Communities as Stakeholders:

Impact of Corporate Bankruptcies on Local Governments *

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Abstract

We provide new evidence on externalities imposed on local communities due to bankruptcy filings of publicly-listed manufacturing firms. Compared to matched counties with similar economic trends, municipal bond yields for counties with firm headquarters increase by 10 bps within a year of a firm's bankruptcy filing. Notably, in counties with a lasting increase in yields, the local communities suffer prolonged economic distress. The effect is more pronounced for counties with budgetary restrictions. Meanwhile, counties in pro-business states are less affected. Our results highlight local communities as stakeholders to public firms whereby firm financial distress affects the municipal bond market.

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1 Introduction

Stakeholders of public firms include customers, suppliers, employees, debt holders, shareholders, and local communities. 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, since 1997, the Business Roundtable[1] has emphasized shareholder primacy. Recently, however, this focus on maximizing shareholder value has been criticized (Edmans, 2020). Business Roundtable recently issued a statement that redefined the purpose of corporations as benefiting all their stakeholders.

A large literature in finance has addressed how corporate policies can impact various stakeholders of firms. However, there is limited evidence on the impact of corporate policies on the local governments and communities in which the firms have a significant presence despite them being important stakeholders in businesses (Zingales, 2000; Freeman, 2010). Business taxes (i.e., property taxes and sales taxes) account for more than one-third of local government revenues. Any shocks to these revenue streams can impact local governments' borrowing costs and public investment. In this paper, we analyze the real impact of a public manufacturing firm's bankruptcy on the communities in which the bankrupt firm has a significant presence and document how local communities are a major stakeholder of the affected firms.

It is not clear how the financial distress of a public firm would impact the communities in which the firm has a major presence. Extensive theoretical literature documents that the agency problem and conflicts of interest among claim holders may lead to the continuation of inefficient firms after bankruptcy filing (Gertner and Scharfstein, 1991; Hart, 1995). If the physical and human capital of the bankrupt firm can be redeployed toward more efficient uses quickly, then the local communities may not be affected. In other words, the bankruptcy of an inefficient firm can trigger "creative destruction".

[1] Business Roundtable `https://www.businessroundtable.org/` is an association of chief executive officers of America's leading companies

This may help local areas attract new residents and businesses, thus increasing sales tax and property tax revenue. Moreover, if the affected workers can quickly find alternative jobs after their employers financial distress, or if the local communities can replace the tax revenue from the distressed firms with other sources of revenue, then a public firm's financial distress may not negatively impact the local community. In fact, the positive economic trend and higher tax revenue may reduce the risk of defaulting on existing municipal debt. Consequently, this may reduce the borrowing costs of local public service providers and help increase investment in public services. Otherwise, the finances of the local communities may be affected, making them long-term stakeholders in the firm[2].

We find that a large manufacturing firm filing for bankruptcy leads to a 10% increase in yields of municipal bonds of counties where the distressed firm has headquarters, compared to a matched county with similar economic trends. The counties suffer prolonged economic distress when the increase in municipal yields lasts beyond three years after the firm's bankruptcy filing. The results are driven by counties subject to budgetary restrictions on their debt, property tax or expenditure limits. However, counties located in pro-business states are less affected. This negative impact is further amplified when the county is more dependent on the industry of the bankrupt firm and its upstream suppliers. The higher borrowing cost results in reduction in total expenditures, teaching staff and the number of public schools.

We focus on the impact of a manufacturing firm's bankruptcy on the headquarter (HQ) county and other locations where the firm has a significant presence for two reasons. First, the role manufacturing plays in the US economy has decreased in the past few decades and remains an important public policy issue[3]. Second, large manufacturing

[2]For example, the decline of Detroit with the fall of auto manufacturing highlights the hardships communities face in replacing the auto industry with other employers. On the other hand, Rochester thriving despite Kodak's turbulent phase is a contrasting example of how communities can come out of the contagion effects of large bankruptcies.

[3]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.

firm bankruptcies are unlikely to stem from local economic conditions, as they are less dependent on the local demand for their revenue in the HQ county or at the other plant locations.

We utilize firm bankruptcy data during 2006-2016 from Chava, Stefanescu, and Turnbull (2011) and Alanis, Chava, and Kumar (2018). For bankrupt manufacturing firms, we begin with HQ county as the treatment county. Identifying the causal impact of the announcement of a corporate bankruptcy filing on the borrowing costs of local governments is challenging since we cannot observe what would have happened if the firm did not file for bankruptcy. We identify the nearest neighbor based on the level and changes in unemployment rate and labor force and ex-ante average municipal bond yields as the counterfactual county.[4]

Using 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 treatment and control counties follow similar trends, and the difference between the two groups is statistically insignificant before the announcement of a bankruptcy filing by a publicly-listed manufacturing firm (here after "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 treatment counties, but there is no change in the yields for the bonds of control counties. The tax-adjusted yield spreads for treatment counties increase by 10.01 basis points (bps) compared to the control counties within 36

[4]We provide evidence for the validity of the identifying assumption, i.e., the treatment county and the matched control county follow similar economic trends before the shock. 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 treatment and control counties. We also impose a geographic restriction of identifying the control counties from within the same region in the US, besides considering the matching to three nearest neighbors and find consistent results.

months after the firm bankruptcy announcement. To understand this impact, we compare it against the median credit spread between AA- and AAA-rated municipal bonds in the sample, which equals 93 basis points. This suggests that the average yield increase after the bankruptcy filing represents $\sim$ 10% (=10.01/93) of the credit spread.

One of our empirical strategy's identifying assumptions is that the treatment and control counties follow similar economic trends before the firm bankruptcies. In line with this assumption, we confirm that the treatment and control counties' bond yields follow similar pre-trends. The difference between the two groups is statistically insignificant before the announcement of the manufacturing firm bankruptcy. Using the bankruptcy of large manufacturing firms may help alleviate the concern if declining local demand may be driving both the firm bankruptcy and our municipal bond market results. Our tests on consumer bankruptcies further address the above concern. We find that for treatment and control counties, consumer bankruptcies follow a similar pre-trend. The treatment counties (i.e., those with large firm bankruptcies) observe a significant increase in consumer bankruptcies in the post-period, with no impact on control counties. These results suggest that declining local consumer demand does not drive our results. The parallel pre-trend for both treatment and control counties in the unemployment rate and an upward trend for unemployment rate for treatment counties after the shock further suggests how mass layoffs after firm bankruptcies may deteriorate the local economy.

In addition to HQ counties, we also analyze the impact on other plant locations of the bankrupt firms. We utilize the plant location data both 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. Meanwhile, on sorting across all firm locations, we find that the counties with 25% or more operations (measured using the number of employees at the site) are affected the most. 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. We control for coupon rate, size of issuance, 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 level and changes in the unemployment rate and labor force, the average per capita county income, and house prices to control for local economic conditions. As a restrictive specification, we control for bond fixed effects to further control for bond-specific unobservables. To control for any county and region-specific unobservables, in some specifications, we also include the county and region/region-year fixed effects. 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.

Various states impose tax and expenditure limits that control the taxing and spending ability of local governments. To minimize defaults and over-borrowing, various debt limits restrict the ability of local governments to access the public debt market through the issuance of municipal bonds. Poterba (1994) finds that state budgetary rules including tax and expenditure limitations are correlated with more rapid fiscal adjustments to unexpected deficits. Later, Poterba and Rueben (1999) show that bond market participants consider fiscal institutions in assessing the risk characteristics of tax-exempt bonds. We test and find that local budgetary restrictions imposed by states reduce the ability of local governments to respond to large firm bankruptcies in their counties. We also find that counties located in pro-business states experience a muted impact on their municipal bond yield spreads. Thus, our results highlight how the impact on local governments' borrowing costs after a large firm bankruptcy varies with state-imposed budgetary rules and state-sponsored business incentives.

Early work by Lang and Stulz (1992) documents intra-industry negative stock reactions to a competitor's bankruptcy filing. Similarly, Hertzel, Li, Officer, and Rodgers

(2008) show that suppliers to filing firms experience negative stock price reactions around filing dates. In a similar spirit, we create two measures of a county's dependence on an industry that observes large firm bankruptcy: a) direct dependence, and b) indirect dependence via inter-industry linkages. We find that the negative impact is further amplified when the county is more dependent on the industry of the bankrupt firm and its upstream suppliers.

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 2 times. We find that the treatment counties' primary market offering yield spreads increase by 6.14 bps compared to the control group. Overall, for the local economy we find an increase in the unemployment rate, a decline in county GDP growth, lower tax revenue, lower property tax revenue, and reduced public expenditure, especially on public schools.

Our paper relates to the literature discussing shareholder vs. stakeholder theories (Friedman, 1970; Zingales, 2000; Freeman, 2010). Moreover, (Baker, Gibbons, and Murphy, 2002; 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, especially in the context of public investment. Our paper also relates to the recent academic literature that has documented 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 public investment is limited. In this paper, we identify a new channel by which bankrupt firms impose negative externalities on local communities; namely, 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 local communities are adversely impacted by financial distress of large firms. In this regard, we also contribute to the recent literature on municipal bonds (Adelino, Cunha, and Ferreira, 2017; Schwert, 2017; Gao, Lee, and Murphy, 2019a,b).

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, and we conclude in Section 5.

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 cost and then describe our empirical specification.

2.1 Identification Challenges

The first econometric challenge is to identify if the underlying local economic conditions drive firms to file for bankruptcy. In that case, certain omitted variables (e.g., reduced local demand) could affect the firm and the corresponding county's municipal bond yields. To overcome this threat, we specifically focus on bankruptcies in the manufacturing sector, which are arguably relatively exogenous to demand in the local area. 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 economics and, thus we 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. We find that the patterns look very similar, with no appreciable difference between the two groups. This affords us considerable comfort about 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). As before, 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. This suggests that local economic conditions were not deteriorating before the bankruptcy filing (see Section 4.1.2).

Finally, there may be a concern that our primary dependent variable – the bond yield spreads – is driven by unobserved factors at the county level other than the bankruptcy filing. 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 cost. 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. Our sample period is restricted by the availability of ex-ante municipal bond yields in the secondary market at the beginning year and the availability of 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:

$$\begin{aligned} y_{i,c,e,t} = \alpha + \beta_0 * Treated_{i,c,e} * Post_{i,c,e,t} + \beta_1 * Treated_{i,c,e} + \beta_2 * Post_{i,c,e,t} \\ + BondControls + CountyControls + \eta_e + \delta_c + \gamma_t + \epsilon_{i,c,e,t} \end{aligned} \quad (1)$$

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 (2019a) to include amount issued, coupon rate, dummy for status of bond insurance, dummy for the type of bid (competitive versus negotiated), and 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 for local economic conditions. It includes log of the lagged value of 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 (2019a) 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. 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 Muncipal Securities Rulemaking Board (MSRB).

3.1 Corporate Bankruptcies

Our data on corporate bankruptcies comes 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 headquarter location from Compustat. To avoid potential endogeneity problems in our research design, we drop headquarter counties with multiple bankruptcies filed in the same calendar year. Further, our focus on the manufacturing sector (using NAICS codes 31-33) during 2006-2016 results 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 used 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, our nearest neighbor matching strategy uses five county-level variables: unemployment rate, change in unemployment rate, log(labor force), change in 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 1. 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 terms of our matching.

3.2 Municipal Bonds

Municipal bond characteristics are obtained 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. 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 on 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[5]. Out of 63,754 unique issuer identities (6-digit CUSIPs), Bloomberg provides us with county-state names on 59,901 issuers. For these issuers, we match the Federal Information Processing Standards (FIPS) code. The FIPS is then used as the matching key between bonds and bidding counties involved in offering corporate subsidies. 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 the 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

[5]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.

(2019a) in aggregating the volume-weighted trades to a monthly level. Following Downing and Zhang (2004); Gao, Lee, and Murphy (2019b), we only use customer buy trades to eliminate the possibility of bid-ask bounce effects. Given our primary focus on the borrowing cost 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 (from FTSE Russell), we rely on the CUSIP as the key identifier. In Figure 2, we provide kernel densities for secondary market bond features like after-tax yield spreads and remaining maturity between the treatment and control groups. Figure 3 provides a representation of 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 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. We calculate the bond's coupon-equivalent risk-free yield as in Gao, Lee, and Murphy (2019a)[6]. 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 on both issuers and investors in the form of higher yields. In particular, we use:

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

[6]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). Using this price of the coupon-equivalent risk-free bond, the coupon payments, and the face-value payment, we get the risk-free yield to maturity. 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).

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 maturity–matched 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 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 measures of gross domestic product (GDP) 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 employment and wages data comes 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. For the county-level population, we use data from the Surveillance, Epidemiology, and End Results (SEER) Program under the National Cancer Institute. 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 present robustness tests in Section 4.2. We propose the potential mechanism to explain our results in Section 4.3. In Section 4.4, we highlight the county's exposure to the industry of firm bankruptcy. Finally, in Section 4.5, we analyze the heterogeneity in our main result before discussing the impact on the primary market for municipal bonds (Section 4.6) and the local economy (Section 4.7).

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. We present results on headquarters locations versus other firm locations in Section 4.1.3.

4.1.1 Dynamics and Baseline Results

We begin our analysis by plotting the after-tax yields and after-tax yield spreads observed in the secondary market for the treated group. Our event window comprises three years before and after the filing of firm bankruptcy. We use the first 12 months in the window during T=-25 to T=-36 months as the benchmark period to evaluate the pre-trends before the bankruptcy announcements. By depicting the observations aggregated to a quarterly scale, we overcome 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, in Figure 4a and Figure 4b, we show the impact on the treatment group alone. We find a visible upward trend in after-tax yields and after-tax yield spreads in the quarters after the bankruptcy filing. This impact is in excess of 10bps and increases over time until the eleventh quarter after the shock. Next, to better understand the impact, we evaluate the difference between the treated and control groups. In Figure 5a, 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. We replicate a similar figure using our primary dependent variable, i.e., after-tax yield spread, shown in Figure 5b. 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, the treatment group's yields become higher than those of the control group on average just one quarter before the announcement. However, this effect is statistically indistinguishable from zero. The effect becomes statistically significant from the third quarter onward. Finally, we find that the difference between the two groups persists until the 12th quarter, when it shrinks marginally. We test these observations in our baseline regressions.

Note that the above results only 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, Panel A 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 provide the description of key variables in Table A1. In Column (3), we 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[7]. Thereafter, we follow the same scheme and show our results using after-tax yield spread as the dependent variable.

Using Column (6) with after-tax yield spread as our baseline case 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

[7]We report these coefficients for bond level and county level controls in Table IA2. For robustness, we further include the lagged values of log(personal income) and log(house price index). The magnitude of our main effect reported in Section 4.2 remains similar.

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.

To evaluate the sensitivity of our baseline results against the choice of window used, we show our main result in Panel B using different duration of the event windows. 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 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 both in the preceding and succeeding periods around the announcement to arrive at sharper estimates of the effect, especially given the limited trading in the municipal bond market. Next, in Panel C we show the baseline result of Column (6) 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 continue to be statistically significant and economically meaningful, ranging between 4 and 8 bps. 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. To further alleviate concerns about underlying economic differences between the treated and control counties, we provide evidence from consumer bankruptcies. 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 6 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 that there is 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. The difference becomes statistically significant beyond the fourth 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 drive 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 (2021) document that value creation has become heavily concentrated in a few headquarter cities over the last 20 years as per stock market indicators. One may argue that while the location of the headquarters of a firm may represent a hub of economic activity of the firm, 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 on the intensive margin. We report our results in Table 3. We obtain data on other locations using two sources: Environmental Protection Agency and Mergent Intellect. Our data on from Mergent Intellect is hand-collected and allows us to observe facility level DUNS number and employment information for each firm. 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 is coming from the headquarter location (12.74 bps). We find similar results by 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 that there is 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 also include larger firms with a greater 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 bankruptcy (See Section 4.3). To further consolidate our finding 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. We report our results in Column (7) where we find the greatest impact (of 14.77 bps) due to facilities which are ranked highest (for counties with greatest employment share in the firm). The effect on counties with lower ranked facilities is insignificant.

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

4.2 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 4.

4.2.1 Other dependent variables

We test our main result from Equation 1 by changing the dependent variable. In Column (1), we use 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 (2), we find that the baseline effect is 6.51 bps. Both of 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 dependent variable used in the analysis.

4.2.2 Is the effect driven by size of trades?

One potential concern with the interpretation of 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 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[8]. Accounting for such differences, we dissect our results

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

into sub-samples of trades constituting various buckets. Columns (3)-(6) depict the main effect, as 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 (4), which has substantially fewer trades due to the sample restriction. The results continue to be comparable to the baseline effect in terms of statistical significance and economic meaning. This seems to suggest that our main result is not driven by any specific group of clientele.

4.2.3 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 start from 2005, we further exercise caution in this regard by dropping bonds issued within a few months of the bankruptcy filing, shown in Columns (7)-(8). For example, in Column (7), 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 of these sub-samples, the coefficient of interest is higher in magnitude than in the baseline result. Based on this evidence, we argue that our results are not driven by trades from recently issued bonds.

4.2.4 Additional controls

In Column (11), 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. We find that the bond yield spreads increase by 9.60 bps in this case. Our next check on 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 this restricts the sample by requiring bond ratings, we do not impose the rating requirement in the baseline. We show our results by controlling for bond ratings in Column (12) of Table 4. We find that the bond yield spreads increase by 8.28 bps. This test mitigates any concern about unrated bonds solely driving our results.

4.2.5 Controlling for Duration

To verify the robustness of our results to the duration effects of bonds, we modify the baseline specification in Columns (13)-(14). First, in Column (13), we show our main effect by replacing years to maturity and inverse years to maturity at the bond level by the corresponding duration for the bond-month observation. We report a higher impact of 10.60 bps. Next, in Column (14), we show the same result by calculating duration based on after-tax yields. This tax adjustment also increases the impact to 10.61 bps. We find that 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.

4.2.6 Other unobservables

Further, we consider whether our results are robust to a host of unobserved factors at the bond, county, and geographic region level. First, we additionally absorb all bond level, time-invariant variation to control for unobserved bond features in Column (15). This yields a baseline effect of 8.23 bps. To account for unobserved regional variation in the US, we show our results in Columns (16)-(18). First, in Column (16), we show the result after introducing region fixed effects to the baseline specification. Our magnitude changes to 10.01 bps. Further, we impose region $\times$ year fixed effects to account for time-varying unobserved factors at the regional level. The main result in Column (17) is 10.42 bps. Finally, in Column (18), we show our results by controlling for a more granular time-varying component by introducing region $\times$ 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 $\times$ GO bond indicator fixed effects in Column (19). We find that the bond yield spreads increase by 8.74 bps. Likewise, in Column (20), we control for year-month $\times$ insured bond status fixed effects to control for unobserved market-wide factors for insured bonds. By using bond purpose $\times$ year-month fixed effects, we report the baseline effects as 7.88 bps in Column (21). 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. Overall, our analysis suggests that our main effect is robust to unobserved factors at the bond, county, region and municipal bond market levels.

4.2.7 Other Sub-samples

It is possible that our matching control counties may also face the repercussions of a recent firm bankruptcy. If so, this might under-estimate our main result. We address this concern in Column (22) by restricting the set of control counties to include only those which do not also see a firm bankruptcy from our sample within 24 months 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 likely could 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. Thus, the differential size effect dominates the expected advantage of removing recently-affected counties. Further, we test our specification using a different set of bonds. First, 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 (23). Our classification of industries is based on Bernstein, Colonnelli, Giroud, and Iverson (2019). We exclude all service industries as per the Census [9] 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 (23), we report our baseline effect as 8.27 bps by using firm bankruptcies in tradeables. We also show the dynamic impact on the bond yield coefficients following our baseline strategy for this sample of tradeable bankruptcies in Figure IA8. Next, we address the concern that our results may be capturing the differences in taxation of income from municipal bonds across states in Column (24). 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. Overall, we provide evidence to show that our results are not sensitive to redefining the control group and the choice of focusing on manufacturing firm bankruptcies. Our results are also robust to dropping taxable bonds from the sample.

4.2.8 Financial Crisis of 2008-09

Another potential worry stems from the fact that the sample period spans the financial crisis of 2008-2009. Understandably, 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 (25) shows the baseline results from Equation (1) by excluding all trades up to 2010. We find that the increase in bond yield spreads among the treated counties is 11.04 bps higher than 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.

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

4.2.9 Alternative specifications

To evaluate potential concerns about alternative specifications that might be used in this setting, we also consider some modifications to the baseline. We report our results in Columns (26)-(28). First, 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 (26), 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 (27). 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 to replacing year-month fixed effects with year fixed effects in Column (28). The increase in bond yields is reported as 10.35 bps. Overall, we show robustness to alternative specifications to our baseline Equation 1.

4.2.10 Assumptions on correlation of standard errors

Finally, 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, 2019a,b). In Columns (29)-(31), 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 (29)). It could also be that the error term in our main specification is correlated over specific bond issuers. We report our results for this in Column (30). Finally, we also consider clustering standard errors at the county bond issue level in Column (31). In all these specifications, we find results similar to our baseline specification. Thus, our results are robust to these alternative choices of clustering standard errors.

In summary, 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.

4.3 Mechanism

In order to explain our results, we propose a mechanism based on the underlying state-specific budgetary regulations and incentive policies.

4.3.1 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 5 based on a modified version of the baseline Equation (1). 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.

4.3.2 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[10], 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-

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

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 6 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 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.

To demonstrate further support for our mechanism, we compare the county level unemployment rate between our treated and control counties. We use the annualized version of Equation (4) at the county level since the unemployment rate is reported annually. We replace month fixed effects by year fixed effects and cluster standard errors at the event pair level. We show our results from the regression coefficients in Figure 7a. 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 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 in later years. We substantiate this evidence further in light of the county level budgetary restrictions discussed above. Specifically, in Figure 7b, we show that treated counties with higher budgetary restrictions seem to drive the overall result in unemployment rate. The differential effect is relatively muted and statistically insignificant for counties with low budgetary restrictions. We provide similar evidence using county exposure based on employment share and wages share in Figure IA9. Once again, we find that treated counties with a higher dependence on the industry of the firm filing for bankruptcy are more severely affected. We provide more details on how we measure the county-level exposure in the next section.

4.4 County Exposure

We use the 3-digit NAICS code to identify the industry of a bankrupt firm. For counties that are heavily reliant on the bankrupt firm's industry, we expect the baseline effect to be higher. Additionally, we also evaluate the upstream linkages in the county using Bureau of Labor Statistics (BLS) input-output tables. We expect that the municipal bond yield spreads would increase more for counties which show greater linkages to the industry of the firm filing for bankruptcy. Our data on county wages and employment in this section is from the Quarterly Census of Employment and Wages (QCEW) by the US Bureau of Labor Statistics.

First, as a direct measure of the county's dependence on the industry, we calculate the county's share of wages and employment from the NAICS 3-digt industry of the bankrupt firm. 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 Columns (1) and (2) of Table 7. Using Column (1), we show that a higher dependence based on wages results in an increase in yield spreads of 15.01 bps. The difference between the high and low groups is statistically significant. Similarly, in Column (2), we show that a greater dependence via fraction of employment results in an impact of 13.73 bps, but the effect is insignificant for the low group.

We also use real valued input-output use tables from the Bureau of Economic Analysis (BEA) to quantify the county linkages in the industry of bankruptcy filing. Using the sector-level NAICS mapping provided by BEA, we first aggregate the county level wages and employment at the county-sector-year level. For our firm bankruptcies in manufacturing, we represent the county's exposure in the industry using the upstream sector (s) based on the fraction of total wages and employment ($\eta_{s,t}^{county}$). The input-out tables also provide us with a share of value added by upstream industries in manufacturing ($w_{s,t}^{mfg}$). This upstream sector-level value added changes over the years and is used as a weight. To arrive at the county's sector level exposure ($e_{s,t}^{county}$) by summing up the upstream sector linkages, we follow:

$$e_{s,t}^{county} = \sum_{s} w_{s,t}^{mfg} * \eta_{s,t}^{county} \tag{5}$$

In Columns (3) and (4) of Table 7, we report our results using the county's exposure based on upstream linkages. Following a similar approach as before, we divide the treated counties into two groups of low and high exposure based on wages. We find that the bond yield spreads increase by about 18.75 bps when the bankrupt firm is in a county with high wages exposure. On the other hand, the impact is insignificant in the low group.

Similarly, we report the impact using employment exposure in Column (4). The yield spreads go up by about 15.85 bps in this case. The differential impact between the high and low groups is statistically significant and economically meaningful in both cases.

To summarize, our results in this subsection provide evidence in favor of the county's dependence on the industry of 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 (13-19 bps) in bond yield spreads. These counties also seem to drive the result showing an increase in unemployment rate, in aggregate.

4.5 Heterogeneity

In this sub-section, we test the heterogeneity of our results. We study how various factors including the relative importance of the firm filing for bankruptcy, the type of bankruptcy, and bond-specific features may affect our results. We report our results in Table 8. We suitably modify our baseline Equation (1) to include group-year month fixed effects.

4.5.1 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). 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 which have a relatively large size in comparison to the county in which they are located, 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 results in an increase in bond yields of 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 property tax revenue of the county. 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.

4.5.2 Purpose of Bonds

Next, we evaluate the differential impact among different types of municipal bonds. Specifically, we classify them into groups based on the use of proceeds associated with the municipal bonds. We report our results in Column (4). We find that the highest impact (32.55 bps) occurs with bonds associated with improvement and development purposes. 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 in the locality. For example, industrial revenue bonds may specifically be for building or acquiring factories or heavy equipment and tools. In Section 4.7, we also discuss the impact on average house price index. We do not find a large impact on bonds that are linked to services and fee-based revenue models.

Overall, our results in this section provide evidence on 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.

4.6 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 impact of the event on the primary market using new bonds 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:

$$\begin{aligned} y_{i,c,e,t} = {} & \beta_0 * Treated_{i,c,e} * Post_{i,c,t} + \beta_1 * Treated_{i,c,e} + \beta_2 * Post_{i,c,t} \\ & + BondControls + CountyControls + \eta_e + \delta_c + \gamma_j + \kappa_t + \epsilon_{i,c,e,t} \end{aligned} \tag{6}$$

where index i refers to bond, c refers to the county, e denotes the event pair, and t indicates the time (in months). We use yields at the time of bond issuance obtained from the primary market as the dependent variable in $y_{i,c,e,t}$. 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 9 shows the results for the specification above. In Column (1), we estimate the difference-in-differences coefficient from within the same event-pair, absorbing for issuer fixed effects. We report an increase of 8.25 bps in the offering yields. However, this does not account for bond level and county level heterogeneity in the sample. In Column (2), we introduce bond level controls to show an increase in offering yields of new issuances by 6.29 bps. Next, we introduce county-level time-varying characteristics along with county fixed effects in Column (3) to get a 3.24 bps increase in offering yields for the treated counties as compared to the control group. Subsequently, we show our results for the specification in Equation (6) using after-tax yields as the dependent variable. Since municipal bonds are tax-exempt at the state level, we argue that it is relevant to adjust for variation in tax effects across states in our sample. Column (4) shows that the tax-adjusted offering yields increase by 5.78 bps after a firm bankruptcy filing.

Finally, in Column (5), we report the impact on the tax-adjusted offering yield spread and find that the issuance cost increases by 6.14 bps for the treated counties. The spread is measured over maturity-matched zero coupon yields from treasury bonds. Overall, our results suggest that the primary market for bond issuance is associated with an increase in yields by 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 IA4. In Figure 8, 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.

4.7 Impact on County's Crime and 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.

We consider the county-level crime represented by the number of robberies reported in a month using Kaplan (2021), on a per-capita basis. We present our results using Equation (1) using monthly data in Table 10. To enhance readability of the magnitude, we report our results by re-scaling the dependent variable for every million units of population.

Our results show that there is an overall increase of about 6 robberies per million population in the three years after the bankruptcy event (Column (1)). In Columns (2)-(5), we provide results based on the proposed mechanism using county-level budgetary restrictions. We interact the equation with corresponding dummies and additionally control for average group-month fixed effects. Columns (2)-(4) suggest that counties with budgetary restrictions in the form of debt limit, overall property tax limits or expenditure limits experience an increase in monthly robberies of about 6-15 instances per million of population. Finally, in Column (5), we show that the main result in Column (1) is driven by treated counties with multiple such restrictions. Overall, we find an increase in crime as measured by the number of robberies. We argue that these results are consistent with the evidence on the number of consumer bankruptcies, discussed in Section 4.1.2.

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. Based on the proposed mechanism, we expect the county dependence to result in a detrimental effect on local economic activity. Specifically, we examine the impact on the county's GDP growth (%), house price index, total revenue, property tax revenue, total expenditure and total education expenditure. Our analysis also provides evidence on the number of teachers employed, students enrolled and the number of public schools enlisted in the treated counties. We present our results in Table 11.

To study the county level metrics reported on an annual basis, we modify our baseline

Equation (1) as below:

$$y_{c,e,t} = \beta_0 * Treated_{c,e} * Post_{c,t} + \beta_1 * Treated_{c,e} + \beta_2 * Post_{c,t}$$
$$+ CountyControls + \eta_e + \gamma_c + \epsilon_{c,e,t} \quad (7)$$

where index c refers to 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.

We start by analyzing the overall GDP growth (%) in the county in Column (1). Our results show that there is a 1% decline in GDP growth among headquarter counties in the three years after the listed manufacturing firm files for bankruptcy. However, the associated reduction in house price index is not statistically significant in Column (2). Next, in terms of total revenue and property tax revenue, we show a 2% decline in the treated counties (Columns (3)-(4), respectively), but the result is only weakly significant. Since local governments are heavily reliant on property tax revenue for their income, this result is not surprising given the decline in the house price index. Additionally, a potential decline in revenues from a large tax-paying firm in the county, which is now under financial distress, would likely affect the financing capability of the local governments. Further, in Column (5), we report a similar 2% decline in total expenditure by these counties at the local level.

Finally, we are interested in learning more about the component of their spending activity that local governments choose to adjust. We present our results from the analysis on local education in Columns (6)-(13) of Table 11. First, in Column (6), we report a slight decline in aggregate spending in education by treated counties in the three years after the firm bankruptcy, but the effect is statistically insignificant. However, this evidence is not statistically significant. So, we investigate further based on the proposed mechanism and show our results in Column (7), where we divide the treated counties into two groups based on the state-imposed county-level budgetary restrictions. We use a linear combination of the dummy indicators for restrictions on debt issuance, overall property tax, and expenditure limits by summing them up for each treated county. A *Low* value corresponds to zero or one restriction, while a *High* value indicates multiple restrictions. We do not find any significant difference between the two groups with respect to education expenditures. We follow a similar strategy in the next six columns, where we use elementary school data from the Elementary/Secondary Information System (ELSI) provided by the National Centre for Education Statistics (NCES). In Columns (9) and (13), we report a 2% decline in the number of teachers and public schools, respectively, after a firm bankruptcy among treated counties with high budgetary restrictions. Thus, we find some evidence supporting our proposed mechanism using these additional outcome variables across county-level education.

4.8 Recovery of Treated Counties

Following the evidence about the local economy, we now turn to the long-term implications of a large firm bankruptcy. Our primary identification relies on the impact on municipal bond secondary market yield spreads. As a result, we group the treated counties based on their recovery in after-tax bond yield spreads. We compare the ex-post yields to the three-year average before the event. We classify counties as "recovered" if the average yield spreads in the three years after the event are less than or equal to the pre-period. For counties in which the yields recover during the fourth year after the event, we classify them as "less affected". We label the remaining treated counties as "not recovered".

Using the definition above, we evaluate the long-term economic outcomes of the treated counties. We plot the coefficients after modifying the annualized version of Equation (4) by omitting dummies corresponding to control counties. We cluster standard errors at the event level. In Figure 9a, we show the coefficient estimates using county unemployment rate as the dependent variable. We find that there is a long-term reduction in unemployment rate for treated counties in which bond yields were "less affected"/"recovered". However, this is not the case for counties that did not recover after the firm bankruptcy. We report similar long-term adverse consequences for counties "not recovered" using the county-level growth rate of establishments in Figure 9b.

Overall, our results in this section highlight the potential response of local economies and local governments following a bankruptcy filing by a listed manufacturing firm headquartered in their counties. There is a decline in the county's overall GDP growth and total revenue, resulting in a similar dip in total expenditure by the county. We provide evidence consistent with the proposed mechanism to show that the effect is stronger among treated counties with high budgetary restrictions. The upshot seems to be a reduction in spending on education and a subsequent drop in teachers' employment and the number of public schools in the county.

5 Conclusion

Using bankruptcies of publicly listed manufacturing firms during 2006-2016, we assess the impact on local government finances at headquarter counties and other locations. We focus on listed manufacturing firms since they are less likely to be dependent on local demand and they allow us to identify the impact of their bankruptcy on the HQ county and other locations. We find that in the three years after the bankruptcy filing by a local manufacturing firm, the cost of municipal debt increases by 10 bps in the secondary market. We propose a mechanism based on the state imposed budgetary restrictions on treated counties. For counties with greater budgetary restrictions, the increase in yields is higher (as much as 19 bps). We reinforce our mechanism with evidence from the labor channel using county-level unemployment rate as the outcome variable. This negative impact is further amplified when the county is more dependent on the industry of the bankrupt firm and its upstream suppliers.

We also explore the heterogeneity among firm bankruptcies based on the relative importance of the firm and the purpose of the bond. Using ratios of size of the firm to that of the county, we show that when the firm is relatively more important to the county, the impact on the bond yield spreads is higher. Finally, we document the impact on the primary market of new bond issuances (offering yields go up by 6 bps) and the local economy. Counties experience a 1% decline in GDP growth in the three years after the firm bankruptcy compared to their control group. There is a decline in total expenditure along with a reduction in education expenditures for counties with greater budgetary restrictions. As a result, the number of teachers and public schools also decreases. Overall, our results highlight hitherto undocumented implications borne by local communities as potential stakeholders to firm 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

Variable	Description	Source
Interest burden	Ratio of interest on general debt to total revenue for the county.	CLGF
Interest to debt	Ratio of interest on general debt to total long term debt outstanding for the county.	CLGF
Interest to expenses	Ratio of interest on general debt to total expenditure for the county.	CLGF

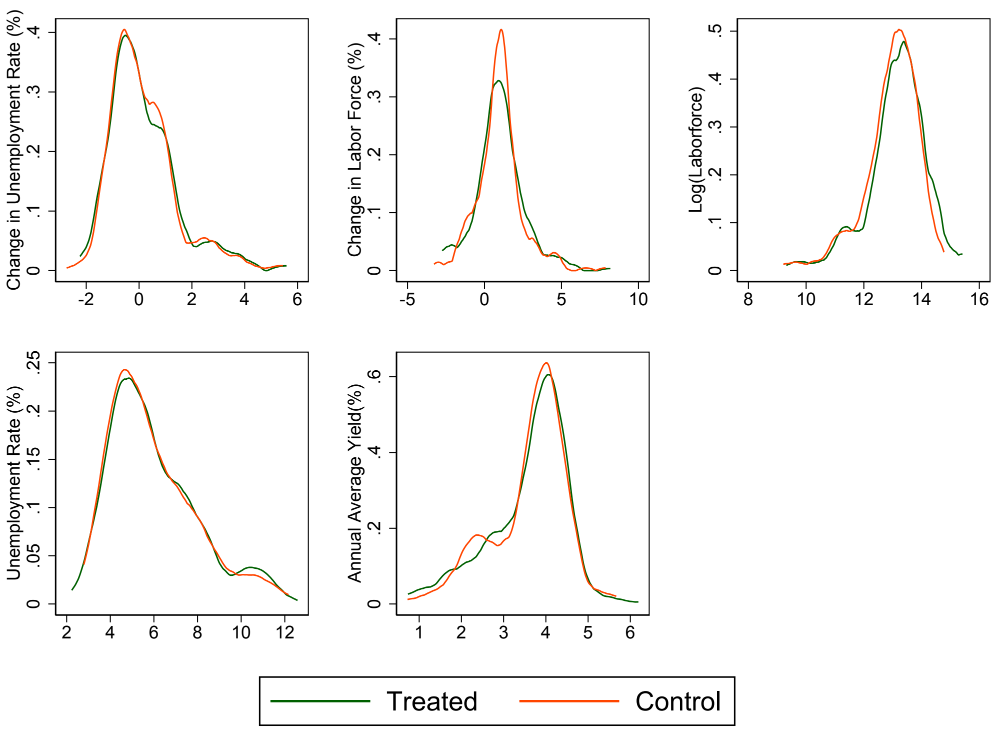


Figure 1: Matching County Characteristics: The figure shows the kernel density plot based on matching the treatment counties using five variables in the year before firm bankruptcy: unemployment rate, change in unemployment rate, log(labor force), change in labor force, and average yield of the county in that year.

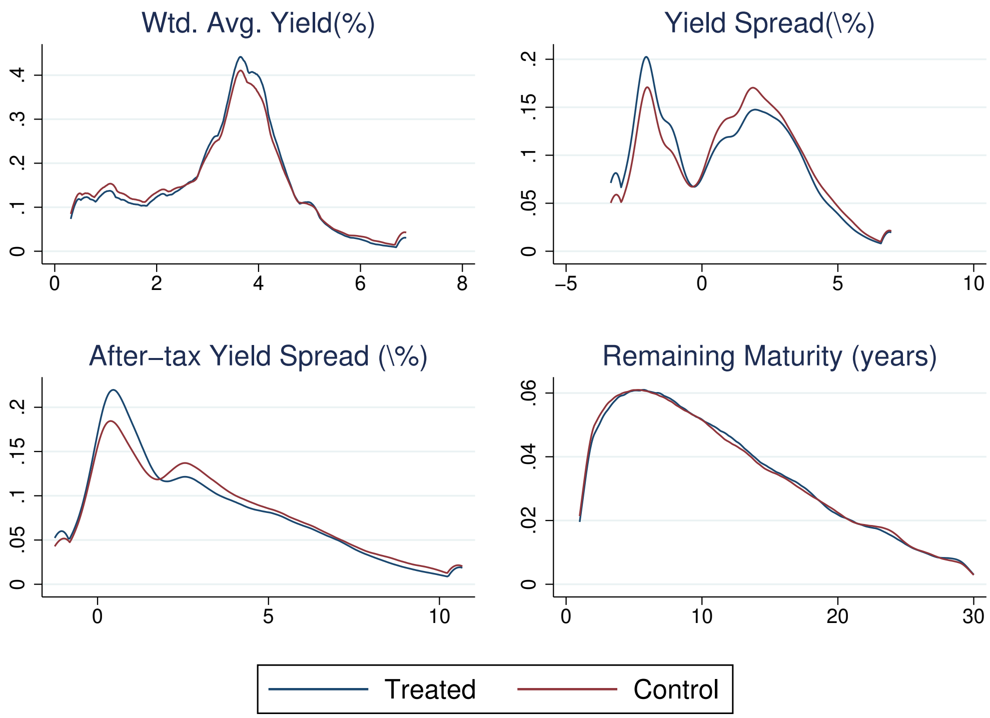


Figure 2: Secondary Market Bond Features: The figure shows the kernel density plot for municipal bond aspects from the secondary market. The county-level matching is based on five variables in the year before firm bankruptcy: unemployment rate, change in unemployment rate, log(labor force), change in labor force, and average yield of the county in that year.

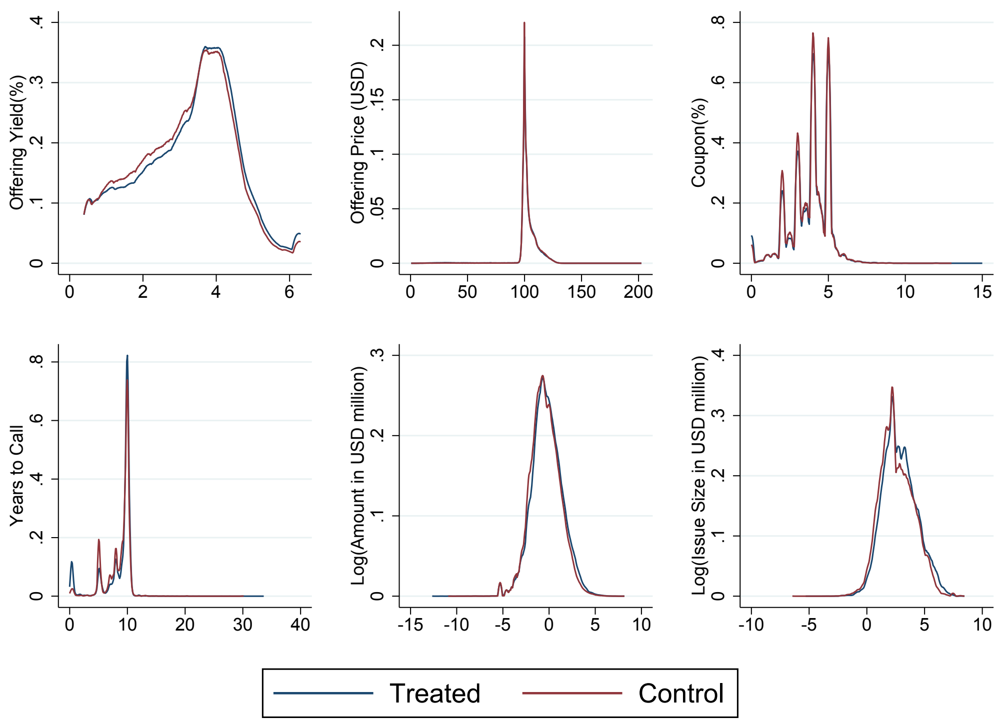
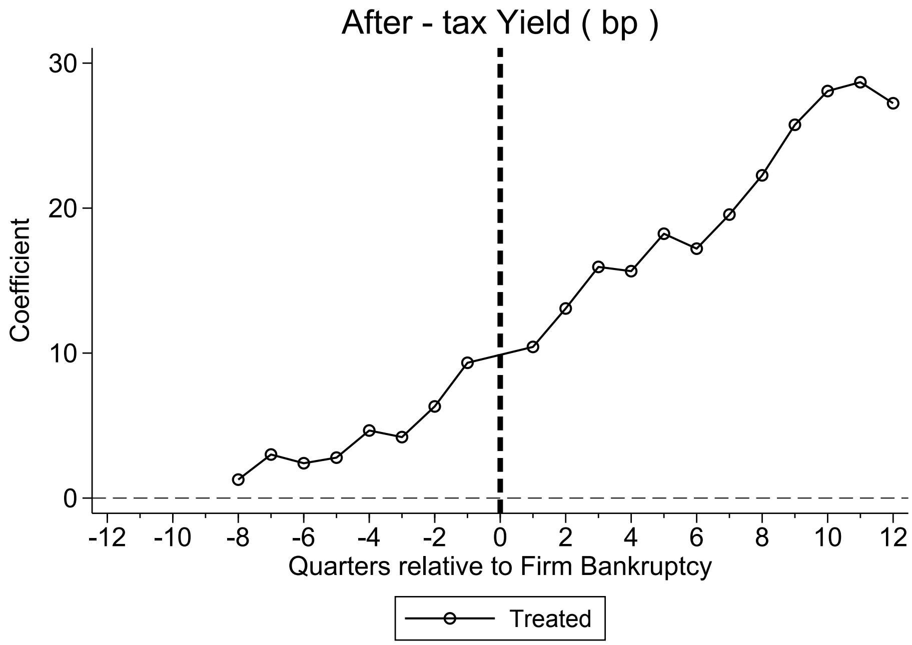


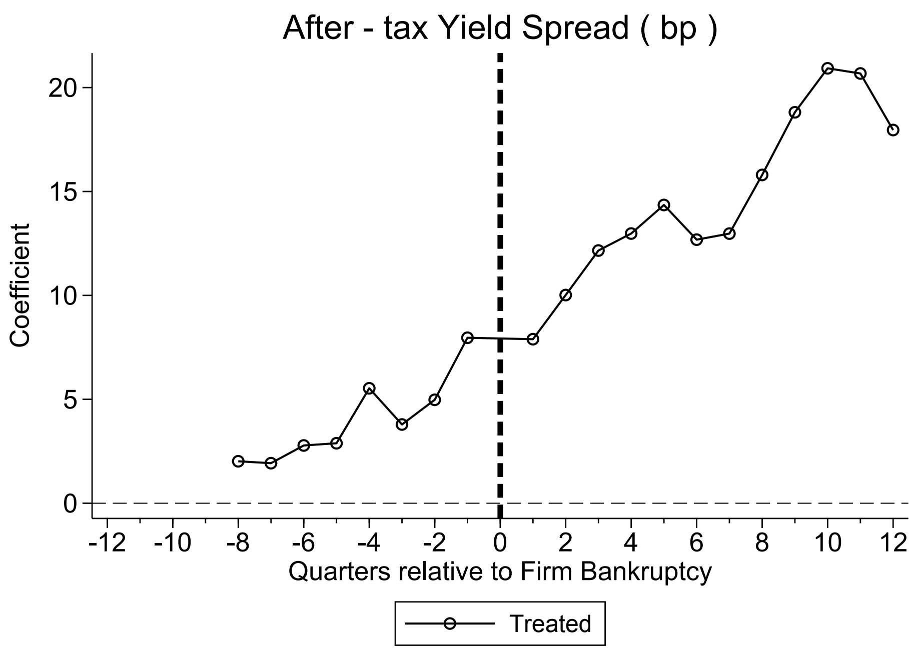
Figure 3: Primary Market Muni Bond Characteristics: The figure shows the kernel density plot for municipal bond aspects from the primary market. The county-level matching is based on five variables in the year before firm bankruptcy: unemployment rate, change in unemployment rate, log(labor force), change in labor force, and average yield of the county in that year.

Panel A:



(a)

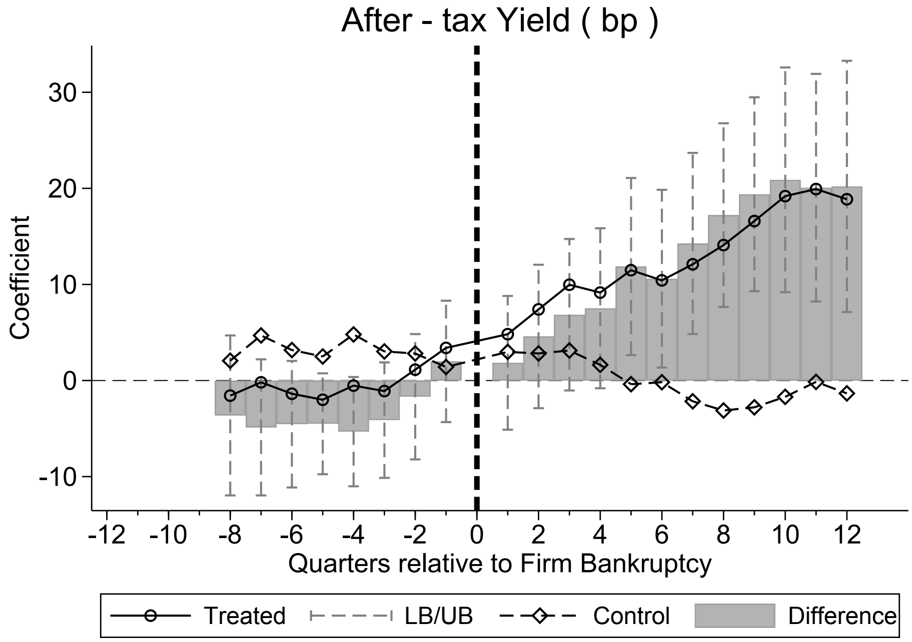
Panel B:



(b)

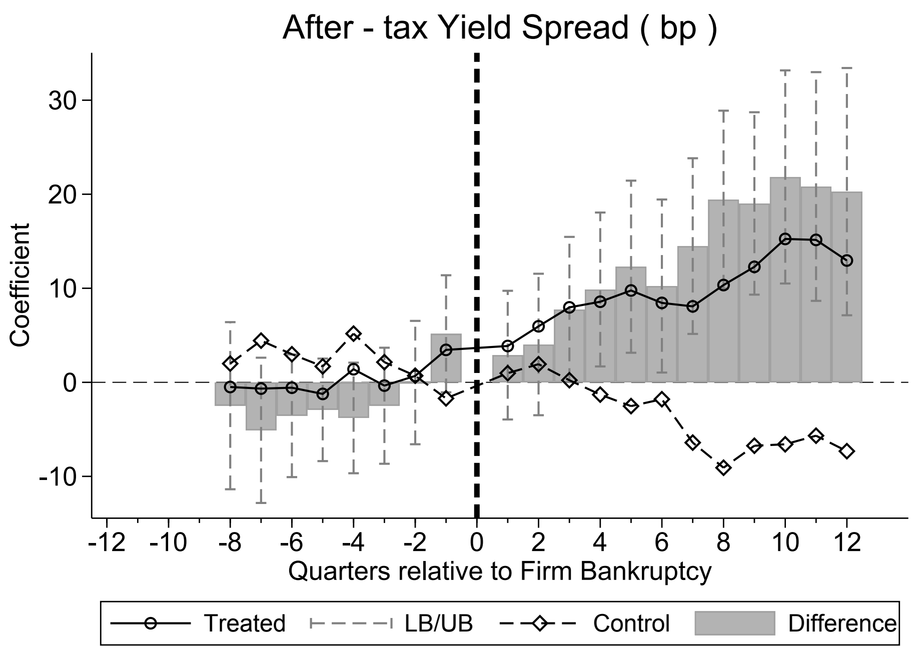
Figure 4: Secondary yields: Treatment Effect: The figure shows the impact on secondary market bond yields for the treated counties. We report the coefficients from Equation 4 by restricting the sample to bonds from the treated counties. We cluster standard errors at the county bond issue and year month level.

Panel A:



(a)

Panel B:



(b)

Figure 5: 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 issue 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.

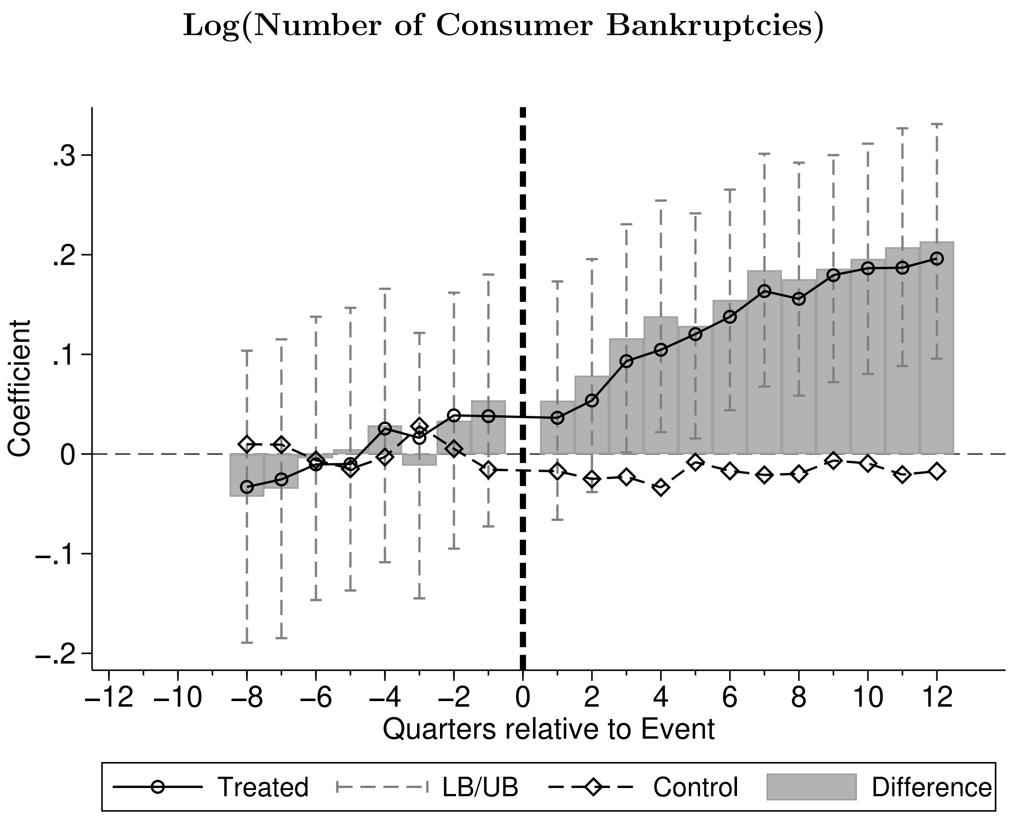
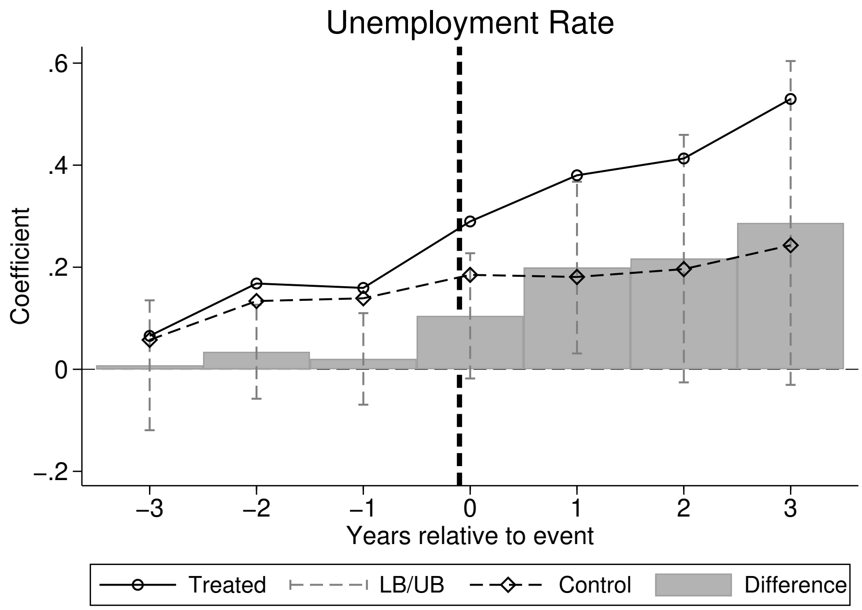


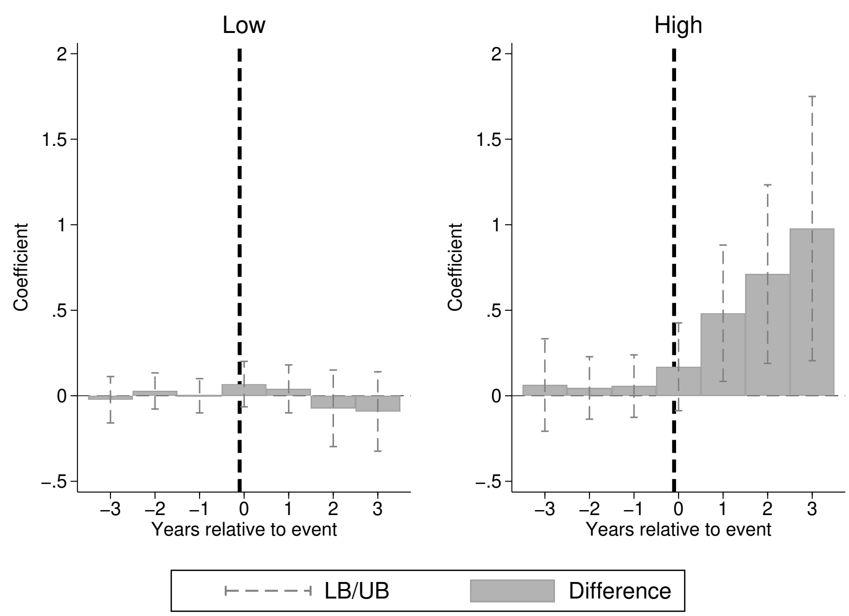
Figure 6: Consumer Bankruptcies : Treatment vs Matched Control: The figure shows the coefficients based on Equation 4. We regress the logged value of the number of consumer bankruptcies as the dependent variable. We cluster standard errors at the event pair level. The control county was matched based 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 Budgetary Restrictions



(b)

Figure 7: 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 counties, and in Panel B, we show the impact on sub-samples of low versus high number of county-level budgetary restrictions. 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.

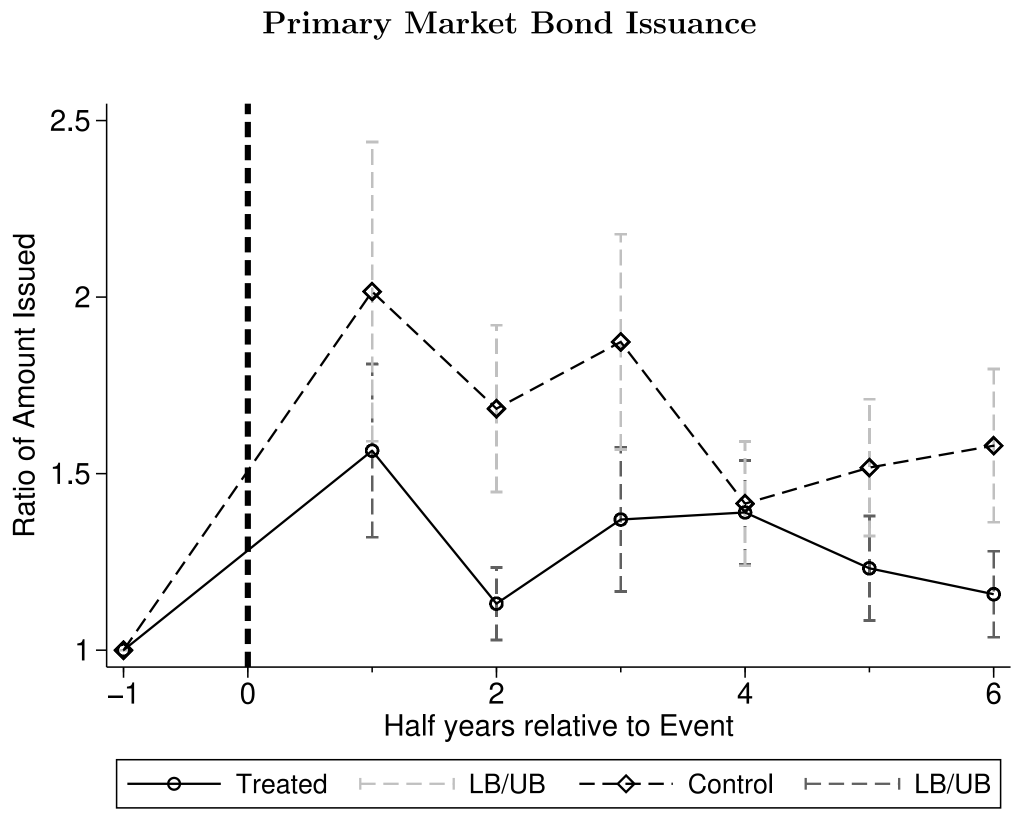
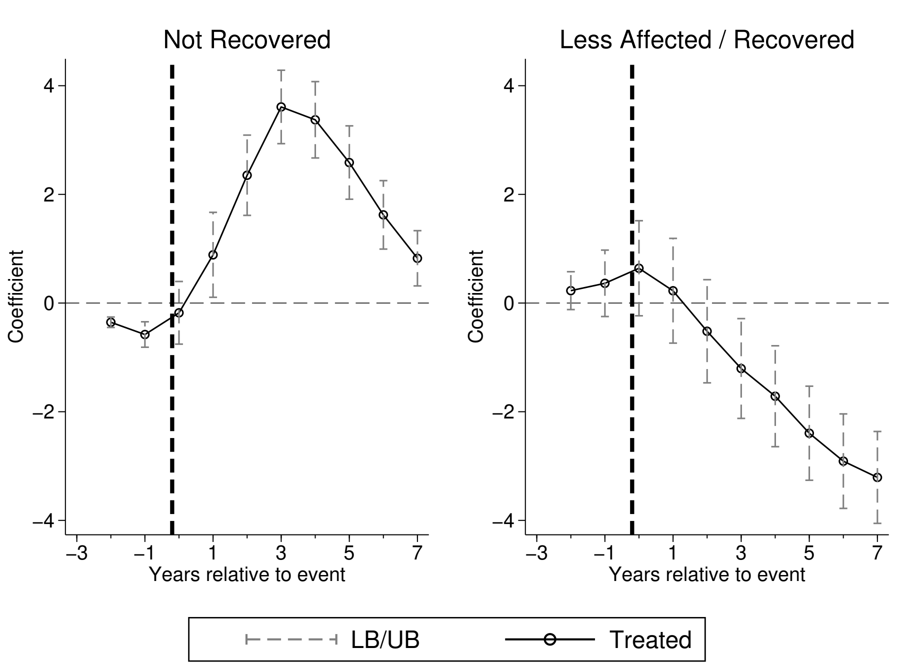


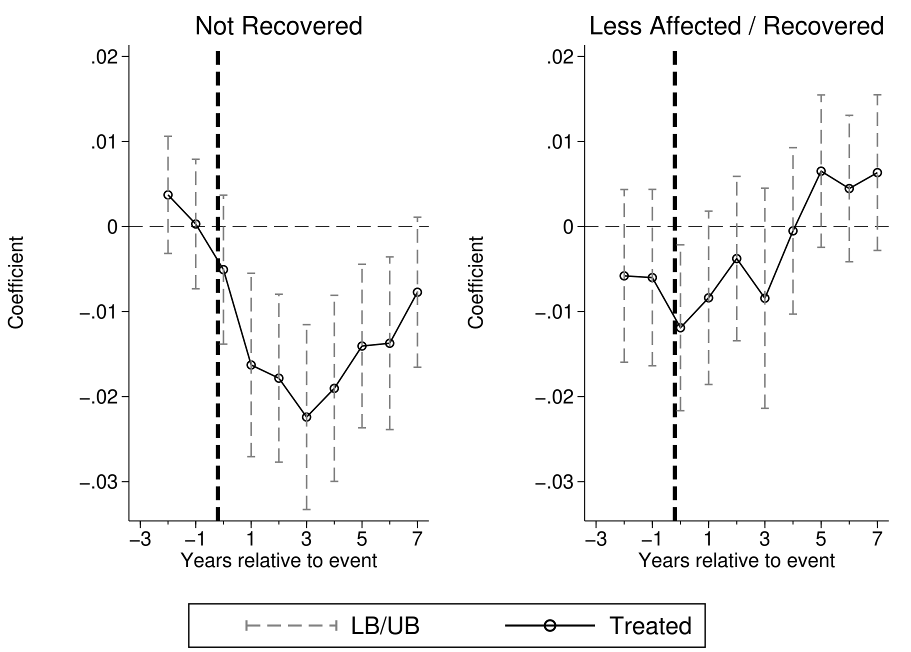
Figure 8: Primary Market Bond Issuance: The figure shows the county level aggregate volume of bond issuance for treated and control counties after the firm bankruptcy filing. For each county, we calculate the total par value of bonds issued in the six month rolling window during T=-1 to T=-6 months before 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. 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.

Panel A: Unemployment Rate (%)



(a)

Panel B: Growth Rate in Establishments



(b)

Figure 9: Recovery of Treated Counties: The figure shows the association of unemployment rate and growth rate in establishments in the treated counties after the firm bankruptcy. We plot the coefficients obtained after modifying the annualized version of Equation 4 by omitting dummies corresponding to the control counties. We cluster standard errors at the event level. In Panel A, we use county unemployment rate as the dependent variable, while Panel B shows the results using growth rate in establishments.

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. 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.

Panel A: Firm Bankruptcies in Manufacturing

	Count	Mean	Median	Std. Dev.
Total Revenue	126	578.5	31.1	1,518.3
EBITDA	125	14.6	-4.7	101.5
Net Income	126	-75.0	-13.2	206.1
Total Assets	126	461.6	43.7	1,142.8
LT Debt - Total	126	105.0	1.6	315.9
Equity	126	-27.5	0.1	337.6
Employees	126	2,168.9	167.5	7,171.5

Panel B: Difference between Treated and Control Counties

	Mean (Treated)	Mean (Control)	Difference	T-Stat	N_1	N_2
Annual Trading Volume (USD mio)	761.9	589.2	172.74	1.23	128	128
Annual Average Yield (%)	3.5	3.5	-0.05	-0.39	128	128
Population	884,806.9	707,202.2	177,604.66	1.71	127	127
Unemployment Rate (%)	0.1	0.1	0.00	0.10	128	128
Laborforce	458,578.2	371,886.1	86,692.05	1.67	128	128
Log(Laborforce)	12.6	12.5	0.13	1.00	128	128
Revenue (USD million)	4.6	3.4	1.22	1.92	127	127
Expenditure (USD million)	4.6	3.4	1.13	1.82	127	127
Surplus (USD million)	0.0	-0.1	0.09	1.58	127	127
Zillow HPI	300,816.0	270,647.2	30,168.80	1.04	125	125

Panel C: Distribution between Treated and Control Counties

	Count	Mean	p25	p50	p75	Std. Dev.
Treated						
Annual Trading Volume (USD mio)	128	761.9	85.0	242.3	880.7	1,176.2
Annual Average Yield (%)	128	3.5	2.9	3.8	4.1	1.0
Population	127	884,806.9	361,000.0	686,600.0	1,023,859.0	1,033,083.0
Unemployment Rate (%)	128	0.1	0.0	0.1	0.1	0.0
Log(Laborforce)	128	12.6	12.1	12.8	13.3	1.0
Revenue (USD million)	127	4.6	1.3	2.7	5.9	6.5
Expenditure (USD million)	127	4.6	1.3	2.8	6.0	6.3
Zillow HPI	125	300,816.0	138,200.0	237,100.0	379,700.0	245,562.8
Control						
Annual Trading Volume (USD mio)	128	589.2	87.0	213.9	601.9	1,059.5
Annual Average Yield (%)	128	3.5	3.1	3.7	4.1	1.0
Population	127	707,202.2	350,858.0	647,187.0	911,626.0	553,934.2
Unemployment Rate (%)	128	0.1	0.0	0.1	0.1	0.0
Log(Laborforce)	128	12.5	12.1	12.7	13.1	1.0
Revenue (USD million)	127	3.4	1.2	2.3	4.8	3.1
Expenditure (USD million)	127	3.4	1.2	2.3	4.9	3.2
Zillow HPI	125	270,647.2	148,300.0	206,000.0	323,600.0	211,200.1

Table 2: Impact on Borrowing Costs of Local Governments: Evidence from Municipal Bonds Secondary Market

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)-(6), the dependent variable is after-tax yield spread, which is calculated using Equations (2) and (3). Our baseline specification comes from Column (6) in Panel A. In Panel B, we report the baseline specification from Column (6) of Panel A by changing the beginning or ending point of the event scale, holding the other period constant at 36 months. In Panel C, we report the baseline results from Column (6) of Panel A by altering the length of the event window duration. 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: Firm Bankruptcies in Manufacturing

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

Panel B: Sensitivity to Choice of Windows - Length of Duration

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]
Event FE	✓	✓	✓	✓	✓	✓
County FE	✓	✓	✓	✓	✓	✓
Year-month FE	✓	✓	✓	✓	✓	✓
Bond Controls	✓	✓	✓	✓	✓	✓
County Controls	✓	✓	✓	✓	✓	✓
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 C: Sensitivity to Choice of Windows - Start/End

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]
Event FE	✓	✓	✓	✓	✓	✓
County FE	✓	✓	✓	✓	✓	✓
Year-month FE	✓	✓	✓	✓	✓	✓
Bond Controls	✓	✓	✓	✓	✓	✓
County Controls	✓	✓	✓	✓	✓	✓
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 3: Headquarter vs Other Firm Locations

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		
	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	✓	✓	✓	✓	✓	✓	✓
Bond Controls	✓	✓	✓	✓	✓	✓	✓
County 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

Table 4: Robustness Tests

In this table we report results for various robustness tests on our baseline specification, i.e., Column (6) of Table 2. Columns (1)-(2) show the baseline effect by changing the dependent variable to average yield and average yield spread, respectively. In Columns (3)-(6), we report results using only customer-buy trades with transaction size $>$ \$25,000, $>$ \$50,000, $\leq$25,000, and $\leq$ \$50,000, respectively. Columns (7)-(10) 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 use additional controls in Columns (11)-(12). In Column (11), we introduce additional county level time-varying covariates using lagged values of log(personal income) and log(house price index). In Column (12), 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 (13)-(14), we control for duration. First, in Column (13), we use duration in the controls by replacing years to maturity and inverse of years to maturity. Second, in Column (14), we use tax-adjusted duration to replace years to maturity and inverse of years to maturity. Columns (15)-(21) report results controlling for unobserved factors at the bond, county, region, and municipal bond market levels. Specifically, in Column (15), we introduce bond fixed effect to the baseline. In Column (16), we impose region fixed effects. Columns (17)-(18) show the results after adding time-varying region-year and region-year month fixed effects, respectively. Column (19) shows the baseline results with year-month $\times$ GO bond indicator fixed effects. Finally, in Column (20), we use year-month $\times$ Insured bond status fixed effects. Column (21) shows the baseline results after including bond purpose $\times$ year-month fixed effects. We use other sub-samples in Columns (22)-(24). In Column (22), 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 criteria is imposed based on the publicly listed manufacturing bankruptcy events in our main sample set. Column (23) shows results by expanding the original sample of events to the broader set of bankruptcy filings from tradeable sector industries. In Column (24), we restrict our sample of bonds to tax-exempt municipal bonds only. In Column (25), we use transactions after the financial crisis of 2010. Columns (26)-(28) show the results with alternative specifications. Specifically, Column (26) shows the results by introducing event-month fixed effects to the baseline. In Column (27), we drop the year month fixed effects and replace them with the event-month fixed effects in the baseline. Column (28) shows the result by changing year-month fixed effect to calendar year fixed effects. Columns (29)-(31) report results with alternative choices for clustering the standard errors, namely: issue and year-month, county bond issuer, and county bond issue. 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									
	Other Dependent Variables		Trade Size (in USD)				Drop Bonds Dated Within			
	Yield	Yield spread	>25,000	>50,000	≤25,000	≤50,000	6 months	12 months	24 months	36 months
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Post × Treated	7.19***	6.51***	7.84**	5.83*	9.43***	9.86***	10.01***	10.42***	11.81***	11.70***
	[3.91]	[3.39]	[2.38]	[1.68]	[2.91]	[3.13]	[3.14]	[3.17]	[3.24]	[2.91]
Adj.-R^2	0.571	0.801	0.646	0.627	0.667	0.661	0.641	0.632	0.615	0.595
Obs.	2,835,820	2,703,829	1,482,804	872,465	1,915,893	2,323,079	2,518,961	2,348,294	2,024,963	1,713,545

	Additional Controls		Controlling for duration		Other unobservables						
	More County Controls	Add Ratings	Use Duration	Use after-tax duration	Use Bond FE	Use Region FE	Use Region-Year FE	Use Region-YM FE	Use GO bond-YM FE	Use Insured-YM FE	Use Purpose-YM FE
	(11)	(12)	(13)	(14)	(15)	(16)	(17)	(18)	(19)	(20)	(21)
Post × Treated	9.60***	8.28**	10.60***	10.61***	8.23**	10.01***	10.42***	10.66***	8.74***	7.74***	7.88***
	[2.95]	[2.32]	[3.35]	[3.32]	[2.57]	[3.23]	[3.38]	[3.45]	[2.95]	[2.62]	[3.02]
Adj.-R^2	0.651	0.663	0.637	0.626	0.833	0.648	0.650	0.651	0.655	0.652	0.677
Obs.	2,504,271	2,016,817	2,688,584	2,688,584	2,697,225	2,703,342	2,703,342	2,703,342	2,703,342	2,703,342	2,703,113

	Other Sub-samples			Financial Crisis	Other Specifications			Clustering		
	Restricted Control Counties	Tradeables	Tax-Exempt Bonds	Trades After 2010	Add event-month FE	Change YM FE to event-month FE	Change YM FE to Year FE	Issue and YM	Issuer	Issue
	(22)	(23)	(24)	(25)	(26)	(27)	(28)	(29)	(30)	(31)
Post × Treated	8.69**	8.27***	11.02***	11.04**	10.03***	23.27***	10.35***	10.01***	10.01***	10.01***
	[2.45]	[3.05]	[3.55]	[2.38]	[3.23]	[3.69]	[3.38]	[4.44]	[3.30]	[4.81]
Adj.-R^2	0.648	0.634	0.662	0.438	0.648	0.416	0.512	0.648	0.648	0.648
Obs.	2,567,695	3,731,872	2,453,646	1,272,300	2,703,342	2,703,342	2,703,342	2,703,342	2,703,342	2,703,342

Table 5: 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 4.3.1. 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 6: 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 4.3.2. 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			
Post × Treated	Net Tax (after incentives) (1)	Total Incentives (2)	Investment Tax Credit (3)	R&D Tax Credit (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

Table 7: County Exposure

This table reports the results for the proposed mechanism at work. We report results for our baseline specification from Equation 1, which is interacted with dummies corresponding to county dependence and county exposure. We additionally include group-month fixed effects in the modified baseline equation. We define county dependence based on the median values of the respective treated county's share of employment (or wages) in the NAICS-3 industry of firm bankruptcy. For county-level exposure measures, we use BLS input-output tables to quantify the weighted linkages for each county in the industry of firm bankruptcy. We again divide counties into two groups based on the median value of the exposure. We provide more details in Section 4.4. Columns (1) and (2) show the results using *Dependence* based on wages and employment, respectively. Columns (3) and (4) show the results using *Exposure* based on wages and employment, respectively. 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			
	Dependence		Exposure	
	(Wages)	(Employment)	(Wages)	(Employment)
Post x Treated	(1)	(2)	(3)	(4)
Low	1.90	3.88	1.03	4.47
	[0.41]	[0.87]	[0.28]	[1.19]
High	15.01***	13.73***	18.75***	15.85***
	[3.69]	[3.34]	[3.74]	[3.23]
Difference	13.11	9.85	17.72	11.39
P-value	0.03	0.10	0.01	0.07
Event FE	✓	✓	✓	✓
County FE	✓	✓	✓	✓
Month FE	✓	✓	✓	✓
Group Month FE	✓	✓	✓	✓
Bond Controls	✓	✓	✓	✓
County Controls	✓	✓	✓	✓
Adj.-R^2	0.649	0.649	0.649	0.649
Obs.	2,703,342	2,703,342	2,703,342	2,703,342

Table 8: Heterogeneity

This table reports the results to show 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. In Column (1), we use the firm's operating income to the county's revenue. Column (2) uses the ratio of the firms' selling, general and administrative (SGA) expenses to the county's revenue. Further, in Column (3), we use a ratio of the firm's plant, property and equipment (PPE) to the property tax revenue of the county. Finally, Column (4) shows the impact across different types of municipal bonds in the treated counties, based on the use of proceeds. 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			
	Relative Importance of Firm			
	Firm EBIT / County Revenue	Firm SG&A / County Revenue	Firm PP&E / Property Tax	Bond Use of Proceeds
Post x Treated	(1)	(2)	(3)	(4)
Low	3.49	5.15	7.80**	
	[0.82]	[1.20]	[1.99]	
High	14.98***	20.84***	11.96**	
	[3.41]	[3.20]	[2.26]	
Education (Primary, Secondary)				6.56
				[1.55]
Improvement & Development				32.55***
				[4.99]
Other Public Services				0.79
				[0.12]
Transpo.-Housing -Hospitality				-5.62
				[-0.68]
Utilities-Water -Sewer				0.09
				[0.01]
Difference	11.49	15.69	4.16	
P-value	0.06	0.06	0.53	
Event FE	✓	✓	✓	✓
County FE	✓	✓	✓	✓
Month FE	✓	✓	✓	✓
Group Month FE	✓	✓	✓	✓
Bond Controls	✓	✓	✓	✓
County Controls	✓	✓	✓	✓
Adj.-R^2	0.651	0.660	0.651	0.656
Obs.	2,555,676	2,210,711	2,555,676	2,703,008

Table 9: 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 6, for offering yields. The dependent variable in Columns (1)-(3) is the offering yield. In Column (1), we show the result by using only the 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 effect. Column (4) shows the results with after-tax offering yield as the outcome variable. Finally, in Column (5), we use after-tax offering yield spread as the dependent variable. 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:	Offering Yield			After-tax Offering Yield	After-tax Offering Yield Spread
	(1)	(2)	(3)	(4)	(5)
Post x Treated	8.25***	6.29***	3.24***	5.78***	6.14***
	[6.23]	[6.38]	[3.46]	[3.73]	[3.91]
Post	-5.94***	-5.18***	-3.76***	-6.02***	-5.05***
	[-4.39]	[-5.13]	[-3.84]	[-3.69]	[-3.09]
Treated	-2.18*	-1.28	0.60	1.42	1.06
	[-1.86]	[-1.54]	[0.74]	[1.04]	[0.79]
Event FE	✓	✓	✓	✓	✓
Issuer FE	✓	✓	✓	✓	✓
Bond Controls		✓	✓	✓	✓
County FE			✓	✓	✓
County Controls			✓	✓	✓
Adj.-R^2	0.502	0.852	0.855	0.845	0.839
Obs.	424,708	424,708	424,708	424,708	424,708

Table 10: County-level Robberies: State Imposed Budgetary Restrictions

This table reports results for the impact on county-level robberies in a month 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 county-level number of robberies per million population in a month. We additionally include group-month fixed effects in the modified baseline equation. We provide more details in Section 4.7. In Column (1), we report the aggregate effect on treated counties, in caparison to the control group. Column (2) 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 (3), we divide the treated counties based on whether they have *Overall Property Tax Restriction* or not. Columns (4) shows the results using *Expenditure Limit* to distinguish between the types of treated counties. Finally, in Column (5), we show the impact based on linear combination of these indicators (in Columns (2)-(4)) 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 clustered at the event-pair level, unless otherwise specified. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Dependent Variable:	Monthly Robberies per million population				
Interaction Variable:		Debt Issuance	Overall Property Tax Restriction	Expenditure Limit	Combination of Restrictions
Post x Treated	(1)	(2)	(3)	(4)	(5)
Overall	6.60**				
	[0.02]				
No		6.28	1.85	4.95*	
		[0.22]	[0.53]	[0.09]	
Yes		6.77**	15.58***	14.28**	
		[0.05]	[0.01]	[0.04]	
None					6.24
					[0.23]
One					-0.93
					[0.80]
Multiple					15.30***
					[0.00]
Difference		0.49	13.73	9.33	9.06
P-value		0.94	0.03	0.20	0.24
Event FE	✓	✓	✓	✓	✓
County FE	✓	✓	✓	✓	✓
Month FE	✓				
Group-Month FE		✓	✓	✓	✓
Controls	✓	✓	✓	✓	✓
Adj.-R^2	0.456	0.512	0.460	0.456	0.509
Obs.	18,153	18,153	18,153	18,153	18,153

Table 11: Impact on County's Local Economy

This table shows the impact of 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 7. Column (1) shows the impact on GDP growth(%). We use the logged value of our dependent variable in Columns (2)-(13). Specifically, Column (2) shows the impact on average house price index. Column (3) shows the impact on total revenue. In Column (4), we use the property tax revenue as our outcome variable. In Column (5), we use total expenditure at the county level as the dependent variable. For Columns (6)-(13), we also show the interaction effect based on state-imposed county level budgetary restrictions. We use a linear combination of the dummy indicators for restrictions on debt issuance, overall property tax and expenditure limits by summing them up for each treated county. A *Low* value corresponds to none or one restriction. Whereas, a *High* value indicates multiple restrictions. We additionally control for group-month fixed effects in the baseline specification. Columns (6)-(7) show the impact on education expenditure. Columns (8)-(9) use the number of teachers as the dependent variable. In Columns (10)-(11), we sue the number of students as the dependent variable. Finally, Columns (12)-(13) show the results for the number of public schools in the county. T-statistics are reported in brackets and standard errors are clustered at the event-pair level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

						Education							
Dependent Variable:	GDP growth(%)	Log(Avg. House Price Index)	Log (Total Revenue)	Log (Total Property Tax)	Log (Total Expenditure)	Log(Education Expenditure)		Log(Total Teachers)		Log(Total Students)		Log(Total Public Schools)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)
Treated × Post	-0.01**	-0.01	-0.02*	-0.02*	-0.02*	-0.01		-0.01		-0.00		-0.00	
	[-2.04]	[-1.43]	[-1.71]	[-1.71]	[-1.77]	[-1.56]		[-1.37]		[-0.48]		[-0.57]	
Treated × Post × Low							-0.01		-0.00		0.00		0.01
							[-1.09]		[-0.43]		[0.40]		[1.15]
Treated × Post × High							-0.02		-0.02*		-0.01		-0.02*
							[-1.15]		[-1.70]		[-0.96]		[-1.74]
Difference							0.01		0.02		0.01		0.03
P-value							0.74		0.25		0.30		0.04
Event FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
County FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Group FE							✓		✓		✓		✓
County Controls	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Adj.-R^2	0.411	0.976	0.995	0.995	0.997	0.996	0.996	0.997	0.997	0.999	0.999	0.998	0.998
Obs.	1,704	1,724	1,732	1,732	1,732	1,721	1,721	1,679	1,679	1,743	1,743	1,744	1,744

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 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 IA3. 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 variable to identify control counties, we show that that the baseline effect ranges between 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 IA1, Figure IA2 and Figure IA3.

Next, if there is a concern that the control groups identified in the baseline approach may be different in terms of the size of bonds issued or the maturity bucket of municipal bonds, we address this 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. We present a comparison of our treated and control county characteristics on these matching variables in Figure IA4 and Figure IA5. 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 should come 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 attributed 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 IA6 and Figure IA7.

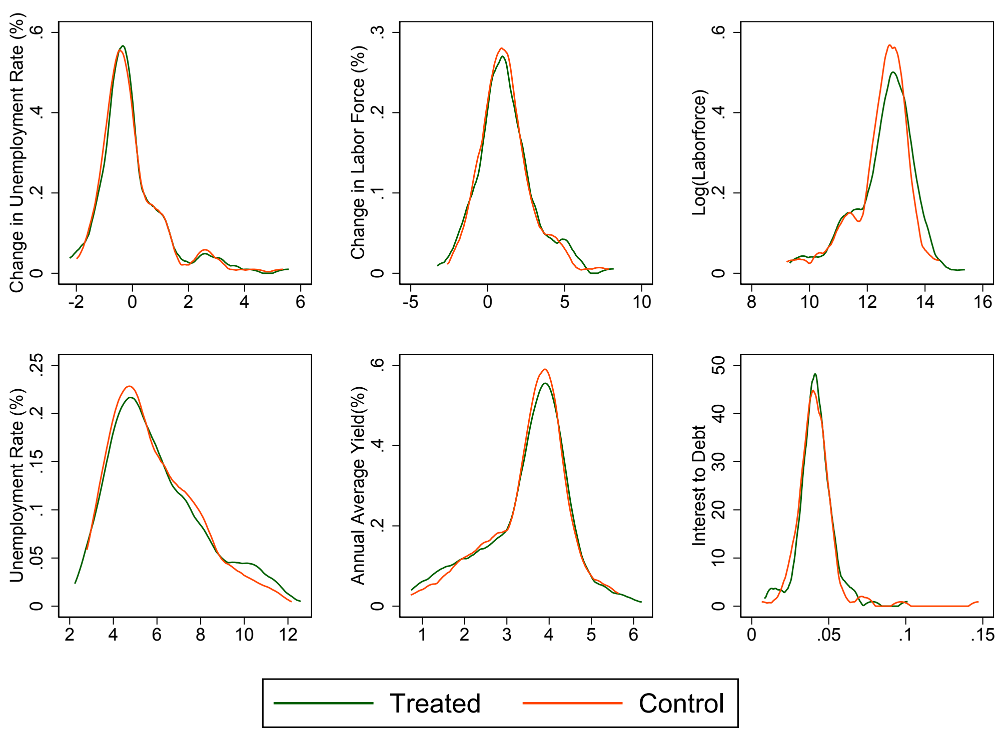


Figure IA1: Matching County Characteristics - Interest to Debt: The figure shows the kernel density plot based on matching the treatment counties using 5 variables in the year before firm bankruptcy: unemployment rate, change in unemployment rate, log(labor force), change in labor force and average yield of the county in that year.

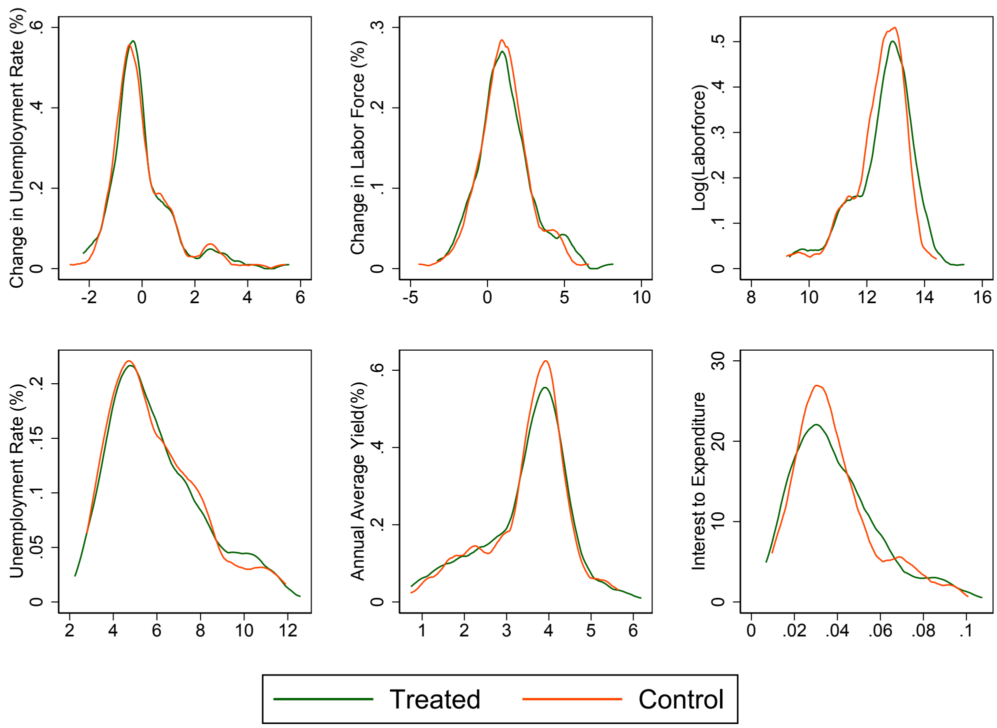


Figure IA2: Matching County Characteristics - Interest to Expenditure: The figure shows the kernel density plot based on matching the treatment counties using 5 variables in the year before firm bankruptcy: unemployment rate, change in unemployment rate, log(labor force), change in labor force and average yield of the county in that year.

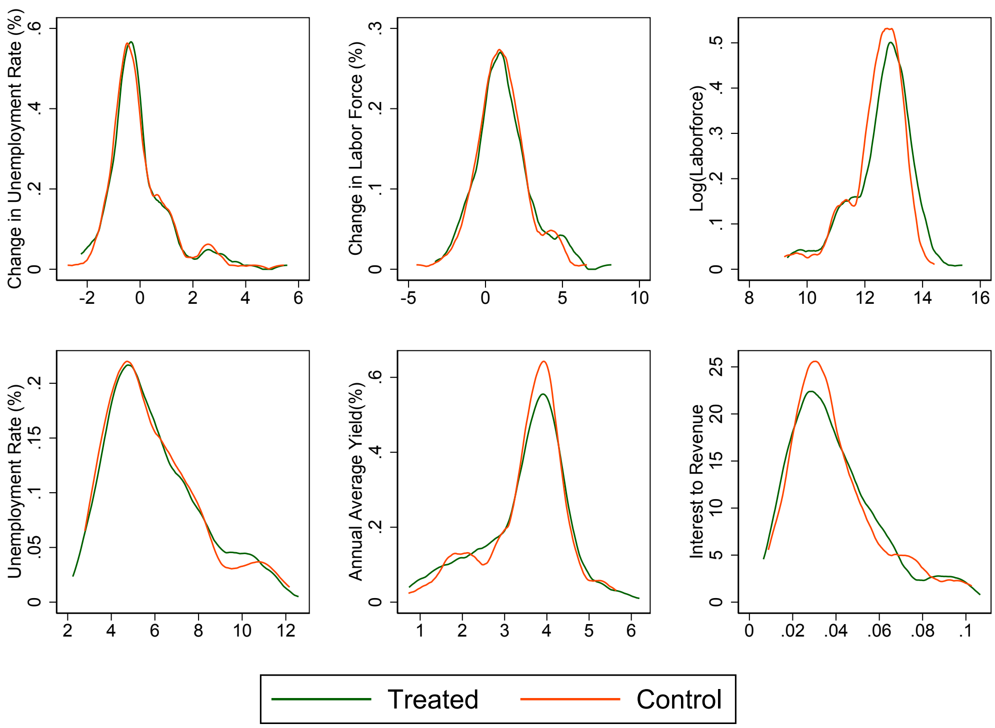


Figure IA3: Matching County Characteristics - Interest to Revenue: The figure shows the kernel density plot based on matching the treatment counties using 5 variables in the year before firm bankruptcy: unemployment rate, change in unemployment rate, log(labor force), change in labor force and average yield of the county in that year.

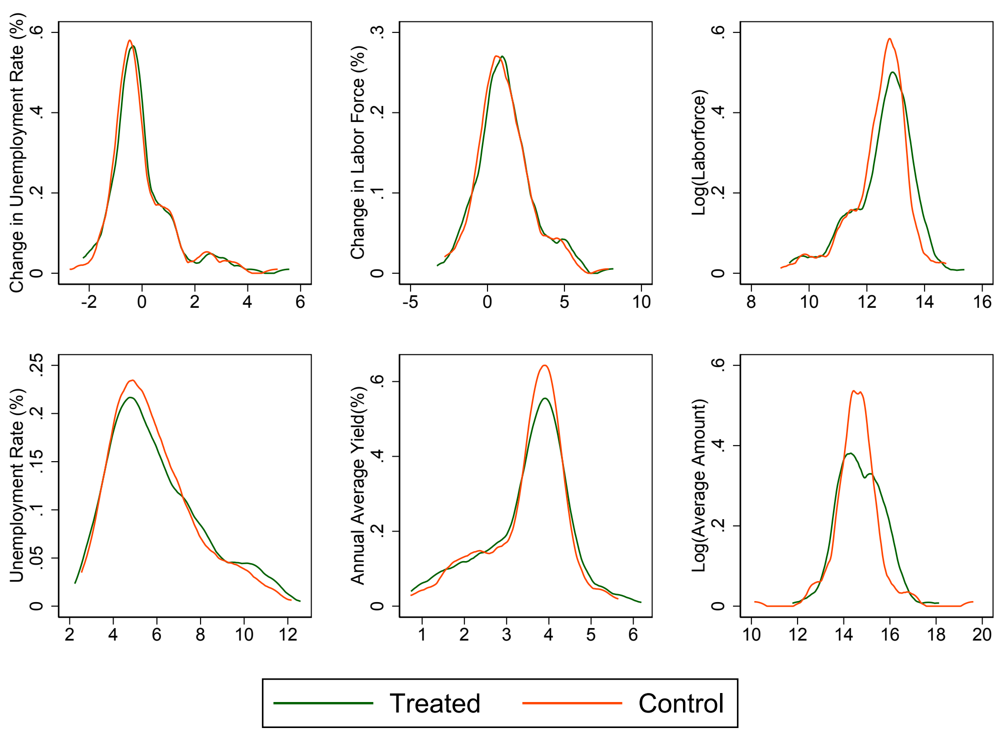


Figure IA4: Matching County Characteristics - Log(Average Amount): The figure shows the kernel density plot based on matching the treatment counties using 5 variables in the year before firm bankruptcy: unemployment rate, change in unemployment rate, log(labor force), change in labor force and average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

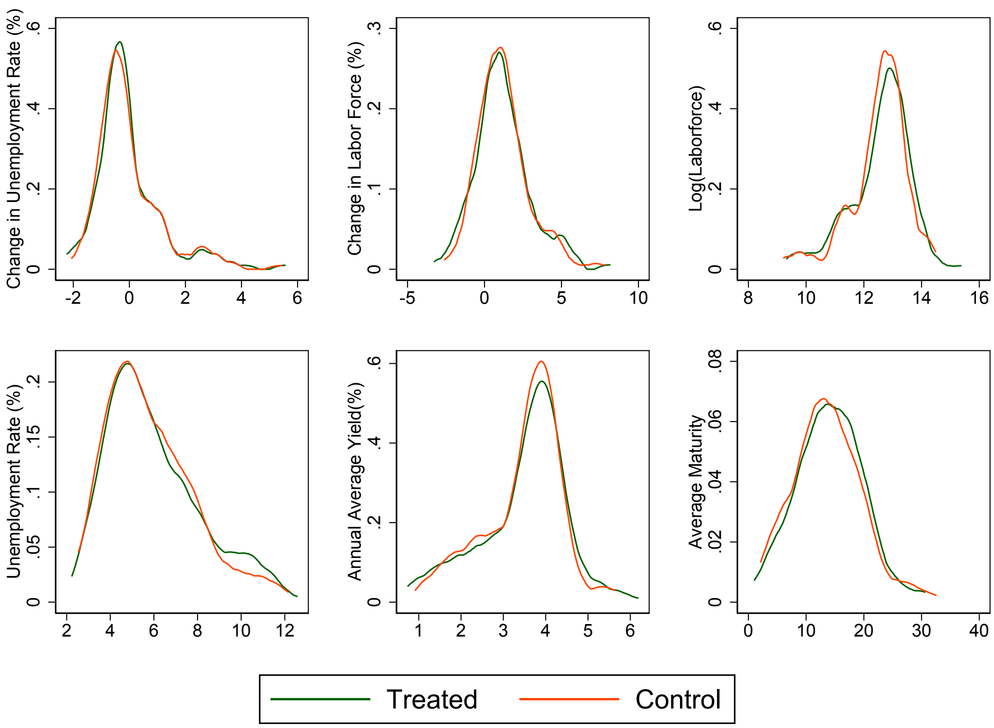


Figure IA5: Matching County Characteristics - Average Maturity: The figure shows the kernel density plot based on matching the treatment counties using 5 variables in the year before firm bankruptcy: unemployment rate, change in unemployment rate, log(labor force), change in labor force and average yield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

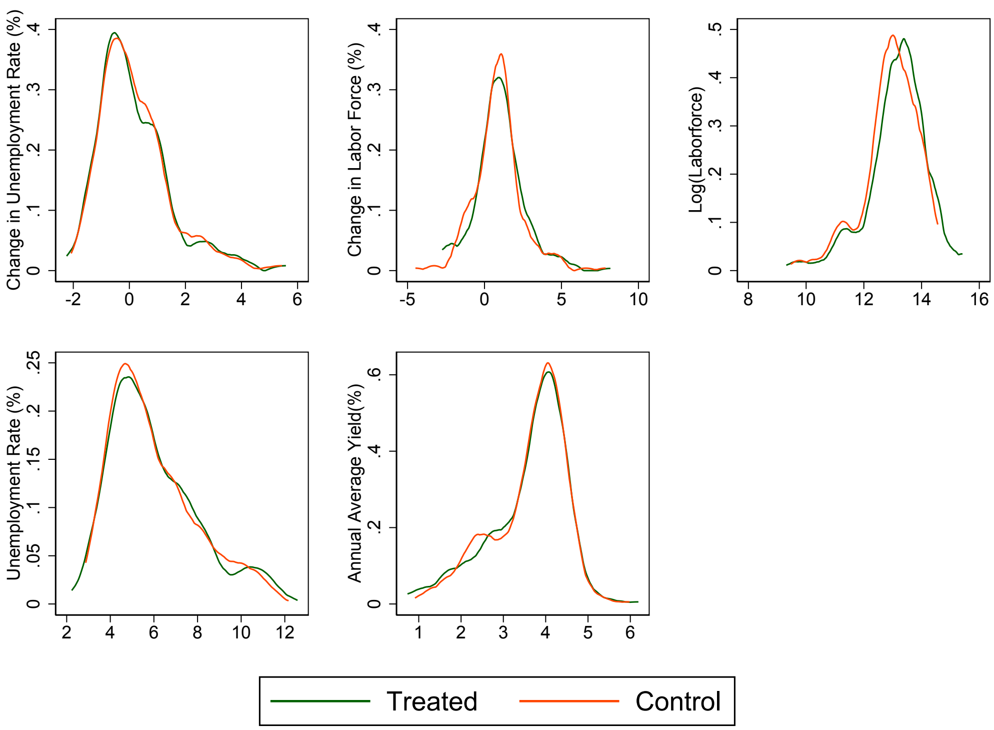


Figure IA6: Matching County Characteristics - same Region: The figure shows the kernel density plot based on matching the treatment counties using 5 variables in the year before firm bankruptcy: unemployment rate, change in unemployment rate, log(labor force), change in labor force and average yield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

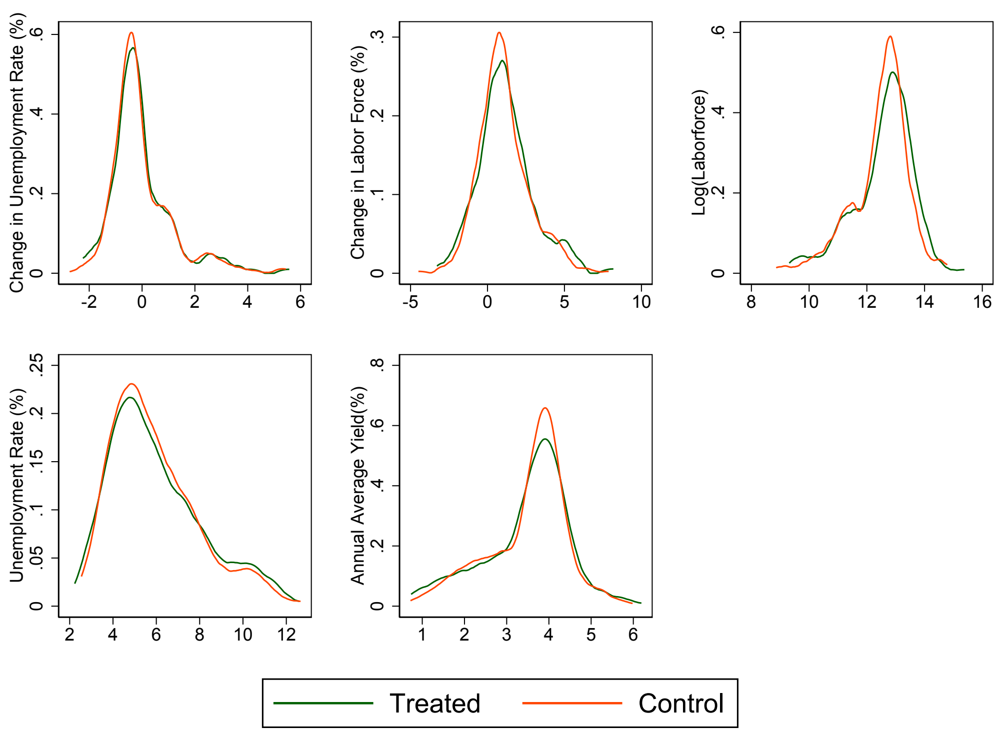
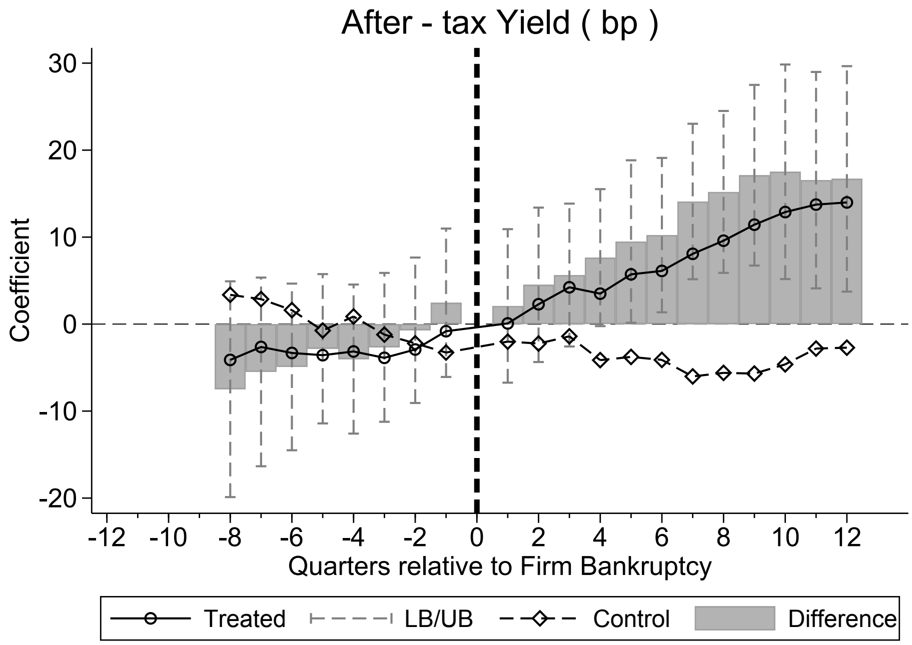


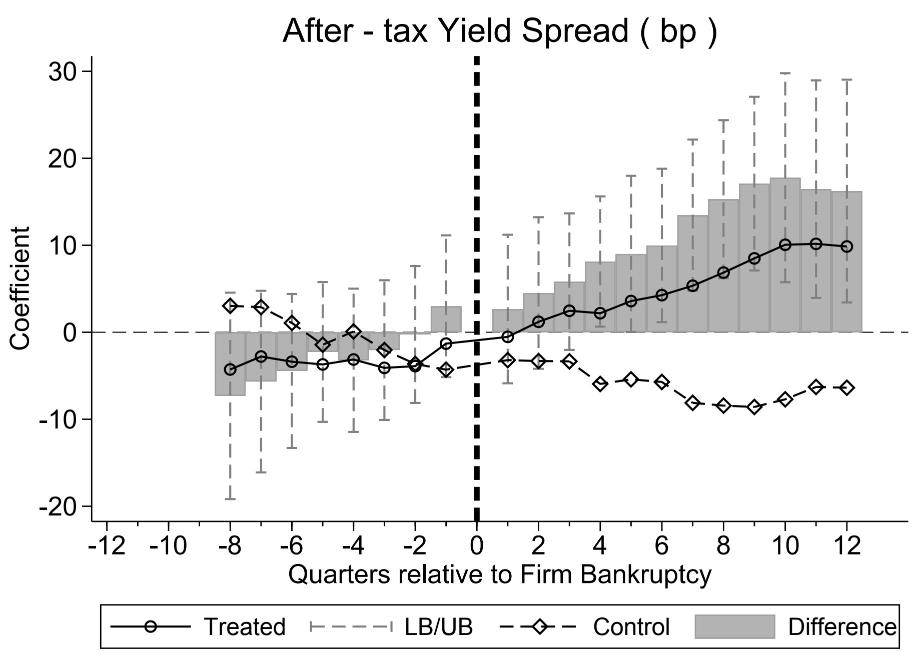
Figure IA7: Matching County Characteristics - KNN(3): The figure shows the kernel density plot based on matching the treatment counties using 5 variables in the year before firm bankruptcy: unemployment rate, change in unemployment rate, log(labor force), change in labor force and average yield of the county in that year.

Panel A:



(a)

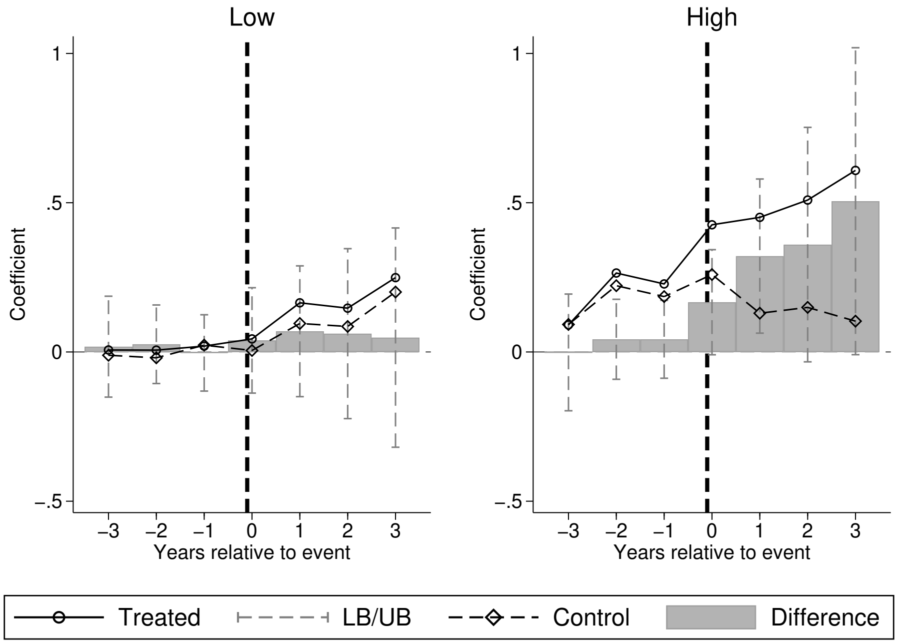
Panel B:



(b)

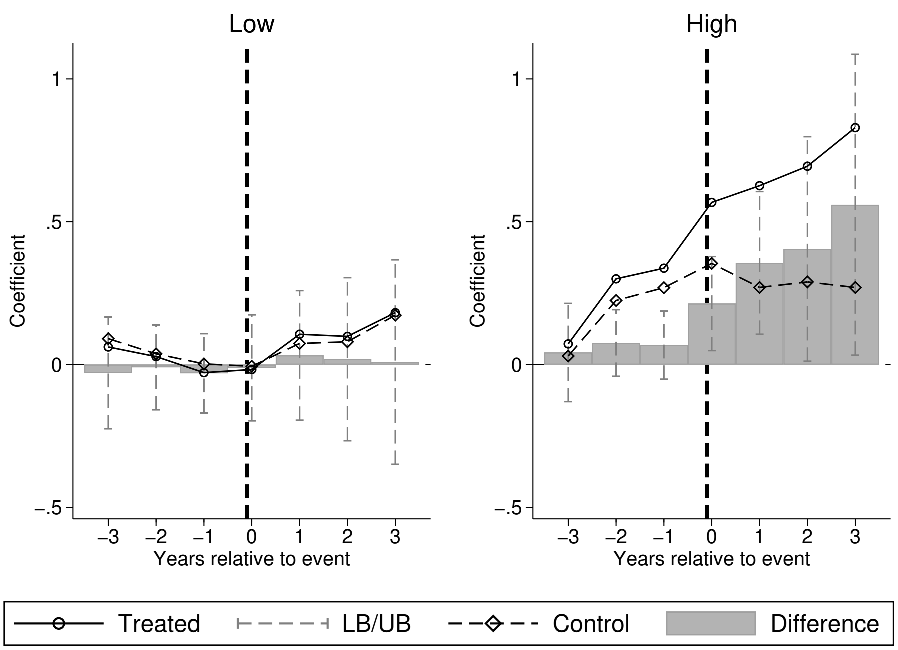
Figure IA8: Secondary yields - Bankruptcies in Tradeables: The figure shows the impact on secondary market bond yields between the treated and control counties. The control county was matched based on 5 variables in the year before firm bankruptcy: unemployment rate, change in unemployment rate, log(labor force), change in labor force and average yield of the county in that year.

Panel A: Based on County's Share of Employment



(a)

Panel B: Based on County's Share of Wages



(b)

Figure IA9: Unemployment Rate: Treatment vs Matched Control: The figure shows the impact on unemployment rates between the treated and control counties. We report coefficients from Equation 7 using unemployment rate as the dependent variable. We cluster standard errors at the event pair level. In Panel A, we shows the differential impact based on treated counties' share of employment. In Panel B, we show the impact on sub-samples of low versus high shares of wages among the treated counties. 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: Baseline Table with All Controls

This table reports the baseline results of Table 2 for our sample using Equation 1 estimating the differential effect on municipal bond yields of treated versus control after the firm bankruptcy filing. Columns (1)-(3) show the results for monthly after-tax yield as the dependent variable. Columns (4)-(6) show the results using after-tax yield spread as the dependent variable which is calculated using Equation 2 and 3. Our preferred specification comes from Column (6). P-values are reported in brackets and standard errors are clustered at issue and year month level, unless otherwise specified. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

	After-tax Yield			After-tax yield spread		
	(1)	(2)	(3)	(4)	(5)	(6)
Post x Treated	16.56***	16.53***	11.35***	15.40***	15.01***	10.01***
	[4.38]	[4.44]	[3.59]	[4.23]	[4.12]	[3.23]
Post	-7.99***	-7.35***	-3.56*	-7.19***	-6.67***	-3.13
	[-3.22]	[-2.92]	[-1.67]	[-2.89]	[-2.64]	[-1.46]
Treated	-8.86**	-4.65	-1.39	-7.29*	-4.69	-1.62
	[-1.99]	[-1.04]	[-0.34]	[-1.85]	[-1.14]	[-0.44]
Coupon		12.23***	12.00***		19.98***	19.77***
		[4.83]	[4.72]		[8.21]	[8.10]
comp_bid		10.52***	10.14***		10.82***	10.45***
		[3.52]	[3.43]		[3.67]	[3.57]
GO_dummy		-57.99***	-57.70***		-55.26***	-54.97***
		[-12.36]	[-12.31]		[-12.13]	[-12.08]
log_amt		-16.06***	-15.98***		-16.21***	-16.13***
		[-10.11]	[-10.04]		[-10.46]	[-10.39]
callable		-41.36***	-41.37***		-36.00***	-36.01***
		[-13.93]	[-13.92]		[-12.34]	[-12.35]
insured3		-26.71***	-27.65***		-23.51***	-24.41***
		[-5.84]	[-6.02]		[-5.23]	[-5.40]
years2maturity		14.15***	14.16***		9.12***	9.13***
		[25.20]	[25.31]		[12.10]	[12.14]
inv_yrs2maturity		-229.10***	-229.12***		37.89***	37.88***
		[-19.53]	[-19.57]		[3.21]	[3.22]
ch_unempRate			14.52***			14.90***
			[8.47]			[8.34]
ch_labourforce			1.32***			1.12**
			[2.81]			[2.29]
lag_log_laborforce			57.70			41.90
			[1.03]			[0.75]
lag_unemp_Rate			29.41***			28.12***
			[12.89]			[12.94]
Event FE	✓	✓	✓	✓	✓	✓
County FE	✓	✓	✓	✓	✓	✓
Year-month FE	✓	✓	✓	✓	✓	✓
Bond Controls		✓	✓		✓	✓
County Controls			✓			✓
Adj.-R^2	0.313	0.554	0.559	0.594	0.645	0.648
Obs.	2,703,342	2,703,342	2,703,342	2,703,342	2,703,342	2,703,342

Table IA3: Additional Matching Strategy

This table reports the results by incorporating additional matching strategies. We report results for our baseline specification from Equation 1. In Columns (1)-(3), we introduce an additional variable in 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 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 identify 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 IA4: 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 6, for offering yields. The dependent variable in Columns (1)-(3) is the offering yield. In Column (1), we show the result by using only the 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. Column (4) shows the results with after-tax offering yield as the outcome variable. Finally, in Column (5), we use after-tax offering yield spread as the dependent variable. 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:	Offering Yield			After-tax Offering Yield	After-tax Offering Yield Spread
	(1)	(2)	(3)	(4)	(5)
Post x Treated	6.33***	5.33***	3.52***	6.34***	7.12***
	[3.92]	[4.47]	[3.03]	[3.30]	[3.66]
Post	-5.22***	-6.12***	-5.47***	-8.91***	-8.72***
	[-3.12]	[-4.96]	[-4.52]	[-4.43]	[-4.31]
Treated	-1.57	-1.30	-0.42	-0.51	-0.31
	[-1.06]	[-1.24]	[-0.41]	[-0.30]	[-0.18]
Event FE	✓	✓	✓	✓	✓
Issuer FE	✓	✓	✓	✓	✓
County FE		✓	✓	✓	✓
Bond Controls		✓	✓	✓	✓
County Controls			✓	✓	✓
Adj.-R^2	0.498	0.862	0.864	0.853	0.849
Obs.	276,988	276,988	276,988	276,988	276,988