Communities as Stakeholders:

Impact of Corporate Bankruptcies on Local Governments

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Abstract

We provide new evidence that the bankruptcy filing of a locally-headquartered and publicly-listed manufacturing firm imposes externalities on the local governments. Compared to matched counties with similar economic trends, municipal bond yields for affected counties increase by 10 bps within a year of the firm's bankruptcy filing. Counties that are more economically dependent on the industry of the bankrupt firm are more affected and do not immediately recover from the negative impact of the corporate bankruptcy. Our results highlight that local communities are major stakeholders in public firms and how they are adversely affected by corporate financial distress.

Keywords: Corporate Bankruptcy, Municipal Debt, Stakeholders

JEL Classification: G33, H74

1 Introduction

While extensive literature in finance has studied how corporate decisions impact various stakeholders of firms like customers, workers, and suppliers (Titman, 1984), less attention has been paid to the impact on local governments. Local communities are essential stakeholders of firms because they provide factors of production (Alchian and Demsetz, 1972; Jensen and Meckling, 1976) and firms require their cooperation to create and capture value (Baker, Gibbons, and Murphy, 2002; Dougal, Parsons, and Titman, 2015). In fact, business taxes account for more than one-third of revenues of local governments (Census Bureau Annual Survey of Local Government Finance, 2019). In this paper, we analyze the impact of the bankruptcy filing of a locally headquartered public manufacturing firm on the bond yields of the affected counties to shed light on the externalities imposed on the local governments.

It is not clear how a local firm's bankruptcy affects the local economies and local government finances. When growth options partially reside in workers' human capital, firm financial distress may boost entrepreneurship (Grossman and Hart, 1986; Hart, 1995; Rajan and Zingales, 2001). Employees may draw upon valuable ideas or projects from financially distressed firms to launch their enterprise. For example, Hacamo and Kleiner (2022) find that labor market declines can lead to more firms and better firms. If the human capital of the bankrupt firm is redeployed productively, the area may attract new residents and businesses. This "creative destruction" may result in higher tax revenue for the affected counties and lower credit risk. Alternatively, as part of the bankruptcy process, firms typically implement many cost-saving policies, including laying off workers. These worker layoffs may reduce local demand and future cash flows to the county, resulting in an increase in local government's bond yields (Auh, Choi, Deryugina, and Park, 2022)[1].

[1]For example, the decline of Detroit with the fall of auto manufacturing highlights the hardships communities face. In contrast, Rochester recovered from Kodak's financial distress (Deaux, 2013)

We find that bankruptcy filing of a publicly listed manufacturing firm leads to a 10% increase in secondary market yields for the headquarter counties, compared to a matched county with similar ex-ante economic trends. We find similar results using new issuance yields from the primary market. This negative impact is amplified when the county is more dependent (based on the share of wages and employment) on the industry of the bankrupt firm. Local gross domestic product (GDP) growth declines more for the affected counties. Using various proxies for a city's appeal to workers (Dougal, Parsons, and Titman, 2022), entrepreneurship, start-up formation rate, patents, and mobility across counties, we find limited evidence supporting the creative destruction argument.

Identifying the causal impact of the announcement of a corporate bankruptcy filing on local governments' borrowing costs is challenging since we cannot observe the counterfactual. Additionally, it is possible that a deterioration in local economic conditions and local demand may have been responsible for the firm filing for a bankruptcy. We attempt to ameliorate these concerns by focusing on the impact of a manufacturing firm's bankruptcy on the headquarter (HQ) county and other locations where the firm has a significant presence. Publicly listed manufacturing firm bankruptcies are less likely to stem from local economic conditions, as they are less dependent on the local demand for their revenue in the HQ county or other plant locations[2]. Second, we use nearest neighbor method to match each HQ county where manufacturing firm filed for bankruptcy to another observably similar county based on the level and changes in the unemployment rate, labor force, and ex-ante average municipal bond yields as the counterfactual county. We provide evidence for the validity of the identifying assumption, i.e., the treated county and the matched control county follow similar economic trends before the shock.

We use firm bankruptcy data during 2006-2016 using Chava, Stefanescu, and Turnbull (2011) which was extended in Alanis, Chava, and Kumar (2018). In our primary

[2]Additionally, the role manufacturing plays in the US economy remains an important public policy issue. According to the Bureau of Economic Analysis (BEA), in 1998, the manufacturing sector accounted for 16% of the GDP by employing 17.6 million workers. In 2019, the manufacturing sector's GDP share reduced to 11% with the employment of 12.8 million workers.

analysis, we use secondary market trades for 162,740 municipal bonds of the counties with HQ of bankrupt manufacturing firms. We estimate an event-study style difference-in-differences regression with event fixed effects (i.e., treatment-control county-pair fixed effects), county fixed effects, and calendar-time month fixed effects (to control for declining trends in the yields during our sample period). First, we confirm that the yields of municipal bonds of treated and control counties follow similar trends before the shock. We find that the difference between the two groups is statistically insignificant before the announcement of a bankruptcy filing by a publicly-listed manufacturing firm (hereafter "firm bankruptcy"). Next, we find that within a quarter after the announcement of firm bankruptcy, there is an upward trend in the yields for bonds of treated counties, but there is no change in the yields for the bonds of control counties. The tax-adjusted yield spreads for treated counties increased by 10.01 basis points (bps) compared to the control counties within 36 months after the firm bankruptcy announcement. The median credit spread between AA- and AAA-rated municipal bonds in the sample is 93 basis points. This suggests that the average yield increase after the bankruptcy filing represents $\sim$ 10% (=10.01/93) of the credit spread.

In robustness checks, we identify the control counties using additional considerations such as county-level debt capacity measures and primary market bond characteristics like the average amount issued and average maturity of bonds to control for underlying differences in treated and control counties. We find consistent results when we impose a geographic restriction of identifying the control counties from within the same region in the US and when we consider matching to three nearest neighbors. We also test if declining local consumer demand derives our results. A similar pre-trend for treatment and control counties in consumer bankruptcies and a significant increase in consumer bankruptcies after firm bankruptcy suggests a decline in local demand may not have been responsible for the firm filing for bankruptcy.

In other robustness specifications, we show results after accounting for the average per capita county income and house prices to control for the local economic conditions. We also control for bond fixed effects to absorb bond-specific unobservables in a restrictive specification. To control for county and region-specific unobservables, we include the county and region/region-year fixed effects in some specifications. We also find similar results when we expand our sample of bankrupt firms beyond the manufacturing sector by including firms in the tradeable sector. Also, our results are robust if we only consider trades after the financial crisis of 2008-2009.

In addition to HQ counties, we also analyze the impact on other plant locations of the bankrupt firms. We utilize the plant location data from the Environmental Protection Agency (EPA) and Mergent Intellect. We find that most of the impact is limited to the location of the firm's headquarters. Next, we sort firm locations based on the number of employees at the site. We find a more significant increase in municipal bond yields for the counties with 25% or more operations (measured using the number of employees at the site). We focus on secondary market trades to avoid any confounding endogeneity due to market timing in the new municipal bond issuance market. Our baseline results include multiple controls specific to the bond. For example, we control for coupon rate, issuance size, remaining maturity, callability, bond insurance, and type of security based on bond repayment source (tax sources for general obligation bonds and specific revenue stream for revenue bonds). Further, we utilize county-specific controls, including lagged levels and changes in the unemployment rate and labor force.

The impact of a large firm's bankruptcy may spill over to competitors or suppliers. For example, Lang and Stulz (1992) shows intra-industry negative stock reactions to a competitor's bankruptcy filing. Using the fraction of county-level employment and county-level wages in the industry of the bankrupt firm, we capture the county's exposure to the bankrupt firm's industry. Our results on municipal bond yield spreads suggest that most treated counties with high (above median) exposure drive the results. Finally, we use an alternative way to understand the firm's impact on the county. To this end, we construct three measures to proxy for the relative size of the bankrupt firm vis-à-vis the HQ county. Here again, we find that relatively larger firms that may be more economically significant to the county drive the results.

We leverage county-level exposure analysis to show that revenue bonds backed by project-specific revenues drive the overall effect. In contrast, general obligation bonds backed by the overall tax revenues of the counties are not affected because of higher revenue diversification. This is consistent with the cash flows based impact of natural disasters in Auh, Choi, Deryugina, and Park (2022). To further examine this result, we also evaluate the heterogeneity in secondary market yield spreads due to the type of project financed by the bond. We find significant increase in yields for improvement-and-development related bonds used to finance infrastructure projects in the treated county. This finding further supports our evidence linked to project-specific revenue bonds.

Finally, we test the implication of higher borrowing costs reflected by the secondary market yields. First, we find that compared to 6 months before a firm bankruptcy, the new municipal bond issuance for the treated counties increases by about 1.5 times in the year after the firm bankruptcy. For the control counties, however, this increase is about two times. We find that the treatment counties' offering yields (after-tax) on new bonds issued increase by about six bps compared to the control group. Overall, we find an increase in the unemployment rate and a decline in county GDP growth for the local economy. This effect is more for treated counties with high (above median) exposure to the industry of bankrupt firms. Importantly, we do not find evidence for "creative destruction" after a manufacturing firm's bankruptcy. Using metrics for county-level attractiveness based on education, weather (Dougal, Parsons, and Titman, 2022), entrepreneurship, and innovation, we do not find support for recovery after firm bankruptcy.

Our paper relates to the literature discussing shareholder vs. stakeholder theories

(Friedman, 1970; Zingales, 2000; Freeman, 2010).[3] Moreover, Baker, Gibbons, and Murphy (2002) and Dougal, Parsons, and Titman (2015) document the role of firms in urban vibrancy, while our paper highlights the importance of firms for the local area. We show the adverse impact of bankruptcy filings by public firms on municipal bond yields. Our paper also relates to the recent academic literature documenting how firms that file for bankruptcies impact competitors, industry peers (Benmelech, Bergman, Milanez, and Mukharlyamov, 2019), and local employment (Bernstein, Colonnelli, Giroud, and Iverson, 2019). However, the evidence on the impact of firm bankruptcies on local governments' borrowing costs and municipal bond issuance is limited. In this paper, we identify a new channel by which bankrupt firms impose negative externalities on local communities through their impact on the yields of municipal bonds issued by local governments, school districts, hospitals, and public service agencies. To our knowledge, this is the first paper that documents how large firms' financial distress adversely impacts financing cost for local communities. In this regard, we also contribute to the recent literature on municipal bonds (Adelino, Cunha, and Ferreira, 2017; Schwert, 2017; Gao, Lee, and Murphy, 2020, 2019; Babina, Jotikasthira, Lundblad, and Ramadorai, 2021; Adelino, Cheong, Choi, and Oh, 2021; Auh, Choi, Deryugina, and Park, 2022; Gao, Lee, and Murphy, 2022).

The rest of the paper proceeds as follows. We discuss our empirical methodology and identification concerns in Section 2. Section 3 provides details about our data and relevant summary statistics. Our main empirical results are presented in Section 4, followed by our conclusion in Section 5.

[3] Friedman (1970) argued that the social responsibility of business is to increase its profits and maximizing shareholder value benefits other stakeholders of the firm. Consistent with this view, the Business Roundtable, an association of chief executive officers of America's leading companies, has emphasized shareholder primacy since 1997. However, policymakers and academics have revisited the idea of maximizing shareholder value (Edmans, 2020). Accordingly, Business Roundtable updated its view that redefined the purpose of corporations as benefiting all their stakeholders.

2 Identification Challenges and Empirical Methodology

In this section, we first discuss the challenges in identifying the impact of firm bankruptcies on local communities' municipal borrowing costs and then describe our empirical specification.

2.1 Identification Challenges

The first econometric challenge in identifying firm bankruptcy's impact on local governments is whether the underlying local economic conditions drive firms to file for bankruptcy. In that case, certain omitted variables (e.g., reduced local demand) could impact the firm and the corresponding county's municipal bond yields. To overcome this threat, we specifically focus on bankruptcies in the manufacturing sector, arguably relatively exogenous to demand in the local area. In the US, manufacturing firms, especially large ones, are less likely to depend on local demand to sell their finished products. Further, we also drop counties that experience bankruptcies filed by multiple publicly listed firms in the same calendar year in any sector. We argue that these potentially are linked to unobserved county-level unobservable trends and, thus, exclude them from our sample.

Since we do not observe the treated counties' counterfactual scenario, we use a matching strategy to identify a suitable control group. To this end, we first show the kernel density plot for the matching variables between the treated and control counties. The patterns look very similar, with no appreciable difference between the two groups. This affords us considerable comfort in matching the counties. Further, we evaluate the robustness of our matching considerations by including additional variables based on debt capacity, primary market issuance, and geographic considerations. We find consistent results using all these approaches (see Section A.1). We also verify the kernel density plots between the treated and control groups for these additional matching strategies.

We also provide evidence from consumer bankruptcies suggesting that deteriorating local economic conditions do not drive our main result. In short, we find no statistical difference in the number of consumer bankruptcies between the treated and control counties before the firm bankruptcy event. The result suggests that local economic conditions were not deteriorating before the bankruptcy filing (see Section 4.1.2).

Finally, there may be a concern for our primary dependent variable–bond yield spreads. Unobserved factors at the county level other than the bankruptcy filing may drive the impact on yields. To address this, we evaluate and verify the pre-trends in the bond yield spreads between the treatment and control counties before the bankruptcy filing. We show that there is no statistical difference during the quarters before the bankruptcy filing. Since we do not find a significant difference in the bond yields before the event dates, we derive more comfort about our identifying assumption. The control groups do indeed represent a suitable counterfactual to the treated counties. Also, the bond yield spreads do not demonstrate any deviation before the filing dates (see Section 4.1.1).

2.2 Methodology

Our baseline event study focuses on the impact of firm bankruptcies on local governments' borrowing costs. Identifying the causal impact in this setting is challenging since we cannot observe what would have happened to the county's municipal bond yields if the firm had not filed for bankruptcy. To overcome the lack of a ready counterfactual available to us, we use the nearest neighbor in Euclidian distance as the control county based on the level of and changes in the unemployment rate and labor force, along with ex-ante average municipal bond yields. We discuss robustness to additional matching considerations to our baseline in Section A.1. We identify 128 treatment-control event pairs of bankruptcy filings during 2006-2016, spanning 43 states in the US. We restrict our sample period based on the availability of ex-ante municipal bond yields in the secondary market at the beginning and county-level information on ex-ante unemployment and labor for the ending year. We use a three-year window before and after the bankruptcy filings. We use secondary market trades as the baseline case because these bonds are already trading in the event county pairs at the time of the bankruptcy filing. This also mitigates any concerns with filing-related bond issuance driving our results.

Using a standard difference-in-differences approach between the treatment and control counties' bond yields in the secondary market for municipal bonds results in the baseline specification as below:

$$y_{i,c,e,t} = \alpha + \beta_0 * Treated_{i,c,e} * Post_{i,c,e,t} + \beta_1 * Treated_{i,c,e} + \beta_2 * Post_{i,c,e,t} \tag{1}$$
$$+ BondControls + CountyControls + \eta_e + \delta_c + \gamma_t + \epsilon_{i,c,e,t}$$

where index i refers to bond, c refers to county, e denotes the county event pair, and t indicates the year-month. The dependent variable $y_{i,c,e,t}$ is after-tax yield spread and is obtained from secondary market trades in local municipal bonds (described in Section 3). *Treated* is a dummy equal to one for a county where the headquarter of the firm filing bankruptcy is located. This dummy equals zero for the control county in that event pair. Our baseline specification uses a three-year event window around the firm bankruptcy filing. *Post* represents a dummy that is assigned a value of one for months after the bankruptcy is filed and zero otherwise. The coefficient of interest is β_0. The baseline specification also includes three sets of fixed effects: county event pair fixed effects (η_e), so the comparisons are within bonds mapped to a treated-control pair; δ_c, corresponding to county fixed effects; and γ_t, denoting year-month fixed effects to control for time trends. We follow Bergstresser, Cohen, and Shenai (2013); Gao, Lee, and Murphy (2020) to include the amount issued, coupon rate, a dummy for the status of bond insurance, a dummy for the type of bid (competitive versus negotiated), and a dummy based on general obligation versus revenue bond security type, collectively represented as

BondControls. *CountyControls* refers to a vector of county-level measures to control local economic conditions. It includes a log of the lagged value of the labor force in the county, lagged county unemployment rate, the percentage change in the annual labor force level, and the percentage change in the annual unemployment rate. All our specifications are similar to Gao, Lee, and Murphy (2020) in double clustering standard errors at the county bond issuer and year-month level unless specified otherwise.

3 Data

We use data on firm-level corporate bankruptcies matched to municipal bonds corresponding to the locations of firms. Our firm locations come from Compustat for the headquarters. In addition, we use data from Mergent Intellect and the Environmental Protection Agency (EPA) to identify other firm facilities. Our municipal bonds data is based on FTSE Russell (formerly known as Mergent) and the Municipal Securities Rulemaking Board (MSRB).

3.1 Corporate Bankruptcies

Our data on corporate bankruptcies come from the data collected by Chava, Stefanescu, and Turnbull (2011) and Alanis, Chava, and Kumar (2018). We supplement the firm-level bankruptcy announcement dates with headquarters locations from Compustat. To avoid potential endogeneity problems in our research design, we drop headquarters counties with multiple bankruptcies filed in the same calendar year. Further, our focus on the manufacturing sector (using two-digit NAICS codes 31-33) during 2006-2016 resulted in a sample of 128 firm bankruptcies. We present summary statistics for our firms in the sample from Compustat in Panel A of Table 1. We use the most recent information available before the bankruptcy date. The sample cut-off years are based on the nearest-neighbor matching strategy to identify the control group. The median firm employs 167 personnel and has total assets of $43 million with total revenue of $31 million.

To identify counties similar to the treated headquarter locations, we use five county-level variables for the nearest neighbor matching strategy: unemployment rate, change in unemployment rate, log(labor force), change in the labor force, and average yield in the year before the bankruptcy. We provide the kernel density plot of these matching variables between the treated and control groups in Figure IA1. The two groups look similar in terms of these matching characteristics. In Panel B of Table 1, we tabulate the difference between additional county-level economic variables. On average, the treated counties are larger than the control counties in terms of population, average municipal bond trading volume, revenues, and expenditures. A closer look at the distribution of these metrics in Panel C of Table 1 provides greater comfort in our matching.

3.2 Municipal Bonds

We obtain Municipal bond characteristics from the Municipal Bonds dataset by FTSE Russell (formerly known as Mergent MBSD). We retrieve the key bond characteristics such as CUSIP, dated date, the amount issued, size of the issue, state of the issuing authority, name of the issuer, yield to maturity, tax status, insurance status, pre-refunding status, type of bid, coupon rate, and maturity date for bonds issued after 1990. We also use S&P credit ratings for these bonds by reconstructing the time series of the most recent ratings from the history of CUSIP-level rating changes. Finally, we encode character ratings into numerically equivalent values ranging from 28 for the highest quality to 1 for the lowest quality.

An important step in our data construction is linking the bonds issued at the local level to the counties, which form the treatment and control pairs. This geographic mapping allows us to study the implications of other economic variables using data on demographics and county-level financial metrics. Since the FTSE Municipal Bonds dataset does not have the county name of each bond, we need to supplement this information from other sources like Bloomberg. However, in light of Bloomberg's download limit, it is not feasible to search for information on each CUSIP individually. Therefore, we first extract the first six digits of the CUSIP to arrive at the issuer's identity[4]. Out of 63,754 unique issuer identities (6-digit CUSIPs), Bloomberg provides us with county-state names on 59,901 issuers. Next, we match these issuers' Federal Information Processing Standards (FIPS) code. The FIPS is then used as the matching key between bonds and treatment/control counties. We also match the names of issuers to the type of (issuer) government (state, city, county, other) on Electronic Municipal Market Access (EMMA) data provided by the Municipal Securities Rulemaking Board. We use this information to distinguish local bonds from state-level bonds because we are interested in non-state bonds.

We use the Municipal Securities Rulemaking Board (MSRB) database on secondary market transactions during 2005-2019. Our paper closely follows Gao, Lee, and Murphy (2020) in aggregating the volume-weighted trades to a monthly level. Following Downing and Zhang (2004); Gao, Lee, and Murphy (2019), we only use customer buy trades to eliminate the possibility of bid-ask bounce effects. Given our primary focus on borrowing costs from secondary market yields, our sample is derived from the joint overlap between the bond characteristics and bond trades at the CUSIP level. In matching the bond transactions from secondary market data to their respective issuance characteristics (FTSE Russell), we rely on the CUSIP as the key identifier. In Figure IA2, we provide kernel densities for secondary market bond features like after-tax yield spreads and remaining maturity between the treatment and control groups. Figure IA3 represents corresponding municipal bond characteristics from the primary market. We describe the key variables in Table A1. Importantly, we find that the two groups look similar in the pattern of their

[4]The CUSIP consists of 9-digits. The first six characters represent the base that identifies the bond issuer. The seventh and eighth characters identify the type of the bond or the issue. The ninth digit is a check digit that is generated automatically.

distributions. We also tabulate these characteristics in Table IA1.

The primary outcome variable used in Equation 1 is the tax-adjusted spread over the risk-free rate. First, we calculate the bond's coupon-equivalent risk-free yield as in Gao, Lee, and Murphy (2020)[5]. Tax adjustment follows Schwert (2017) wherein the marginal tax rate impounded in the tax-exempt bond yields is assumed to be the top statutory income tax rate in each state. This is consistent with the broad base of high-net-worth individuals and households who form a major section of investors in the US municipal bond market (often through mutual funds). A detailed study on tax segmentation across states by Pirinsky and Wang (2011) shows significant costs for both issuers and investors in the form of higher yields. In particular, we use:

$$1 - \tau_{s,t} = (1 - \tau_t^{\text{fed}}) * (1 - \tau_{s,t}^{\text{state}}) \tag{2}$$

To compute the tax-adjusted spread on secondary market yields, we use:

$$spread_{i,t} = \frac{y_{i,t}}{(1 - \tau_{s,t})} - r_t, \tag{3}$$

where r_t corresponds to the coupon-equivalent risk-free yield for a bond traded at time t. From Schwert (2017), we use the top federal income tax rate as 35% from 2005 to 2012, 39.6% from 2013 to 2017, and 37% from 2018 to 2019.

3.3 Other Variables

We use Census data from the Census Bureau Annual Survey of Local Government Finances to get details on revenue, property tax, expenditures, and indebtedness of the

[5] First, we calculate the present value of coupon payments and the face value of a municipal bond using the US treasury yield curve based on zero-coupon yields as given by Gürkaynak, Sack, and Wright (2007). We get the risk-free yield to maturity using this price of the coupon-equivalent risk-free bond, the coupon payments, and the face-value payment. Finally, the yield spread is calculated as the difference between the municipal bond yield observed in the trades and the risk-free yield to maturity calculated. This yield spread calculation is similar to Longstaff, Mithal, and Neis (2005).

local bodies. This gives us detailed constituents of revenue and tax components at the local level, which we use in additional tests to examine the implications for our main results. We obtain the gross domestic product (GDP) measures by county from the Bureau of Economic Analysis (BEA). Our data on county-level household income is from the Internal Revenue Service (IRS) and is used as the total personal income at the county level. Our county-level establishments, employment, and wages data come from the Quarterly Census of Employment and Wages (QCEW) by the Bureau of Labor Statistics (BLS). We use unemployment data from the Bureau of Labor Statistics. We use data from the Surveillance, Epidemiology, and End Results (SEER) Program under the National Cancer Institute for the county-level population. As a proxy for the risk-free rate, we use the zero-coupon yield provided by FEDS, which provides continuously compounded yields for maturities up to 30 years. To get tax-adjusted yield spreads, we use the highest income tax bracket for the corresponding state of the bond issuer from the Federation of Tax Administrators.

4 Results

We discuss our baseline results for Equation (1) documenting the impact on the borrowing costs of local governments in Section 4.1. Next, we propose the potential mechanism to explain our results in Section 4.2. In Section 4.3, we analyze the heterogeneity in our main result. Finally, we present robustness tests in Section 4.4, before discussing the impact on the primary market for municipal bonds (Section 4.5) and the local economy (Section 4.6).

4.1 Impact on Borrowing Costs of Local Governments

In this section, we start with dynamic evidence from the raw data on yields (Section 4.1.1), followed by evidence against underlying economic differences driving our results in Section 4.1.2. Finally, we present results on headquarters versus other locations of the firm filing for bankruptcy in Section 4.1.3.

4.1.1 Dynamics and Baseline Results

We begin our analysis by depicting the after-tax yields aggregated to a quarterly scale in order to overcome the inherent limitations of liquidity in the municipal bond market. We use Equation (4) below to represent our approach in comparing the raw difference between the treatment and control groups:

$$y_{i,c,e,t} = \beta_q * \sum_{n=-12}^{n=12} Treated_{i,c,q} * Post_{i,c,q} + \delta_q * \sum_{n=-12}^{n=12} Control_{i,c,q} * Post_{i,c,q} \quad (4)$$
$$+ \eta_e + \gamma_c + \kappa_t + \epsilon_{i,c,e,t}$$

where index i refers to bond, c refers to county, e denotes the event pair, t indicates the event year-month, and q refers to the quarter corresponding to the event month t. The dependent variable $y_{i,c,d,t}$ is obtained from secondary market trades in local municipal bonds. η_e represents event pair fixed effects, γ_c corresponds to county fixed effects, and κ_t denotes year-month fixed effects. We cluster standard errors by the county bond issuer and year-month.

First, we evaluate the difference between the treated and control groups around bankruptcy filing. In Figure 1, the solid line with circles plots the after-tax yields over the 3-year window for the treated group on average. The control group is depicted using a dashed line. First, the figure reveals no statistical difference between the two groups during the eight quarters before the bankruptcy announcement; the treatment and control groups trend parallel. Second, on average, the treatment group's yields become higher than those of the control group just one quarter before the announcement. However, this effect is statistically indistinguishable from zero. Third, the effect increases and becomes statistically significant in subsequent quarters. Finally, the difference between the two groups persists until the 12^{th} quarter, when it shrinks marginally. We test these observations in our baseline regressions.

Note that the above results represent the raw difference in yield spreads between the two groups by stacking the 128 event pairs in our sample into an aggregated set. These findings do not control for differences in bond characteristics and local economic conditions over time. Next, we estimate our difference-in-differences using our baseline Equation (1). Here, the coefficient β_0 of the interaction term $Treated_{i,e} * Post_{i,t}$ identifies the differential effect after the bankruptcy announcement on average yields of treated counties in comparison to the control groups by additionally accounting for observable characteristics. To revisit our identifying assumption: the control county serves as an adequate counterfactual to map how the treated county's yields would have changed in the absence of the bankruptcy filing. We discuss robustness to additional matching considerations in Section A.1. The event fixed effects ensure estimation from within each event pair. We absorb the unobserved county-level variation using the county-fixed effects. The year-month fixed effects control for declining yields in the overall municipal bond market during our sample period, over and above the treasury adjustment for spreads.

Table 2, reports the effect of firm bankruptcy on the municipal bond yields using Equation (1). In Column (1) - Column (3), we estimate the regression equation using the after-tax yield as the dependent variable. Specifically, Column (1) denotes the estimates without using any controls. We use bond level controls in Column (2), which consist of coupon (%); log(amount issued in USD); dummies for callable bonds, bond insurance, general obligation bond, and competitively issued bonds; remaining years to maturity; and inverse years to maturity. We describe key variables in Table A1. In Column (3), we control for the county-level variation in the unemployment rate and labor force. We use the lagged values (to the year of bankruptcy filing) for log(labor force) and unemployment rate and the percentage change in the unemployment rate and labor force, respectively.

We show results with after-tax yield spread as the dependent variable in Columns (4)-

(8) by following a similar progression across columns. Using Column (6) with after-tax yield spread as our baseline specification implies that the yield spread for treated counties increases by 10.01 bps after the bankruptcy filing compared to the control counties. The effect is statistically significant and economically meaningful. To understand the magnitude in context better, consider the average credit spread between AA- and AAA-rated municipal bonds, which amounts to 93 bps for our sample of municipal bonds in the treated group before the firm bankruptcy. This implies that the average increase in yield spreads after the bankruptcy represents $\sim 10\%$ (= 10.01/93) of the credit spread. Further, we introduce more granular fixed effects in Columns (7) and (8). In Column (7), we also introduce the event pair $\times$ county fixed effect. The baseline magnitude increases to 10.15 bps, with similar statistical significance. As expected, we find that the "treated" dummy gets absorbed in this estimation. Finally, we introduce the event pair $\times$ year-month fixed effect in Column (8). Both "treated" and "post" dummies get absorbed, but the overall effect remains similar to the baseline specification in Column (6). In the next sub-section, we provide evidence against underlying local economic differences driving our main result.

4.1.2 Do bond yields respond to underlying local economic differences?

We have already shown that the secondary market and primary market bond features look almost identical in their density plots. We provide evidence from consumer bankruptcies to further alleviate concerns about underlying economic differences between the treated and control counties. If it is the case that the increase in bond yield spreads is due to worse local economic conditions in the treated counties, similar evidence should show up at the consumer level. However, in Figure 2 we do not find such evidence. This plot shows the regression coefficients corresponding to the treated and control groups similar to Equation (4). The dependent variable is the log transformation of the number of consumer bankruptcies filed in the county in a given quarter. Our evidence shows no statistical difference between the number of consumer bankruptcies filed between the two groups in the quarters before the firm bankruptcy filing. The solid line representing the treated group shows an increase in consumer bankruptcies after the penultimate quarter of firm bankruptcy filing. The difference becomes statistically significant beyond the second quarter and persists until the end of the event window (three years after). This evidence suggests that it is unlikely that the deterioration in ex-ante local economic conditions drives the results in bond yield spreads. We discuss the importance of the headquarter versus other firm locations in the next sub-section.

4.1.3 Headquarters vs Other Firm Locations

Dougal, Parsons, and Titman (2022) document that value creation has become heavily concentrated in a few headquarters cities over the last 20 years as per stock market indicators. One may argue that while the location of a firm's headquarters may represent a hub of economic activity, it may not be the only location. Manufacturing firms may have operations in other locations besides their headquarters. To this end, we analyze the impact on additional facilities for the firms in our sample. We report our results in Table 3. We obtain data on other locations using two sources: Environmental Protection Agency and Mergent Intellect. Our data from Mergent Intellect is hand-collected and allows us to observe each firm's facility level DUNS number and employment information. In Column (1), we show the baseline result by including non-headquarter locations from the EPA and find the impact as 4.02 bps. However, Column (2) reveals that most of this effect comes from the headquarter locations (12.74 bps). We find similar results using non-headquarter facilities from Mergent Intellect in Columns (3)-(4). Column (3) shows that the aggregate effect across all facilities amounts to an increase in bond yield spreads of 6.34 bps. Once again, Column (4) reveals that the headquarter locations account for 10.82 bps. The effect is insignificant and too small for other locations.

Further, we also investigate the importance of facilities based on employment at the site. We show these results in Columns (5)-(7), where we consider all facilities from Mergent Intellect, which may or may not overlap with the headquarter site. However, this sample does not include all headquarters from the baseline. First, in Column (5), we find an overall increase in bond yield spreads of 3.83 bps among these facilities listed in Mergent Intellect. However, this effect only seems to be weakly significant. Column (6) shows that most of this effect belongs to multi-facility firm bankruptcies, which might include larger firms with a more significant economic footprint. We find this magnitude to be 6.63 bps. The effect is statistically significant and about half the baseline magnitude. On the other hand, the impact on single facility locations seems to be insignificant. This result is consistent with our mechanism discussing the county's dependence on the firm filing for bankruptcy (See Section 4.2). To further consolidate our findings on the relative economic importance of facilities, we rank them based on the fraction of employees. Sites with $\geq 25\%$ of total employees are ranked 1, followed by rank 2 for sites with $> 5\%$ of total employees. Rank 3 is assigned to the remaining locations. Finally, we report our results in Column (7), where we find the greatest impact (of 14.77 bps) due to facilities that are ranked highest (for counties with the highest employment share in the firm). The effect on counties with lower-ranked facilities is insignificant.

Our results in this section show that the increase in bond yield spreads after the firm filing for bankruptcy is mainly due to the headquarters locations. Counties with non-headquarter facilities are not affected as much. Additionally, counties with a higher fraction of employment show a more significant impact on bond yield spreads. We argue that this is because the employment share may represent the site's economic importance.

4.2 Mechanism

To explain our results, we propose a mechanism based on the exposure of treated (HQ) counties to the bankrupt firm's industry.

4.2.1 Source of Repayment and County's exposure to the bankrupt firm's industry

Our analysis in this section begins with the type of repayment among municipal bonds. We identify the two major classes of bond repayment in the sample. General obligation bonds correspond to the underlying taxing power of the issuer. Revenue bonds generally have their cash flows linked to the specific project backing the bonds. We show results for our baseline specification in Equation (1) in Panel A of Table 4.

In Column (1), we report the baseline effect of 10.01 bps across all bonds. Column (2) shows that the effect on revenue bonds is higher, amounting to 12.56 bps. This effect is primarily driven by the sub-sample of uninsured revenue bonds (Column (3)). Insured revenue bonds show a weak and statistically insignificant effect in Column (4). Finally, we do not find any significant effect on general obligation bonds in Column (5). The fact that we do not find any effect on general obligation bonds suggests that the underlying mechanism of firm bankruptcies affecting local bonds runs through cash flows linked to project revenues (Auh, Choi, Deryugina, and Park, 2022) of revenue bonds. General obligation bonds are backed by the taxing authority of local institutions and, therefore, relatively immune to such shocks based on the local economy's exposure. We build on this argument in subsequent analysis using the county-level exposure described below.

We use the 3-digit NAICS code to identify the industry of a bankrupt firm. We expect the baseline effect to be higher for counties that rely heavily on the bankrupt firm's industry. As a direct measure of the county's dependence on the industry, we calculate the county's ex-ante share of wages and employment from the NAICS 3-digit industry of the bankrupt firm in the year before firm bankruptcy. A higher proportion would reflect a county's greater reliance on that sector.

We divide the treated counties into two groups based on the median values of the share of wages and share of employment in the industry. Our specification is suitably modified to additionally include group-month fixed effects. We report our results in Panel B of

Table 4. We split the treated counties based on the share of aggregate industry wages in Columns (1)-(3). Using all bonds in Column (1), we show that a higher dependence based on wages results in an increase in yield spreads of 15.01 bps. This effect is negligibly small and statistically insignificant for treated counties with low exposure (below median). The difference between the high and low groups is statistically significant. Thereafter, we consider the sub-samples of revenue bonds and uninsured revenue bonds in Columns (2) and (3). As before, bonds associated with treated counties of high exposure are more strongly affected.

Next, we consider the heterogeneity among treated counties based on their share of aggregate employment in the industry of the bankrupt firm. We present these results in Columns (4)-(6). Once again, we examine the impact among all municipal bonds in the sample. Column (4) shows that the after-tax yield spreads increase in treated counties with high exposure to firm bankruptcy by 13.73 bps. The corresponding effect on counties with below median exposure is only 3.88 bps and is statistically insignificant. Our results show that the difference between these two groups is statistically significant. We perform a similar analysis using the sub-samples of revenue bonds (Column (5)) and uninsured revenue bonds (Column (6)). Once again, the heterogeneity seems to be driven by counties with high exposure to the industry of bankrupt firm.

To demonstrate further support for our mechanism, we compare the county-level unemployment rate between the treated and control counties. Since the unemployment rate is reported annually, we use the annualized version of Equation (4) at the county level. We replace month-fixed effects with year-fixed effects and cluster standard errors at the event pair level. We show our results from the regression coefficients in Figure 3a. We find that in the three years before a firm's bankruptcy filing, the unemployment rates in the treated and control counties trend similarly, with negligible differences between them. The difference in the unemployment rate begins to pick up in the year of the bankruptcy filing by the firm and becomes statistically significant in the first year after the filing. Subsequently, the difference between the two groups tends to increase later. We substantiate this evidence further in light of the county-level dependence using the share of employment in the industry of firm bankruptcy. Specifically, in Figure 3b, we show that treated counties with higher dependence seem to drive the overall unemployment rate. However, the differential effect is relatively muted and statistically insignificant for counties with low exposure to the industry of bankrupt firms. We provide similar evidence using county exposure based on the county-level share of wages in the industry of firm bankruptcy in Figure IA11. Once again, we find that treated counties with a higher dependence on the industry of the firm filing for bankruptcy are more severely affected. Separately, we also discuss our results in light of state-specific budgetary regulations and incentive policies in Section A.2.

To summarize, our results in this subsection provide evidence in favor of the county's dependence on the industry for firm bankruptcy. We show that treated counties with a higher dependence and a greater exposure to the industry of firm bankruptcy experience a higher increase (13-15 bps) in bond yield spreads. These counties also seem to drive the result showing an aggregate increase in the unemployment rate.

4.3 Heterogeneity

In this sub-section, we test the heterogeneity of our results. We study how factors such as the relative importance of the firm filing for bankruptcy and bond-specific features may affect our results. We report our results in Table 5 and in Figure 4, respectively. In these analyses, we suitably modify our baseline Equation (1) to include group-year month fixed effects.

4.3.1 Relative Importance of Firm

To understand how the relative importance of a firm may determine its impact on the local economy, we construct three measures in Columns (1)-(3) of Table 5. First, we use a ratio of the firm's operating income (EBIT) to the county's revenue. We divide the bankruptcy events into two groups based on the median value of this ratio. We find that a high value of the ratio results in an increase in bond yield spreads of 14.98 bps. The effect on the low group is negligible and insignificant. This result suggests that firms with a relatively large size compared to the HQ county have a greater impact on the borrowing cost of the treated county. We replicate this approach using two other measures of a firm's importance to the county. Since we do not observe the number of employees on the firm's payroll for each location, we proxy for this by using selling, general, and administrative (SG&A) expenses. In Column (2), we use the ratio of SG&A expenses of the firm to the county's revenue. We find that a higher ratio of expenses to county revenue increases bond yields by 20.84 bps. The differential effect in the high group counties is statistically different from the treated counties with low values. Finally, in Column (3), we evaluate the relative importance of the firm by comparing the firm's plant, property, and equipment to the county's property tax revenue. Since property tax consists of a large portion of a county's income, we argue that in the absence of precise data on commercial property tax revenue from the bankrupt firm, this measure would capture the economic significance of the firm. Consistent with our previous results, we find that counties with a high value of this ratio experience a yield spread increase of 11.96 bps.

4.3.2 Purpose of Bonds

Next, we evaluate the differential impact among different types of municipal bonds. Specifically, we classify them based on the use of proceeds associated with municipal bonds. We report our results in Figure 4. The highest impact (32.55 bps) occurs with bonds associated with improvement and development. This is understandable because many of these bonds are associated with constructing new amenities and features in the locality with the prospect of providing infrastructure for private sector firms. For example, industrial revenue bonds may be for building or acquiring factories or heavy equipment and tools. We do not find a large impact on bonds that are linked to services and fee-based utilities.

Overall, our results in this section provide evidence of the heterogeneity of the impact of firm bankruptcies. We show that the impact is higher if the relative importance of the firm in the county is higher. Further, the increase in bond yields comes from specific types of bonds that are likely affected due to the firm bankruptcy.

4.4 Robustness Tests

In this section, we test the robustness of our main result in Column (6) of Panel A in Table 2 to various potential confounding factors. We present the results of these robustness checks in Table 6.

4.4.1 Other unobservables

We consider whether our results are robust to various unobserved factors at the bond, county, and geographic region levels. First, we absorb all bond level, time-invariant variation to control for unobserved bond features in Column (1). This yields a baseline effect of 8.23 bps. To account for unobserved regional variation in the US, we show our results in Columns (2)-(4). First, in Column (2), we show the result after introducing region-fixed effects to the baseline specification. Our magnitude changes to 10.01 bps. Further, we impose region × year fixed effects to account for time-varying unobserved factors at the regional level. The main result in Column (3) is 10.42 bps. Finally, in Column (4), we show our results by controlling for a more granular time-varying component by introducing region × year-month fixed effects. Even with this restrictive specification, we report an increase in yield spreads of 10.66 bps. Moreover, if there is a concern that the municipal bond market yields were different for general obligation versus revenue bonds in a given year-month, we control for year-month × GO bond indicator fixed effects in Column (5). We find that the bond yield spreads increase by 8.74 bps. Likewise, in Column (6), we control for year-month × insured bond status fixed effects to control for unobserved market-wide factors for insured bonds. Using bond purpose × year-month fixed effects, we report the baseline effects as 7.88 bps in Column (7). This specification controls for any time-varying unobservables that may drive our result in yields for a given use of proceeds raised in the municipal bond market. Our analysis suggests that our main effect is robust to unobserved factors at the bond, county, region, and municipal bond market levels.

4.4.2 Additional controls

Following unobserved factors, we now control for additional observables in Panel B. In Column (1), we present our results by introducing some more county-level time-varying covariates. We introduce the lagged values of log(personal income) and log(house price index) to account for any changes in these metrics that may be simultaneously changing at the time of firm bankruptcy filing. In this case, the bond yield spreads increase by 9.60 bps. Our next check on the robustness of our main results involves using only those bonds for which the most recent bond ratings are available from Standard & Poor's credit ratings in the FTSE Russell municipal bonds database. Since the restriction of requiring bond ratings reduces the sample, we do not impose the rating requirement on the baseline specification. We show our results by controlling for bond ratings in Column (2) of Table 6. We find that the bond yield spreads increase by 8.29 bps. This test mitigates any concern about unrated bonds solely driving our results.

To verify our results' robustness to the bonds' duration effects, we modify the baseline specification in Columns (3)-(4). First, Column (3) shows our main effect by replacing years to maturity and inverse years to maturity at the bond level with the corresponding duration for the bond-month observation. We report a higher impact of 10.60 bps. Next, in Column (4), we show the same result by calculating duration based on after-tax yields.

This tax adjustment also increases the impact to 10.61 bps. Our main result is robust to altering the baseline specification to include bond level duration instead of years to maturity and inverse years to maturity among the bond level controls.

4.4.3 Is the effect driven by recently issued bonds?

Schultz (2012) documents that price dispersion in municipal bond trades in the days after issuance has declined dramatically since the introduction of 15-minute trade reporting by the MSRB in 2005. While our municipal bond trades started in 2005, we further exercise caution in this regard by dropping bonds issued within a few months of the bankruptcy filing. We show these results in Columns (1)-(4) in Panel C. For example, in Column (1), we report the baseline result by dropping bonds dated within 6 months of the bankruptcy filing and find that the increase in bond yield spreads is 10.01 bps. In each sub-sample, the coefficient of interest is higher in magnitude than in the baseline result. Based on this evidence, we argue that trades from recently issued bonds do not drive our results.

4.4.4 Alternative specifications

We also consider some modifications to the baseline equation to evaluate potential concerns about alternative specifications that might be used in this setting. We report our results in Panel D. First, we consider our choice of clustering standard errors in the baseline specification in Equation (1), which is similar to the existing literature (Gao, Lee, and Murphy, 2020, 2019). In Columns (1)-(3), we show our main result using alternative definitions for clustering. First, we consider the possibility that the standard errors may be correlated across different bond issues over the calendar months (see Column (1)). It could also be that the error term in our main specification is correlated with specific bond issuers. We report our results for this in Column (2). Finally, we also consider clustering standard errors at the county bond issue level in Column (3). In all these specifications, we find results similar to our baseline specification. Thus, our results are robust to these choices of clustering standard errors.

Next, we add event-month fixed effects to the baseline. If there is a concern that the impact on bond yields may be driven by specific timing effects on the event scale, this modification should account for that. In other words, if the relative age of the bond trades with respect to the bankruptcy filing may affect our main result, then these fixed effects should absorb that variation. In Column (4), the baseline effect is reported as 10.03 bps. Further, if one believes that controlling for event-month fixed effects is more important than absorbing calendar year-month fixed effects, we address this in Column (5). We replace the year-month fixed effects with event-month fixed effects. The overall magnitude goes up to 23.27 bps and is statistically significant. Finally, we show robustness in replacing year-month fixed effects with year-fixed effects in Column (6). The increase in bond yields is reported as 10.35 bps. Overall, we show robustness to alternative specifications to our baseline Equation (1).

4.4.5 Is the effect driven by size of trades?

One potential concern with interpreting the results presented so far is that they may be driven by different customer groups in the municipal bond market. Green, Hollifield, and Schürhoff (2007) show that municipal bond dealers earn lower average markups on larger trades even though they bear a higher risk of losses on such transactions. In this market, retail customers dominate the holdings (Schwert, 2017) through direct ownership or via investment vehicles like bond mutual funds. However, there has been a recent rise in holdings by institutional investors as well[6]. Accounting for such differences, we dissect our results into sub-samples of trades constituting various buckets and show our results in Panel E. Columns (1)-(4) depict the main effect derived from trade sizes with threshold cut-offs worth \$25,000 or \$50,000. The increase in borrowing cost is around 8 bps or more in each of these specifications except in Column (2), which has substantially fewer

[6] http://www.msrb.org/msrb1/pdfs/MSRB-Brief-Trends-Bond-Ownership.pdf

trades due to the sample restriction. The results are comparable to the baseline effect regarding statistical significance and economic meaning. This suggests that any specific group of clientele does not seem to drive our main result.

4.4.6 Additional Considerations

We focus on some additional considerations regarding our sample and specification in Panel F. First, we address the concern that our results may be capturing the differences in taxation of income from municipal bonds across states in Column (1). By focusing only on tax-exempt bonds in the sample, we try to minimize the differential impact due to taxation and report our main result as 11.02 bps.

Another potential worry stems from the fact that the sample period spans the financial crisis of 2008-2009. This was a period of major unrest in the financial markets across asset classes, and municipal bonds were not immune to this unrest. As a result, we report our findings by excluding this period from our data. Column (2) shows the baseline results from Equation (1) by excluding all trades up to 2010. The increase in bond yield spreads among the treated counties is 11.04 bps higher than those of the control counties. The sample size is reduced by almost half, but the main effect is still statistically significant and economically meaningful with this reduced sample. This suggests that our result is not primarily driven by the financial crisis of 2008-2009.

Next, we test our main result from Equation (1) by changing the dependent variable. Column (3) uses the monthly average yield observed from MSRB as the outcome variable. We find that the average increase in bond yields is 7.19 bps. The effect is statistically significant and economically meaningful. Similarly, by using the yield spread as the dependent variable in Column (4), we find that the baseline effect is 6.51 bps. These outcome variables do not account for the state-level tax exemption on income from municipal bonds. Overall, our results suggest that our baseline effect is robust to the choice of the dependent variable used in the analysis.

It is possible that our matching control counties may also face the repercussions of recent firm bankruptcy. If so, this might underestimate our main result. We address this concern in Column (5) by restricting the control counties to include only those that do not also see a firm bankruptcy from our sample within two years of the treated county's firm bankruptcy filing. We find that the main result shows up as 8.69 bps in this revised sample of restricted counties for the control set. We argue that this restriction could likely reduce the availability of matching counties based on size. As a result, the matched counties may likely be smaller than the treated group, leading to higher yields.

Finally, to evaluate if our results are sensitive to the choice of using manufacturing firm bankruptcies, we broaden the set of firms to include all tradeables in Column (6). Our classification of industries is based on Bernstein, Colonnelli, Giroud, and Iverson (2019). We exclude all service industries as per the Census [7] and classify the nontradable sector as retail trade (NAICS 44-45) and accommodation and food services (NAICS 72). All remaining industries are considered under tradeables, which are mostly manufacturing (NAICS 31-33). In Column (6), we report our baseline effect as 8.27 bps by using firm bankruptcies in tradeables.

Overall, we provide evidence to show that our results are not sensitive to redefining the control group or to the choice of focusing on manufacturing firm bankruptcies. Our results are also robust to dropping taxable bonds from the sample.

4.4.7 Sensitivity to Length of Event Window

To evaluate the sensitivity of our baseline results against the choice of event window used, we show our main result in Panel G using different lengths of the event window. We modify the baseline choice of a 36-month event window to range between 12 and 60-month event windows. In Column (1), we find that even over a short window of one year, the magnitude of 3.86 bps is statistically significant. The increase in yield spreads

[7] www.census.gov/econ/services.html

ranges between 5 to 12 bps when we consider alternative windows, as shown in Columns (2)-(6). We argue that a longer period is needed in the preceding and succeeding periods around the announcement of firm bankruptcy to arrive at sharper estimates of the effect, especially given the limited trading in the municipal bond market.

4.4.8 Sensitivity to Start/End of Event Window

Next, in Panel H, we show the baseline result of Column (6) in Table 2 by changing either the starting or the ending point in the event window. Specifically, in Columns (1)-(3), we use shorter pre-event periods ranging from 6 to 24 months before the bankruptcy filing. We find that the magnitude of the baseline impact ranges between 6 to 10 bps. In Columns (4)-(6), we keep the pre-event window similar to the baseline specification (at 36 months) and modify the post-event window. Our results remain statistically significant and economically meaningful, ranging between 4 and 8 bps.

Overall, this section demonstrates robustness to our baseline specification using alternative considerations. We consider additional matching strategies to identify the counterfactual counties for our setting in Section A.1. In the following sections, we present results from new municipal bonds and the local economy.

4.5 Impact on Primary Market of Municipal Bonds

As a related implication of our first result on increasing yields for the treated county in the secondary market, we evaluate the event's impact on the primary market using new bond issuance data. The average new issuance in the sample is $53 million (median is $15 million) for the treated counties. Therefore, it becomes important to understand the borrowing cost implications of raising new money. To this end, we use a similar estimate as the difference-in-differences test of the secondary market, with suitable modifications to Equation (1). The equation is presented below:

$$y_{i,c,e,t} = \beta_0 * Treated_{i,c,e} * Post_{i,c,t} + \beta_1 * Treated_{i,c,e} + \beta_2 * Post_{i,c,t} \\ + BondControls + CountyControls + \eta_e + \delta_c + \gamma_j + \kappa_t + \epsilon_{i,c,e,t} \quad (5)$$

where index i refers to the bond, c refers to the county, e denotes the event pair, and t indicates the time (in months). We use tax-adjusted yields at the time of bond issuance obtained from the primary market as the dependent variable in $y_{i,c,e,t}$. Since municipal bonds are tax-exempt at the state level, we argue that adjusting for variation in tax effects across states is relevant in our sample. The coefficient of interest is β_0. We use the same items for *BondControls* and *CountyControls* as in Equation (1). η_e refers to event pair fixed effects, δ_c corresponds to county fixed effects, and γ_j represents issuer fixed effect for the bond i in county c. The issuer fixed effects help us to control for characteristics specific to the issuer, especially in the context of the purpose for which the bond may be issued. We also control for the month in which the bond was issued by including year-month fixed effects in the form of κ_t. We cluster standard errors at the bond issue level.

Table 7 shows the results for the specification above. Column (1) estimates the difference-in-differences coefficient from within the same event-pair, absorbing for issuer fixed effects. We report an increase of 13.69 bps in the after-tax offering yields. However, this does not account for the sample's bond level and county-level heterogeneity. In Column (2), we introduce bond level controls to show an increase in offering yields of new issuances by 10.49 bps. Next, we introduce county-level time-varying characteristics along with county-fixed effects in Column (3) to get a 5.79 bps increase in offering yields for the treated counties compared to the control group.

Overall, our results suggest that the primary market for bond issuance is associated with an increase in tax-adjusted yields of about 6 bps after a firm bankruptcy filing. However, we admit a major caveat. If counties and local governments rationally expect a higher borrowing cost in anticipation of the bankruptcy of a distressed firm, they may try to time the market and raise money well before or after the event. This inherent endogeneity problem limits our ability to test our hypothesis in this market. Existing bonds that are already trading in the secondary market are not riddled with this limitation and, hence are used in our baseline analysis to evaluate the impact on borrowing cost. Since the information on bond ratings reduces our sample size, we do not consider this in the main analysis above. However, we show similar results after controlling for bond ratings, resulting in a smaller sample shown in Table IA3.

In Figure 5, we also show evidence indicating lower municipal bond issuance among the treated counties when compared to their matching control counties in the three years after the firm bankruptcy. This is consistent with the increase in offering yields of new bond issuances. We substantiate the evidence on the underlying mechanism discussed in Section 4.2 by showing these results based on the treated county's exposure. We find that the reduction in issuance is driven by counties with a high dependence on the industry of the bankrupt firm. As before, we measure dependence based on the fraction of aggregate employment of the county in the industry of a firm filing for bankruptcy. Alternatively, we find similar results when we measure the dependence based on the aggregate share of wages belonging to the industry of a bankrupt firm (Figure IA12).

4.6 Impact on County's Local Economy

Having shown thus far that a county's borrowing cost is affected following the bankruptcy of a listed manufacturing firm, we now turn to some real economy effects after this event.

Business taxes account for more than one-third of local government revenue. Since the bankruptcy filing of a listed manufacturing firm can be interpreted as a major shock to expected future revenues, we investigate the implications on the local economy in this section. We examine the impact on the county's GDP growth (%) and growth in county-level establishments. We present our results in Table 8.

To study the county-level metrics reported on an annual basis, we modify our baseline Equation (1) as below:

$$y_{c,e,t} = \beta_0 * Treated_{c,e} * Post_{c,t} + \beta_1 * Treated_{c,e} + \beta_2 * Post_{c,t} + CountyControls + \eta_e + \gamma_c + \epsilon_{c,e,t} \quad (6)$$

where index c refers to the county, e denotes the event pair, and t indicates the year. The dependent variable $y_{c,e,t}$ represents the county level outcome variables. The coefficient of interest is β_0, representing the differential impact on treated counties compared to the control group after the bankruptcy filing by the firm. The above specification also includes two sets of fixed effects: event pair fixed effects (η_e), so the comparisons are within counties mapped to an event pair; and county fixed effects (γ_c) to account for unobserved variation across counties. Here, we cluster standard errors at the event level.

First, we show our results using the baseline difference-in-differences strategy in Panel A. We start by analyzing the overall GDP growth (%) in the county in Column (1) for the baseline event window of three years around the firm bankruptcy. Our results show a 0.67% decline in GDP growth among headquarters counties in the three years after the listed manufacturing firm filed for bankruptcy. This effect persists over five years (Column (2)) and seven years (Column (3)) after the event. However, the magnitude of the impact declines marginally. Next, we follow a similar estimation for the growth in county-level establishments in Columns (3)-(6). Again, we find weak evidence for a decline in the coefficient of interest, but the magnitude is much smaller and statistically insignificant.

Next, we examine these county-level dependent variables per our proposed county-level exposure mechanism. We show these results in Panel B, by splitting the treated counties into two groups based on the median value of county-level employment dependence. Using county GDP growth as the dependent variable in Columns (1)-(3), we find evidence consistent with our mechanism. The decline in GDP growth is driven by counties with above median (high) dependence on the industry of bankrupt firm. The difference between the high and low groups is statistically significant during the three, five, and seven years after the firm's bankruptcy. The evidence of county-level growth in establishments is not clear.

4.7 Recovery of Treated Counties

Following the evidence about the local economy, we now turn to the long-term implications of large firm bankruptcy. We consider conventional proxies for a city's appeal to workers (Dougal, Parsons, and Titman, 2022) like education rate and weather to understand the probable recovery over time. We extend this argument to other factors like entrepreneurship, start-up formation rate, patent formation, race, and mobility across counties. Using the county-level measures of growth in GDP and the growth in establishments discussed in Section 4.6, we show results based on the heterogeneity among treated counties.

Table 9 shows these results using the corresponding interactions for Equation (6). First, based on the county-level rate of bachelor's education (Panel A), we find no difference between high and low groups of treated counties for growth in GDP. This analysis represents the top quartile of treated counties in the "high" group. The remaining counties are classified as the low group. As before, we perform this analysis for the baseline event window of three years around bankruptcy and extended periods of five and seven years after the event, respectively. However, Columns (4)-(6) show some evidence for higher county-level growth in establishments for treated counties with high levels of bachelor's education.

In Panel B, we use the number of pleasant days to group the treated counties into low versus high categories. Here, we find that growth in county GDP declines more for counties with a high number of pleasant days. This difference is statistically significant and economically large. However, the corresponding evidence from the growth in establishments in Columns (3)-(6) is inconclusive. We report similar mixed evidence in Panels C to H. Thus, along these observable dimensions, we find limited evidence supporting the recovery of treated counties after bankruptcy.

5 Conclusion

Using bankruptcies of publicly listed manufacturing firms during 2006-2016, we assess the impact on local government finances at headquarters counties and other locations. We focus on listed manufacturing firms since they are less likely to depend on local demand. This allows us to identify the impact of firm bankruptcy on HQ county and other locations. We find that the cost of municipal debt increases by 10 bps in the secondary market three years after the bankruptcy filing by a local manufacturing firm. We approximate the treated county's exposure to the firm bankruptcy using aggregate industry-county-level employment and wage data. This negative impact is higher when the county is more dependent on the industry of the bankrupt firm.

We also explore the heterogeneity based on the economic exposure of the county to the bankrupt firm and the bankrupt firm's industry. When the firm is relatively more important to the county, the impact on the bond yield spreads is higher. Consistent with the increase in the secondary market yields, we document an increase in yields for new municipal bond issuances also. Counties experience a decline in GDP growth in the three years after the firm bankruptcy compared to their control group. Overall, our results highlight local communities are an important stakeholder in corporations and how they get affected by corporate bankruptcies.

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Table A1: Description of Key Variables

This table reports variable definitions. Data sources include municipal bond transaction data from the Municipal Security Rulemaking Board (MSRB), FTSE Russell's Municipal Bond Securities Database (FTSE, formerly known as Mergent MBSD), zero coupon yield provided by FEDS, highest income tax bracket for the corresponding state of the bond issuer from the Federation of Tax Administrators (FTA), and Census data from the Census Bureau Annual Survey of Local Government Finances (CLGF). Our data on firm bankruptcy filings comes from Chava, Stefanescu, and Turnbull (2011) and Alanis, Chava, and Kumar (2018) (CST-ACK).

Variable	Description	Source
Treated	Dummy set to one for a county that has a firm bankruptcy filing. This dummy equals zero for the control group county in that event pair.	CST-ACK
Post	Dummy that is assigned a value of one for months after the bankruptcy filing and zero otherwise.	CST-ACK, MSRB
Average Yield	Volume-weighted average yield for a CUSIP in a given month. Volume refers to the par value of the trade.	MSRB
Yield Spread	Calculated as the difference between the *Average Yield* and the coupon-equivalent risk free yield. The risk free yield is based on the present value of coupon payments and the face value of the municipal bond using the US treasury yield curve based on maturity-matched zero-coupon yields as given by Gürkaynak, Sack, and Wright (2007). This yield spread calculation is similar to Longstaff, Mithal, and Neis (2005).	MSRB, FEDS

Variable	Description	Source
After-tax Yield Spread	Calculated as the difference between the tax-adjusted *Average Yield* and the coupon-equivalent risk free yield. The risk free yield is based on the present value of coupon payments and the face value of the municipal bond using the US treasury yield curve based on maturity-matched zero-coupon yields as given by Gürkaynak, Sack, and Wright (2007). This yield spread calculation is similar to Longstaff, Mithal, and Neis (2005). We follow Schwert (2017) in applying the tax adjustment. It is calculated as below: $$spread_{i,t} = \frac{y_{i,t}}{(1-\tau_t^{\text{fed}}) * (1-\tau_{s,t}^{\text{state}}))} - r_t$$	MSRB, FEDS, FTA
Competitive Bond Dummy	Dummy variable that equals 1 if the issue is sold to underwriters on a competitive basis, and 0 otherwise.	FTSE
GO Bond Dummy	Dummy variable for general obligation bond. A GO bond is a municipal bond backed by the credit and taxing power of the issuing jurisdiction rather than the revenue from a given project.	FTSE
Log(Amount)	Log transformation of the dollar amount of the individual bond's (9-digit CUSIP) original offering.	FTSE
Callable Dummy	Dummy variable that equals 1 if the issue is callable, and 0 otherwise.	FTSE
Insured Dummy	Dummy variable that equals 1 if the issue is insured, and 0 otherwise.	FTSE
Remaining Maturity	Individual bond maturity measured in years.	FTSE, MSRB
Inverse Maturity	Inverse of the value of *Remaining Maturity*; to account for non-linearity.	FTSE, MSRB

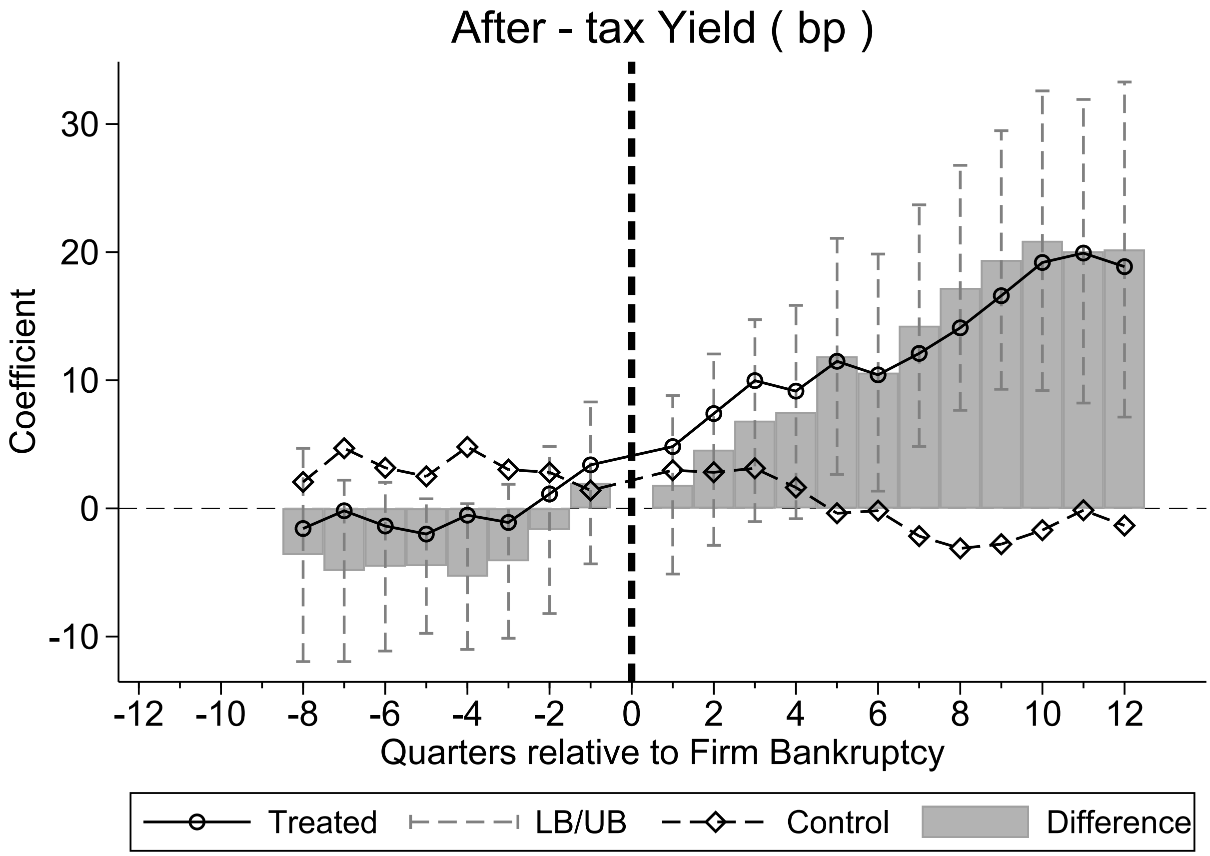


Figure 1: Municipal Bonds Secondary Yields: Treatment vs Matched Control: The figure shows the impact on secondary market bond yield spreads between the treated and control counties. We report the coefficients from Equation 4. We cluster standard errors at the county bond issuer and year-month level. The control county was matched based on five variables in the year before firm bankruptcy: unemployment rate, change in unemployment rate, log(labor force), change in labor force, and average yield of the county in that year.

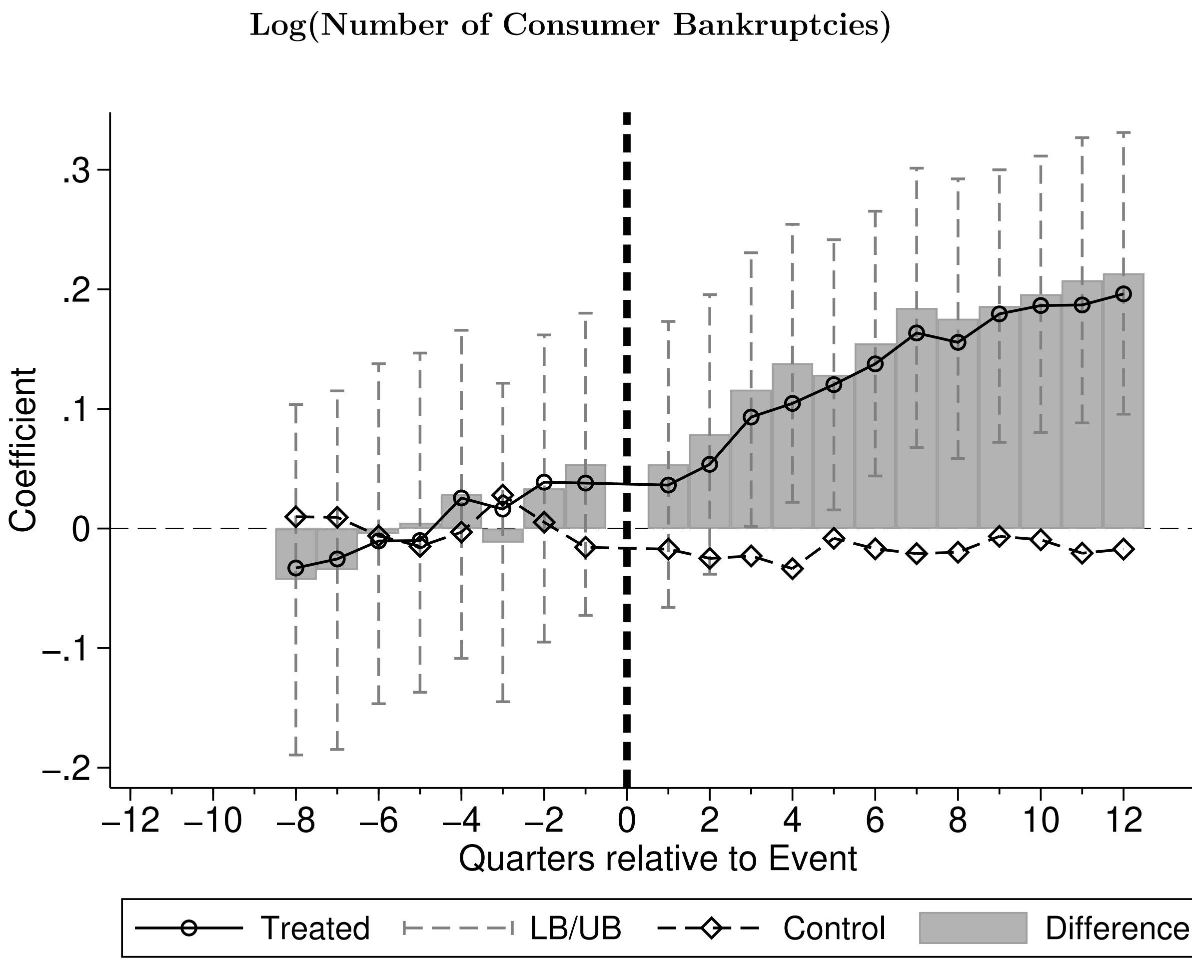
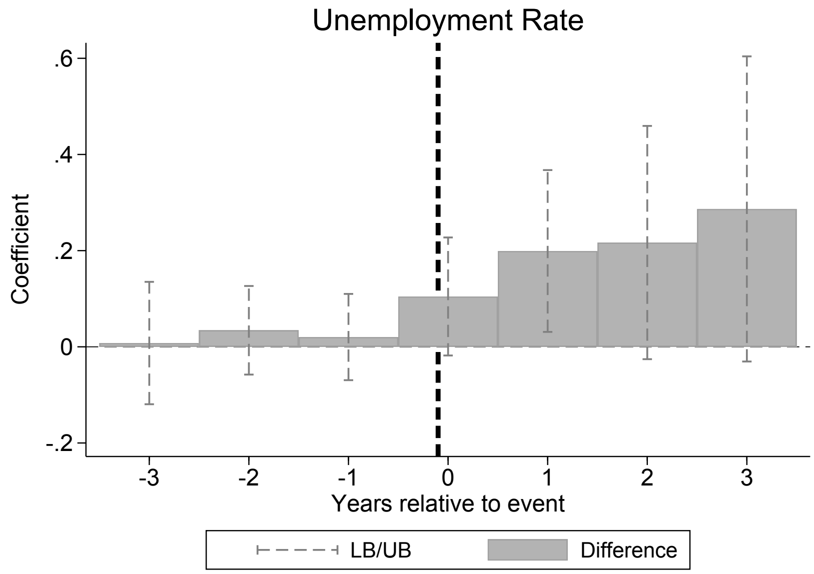


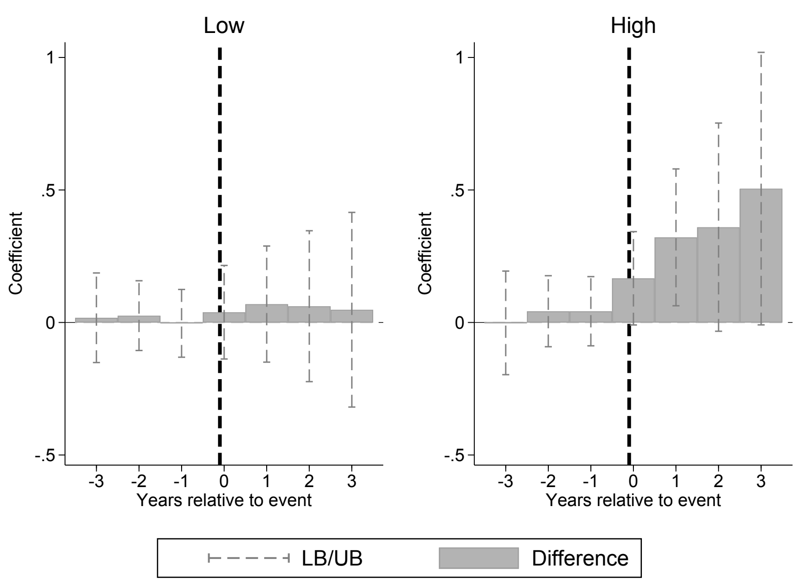
Figure 2: Consumer Bankruptcies : Treatment vs Matched Control: The figure shows the coefficients based on Equation 4. We regress the logged value of the number of consumer bankruptcies as the dependent variable. We cluster standard errors at the event pair level. The control county was matched based on five variables in the year before firm bankruptcy: unemployment rate, change in unemployment rate, log(labor force), change in labor force, and average yield of the county in that year.

Panel A: All Counties



(a)

Panel B: Based on County Dependence (Employment)



(b)

Figure 3: Unemployment Rate: Treatment vs Matched Control: The figure shows the impact on unemployment rates between the treated and control counties. We report coefficients from the annualized version of Equation 4 using unemployment rate as the dependent variable. We cluster standard errors at the event pair level. In Panel A, we show all treated counties versus their matching control counties. In Panel B, we show the impact on sub-samples of low versus high level of county-level dependence on the industry of bankrupt firm among treated counties. We measure dependence based on the share of aggregate employment in the county. The control county was matched based on five variables in the year before firm bankruptcy: unemployment rate, change in unemployment rate, log(labor force), change in labor force, and average yield of the county in that year.

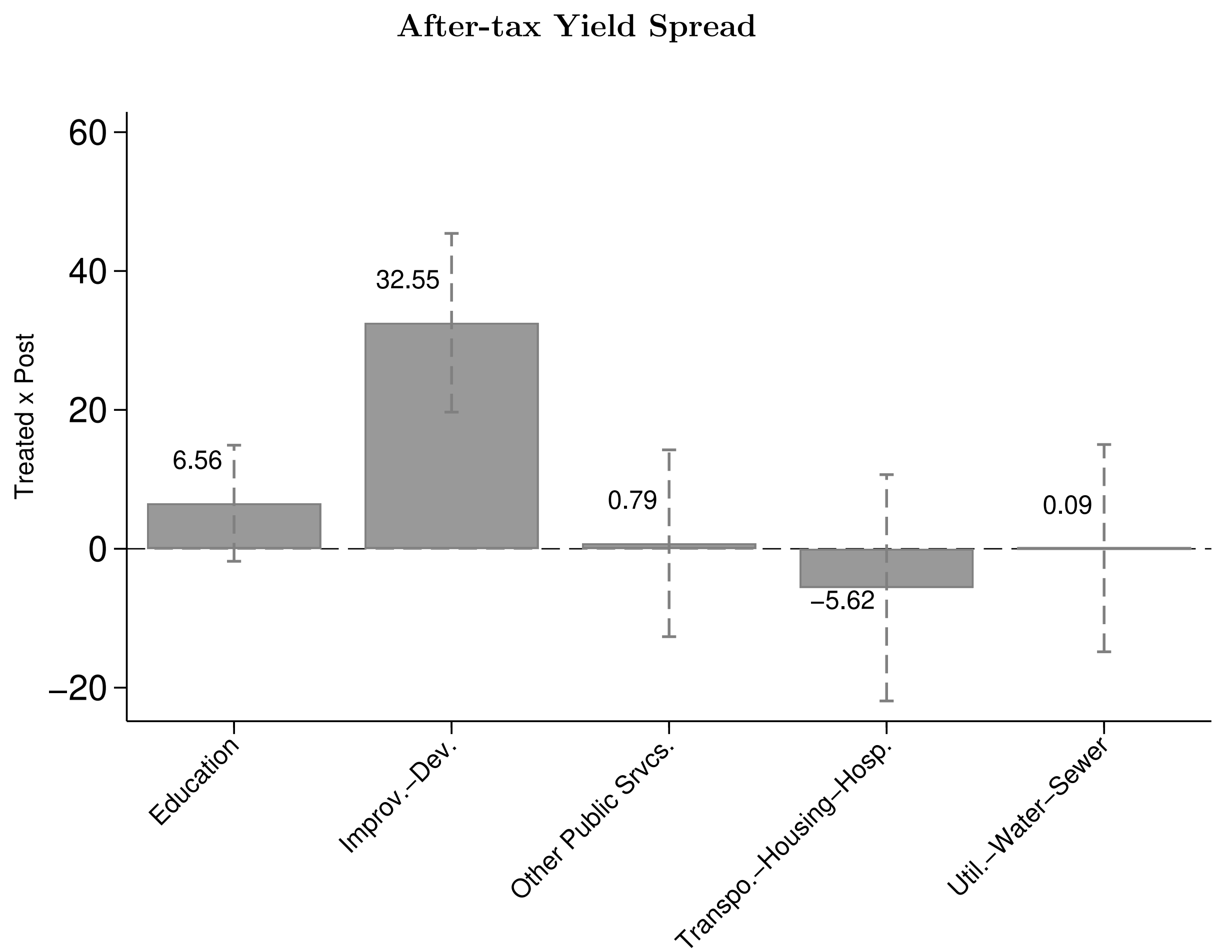
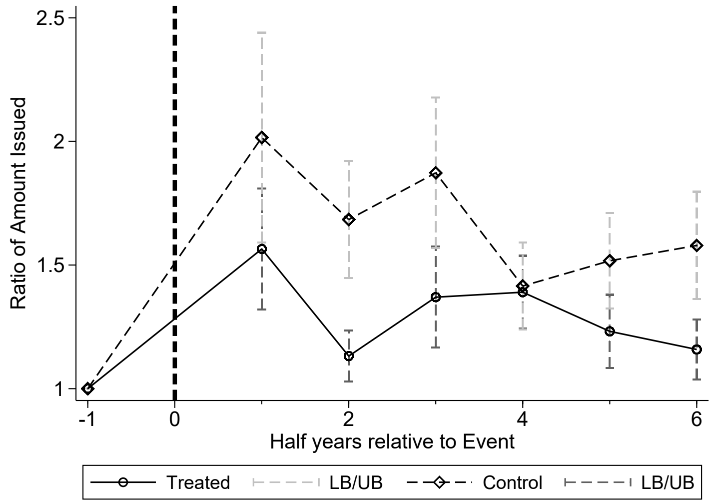


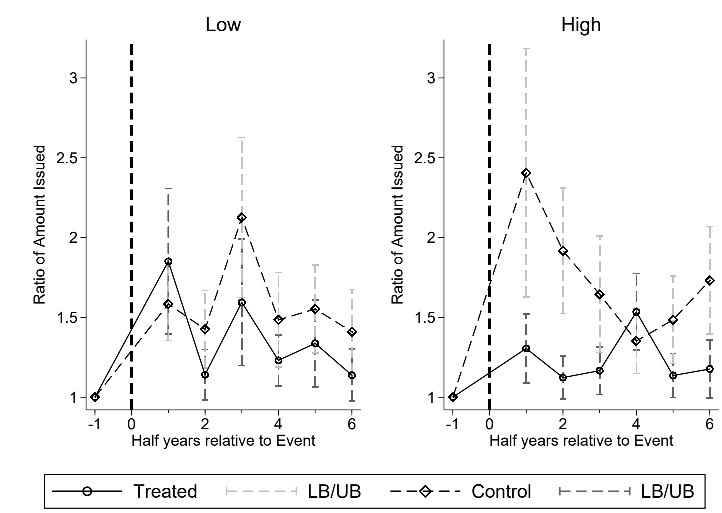
Figure 4: Heterogeneity based on Purpose of Bonds: The figure shows results for our main interaction term, β_0, from Equation (1). We modify the baseline equation to interact with indicators corresponding to the different types of municipal bonds, based on the use of proceeds. We additionally control for group-month fixed effects in the regression. Standard errors are double clustered by county bond issuer and year-month. The dashed lines represent 95% confidence intervals.

Panel A: Overall



(a)

Panel B: Based on County Dependence (Employment)



(b)

Figure 5: Primary Market Bond Issuance: The figure shows the county level aggregate volume of bond issuance for treated and control counties after the firm bankruptcy filing. For each county, we calculate the total par value of bonds issued in the six month rolling window before the corresponding bankruptcy event. We normalize this value to one and compute total par value of new issues relative to this amount in the half years after the announcement. The ratio represents the relative growth in issuance among treated counties, compared to the corresponding growth of control counties' issuance. The vertical bars show the upper and lower limits based on the standard errors of the mean values. In Panel A, we show all treated counties versus their matching control counties. In Panel B, we show the impact on sub-samples of low versus high level of county-level dependence on the industry of bankrupt firm among treated counties. We measure dependence based on the share of aggregate employment in the county. The control group counties are matched using five variables in the year before firm bankruptcy: unemployment rate, change in unemployment rate, log(labor force), change in labor force, and average yield of the county in that year.

Table 1: Summary Statistics

This table summarizes the ex-ante financial characteristics for our sample of bankruptcy firms during 2006-2016 and the corresponding headquarter counties. All figures in Panel A are in USD Million, except the number of employees. In Panel B, we report the difference between the average values of the ex-ante characteristics between the treatment (HQ) and control counties. Panel C provides more details about the distribution of those variables at the county level.

Panel A: Firm Bankruptcies in Manufacturing

	Mean	Median	Std. Dev.
Total Revenue	578.5	31.1	1,518.3
EBITDA	14.6	-4.7	101.5
Net Income	-75.0	-13.2	206.1
Total Assets	461.6	43.7	1,142.8
LT Debt - Total	105.0	1.6	315.9
Equity	-27.5	0.1	337.6
Employees	2,168.9	167.5	7,171.5

Panel B: Difference between Treated and Control Counties

	Mean (Treated)	Mean (Control)	Difference	T-Stat
Annual Trading Volume (USD mio)	761.9	589.2	172.74	1.23
Annual Average Yield (%)	3.5	3.5	-0.05	-0.39
Population	884,806.9	707,202.2	177,604.66	1.71
Unemployment Rate (%)	0.1	0.1	0.00	0.10
Laborforce	458,578.2	371,886.1	86,692.05	1.67
Log(Laborforce)	12.6	12.5	0.13	1.00
Revenue (USD million)	4.6	3.4	1.22	1.92
Expenditure (USD million)	4.6	3.4	1.13	1.82
Surplus (USD million)	0.0	-0.1	0.09	1.58
Zillow HPI	300,816.0	270,647.2	30,168.80	1.04

Panel C: Distribution between Treated and Control Counties

	Mean	p25	p50	p75	Std. Dev.
Treated					
Annual Trading Volume (USD mio)	761.9	85.0	242.3	880.7	1,176.2
Annual Average Yield (%)	3.5	2.9	3.8	4.1	1.0
Population	884,806.9	361,000.0	686,600.0	1,023,859.0	1,033,083.0
Unemployment Rate (%)	0.1	0.0	0.1	0.1	0.0
Log(Laborforce)	12.6	12.1	12.8	13.3	1.0
Revenue (USD million)	4.6	1.3	2.7	5.9	6.5
Expenditure (USD million)	4.6	1.3	2.8	6.0	6.3
Zillow HPI	300,816.0	138,200.0	237,100.0	379,700.0	245,562.8
Control					
Annual Trading Volume (USD mio)	589.2	87.0	213.9	601.9	1,059.5
Annual Average Yield (%)	3.5	3.1	3.7	4.1	1.0
Population	707,202.2	350,858.0	647,187.0	911,626.0	553,934.2
Unemployment Rate (%)	0.1	0.0	0.1	0.1	0.0
Log(Laborforce)	12.5	12.1	12.7	13.1	1.0
Revenue (USD million)	3.4	1.2	2.3	4.8	3.1
Expenditure (USD million)	3.4	1.2	2.3	4.9	3.2
Zillow HPI	270,647.2	148,300.0	206,000.0	323,600.0	211,200.1

Table 2: Impact of Corporate Bankruptcies on Borrowing Costs of Local Governments

This table reports the baseline results for our sample using Equation (1) estimating the differential effect on municipal bond yields of treated versus control counties after a firm bankruptcy filing. The primary coefficient of interest, β_0, is captured by the interaction term of Treated × Post. Panel A compares treatment and control bonds in the secondary market around an equal window of 3 years of the event. Columns (1)-(3) show the results for monthly after-tax average yield as the dependent variable. Specifically, Column (1) reports the effect using event pair fixed effects and year month fixed effects. In Column (2), we also introduce bond level controls consisting of coupon (%); log(amount issued in $); dummies for callable bonds, bond insurance, general obligation bonds and competitively issued bonds; remaining years to maturity; and inverse years to maturity. We provide descriptions of key variables in Table A1. In Column (3), we additionally control for the county-level variation in unemployment rate and labor force. We use the lagged values (to the year of bankruptcy filing) for log(labor force) and unemployment rate, and the percentage change in unemployment rate and labor force, respectively. We use a similar scheme for the remaining columns. In Columns (4)-(8), the dependent variable is after-tax yield spread, which is calculated using Equations (2) and (3). Our baseline specification comes from Column (6). Column (7) shows results by introducing event × county fixed effect. In Column (8), we also add event × year-month fixed effect. T-statistics are reported in brackets and standard errors are double clustered at county bond issuer and year month level, unless otherwise specified. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Dependent Variable:	After-tax yield			After-tax yield spread				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Post × Treated	16.56***	16.53***	11.35***	15.40***	15.01***	10.01***	10.15***	10.47***
	[4.38]	[4.44]	[3.59]	[4.23]	[4.12]	[3.23]	[3.29]	[3.48]
Post	-7.99***	-7.35***	-3.56*	-7.19***	-6.67***	-3.13	-3.21	0.00
	[-3.22]	[-2.92]	[-1.67]	[-2.89]	[-2.64]	[-1.46]	[-1.50]	[0.00]
Treated	-8.86**	-4.65	-1.39	-7.29*	-4.69	-1.62	0.00	0.00
	[-1.99]	[-1.04]	[-0.34]	[-1.85]	[-1.14]	[-0.44]	[.]	[0.00]
Event FE	✓	✓	✓	✓	✓	✓	✓	✓
County FE	✓	✓	✓	✓	✓	✓	✓	✓
Year-month FE	✓	✓	✓	✓	✓	✓	✓	✓
Bond Controls		✓	✓		✓	✓	✓	✓
County Controls			✓			✓	✓	✓
Event × County FE							✓	✓
Event × Year-month FE								✓
Adj.-R^2	0.313	0.554	0.559	0.594	0.645	0.648	0.648	0.655
Obs.	2,703,342	2,703,342	2,703,342	2,703,342	2,703,342	2,703,342	2,703,342	2,703,273
HQ Location	✓	✓	✓	✓	✓	✓	✓	✓
Other Locations	×	×	×	×	×	×	×	×

Table 3: Why do we focus on Headquarters?

This table reports the results from our analysis on the intensive margin. We use the baseline Equation (1), with suitable modifications, where necessary. In Columns (1)-(4), we use data from the EPA and Mergent Intellect on other non-HQ locations for firms in the sample. Columns (1) and (3) show the baseline results with aggregated locations across HQ and non-HQ counties from the baseline Equation 1. In Columns (2) and (4), we show the interaction effect by adding group-month fixed effects. Columns (5)-(7) show results from using all facilities from the Mergent Intellect database only. This may or may not include all the HQ locations. We report the results using all such locations in Column (5) based on Equation 1. We show the interaction effect from multi-facility locations in Column (6), while additionally controlling for group-month fixed effects. Finally, Column (7) shows results from ranking only the multi-facility locations in Column (6). We use the fraction of employment to assign the ranks. Sites with $\geq 25\%$ firm employees are ranked 1, followed by rank 2 for sites with $> 5\%$ of the firm employment. Rank 3 is assigned to the remaining locations. T-statistics are reported in brackets and standard errors are double clustered at county bond issuer and year month level, unless otherwise specified. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Dependent Variable:	After-tax yield spread						
	Compustat HQ vs Other Locations				All Facilities from Mergent		
Other Locations:	From EPA		From Mergent				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Post × Treated	4.02**		6.34***		3.83*		
	[2.18]		[3.16]		[1.66]		
Post × Treated × HQ County		12.74***		10.82***			
		[3.96]		[3.45]			
Post × Treated × Facility County		-2.83		2.92			
		[-1.17]		[1.24]			
Post × Treated × Multi-Facility						6.63**	
						[2.54]	
Post × Treated × Single-Facility						-2.86	
						[-0.59]	
Post × Treated ×Multi-facility Rank=1							14.77**
							[2.43]
Post × Treated ×Multi-facility Rank=2							4.76
							[0.56]
Post × Treated ×Multi-facility Rank=3							0.46
							[0.20]
Difference		16.89		8.75		9.38	
P-value		0.00		0.02		0.10	
Event FE	✓	✓	✓	✓	✓	✓	✓
County FE	✓	✓	✓	✓	✓	✓	✓
Year-month FE	✓	✓	✓	✓	✓	✓	✓
Controls	✓	✓	✓	✓	✓	✓	✓
Adj.-R^2	0.620	0.620	0.693	0.693	0.711	0.711	0.742
Obs.	6,369,196	6,369,196	6,675,920	6,675,920	4,347,334	4,347,334	3,146,863
HQ Location	✓	✓	✓	✓	✓	✓	✓
Other Locations	✓	✓	✓	✓	✓	✓	✓

Table 4: Mechanism: Source of Repayment and County's exposure to the bankrupt firm's industry

This table reports the results for the proposed mechanism at work. Panel A reports results from the baseline specification of (1) using subsamples of bonds, as shown. In Panel B, we report results for our baseline specification from Equation (1), which is interacted with dummies corresponding to county dependence (or county exposure). We additionally include group-month fixed effects in the modified baseline equation. We define county dependence based on the ex-ante median values of the respective treated county's share of employment (or wages) in the NAICS-3 industry of firm bankruptcy. Columns (1)-(3) show the results using *Dependence* based on aggregate share of wages. We use the aggregate share of employment in Columns (4)-(6). Further, we show results among all bonds in Column (1), followed by the sub-sample of revenue (RV) bonds in Column (2) and uninsured revenue (RV) bonds in Column (3). We follow a similar approach in Columns (4)-(6). T-statistics are reported in brackets and standard errors are double clustered at county bond issuer and year month level, unless otherwise specified. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Panel A: By Source of Repayment

Dependent Variable:	After-tax Yield Spread				
Sample:	All bonds	RV bonds	Uninsured RV	Insured RV	GO bonds
	(1)	(2)	(3)	(4)	(5)
Post × Treated	10.01***	12.56***	13.70**	9.67	3.59
	[3.23]	[2.74]	[2.26]	[1.62]	[1.11]
Event FE	✓	✓	✓	✓	✓
County FE	✓	✓	✓	✓	✓
Year-month FE	✓	✓	✓	✓	✓
Controls	✓	✓	✓	✓	✓
Adj.-R^2	0.648	0.644	0.576	0.701	0.678
Obs.	2,703,342	1,544,732	694,988	849,744	1,158,610

Panel B: By County Exposure using Dependence

Dependent Variable:	After-tax yield spread					
Type of Bonds:	All	RV	Uninsured RV	All	RV	Uninsured RV
Split by:	Share of Agg. Industry Wages			Share of Agg. Industry Employment		
	(1)	(2)	(3)	(4)	(5)	(6)
Post × Treated ×						
Above Median	15.01***	16.45***	17.84**	13.73***	16.46***	18.11**
	[3.69]	[2.83]	[2.14]	[3.34]	[2.78]	[2.08]
Below Median	1.90	5.33	9.18	3.88	5.96	9.66
	[0.41]	[0.84]	[1.11]	[0.87]	[0.96]	[1.23]
Difference	13.11	11.12	8.66	9.85	10.50	8.45
p-val	0.03	0.17	0.45	0.10	0.20	0.47
Event FE	✓	✓	✓	✓	✓	✓
County FE	✓	✓	✓	✓	✓	✓
Group Month FE	✓	✓	✓	✓	✓	✓
Controls	✓	✓	✓	✓	✓	✓
Adj.-R^2	0.649	0.645	0.577	0.649	0.645	0.577
Obs.	2,703,342	1,544,732	694,988	2,703,342	1,544,732	694,988

Table 5: Heterogeneity: Importance of the Bankrupt Firm for the HQ County

This table reports the results showing the heterogeneity in our main effect. We report results for our baseline specification from Equation (1), which is interacted with dummies corresponding to sub-groups indicated in each column. We additionally include group-month fixed effects in the modified baseline equation. Specifically, Columns (1)-(3) show the relative importance of the firm filing for bankruptcy. We divide the treated counties into two groups based on the median value of the ratios defined hereafter. These ratios are calculated using the corresponding values in the year before the bankruptcy filing. In Column (1), we use the ratio of the firm's operating income (EBIT) to the county's revenue. Column (2) uses the ratio of the firms' selling, general and administrative (SGA) expenses to the county's revenue. In Column (3), we use the ratio of the firm's plant, property, and equipment (PPE) to the property tax revenue of the county. T-statistics are reported in brackets and standard errors are double clustered at county bond issuer and year month level, unless otherwise specified. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Dependent Variable:	After-tax yield spread		
Split by:	$\frac{FirmEBIT}{CountyRevenue}$ (1)	$\frac{FirmSG\&A}{CountyRevenue}$ (2)	$\frac{FirmPP\&E}{PropertyTax}$ (3)
Post × Treated			
High	14.98***	20.84***	11.96**
	[3.41]	[3.20]	[2.26]
Low	3.49	5.15	7.80**
	[0.82]	[1.20]	[1.99]
Difference	11.49	15.69	4.16
P-value	0.06	0.06	0.53
Event FE	✓	✓	✓
County FE	✓	✓	✓
Group Month FE	✓	✓	✓
Controls	✓	✓	✓
Adj.-R^2	0.651	0.660	0.651
Obs.	2,555,676	2,210,711	2,555,676

Table 6: Robustness Tests

In this table we report results for various robustness tests on our baseline specification, i.e., Column (6) of Table 2. In Panel A, results controlling for unobserved factors at the bond, county, region, and municipal bond market levels. Specifically, in Column (1), we introduce bond fixed effect to the baseline. In Column (2), we impose region fixed effects. Columns (3)-(4) show the results after adding time-varying region-year and region-year month fixed effects, respectively. Column (5) shows the baseline results with year-month × GO bond indicator fixed effects. Thereafter, in Column (6), we use year-month × Insured bond status fixed effects. Finally, Column (21) shows the baseline results after including bond purpose × year-month fixed effects. Panel B corresponds to robustness checks using additional controls. In Column (1), we introduce additional county level time-varying covariates using lagged values of log(personal income) and log(house price index). In Column (2), we show the results for bonds with non-missing S&P credit ratings. We use the most recent ratings for a given CUSIP transaction. In Columns (3)-(4), we control for duration. First, in Column (3), we use duration in the controls by replacing years to maturity and inverse of years to maturity. Second, in Column (4), we use tax-adjusted duration to replace years to maturity and inverse of years to maturity. We drop bonds recently issued with respect to firm bankruptcy in Panel C. Columns (1)-(4) report regression results where we drop bonds that are dated within 6 months, 12 months, 24 months, and 36 months of the bankruptcy filing, respectively. We consider alternative econometric specifications in Panel D. Specifically, Columns (1)-(3) report results with alternative choices for clustering the standard errors, namely: issue and year-month, county bond issuer, and county bond issue, respectively. Column (4) shows the results by introducing event-month fixed effects to the baseline. In Column (5), we drop the year month fixed effect and replace it with the event-month fixed effect in the baseline. Finally, Column (6) shows the result by changing year-month fixed effect to calendar year fixed effects. We focus on the sensitivity to trade size in Panel E. In Columns (1)-(4), we report results using only customer-buy trades with transaction size > \$25,000, > \$50,000, ≤25,000, and ≤ \$50,000, respectively. Panel F shows robustness in results to additional considerations. In Column (1), we restrict our sample of bonds to tax-exempt municipal bonds only. In Column (2), we only use transactions after the financial crisis of 2010. Columns (3)-(4) show the baseline effect by changing the dependent variable to average yield and average yield spread, respectively. In Column (5), we restrict the choice of control group among counties which do not also observe a publicly listed manufacturing firm bankruptcy within two years of a given treated county. This exclusion criterion is imposed based on the publicly listed manufacturing bankruptcy events in our main sample set. Column (6) shows results by expanding the original sample of events to the broader set of bankruptcy filings from tradeable sector industries. In Panel G, we report the baseline results by altering the length of the event window duration. Finally, Panel H reports the baseline specification by changing the beginning or ending point of the event scale, holding the other period constant at 36 months. T-statistics are reported in brackets and standard errors are double clustered at county bond issuer and year month level, unless otherwise specified. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Panel A: Other Unobservables

Dependent Variable:	After-tax yield spread						
Add:	Bond FE	Region FE	Region × Year FE	Region × YM FE	GO bond × YM FE	Insured × YM FE	Purpose × YM FE
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Post × Treated	8.23**	10.01***	10.42***	10.66***	8.74***	7.74***	7.88***
	[2.57]	[3.23]	[3.38]	[3.45]	[2.95]	[2.62]	[3.02]
Adj.-R^2	0.833	0.648	0.650	0.651	0.655	0.652	0.677
Obs.	2,697,225	2,703,342	2,703,342	2,703,342	2,703,342	2,703,342	2,703,113

Panel B: Additional Controls

Dependent Variable:	After-tax yield spread			
Add:	More Controls	Rating	Duration	After-tax Duration
	(1)	(2)	(3)	(4)
Post × Treated	9.60***	8.29**	10.60***	10.61***
	[2.95]	[2.33]	[3.35]	[3.32]
Adj.-R^2	0.651	0.663	0.637	0.626
Obs.	2,504,271	2,016,734	2,688,584	2,688,584

Panel C: Drop Recent Bonds

Dependent Variable:	After-tax yield spread			
Dated Within:	6 months	12 months	24 months	36 months
	(1)	(2)	(3)	(4)
Post × Treated	10.01***	10.42***	11.81***	11.70***
	[3.14]	[3.17]	[3.24]	[2.91]
Adj.-R^2	0.641	0.632	0.615	0.595
Obs.	2,518,961	2,348,294	2,024,963	1,713,545

Panel D: Alternative Specifications

Dependent Variable:	After-tax yield spread					
	Clustering			Specification		
Modification:	Issue and YM	Issuer	Issue	Add event-month FE	Change YM FE to event-month FE	Change YM FE to and year FE
	(1)	(2)	(3)	(4)	(5)	(6)
Post × Treated	10.01***	10.01***	10.01***	10.03***	23.27***	10.35***
	[4.44]	[3.30]	[4.81]	[3.23]	[3.69]	[3.38]
Adj.-R^2	0.648	0.648	0.648	0.648	0.416	0.512
Obs.	2,703,342	2,703,342	2,703,342	2,703,342	2,703,342	2,703,342

Panel E: Sensitivity to Trade Size

Dependent Variable:	After-tax yield spread			
in USD:	>25,000	>50,000	≤25,000	≤50,000
	(1)	(2)	(3)	(4)
Post × Treated	7.84**	5.83*	9.43***	9.86***
	[2.38]	[1.68]	[2.91]	[3.13]
Adj.-R^2	0.646	0.627	0.667	0.661
Obs.	1,482,804	872,465	1,915,893	2,323,079

Panel F: Additional Considerations

Dependent Variable:	After-tax yield spread					
	Tax-exempt Bonds	Financial Crisis	Other DV		Restricted Control Counties	Bankruptcies in Tradeables
			Yield	Spread		
	(1)	(2)	(3)	(4)	(5)	(6)
Post × Treated	11.02***	11.04**	7.19***	6.51***	8.69**	8.27***
	[3.55]	[2.38]	[3.91]	[3.39]	[2.45]	[3.05]
Adj.-R^2	0.662	0.438	0.571	0.801	0.648	0.634
Obs.	2,453,646	1,272,300	2,835,820	2,703,829	2,567,695	3,731,872

Panel G: Sensitivity to Length of Event Window

Dependent Variable:	After-tax yield spread					
Window (months):	[-12,+12]	[-18,+18]	[-24,+24]	[-30,+30]	[-48,+48]	[-60,+60]
	(1)	(2)	(3)	(4)	(5)	(6)
Post x Treated	3.86**	5.17**	7.42***	9.28***	10.67***	11.98***
	[2.34]	[2.40]	[2.93]	[3.21]	[3.29]	[3.77]
Adj.-R^2	0.673	0.662	0.651	0.651	0.638	0.628
Obs.	971,594	1,427,506	1,861,432	2,288,998	3,471,961	4,159,651

Panel H: Sensitivity to Start/End of Event Window

Dependent Variable:	After-tax yield spread					
Window (months):	[-6,+36]	[-12,+36]	[-24,+36]	[-36,+6]	[-36,+12]	[-36,+24]
	(1)	(2)	(3)	(4)	(5)	(6)
Post x Treated	6.64***	8.73***	9.56***	4.41*	5.62**	7.89***
	[2.68]	[3.49]	[3.39]	[1.78]	[2.15]	[2.71]
Adj.-R^2	0.632	0.636	0.642	0.679	0.675	0.658
Obs.	1,751,064	1,977,783	2,373,529	1,457,062	1,697,153	2,191,245

Table 7: Impact on Primary Market of Municipal Bonds

This table shows the effect of bankruptcy filing on new bond issuances using a difference-in-differences estimate similar to the baseline specification. It is based on primary market bonds in Equation (5) for offering yields. The dependent variable in Columns (1)-(3) is the after-tax offering yield. In Column (1), we show the result by using only event-pair fixed effects and issuer fixed effects in the baseline equation. Next, in Column (2), we introduce bond level controls. Column (3) shows the results with county controls and county fixed effects. T-statistics are reported in brackets and standard errors are clustered at issue level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Dependent Variable:	After-tax Offering Yield		
	(1)	(2)	(3)
Post × Treated	13.69***	10.49***	5.79***
	[0.00]	[0.00]	[0.00]
Post	-9.41***	-8.19***	-6.02***
	[0.00]	[0.00]	[0.00]
Treated	-3.09	-1.71	1.41
	[0.11]	[0.22]	[0.30]
Event FE	✓	✓	✓
Issuer FE	✓	✓	✓
Bond Controls		✓	✓
County Controls			✓
Adj.-R^2	0.474	0.843	0.845
Obs.	424,655	424,655	424,655

Table 8: Impact on the Local Economy of the County

This table shows the impact of a firm bankruptcy on the local economy. We use the annualized version of our baseline equation as the primary specification for this table, shown in Equation (6). Panel A corresponds to the overall effect on county-level GDP growth and growth in establishments. In Panel B, we show results based on the county-level dependence. We define county dependence using the median values of the respective treated county's share of employment in the NAICS-3 industry of firm bankruptcy. T-statistics are reported in brackets and standard errors are clustered at the event-pair level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Panel A: Overall

Dependent Variable:	County GDP growth (%)			County Establishments growth (%)		
Window (years):	[-3, +3]	[-3, +5]	[-3, +7]	[-3, +3]	[-3, +5]	[-3, +7]
	(1)	(2)	(3)	(4)	(5)	(6)
Treated × Post	-0.67**	-0.53*	-0.52*	-0.13	-0.24	-0.28
	[0.04]	[0.08]	[0.08]	[0.74]	[0.52]	[0.44]
Event FE	✓	✓	✓	✓	✓	✓
County FE	✓	✓	✓	✓	✓	✓
County Controls	✓	✓	✓	✓	✓	✓
Adj.-R^2	0.411	0.377	0.355	0.120	0.130	0.139
Obs.	1,704	2,094	2,423	1,732	2,128	2,462

Panel B: By County Exposure using Dependence (Employment)

Dependent Variable:	County GDP growth (%)			County Establishments growth (%)		
Window (years):	[-3, +3]	[-3, +5]	[-3, +7]	[-3, +3]	[-3, +5]	[-3, +7]
Treated × Post ×	(1)	(2)	(3)	(4)	(5)	(6)
Above Median	-1.38***	-1.23***	-1.22***	-0.08	-0.18	-0.28
	[0.00]	[0.00]	[0.00]	[0.83]	[0.60]	[0.38]
Below Median	0.11	0.26	0.27	-0.18	-0.31	-0.28
	[0.81]	[0.52]	[0.50]	[0.80]	[0.66]	[0.68]
Difference	-1.48	-1.48	-1.48	0.10	0.13	0.00
p-val	0.02	0.01	0.01	0.90	0.88	1.00
Event FE	✓	✓	✓	✓	✓	✓
County FE	✓	✓	✓	✓	✓	✓
Group FE	✓	✓	✓	✓	✓	✓
County Controls	✓	✓	✓	✓	✓	✓
Adj.-R^2	0.412	0.378	0.356	0.119	0.130	0.140
Obs.	1,704	2,094	2,423	1,732	2,128	2,462

Table 9: Evidence on Mild Recovery of the County

This table shows the impact of a firm bankruptcy on the local economy. We use the annualized version of our baseline equation as the primary specification for this table, shown in Equation (6). The dependent variables are county-level GDP growth and growth in establishments. In each of the panels, we split the treated counties along the metric mentioned in the panel. We classify the counties in the top quartile as high, while the remaining are grouped as low. T-statistics are reported in brackets and standard errors are clustered at the event-pair level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Panel A: Recovery (By County Bachelor Education)

Dependent Variable:	County GDP growth (%)			County Establishments growth (%)		
Window (years):	[-3, +3]	[-3, +5]	[-3, +7]	[-3, +3]	[-3, +5]	[-3, +7]
Treated × Post ×	(1)	(2)	(3)	(4)	(5)	(6)
High	-0.34	-0.09	0.06	0.85**	0.62	0.54
	[0.58]	[0.87]	[0.92]	[0.05]	[0.10]	[0.14]
Low	-0.78**	-0.67*	-0.71**	-0.47	-0.54	-0.57
	[0.05]	[0.06]	[0.04]	[0.35]	[0.27]	[0.23]
High vs Low	0.44	0.58	0.77	1.31	1.16	1.10
p-val	0.54	0.39	0.27	0.05	0.07	0.07
Event FE	✓	✓	✓	✓	✓	✓
County FE	✓	✓	✓	✓	✓	✓
Group FE	✓	✓	✓	✓	✓	✓
County Controls	✓	✓	✓	✓	✓	✓
Adj.-R^2	0.411	0.378	0.356	0.121	0.131	0.140
Obs.	1,704	2,094	2,423	1,732	2,128	2,462

Panel B: Recovery (By Number of Pleasant Days)

Dependent Variable:	County GDP growth (%)			County Establishments growth (%)		
Window (years):	[-3, +3]	[-3, +5]	[-3, +7]	[-3, +3]	[-3, +5]	[-3, +7]
Treated × Post ×	(1)	(2)	(3)	(4)	(5)	(6)
High	-1.66***	-1.45***	-1.42***	0.53	0.29	0.14
	[0.00]	[0.01]	[0.00]	[0.27]	[0.52]	[0.74]
Low	-0.37	-0.25	-0.24	-0.34	-0.43	-0.44
	[0.35]	[0.49]	[0.50]	[0.49]	[0.37]	[0.36]
High vs Low	-1.30	-1.21	-1.18	0.86	0.72	0.58
p-val	0.06	0.06	0.05	0.23	0.29	0.37
Event FE	✓	✓	✓	✓	✓	✓
County FE	✓	✓	✓	✓	✓	✓
Group FE	✓	✓	✓	✓	✓	✓
County Controls	✓	✓	✓	✓	✓	✓
Adj.-R^2	0.412	0.378	0.356	0.119	0.130	0.139
Obs.	1,704	2,094	2,423	1,732	2,128	2,462

Panel C: Recovery (By County-level Entrepreneurship Quotient)

Dependent Variable:	County GDP growth (%)			County Establishments growth (%)		
Window (years):	[-3, +3]	[-3, +5]	[-3, +7]	[-3, +3]	[-3, +5]	[-3, +7]
Treated × Post ×	(1)	(2)	(3)	(4)	(5)	(6)
High	-1.48***	-1.36***	-1.30***	0.44	0.31	0.09
	[0.00]	[0.01]	[0.01]	[0.33]	[0.46]	[0.81]
Low	-0.40	-0.24	-0.25	-0.31	-0.43	-0.41
	[0.32]	[0.50]	[0.49]	[0.53]	[0.39]	[0.39]
High vs Low	-1.07	-1.12	-1.05	0.75	0.74	0.51
p-val	0.11	0.08	0.09	0.29	0.28	0.43
Event FE	✓	✓	✓	✓	✓	✓
County FE	✓	✓	✓	✓	✓	✓
Group FE	✓	✓	✓	✓	✓	✓
County Controls	✓	✓	✓	✓	✓	✓
Adj.-R^2	0.411	0.378	0.355	0.122	0.133	0.142
Obs.	1,704	2,094	2,423	1,732	2,128	2,462

Panel D: Recovery (By County-level Start-up Formation Rate)

Dependent Variable:	County GDP growth (%)			County Establishments growth (%)		
Window (years):	[-3, +3]	[-3, +5]	[-3, +7]	[-3, +3]	[-3, +5]	[-3, +7]
Treated × Post ×	(1)	(2)	(3)	(4)	(5)	(6)
High	-1.23***	-1.05***	-1.01**	-0.15	-0.42	-0.49
	[0.01]	[0.01]	[0.01]	[0.75]	[0.38]	[0.26]
Low	-0.49	-0.36	-0.36	-0.13	-0.20	-0.23
	[0.23]	[0.33]	[0.32]	[0.80]	[0.67]	[0.62]
High vs Low	-0.75	-0.69	-0.65	-0.03	-0.22	-0.26
p-val	0.23	0.21	0.23	0.97	0.75	0.69
Event FE	✓	✓	✓	✓	✓	✓
County FE	✓	✓	✓	✓	✓	✓
Group FE	✓	✓	✓	✓	✓	✓
County Controls	✓	✓	✓	✓	✓	✓
Adj.-R^2	0.413	0.378	0.355	0.120	0.130	0.139
Obs.	1,704	2,094	2,423	1,732	2,128	2,462

Panel E: Recovery (By County-level Regional Entrepreneurship Acceleration)

Dependent Variable:	County GDP growth (%)			County Establishments growth (%)		
Window (years):	[-3, +3]	[-3, +5]	[-3, +7]	[-3, +3]	[-3, +5]	[-3, +7]
Treated × Post ×	(1)	(2)	(3)	(4)	(5)	(6)
High	0.11	0.20	0.59	0.54	0.51	0.45
	[0.87]	[0.75]	[0.37]	[0.10]	[0.16]	[0.21]
Low	-0.87**	-0.72**	-0.81**	-0.30	-0.44	-0.48
	[0.02]	[0.03]	[0.01]	[0.52]	[0.34]	[0.29]
High vs Low	0.99	0.91	1.40	0.84	0.96	0.93
p-val	0.20	0.20	0.06	0.14	0.10	0.10
Event FE	✓	✓	✓	✓	✓	✓
County FE	✓	✓	✓	✓	✓	✓
Group FE	✓	✓	✓	✓	✓	✓
County Controls	✓	✓	✓	✓	✓	✓
Adj.-R^2	0.410	0.376	0.355	0.118	0.128	0.138
Obs.	1,704	2,094	2,423	1,732	2,128	2,462

Panel F: Recovery (By County-level Number of Patents)

Dependent Variable:	County GDP growth (%)			County Establishments growth (%)		
Window (years):	[-3, +3]	[-3, +5]	[-3, +7]	[-3, +3]	[-3, +5]	[-3, +7]
Treated × Post ×	(1)	(2)	(3)	(4)	(5)	(6)
High	-1.67***	-1.50**	-1.45**	0.38	0.22	0.17
	[0.01]	[0.01]	[0.01]	[0.48]	[0.67]	[0.73]
Low	-0.36	-0.23	-0.24	-0.30	-0.41	-0.44
	[0.35]	[0.50]	[0.48]	[0.53]	[0.38]	[0.33]
High vs Low	-1.31	-1.26	-1.21	0.68	0.64	0.61
p-val	0.08	0.07	0.07	0.35	0.37	0.36
Event FE	✓	✓	✓	✓	✓	✓
County FE	✓	✓	✓	✓	✓	✓
Group FE	✓	✓	✓	✓	✓	✓
County Controls	✓	✓	✓	✓	✓	✓
Adj.-R^2	0.411	0.378	0.356	0.120	0.131	0.140
Obs.	1,704	2,094	2,423	1,732	2,128	2,462

Panel G: Recovery (By County-level Percentage White)

Dependent Variable:	County GDP growth (%)			County Establishments growth (%)		
Window (years):	[-3, +3]	[-3, +5]	[-3, +7]	[-3, +3]	[-3, +5]	[-3, +7]
Treated × Post ×	(1)	(2)	(3)	(4)	(5)	(6)
High	0.04	-0.07	-0.13	-0.07	-0.21	-0.25
	[0.95]	[0.92]	[0.84]	[0.90]	[0.69]	[0.61]
Low	-0.90**	-0.68**	-0.65*	-0.15	-0.26	-0.30
	[0.01]	[0.04]	[0.05]	[0.75]	[0.58]	[0.52]
High vs Low	0.94	0.61	0.51	0.07	0.04	0.05
p-val	0.26	0.42	0.49	0.93	0.95	0.94
Event FE	✓	✓	✓	✓	✓	✓
County FE	✓	✓	✓	✓	✓	✓
Group FE	✓	✓	✓	✓	✓	✓
County Controls	✓	✓	✓	✓	✓	✓
Adj.-R^2	0.410	0.376	0.354	0.117	0.128	0.138
Obs.	1,704	2,094	2,423	1,732	2,128	2,462

Panel H: Recovery (By County-level Percentage Moved from a different county)

Dependent Variable:	County GDP growth (%)			County Establishments growth (%)		
Window (years):	[-3, +3]	[-3, +5]	[-3, +7]	[-3, +3]	[-3, +5]	[-3, +7]
Treated × Post ×	(1)	(2)	(3)	(4)	(5)	(6)
High	-1.20*	-1.04*	-1.04*	0.36	0.17	-0.01
	[0.08]	[0.07]	[0.06]	[0.43]	[0.69]	[0.99]
Low	-0.51	-0.38	-0.36	-0.28	-0.37	-0.37
	[0.18]	[0.29]	[0.30]	[0.57]	[0.44]	[0.43]
High vs Low	-0.69	-0.66	-0.68	0.64	0.54	0.37
p-val	0.39	0.32	0.30	0.35	0.41	0.56
Event FE	✓	✓	✓	✓	✓	✓
County FE	✓	✓	✓	✓	✓	✓
Group FE	✓	✓	✓	✓	✓	✓
County Controls	✓	✓	✓	✓	✓	✓
Adj.-R^2	0.411	0.378	0.356	0.118	0.128	0.138
Obs.	1,704	2,094	2,423	1,732	2,128	2,462

For Online Publication–Internet Appendix

A Additional Results

In Section A.1, we provide results for additional considerations of matching.

A.1 Additional Matching Strategies

We understand that identifying a suitable match for the treated counties using nearest-neighbor matching based on five key variables may not be the only possible approach. Since we lack a perfect counterfactual in our setting, we demonstrate robustness to the choice of our matching strategy by using eight additional approaches in Table IA2. First, in Columns (1)-(3), we introduce a sixth variable to match counties based on their debt capacity. Our metrics for debt capacity are based on Chava, Malakar, and Singh (2020). Using measures linked to interest expenditure as additional matching variables to identify control counties, we show that that the baseline effect ranges from 6.11 to 10.89 bps. The effect remains statistically significant and economically meaningful. We provide the kernel density plot between treated and control counties for our matching variables with debt capacity using interest expenditure in Figure IA4, Figure IA5 and Figure IA6.

Next, we address the concern that the control groups identified in the baseline approach may be different based on the size of bonds issued or the maturity bucket of municipal bonds in Columns (4)-(5). By using the average amount issued and average maturity of bonds, respectively, we show that the increase in yield spreads amounts to 4.73 and 5.74 bps, respectively. We present a comparison of our treated and control county characteristics on these matching variables in Figure IA7 and Figure IA8. Finally, we show robustness to our matching with respect to the geographic region of the control groups in Columns (6)-(8). In Column (6), we require that the baseline choice of control group comes from within the same geographic region of the United States and find the magnitude to be 5.64 bps. Columns (7)-(8) consider three and five nearest neighbors matching (instead of one) among the control counties, respectively. Our results show an increase in bond yields of 5.12 and 4 bps, respectively. We argue that since the additional neighbors are all given an equal weight among the controls, this may introduce some noise to the estimates. We show the distribution of the matching variables between the treated and control counties under these additional geographic considerations in Figure IA9 and Figure IA10.

A.2 Discussion on Budgetary Restrictions vs State Incentives

A.2.1 State Imposed Budgetary Restrictions

Tax and expenditure limits impose controls on the taxing and spending ability of local governments. Beyond taxes, local governments may also raise money by issuing public debt to finance their expenditures. In this regard, debt limits restrict the ability of local governments to access the public debt market through bond issuances. The primary motivation behind these restrictions is minimizing defaults and over-borrowing. Often, a super-majority of voters is required to exceed such debt limits. Some of these taxing and spending limitations on local governments can be traced back to Proposition 13 in California in 1978. Unfunded budget mandates from the state may also control some local government budget expenditures. Poterba and Rueben (1999) show that bond market participants consider fiscal institutions in assessing the risk characteristics of tax-exempt bonds. In this light, we hypothesize that local budgetary restrictions imposed by states reduce the ability of local governments to respond to large firm bankruptcies in their counties.

We present our results in Table IA4 based on a modified version of the baseline Equation (1) using data from Goodman and Leland (2019). We additionally control for group-year month fixed effects to account for average differences across groups in a given calendar year month. In Columns (1)-(3), we divide the treated counties into two groups based on the presence or absence of local restrictions on debt issuance, overall property tax, and total expenditure, respectively. Column (1) shows that counties with debt issuance limits exhibit a bond yield spread increase of 11.65 bps, while there is no significant effect on counties without such limits in the three years after a firm files for bankruptcy. Similarly, Column (2) shows that counties with an overall limit on property tax exhibit an increase of 19.30 bps in the bond yield spreads after a bankruptcy filing. There is a similar effect when we use expenditure limits in Column (3), although the difference between the two groups is not statistically significant.

Finally, since these budgetary restrictions are not mutually exclusive among the counties, we consider a linear combination of these dummy indicators by summing them up for each treated county. The overall index used in this combination of restrictions (shown in Column (4)) ranges between zero and three. Our results in Column (4) suggest that multiple (two or more) budgetary restrictions result in an increase in bond yield spreads of 19.14 bps. The corresponding impact on counties with one or zero restrictions is much smaller but also statistically insignificant. Taken together, the evidence based on county budgetary restrictions suggests that the municipal bond market yield spreads are more adversely affected when a bankrupt firm is located in a county with greater state-imposed controls over the county's budget/debt issuance.

A.2.2 State Level Business Incentives

The business environment in a given county may differ based on various factors and policies by the corresponding state. One such crucial factor could be the state level business incentives at the time a firm files for bankruptcy. We use the Panel Database on Incentives and Taxes (PDIT) from the Upjohn Institute[8], which includes data on incentives from 33 states in the US from 1990 to 2015. The data provides information on the taxes paid and incentives received by a business for a new hypothetical facility opened in a given year across 45 industries. The resulting data are reported as a percentage of the value-added for that particular industry. For our purposes, we focus on the 19 export-based manufacturing industries. We manually match these to the corresponding 4-digit NAICS industry of the firm filing for bankruptcy. Since these data are produced at the state level, we weigh the percentage of value added incentives by the corresponding proportion of the industry's GDP at the county level relative to the manufacturing GDP of the county. We use the average value by discounting the present value of incentives over the 20-year simulation period using a 12% real discount rate. Such a high discount rate reflects the perspective of many corporate decision makers, who place a higher value on short-term factors than long-term factors.

We report our results in Table IA5 by using the baseline Equation (1) and modify it suitably with group-year month fixed effects to control for the average trends in each group. First, in Column (1), we show the results by dividing the treated counties based on the median value of net tax (after incentives). We find that bond yield spreads increase by 14.04 bps among counties with high net tax. On the other hand, the increase in yield spreads is only 3.59 bps for counties with low net tax, but

[8] https://www.upjohn.org/bied/home.php

the coefficient is statistically indistinguishable from zero. That said, a higher tax incentive likely could be beneficial to businesses. We find consistent results in Columns (2)-(4), suggesting that the impact on bond yield spreads is higher when the state sponsors lower level of incentives before the bankruptcy event. For example, Column (3) compares treated counties using the investment tax credit. We find that counties with low incentives experience an increase in yields of 15.61 bps, while the effect on the high group is statistically insignificant. These results suggest that counties located in states where business incentives are less generous tend to experience a greater impact on their municipal bond yield spreads. Alternatively, state-level incentive provisions may help the counties to mitigate the broader economic impact after a large firm bankruptcy.

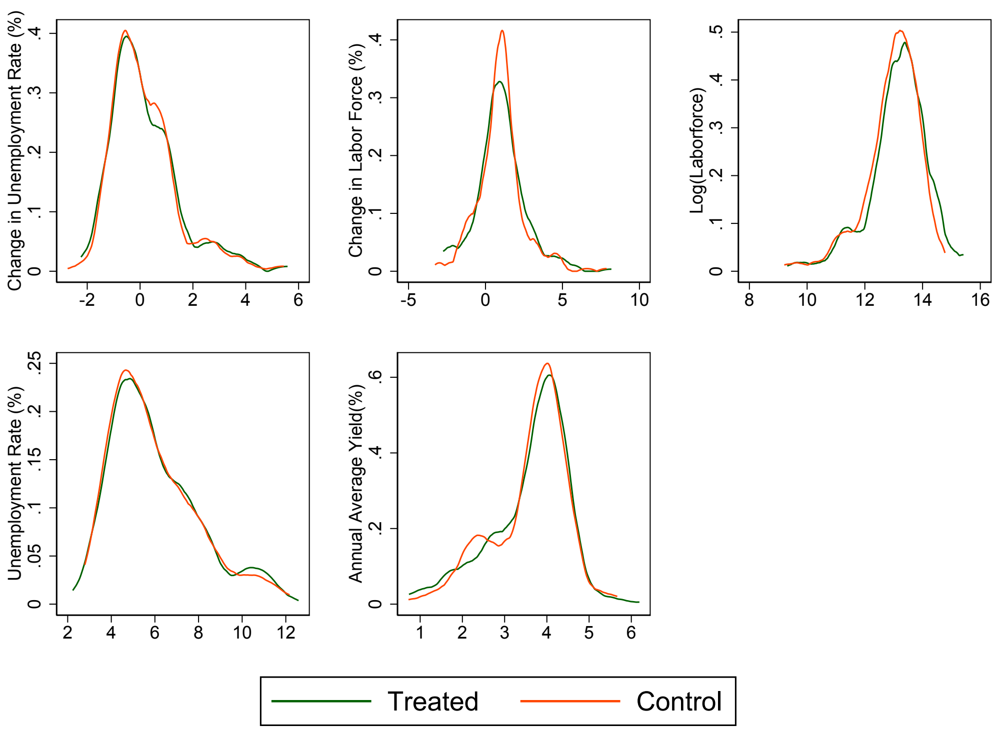


Figure IA1: Matching County Characteristics: The figure shows the kernel density plot based on matching the treatment counties using five variables in the year before firm bankruptcy: unemployment rate, change in unemployment rate, log(labor force), change in labor force, and average yield of the county in that year.

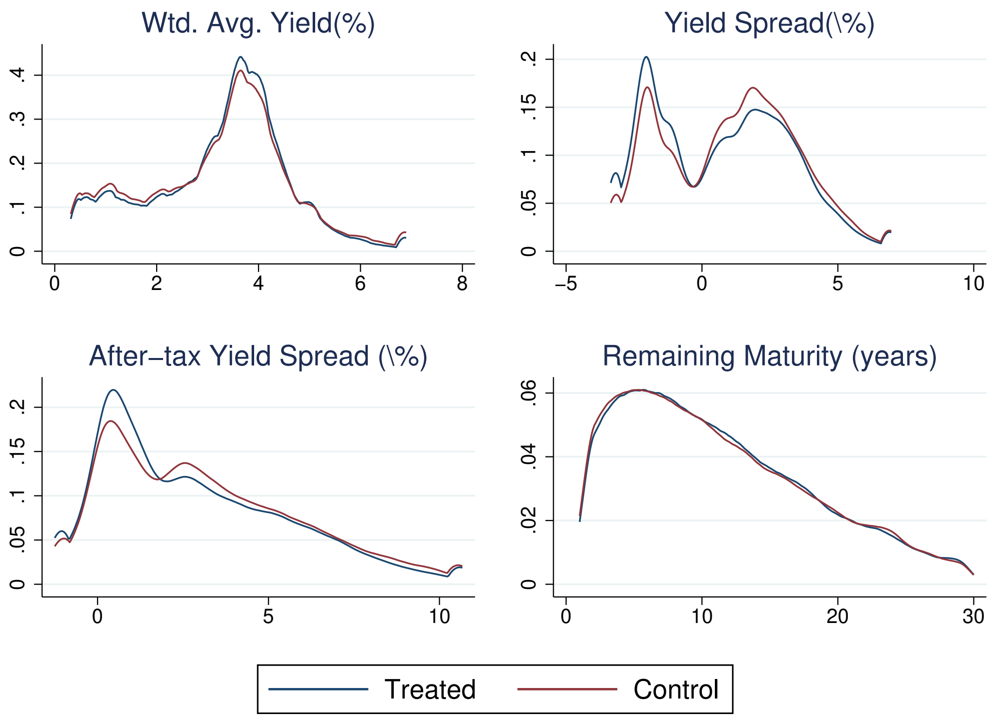


Figure IA2: Secondary Market Bond Features: The figure shows the kernel density plot for municipal bond aspects from the secondary market. The county-level matching is based on five variables in the year before firm bankruptcy: unemployment rate, change in unemployment rate, log(labor force), change in labor force, and average yield of the county in that year.

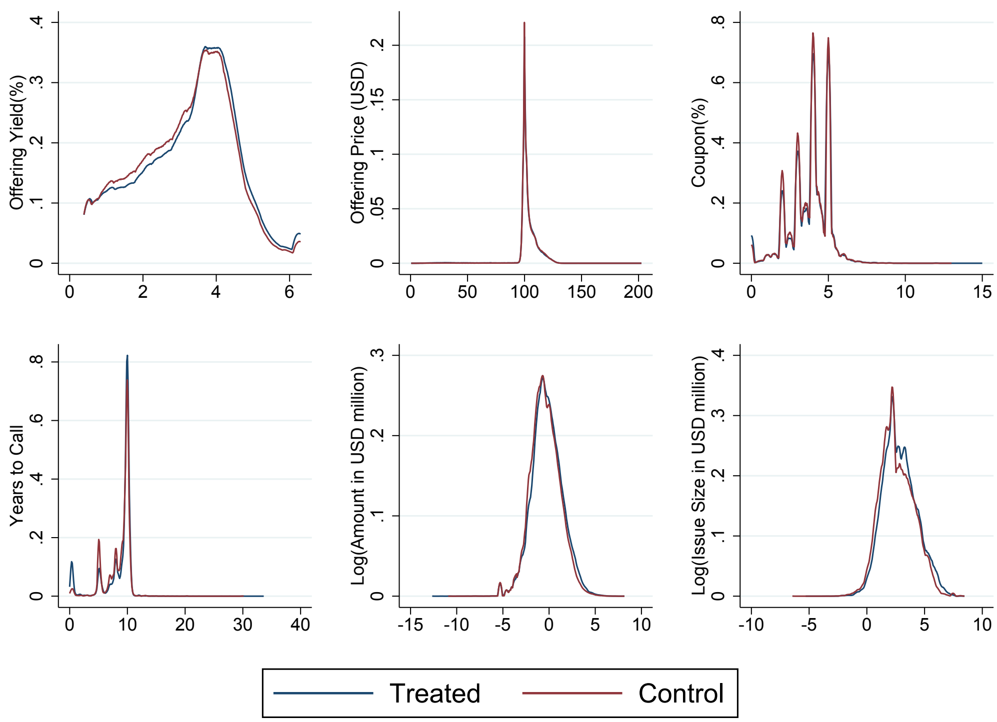


Figure IA3: Primary Market Muni Bond Characteristics: The figure shows the kernel density plot for municipal bond aspects from the primary market. The county-level matching is based on five variables in the year before firm bankruptcy: unemployment rate, change in unemployment rate, log(labor force), change in labor force, and average yield of the county in that year.

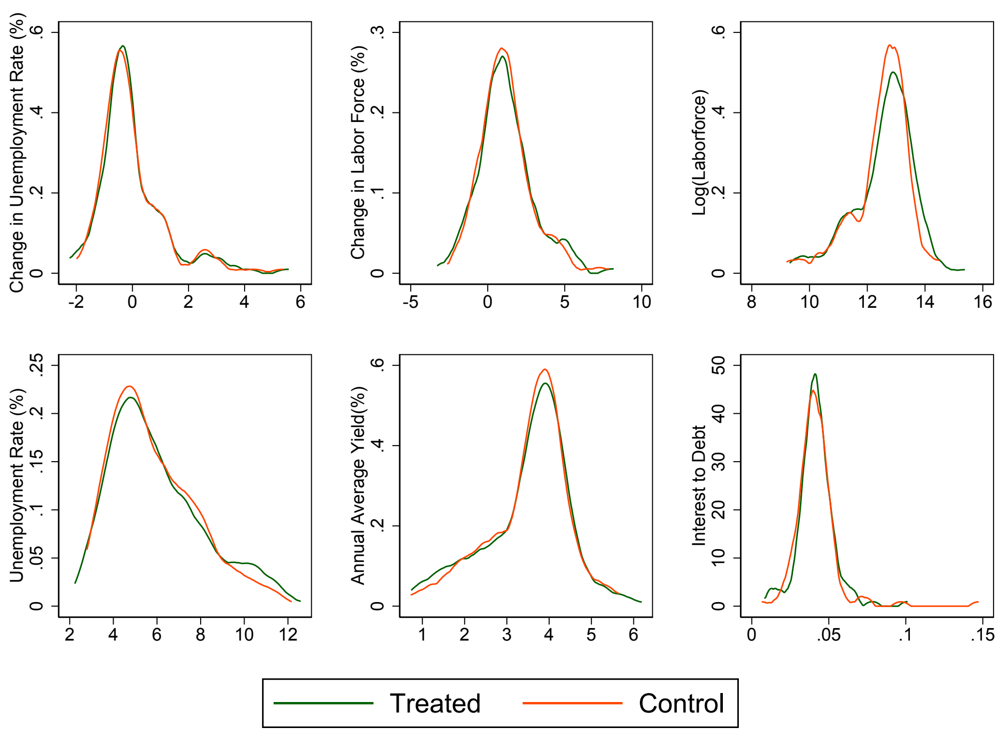


Figure IA4: Matching County Characteristics - Interest to Debt: The figure shows the kernel density plot based on matching the treatment counties using 5 variables in the year before firm bankruptcy: unemployment rate, change in unemployment rate, log(labor force), change in labor force, and average yield of the county in that year.

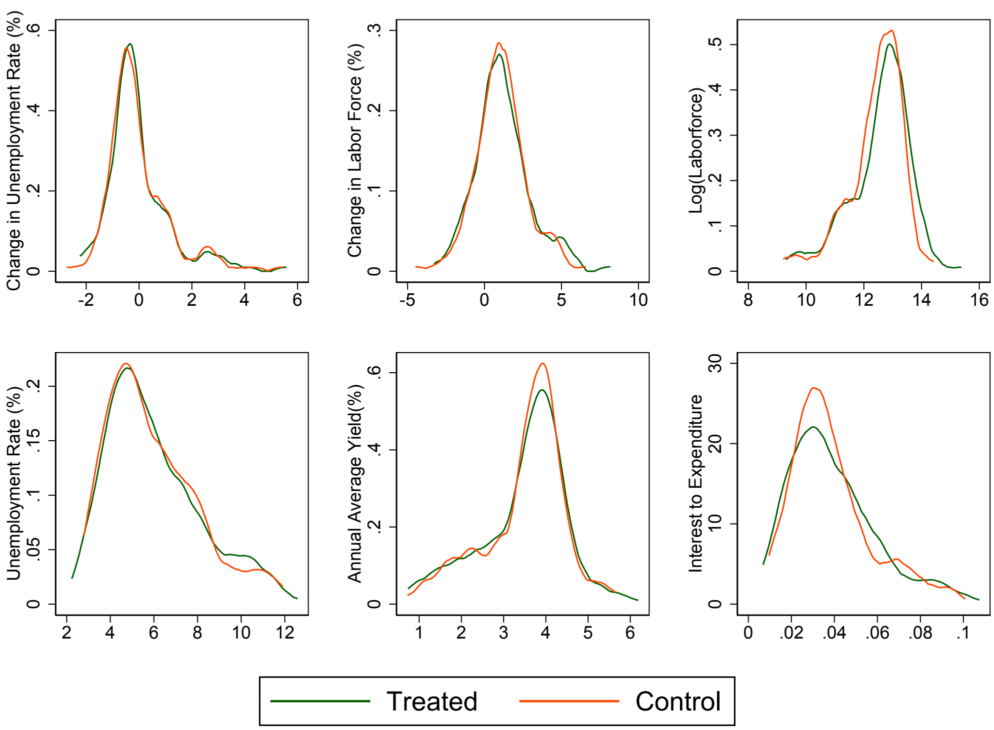


Figure IA5: Matching County Characteristics - Interest to Expenditure: The figure shows the kernel density plot based on matching the treatment counties using 5 variables in the year before firm bankruptcy: unemployment rate, change in unemployment rate, log(labor force), change in labor force, and average yield of the county in that year.

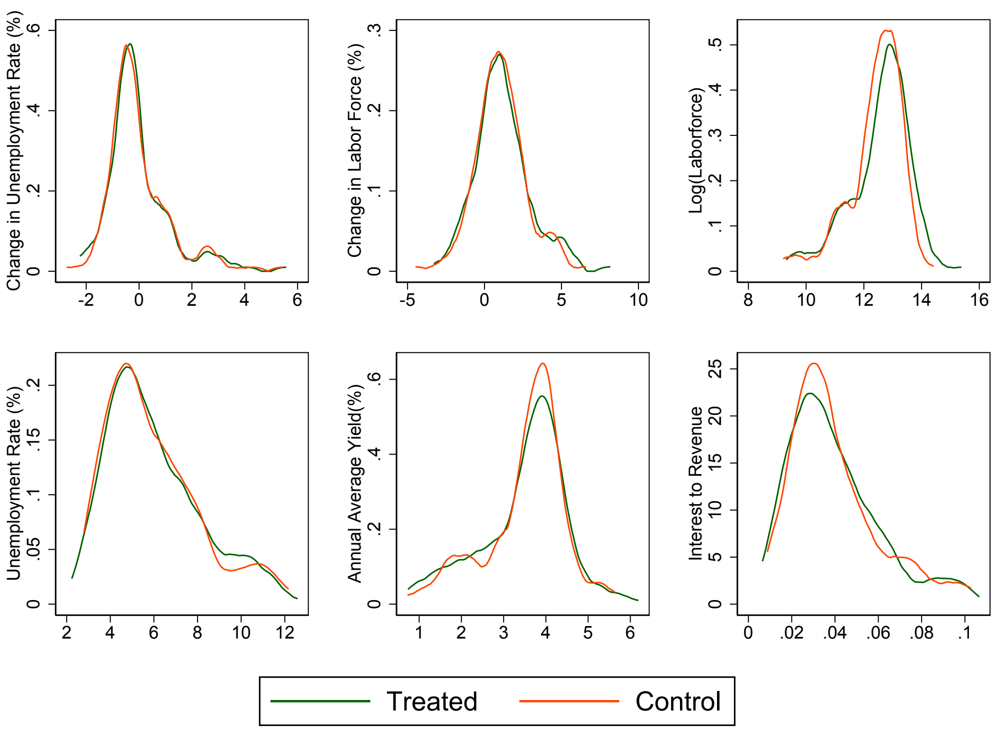


Figure IA6: Matching County Characteristics - Interest to Revenue: The figure shows the kernel density plot based on matching the treatment counties using 5 variables in the year before firm bankruptcy: unemployment rate, change in unemployment rate, log(labor force), change in labor force, and average yield of the county in that year.

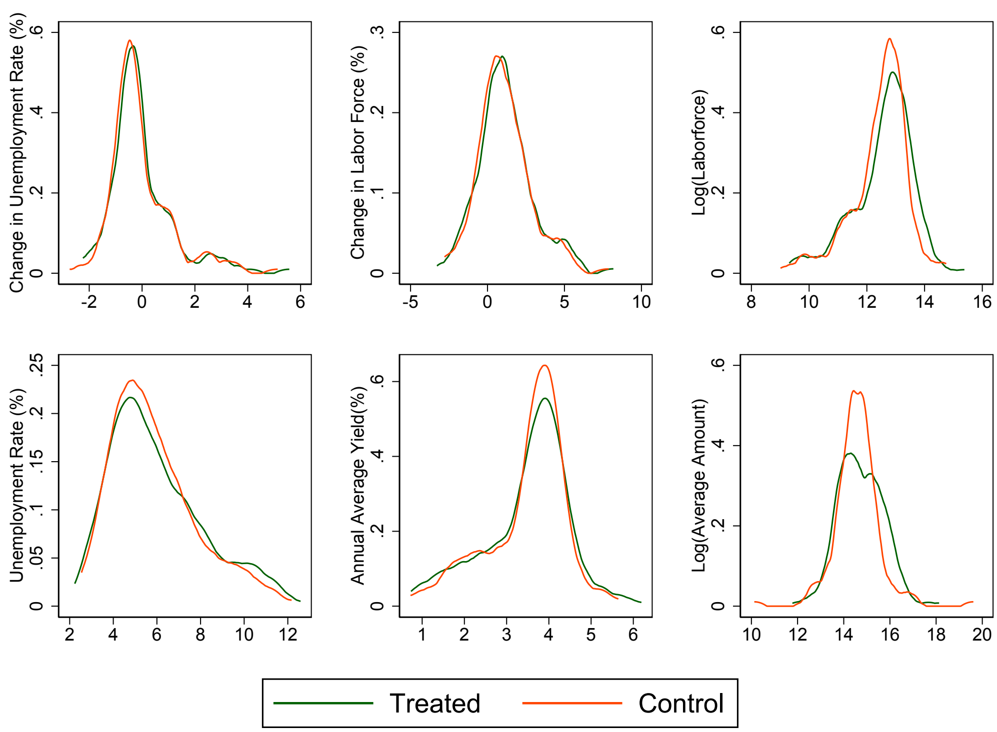


Figure IA7: Matching County Characteristics - Log(Average Amount): The figure shows the kernel density plot based on matching the treatment counties using 5 variables in the year before firm bankruptcy: unemployment rate, change in unemployment rate, log(labor force), change in labor force, and average yield of the county in that year.

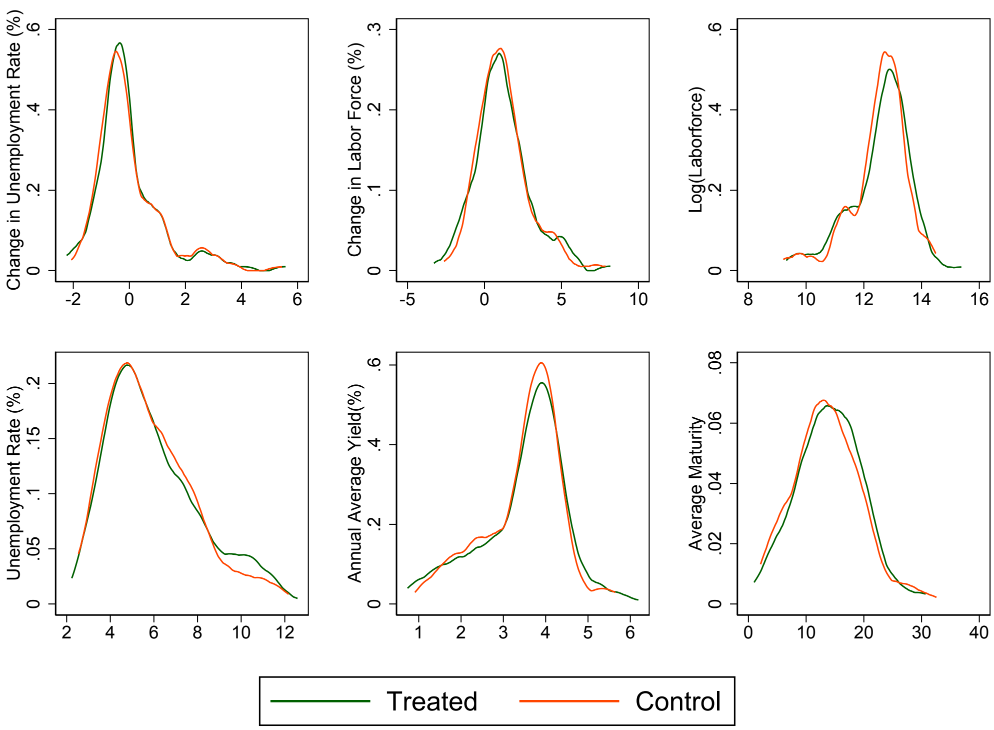


Figure IA8: Matching County Characteristics - Average Maturity: The figure shows the kernel density plot based on matching the treatment counties using 5 variables in the year before firm bankruptcy: unemployment rate, change in unemployment rate, log(labor force), change in labor force, and average yield of the county in that year.

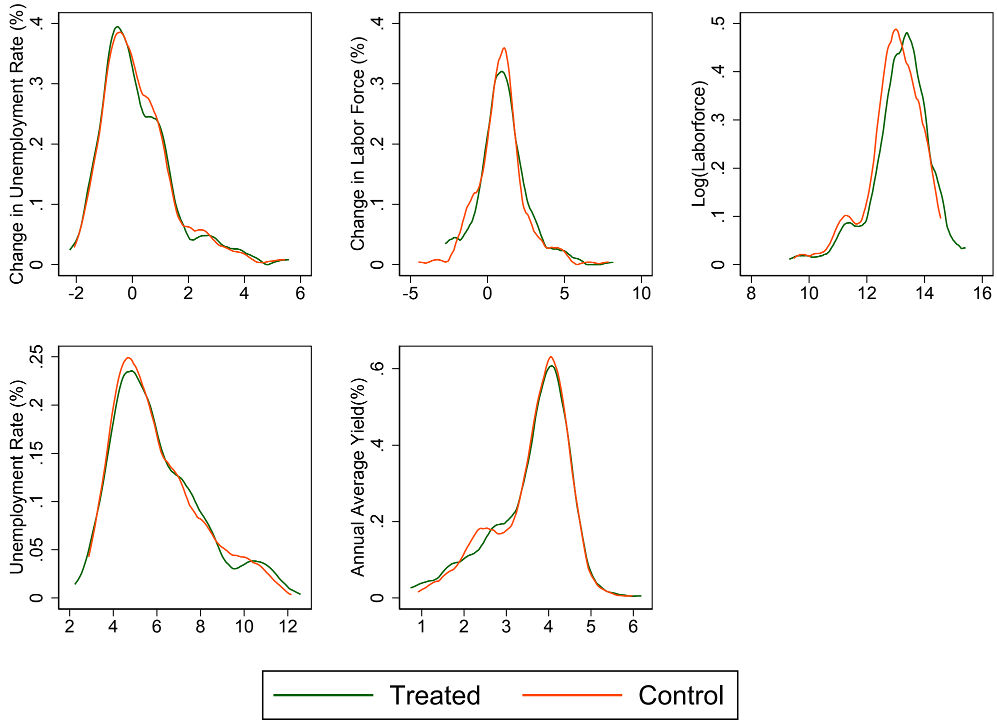


Figure IA9: Matching County Characteristics - Same Region: The figure shows the kernel density plot based on matching the treatment counties using 5 variables in the year before firm bankruptcy: unemployment rate, change in unemployment rate, log(labor force), change in labor force and average yield of the county in that year.

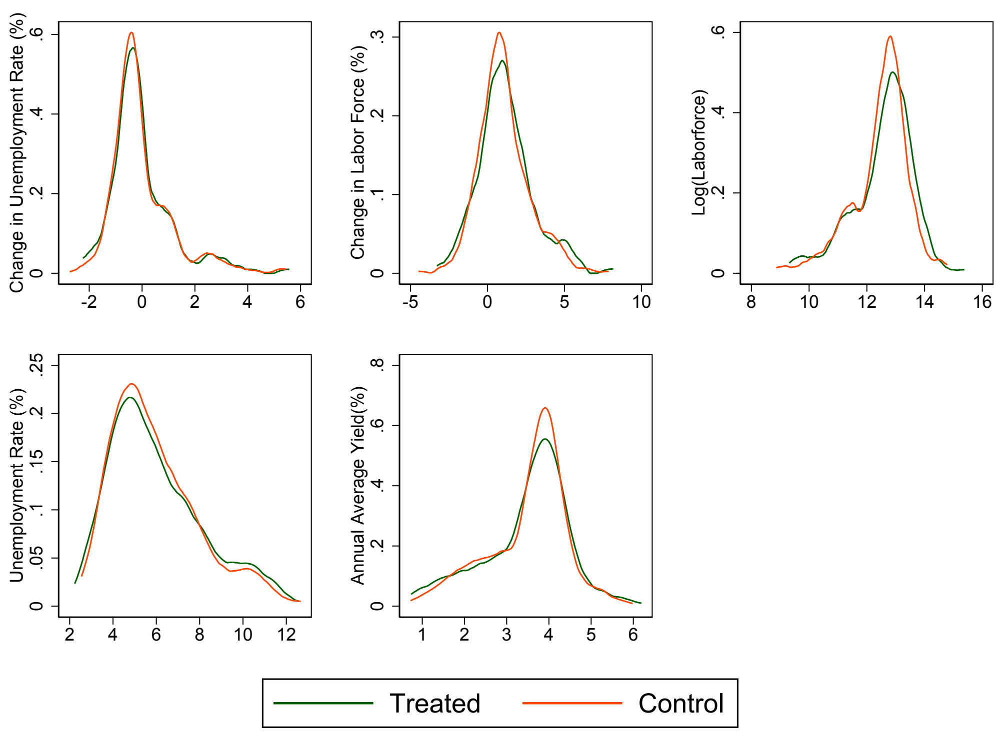


Figure IA10: Matching County Characteristics - KNN(3): The figure shows the kernel density plot based on matching the treatment counties using 5 variables in the year before firm bankruptcy: unemployment rate, change in unemployment rate, log(labor force), change in labor force and average yield of the county in that year.

Based on County Dependence (Wages)

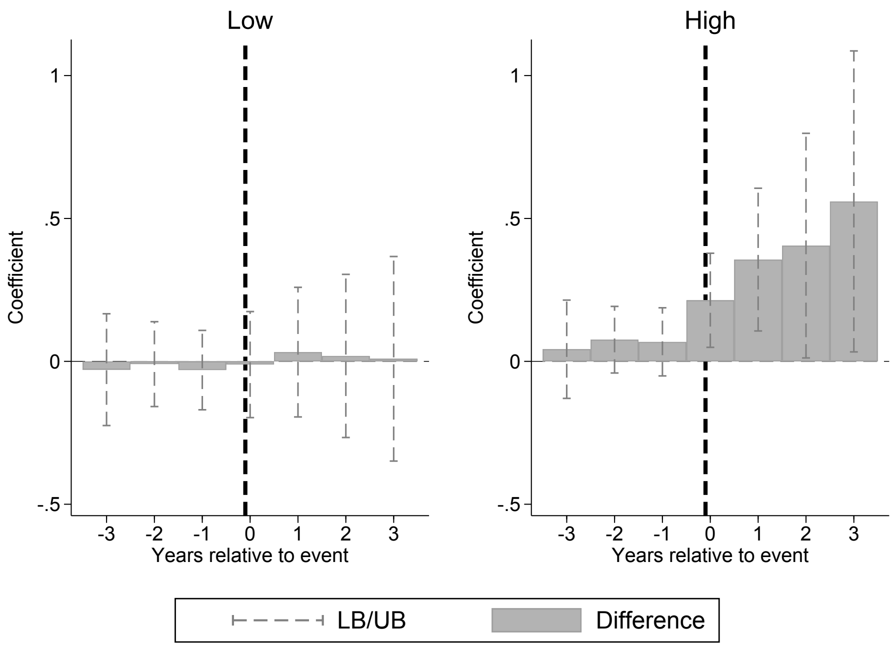


Figure IA11: Unemployment Rate: The figure shows the impact on unemployment rates between the treated and control counties. We report coefficients from Equation (6) using unemployment rate as the dependent variable. We cluster standard errors at the event pair level. We show the impact on sub-samples among the treated counties with low versus high shares of wages in the industry of the bankrupt firm. The control county was matched based on five variables in the year before firm bankruptcy: unemployment rate, change in unemployment rate, log(labor force), change in labor force, and average yield of the county in that year.

Based on County Dependence (Wages)

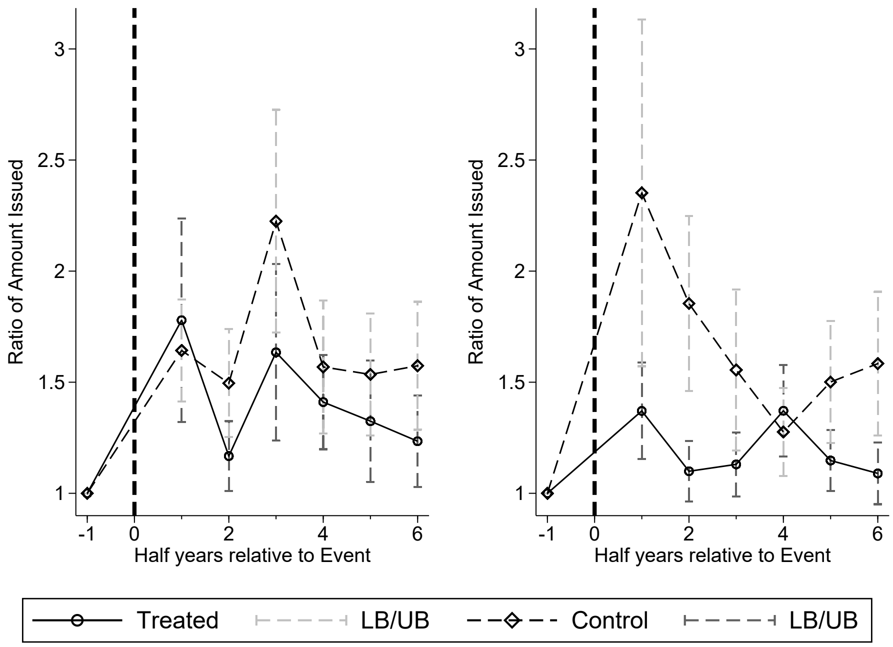


Figure IA12: Primary Market Bond Issuance: The figure shows the impact on unemployment rates between the treated and control counties. We report coefficients from Equation (6) using unemployment rate as the dependent variable. We cluster standard errors at the event pair level. We show the impact on sub-samples among the treated counties with low versus high shares of wages in the industry of the bankrupt firm. The control county was matched based on five variables in the year before firm bankruptcy: unemployment rate, change in unemployment rate, log(labor force), change in labor force, and average yield of the county in that year.

Table IA1: Summary Statistics: Municipal Bonds

This table summarizes the bond level characteristics for our sample of bonds linked to bankruptcy counties during 2005-2019. Panel A reports the secondary market attributes. Panel B reports the primary market features. The key variables are described in Table A1.

Panel A: Secondary market

	Count	Mean	Median	Std. Dev.
Treated				
Wtd. Avg. Yield (%)	1,520,693	3.1	3.3	1.5
Yield Spread (%)	1,520,693	1.4	1.5	2.4
After-tax Yield Spread (%)	1,520,693	3.4	3.0	2.8
Remaining Maturity (years)	1,520,693	11.1	9.8	7.2
Control				
Wtd. Avg. Yield (%)	1,182,649	3.0	3.1	1.5
Yield Spread (%)	1,182,649	1.5	1.6	2.2
After-tax Yield Spread (%)	1,182,649	3.4	3.0	2.7
Remaining Maturity (years)	1,182,649	11.2	9.8	7.2

Panel B: Primary market

	Count	Mean	Median	Std. Dev.
Treated				
Offering Yield (%)	217,814	3.2	3.5	1.3
Offering Price (USD)	217,811	102.3	101.4	11.3
Coupon (%)	217,804	3.8	4.0	1.3
Years to Maturity	217,814	10.2	9.1	6.8
Years to Call	99,464	8.5	9.8	2.8
Amount (USD million)	216,680	3.5	0.8	18.4
Issue Size (USD million)	217,814	53.1	15.0	124.7
Control				
Offering Yield (%)	209,373	3.2	3.4	1.3
Offering Price (USD)	209,370	103.0	101.4	8.9
Coupon (%)	209,372	3.8	4.0	1.2
Years to Maturity	209,373	10.0	9.0	6.6
Years to Call	96,434	8.7	9.6	2.1
Amount (USD million)	208,310	2.8	0.6	20.1
Issue Size (USD million)	209,373	42.2	11.3	121.4

Table IA2: Additional Matching Strategies

This table reports the results after incorporating additional matching strategies. We report results for our baseline specification from Equation 1. In Columns (1)-(3), we introduce additional variables for identifying the nearest neighbor matching county in the control group. These pertain to the county level debt capacity, similar to Chava, Malakar, and Singh (2020). We describe these ratios in Section A1. Next, in Columns (4)-(5), we include ex-ante measures from the primary bond market to identify the nearest neighbor match. Specifically, we use average amount issued and average maturity of new bonds issued, respectively. In Columns (6)-(7), we consider geographical aspects in the matching strategy. First, in Column (6), we restrict the baseline matching to identifying a control county within the same geographic region. In Column (7), we consider three nearest neighbors using the baseline matching strategy. T-statistics are reported in brackets and standard errors are clustered at issue-year month level, unless otherwise specified. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Dependent Variable:	After-tax yield spread						
Additional Dimension:	By Debt Capacity			By Primary Market		By Geography	
	Interest to Debt	Interest to Expenditure	Interest to Revenue	Average Amount Issued	Average Maturity	Same Region	KNN(3)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Post × Treated	9.71***	6.96**	6.64**	5.34*	5.81*	6.00**	4.54*
	[3.33]	[2.35]	[2.28]	[1.77]	[1.93]	[2.20]	[1.82]
Post	-4.24**	-3.39	-2.18	-0.20	-0.41	-3.22*	1.36
	[-2.00]	[-1.60]	[-1.01]	[-0.09]	[-0.20]	[-1.73]	[0.91]
Treated	-2.49	-1.63	-1.65	-5.40	-8.45**	0.35	1.92
	[-0.75]	[-0.46]	[-0.40]	[-1.60]	[-2.40]	[0.07]	[0.84]
Event FE	✓	✓	✓	✓	✓	✓	✓
County FE	✓	✓	✓	✓	✓	✓	✓
Year-month FE	✓	✓	✓	✓	✓	✓	✓
Bond Controls	✓	✓	✓	✓	✓	✓	✓
County Controls	✓	✓	✓	✓	✓	✓	✓
Adj.-R^2	0.654	0.659	0.655	0.656	0.660	0.663	0.642
Obs.	2,634,989	2,598,068	2,615,855	2,746,200	2,589,944	2,720,500	5,041,025

Table IA3: Impact on Primary Market of Rated Municipal Bonds

This table shows the effect of bankruptcy filing on new bond issuances using a difference-in-differences estimate similar to the baseline specification. It is based on primary market bonds in Equation 5 for after-tax offering yields. In Column (1), we show the result by using only event-pair fixed effects and issuer fixed effects in the baseline equation. Next, in Column (2), we introduce bond-level controls. Column (3) shows the results with county controls and county fixed effects. We use S&P credit ratings at the time of issuance. P-values are reported in brackets and standard errors are clustered at issue level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Dependent Variable:	After-tax Offering Yield		
	(1)	(2)	(3)
Post × Treated	10.52***	8.92***	6.34***
	[0.00]	[0.00]	[0.00]
Post	-8.21***	-9.77***	-8.91***
	[0.00]	[0.00]	[0.00]
Treated	-2.33	-2.02	-0.51
	[0.34]	[0.25]	[0.76]
Event FE	✓	✓	✓
Issuer FE	✓	✓	✓
Bond Controls		✓	✓
County Controls			✓
Adj.-R^2	0.466	0.851	0.853
Obs.	276,977	276,977	276,977

Table IA4: Mechanism: State-Imposed Budgetary Restrictions

This table reports the results for the proposed mechanism at work based on county budgetary restrictions. We report results for our baseline specification from Equation (1), which is interacted with dummies corresponding to county budgetary restrictions. The dependent variable is after-tax yield spread. We additionally include group-month fixed effects in the modified baseline equation. We provide more details in Section A.2.1. Column (1) shows the results by dividing the treated counties with firm bankruptcies into those which do or do not have *Debt Issuance* restrictions imposed by their respective states. In Column (2), we divide the treated counties based on whether they have *Overall Property Tax Restriction* or not. Column (3) shows the results using *Expenditure Limit* to distinguish between the types of treated counties. Finally, in Column (4), we show the impact based on a linear combination of these indicators (for Columns (1)-(3)) by summing them. We divide the treated counties into groups based on the total number of restrictions. T-statistics are reported in brackets and standard errors are double clustered at county bond issuer and year month level, unless otherwise specified. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Dependent Variable:	After-tax yield spread			
Post x Treated	Debt Issuance (1)	Overall Property Tax Restriction (2)	Expenditure Limit (3)	Combination of Restrictions (4)
No	5.15	1.93	8.25**	
	[0.83]	[0.53]	[2.22]	
Yes	11.65***	19.30***	12.90**	
	[3.46]	[3.86]	[2.49]	
None				4.14
				[0.67]
One				3.65
				[0.92]
Multiple				19.14***
				[3.85]
Difference	6.50	17.37	4.66	15.00
P-value	0.34	0.01	0.46	0.05
Event FE	✓	✓	✓	✓
County FE	✓	✓	✓	✓
Month FE	✓	✓	✓	✓
Group Month FE	✓	✓	✓	✓
Controls	✓	✓	✓	✓
Adj.-R^2	0.649	0.649	0.650	0.650
Obs.	2,703,342	2,703,342	2,703,342	2,703,342

Table IA5: Mechanism: State-Level Business Incentives

This table reports the results for the proposed mechanism at work based on state-level business incentives. We report results for our baseline specification from Equation 1, which is interacted with dummies corresponding to the level of business incentives from the state. The dependent variable is after-tax yield spread. We additionally include group-month fixed effects in the modified baseline equation. We divide the treated counties into two groups based on the median value of the incentives. We provide more details in Section A.2.2. Column (1) shows the results by dividing the treated counties into two groups using *Net Tax (after incentives)*. In Column (2), we divide the treated counties based on *Total Incentives*. Next, in Column (3), we show the impact based on the level of *Investment Tax Credit*. Finally, Column (4) shows the main result based on *R&D Credit*. T-statistics are reported in brackets and standard errors are double clustered at county bond issuer and year month level, unless otherwise specified. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Dependent Variable:	After-tax yield spread			
	Net Tax (after incentives)	Total Incentives	Investment Tax Credit	R&D Tax Credit
Post × Treated	(1)	(2)	(3)	(4)
Low	3.59	10.35**	15.61***	13.72***
	[0.73]	[2.42]	[3.86]	[2.79]
High	14.04***	9.06**	-0.83	7.24**
	[3.01]	[1.99]	[-0.16]	[2.01]
Difference	10.45	1.29	16.44	6.48
P-value	0.15	0.84	0.01	0.26
Event FE	✓	✓	✓	✓
County FE	✓	✓	✓	✓
Month FE	✓	✓	✓	✓
Group-Month FE	✓	✓	✓	✓
Controls	✓	✓	✓	✓
Adj.-R^2	0.648	0.648	0.649	0.648
Obs.	2,625,214	2,625,214	2,625,214	2,625,214