: Issuers and main control group						
Variables used in the matching process						
Lag of Size	447	7.46	447	6.41	1.05***	(12.90)
Lag of RoA	447	0.14	447	0.14	-0.00	(-1.09)
Capex/Assets	447	0.12	447	0.11	0.02***	(5.06)
PPE/Assets	447	0.80	447	0.70	0.10***	(6.07)
Sales growth	447	0.14	447	0.13	0.00	(0.58)
Other variables						
Lag of Leverage	447	0.49	447	0.48	0.01	(0.94)
Log(Employment)	445	2.34	444	1.79	0.55***	(6.80)
Cash/Assets	447	0.04	447	0.04	-0.01	(-1.51)
Payout ratio	435	0.67	430	0.63	0.04	(1.25)
Market-to-book	388	1.03	400	1.07	-0.04*	(-1.80)
R&D/Assets, imputed	447	0.01	447	0.01	0.00	(0.58)
R&D/Assets	134	0.03	102	0.03	-0.01	(-1.50)
Panel B: Issuers and control group with additional matching on size-by-industry quintile						
Variables used in the matching process						
Lag of Size	282	7.49	282	7.01	0.48***	(-5.39)
Lag of RoA	282	0.14	282	0.14	0.00	(-0.69)
Capex/Assets	282	0.13	282	0.11	0.02***	(-4.22)
PPE/Assets	282	0.79	282	0.69	0.09***	(-4.20)
Sales growth	282	0.14	282	0.14	-0.01	(-0.82)
Other variables						
Lag of Leverage	282	0.48	282	0.48	0.01	(-0.48)
Log(Employment)	280	2.44	279	2.16	0.27***	(-2.67)
Cash/Assets	282	0.04	282	0.04	0.00	(-0.36)
Payout ratio	270	0.68	263	0.61	0.07**	(-2.06)
Market-to-book	243	1.04	260	1.08	-0.04	(-1.34)
R&D/Assets, imputed	282	0.01	282	0.01	0.00	(-0.37)
R&D/Assets	92	0.03	85	0.03	0.00	(-0.09)

Table C.4
Pollution control bond issuance and firms' sales growth: issue year between 1980 and 1982

The table reports the results of difference-in-differences regressions for the association between PCB issuance and sales growth. The sample consists of PCB issuers that tap the PCB market between 1980 and 1982, and their matched control firms, following the matching procedure described in Section 3.5.1. Other than that, the specification follows equation 3.3. Post issuance is a dummy that equals one in the three years following the firm's first PCB issuance. For Panel A, the treatment measure PCB issuer$_{80,81,82}$ is a dummy that equals one if the firm issues any new money PCB between 1980 and 1982. For Panel B, the treatment measure is the natural logarithm of the volume of the first new money PCB issuance between 1980 and 1982 plus one, and is zero for the matched control group. Firm controls are lag of size, lag of return on assets, and lag of leverage. The sample period is from three years before the PCB issuance to three years thereafter. T-statistics based on Huber/White robust standard errors clustered by firm are presented in parentheses. ***, ** and * indicate significance at the 1%-, 5%- and 10%-levels, respectively. Appendix C.1 provides a detailed description of all variables.

	(1)	(2)	(3)
	Sales growth		
Panel A: PCB issuer dummy			
PCB issuer$_{80,81,82}$ x Post issuance	0.016	0.023*	0.018*
	(1.443)	(1.944)	(1.905)
Firm controls	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes
Event time FE	Yes	Yes	Yes
Issue year FE	Yes	No	No
State x Issue year FE	No	Yes	Yes
Industry x Year FE	No	No	Yes
Number of observations	1,580	1,461	1,447
Adjusted R^2	0.213	0.176	0.408
Panel B: PCB volume			
Log(PCB volume$_{80,81,82}$) x Post issuance	0.007**	0.009***	0.007**
	(2.216)	(2.747)	(2.391)
Firm controls	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes
Event time FE	Yes	Yes	Yes
Issue year FE	Yes	No	No
State x Issue year FE	No	Yes	Yes
Industry x Year FE	No	No	Yes
Number of observations	1,580	1,461	1,447
Adjusted R^2	0.215	0.179	0.409

Table C.5

Pollution control bond issuance and firms' sales growth: other control group

The table reports the results of difference-in-differences regressions for the association between PCB issuance and sales growth. The control group for this table also requires exact matching based on size-by-industry quintile in addition to the control group characteristics described in Section 3.5.1. Other than that, the specification follows equation 3.3. The dependent variable sales growth is the difference in log(sales) between year t and year t-1. Post issuance is a dummy that equals one in the three years following the firm's first PCB issuance. In Panel A, the treatment measure PCB issuer$_{\text{new money}}$ is a dummy that equals one if the firm issues any PCB between 1980 and 1984. In Panel B, the treatment measure is the natural logarithm of the volume of the first PCB issuance between 1980 and 1984 plus one, and is zero for the matched control group. Firm controls are lag of size, lag of return on assets, and lag of leverage. The sample period is from three years before the PCB issuance to three years thereafter. T-statistics based on Huber/White robust standard errors clustered by firm are presented in parentheses. ***, ** and * indicate significance at the 1%-, 5%- and 10%-levels, respectively. Appendix C.1 provides a detailed description of all variables.

	(1)	(2)	(3)	(4)
	Sales growth			
Panel A: PCB issuer dummy				
PCB issuer$_{\text{new money}}$ x Post issuance	0.024*	0.030**	0.027**	0.029**
	(1.916)	(2.267)	(2.403)	(2.552)
Firm controls	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
Event time FE	Yes	Yes	Yes	Yes
Issue year FE	Yes	No	No	No
State x Issue year FE	No	Yes	Yes	Yes
Industry x Year FE	No	No	Yes	Yes
Size-by-industry quintile x Year FE	No	No	No	Yes
Number of observations	1,306	1,188	1,172	1,170
Adjusted R^2	0.212	0.165	0.385	0.404
Panel B: PCB volume				
Log(PCB volume$_{\text{new money}}$) x Post issuance	0.009**	0.010***	0.009***	0.009***
	(2.498)	(2.738)	(3.021)	(3.087)
Firm controls	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
Event time FE	Yes	Yes	Yes	Yes
Issue year FE	Yes	No	No	No
State x Issue year FE	No	Yes	Yes	Yes
Industry x Year FE	No	No	Yes	Yes
Size-by-industry quintile x Year FE	No	No	No	Yes
Number of observations	1,306	1,188	1,172	1,170
Adjusted R^2	0.214	0.167	0.387	0.405

Figure C.1
Inflation-adjusted issuance volume of pollution control bonds over time

This figure illustrates the volume of pollution control bonds issued over time, with the volume of issuance adjusted for inflation and expressed in 2013 USDb. I separately show the issuance of new money bonds (■) and refunding bonds (■) per calendar year.

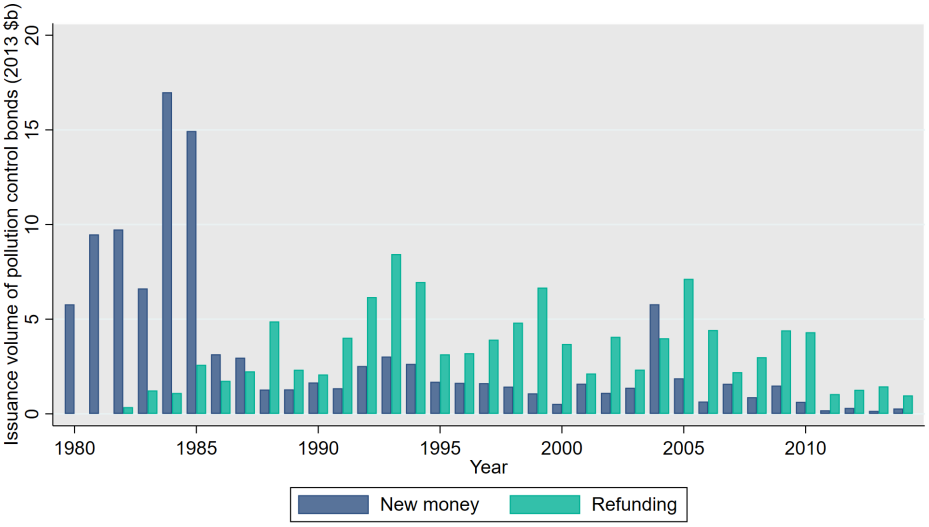
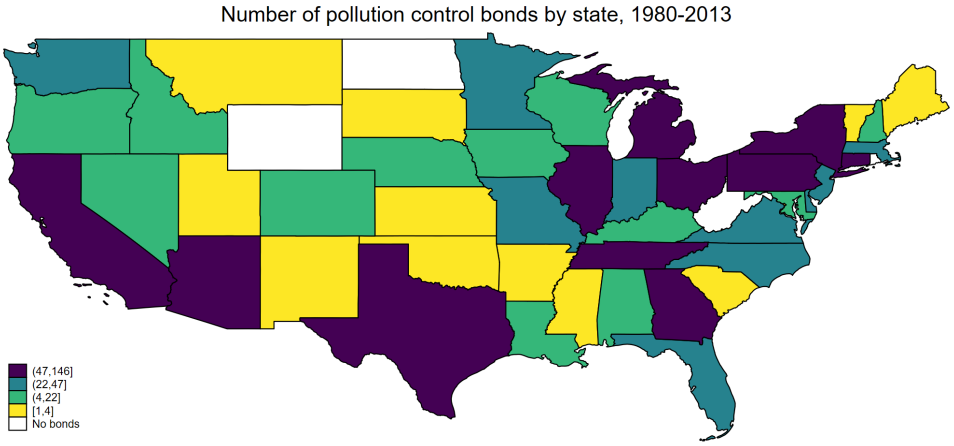


Figure C.2
Issuance of tax-subsidized pollution control bonds by state between 1980 and 2013

This figure illustrates the issuance of pollution control bonds by state, based on the location of the issuing firms' headquarters. The sample consists of new money PCBs issued by Compustat firms between 1980 and 2013. States marked in white have no PCB issues between 1980 and 2013. States marked in yellow (■) are in the bottom quartile in terms of total state-level PCB issuance. States marked in green (■) are in the second quartile regarding total PCB issuance. States marked in blue are in the third quartile (■), while states marked in violet (■) are in the top quartile of states in terms of total PCB issuance. Subfigure (a) shows the map for the total number of bonds by state. Subfigure (b) shows the map for the total issuance volume of bonds by state (adjusted for inflation and expressed in 2013 USDb).

(a) Number of pollution control bonds by state



(b) Issuance volume of pollution control bonds by state

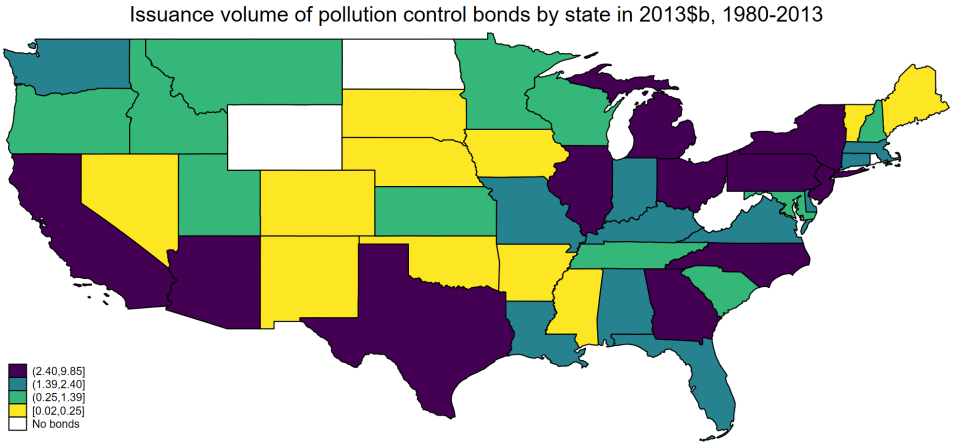


Figure C.3
Characteristics of pollution control bond issuers

This figure illustrates the unadjusted difference in means of firm characteristics between firms that issue a pollution control bond between 1980 and 2013, and firms covered by Compustat that do not. The bars mark the 90% confidence intervals. To measure differences in characteristics prior to issuance, all characteristic variables are measured in t-1. Appendix C.1 provides a detailed description of all variables.

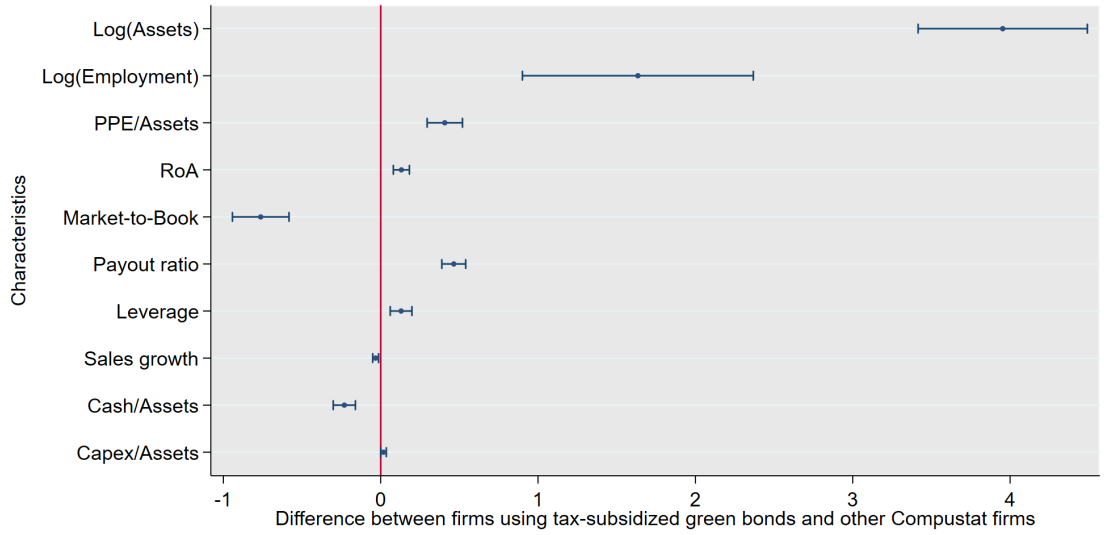
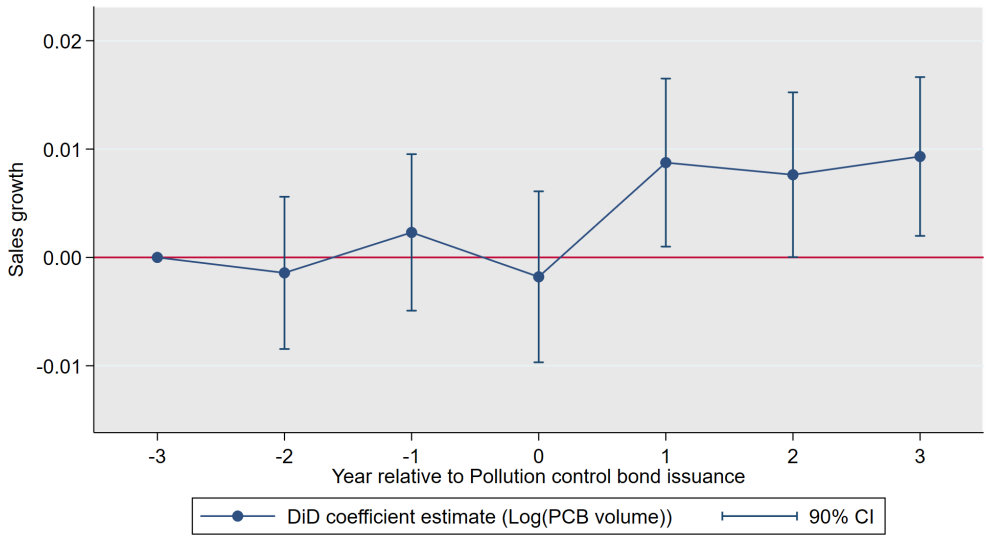


Figure C.4
Pollution control bond issuance and firms' sales growth: effect of issuance volume

This figure illustrates the development of sales growth for firms that issue a pollution control bond between 1980 and 1984, relative to a matched control group. It shows coefficient estimates and the 90% confidence interval on the logarithm of the PCB volume interacted with event-time dummies. The regression specification follows column (3) in Table 3.7. Sales growth is measured as the difference in log(sales) between year t and year t-1. Appendix C.1 provides a detailed description of all variables.



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