Biased Expectations and Credit Risk in the Municipal Bond Market *

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Abstract

We show that the pricing of credit risk in the municipal bond market depends on the salience of its underlying cash-flow shocks. We find that public mass shootings raise borrowing costs of issuers in affected counties by an average of 6 (5.2) basis points in the secondary (primary) market. This increase in tax-adjusted yield spreads is not driven by any material change in the issuers' fundamentals, nor by an increase in liquidity risk, risk aversion, or excess debt supply. In contrast, we find no evidence of the violent crime rate in the county being a priced risk factor. The evidence supports an explanation based on biased expectations of fundamentals growth brought about by media driven salience.

Keywords— Biased Beliefs, Public Mass Shootings, Municipal Debt, Salience

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1. Introduction

Variation in credit spreads are driven by investors' expectations of default risk of the borrower and expected returns on the debt security. Both expected default and expected returns cannot be observed. This makes the understanding of how investors form expectations of issuer's fundamentals, or how they perceive risk, critical to the pricing of securities. This has led to a recent surge in both empirical and theoretical work on understanding belief formation, as well as its implications across asset markets.[1] The municipal (muni) debt market is a particularly interesting setting, since direct holdings by households account for over 50% of total holdings (Cestau et al. (2019)), thus making retail investors the likely marginal investors in this market.[2] How do retail investors form expectations about credit losses in an illiquid asset market with limited information disclosure about issuer fundamentals (Cuny and Dube (2017))? Can salient news shocks distort investor expectations about future cash flows, and thereby credit risk? The extant literature has uncovered some important credit risk determinants of issuers in the municipal debt market[3]. Yet even though some of the default risk factors considered in the earlier papers are easily constructed from the econometrician's information set, how do unsophisticated retail investors price such hard-to-observe risk measures? In contrast, this paper aims to fill this gap by studying the effect, of a salient local shock, in this case - public mass shootings, on the borrowing costs of local governments in the municipal debt market.

The data on public mass shootings is obtained from the Washington Post. Since mass shootings are more likely to occur in urban & more populated counties, our identification relies on estimating the yield spread differential between the treated county and a set of counties with similar population levels as the treated county. Constructing the matched sample of control counties leaves us with 75 mass shootings over the sample period 2000-2018. Based on secondary market trades, the differential impact on the tax-adjusted yield spreads (raw yields) is 6.0 (3.9) basis points (bps) between issuers in affected counties and those in the control counties. To put this in perspective, the average spread between the highest rated (AAA) bonds and those just below investment grade (Ba1) bonds equals 47 bps. This implies that the average increase in the raw yield represents about 8.3% of the default spread.[4] The yield spread differential persists for around three years

[1] For instance, the tendency to extrapolate recent trends affects stock returns (Greenwood and Shleifer (2014)), credit spreads Bordalo et al. (2019), earnings expectations and its implications for stock market anomalies (Bordalo et al. (2020)), and finally, individual beliefs & portfolio allocation decisions (Giglio et al. (2021)).

[2] In contrast, households account for less than 10% of all Treasury and corporate bond holdings.

[3] Such default risk determinants include underfunded pensions of state governments (Novy-Marx and Rauh (2012)), existence of state policies for assisting distressed municipalities (Gao et al. (2019)), opioid epidemics (Cornaggia et al. (2021)), newspaper closures (Gao et al. (2020)).

[4] The difference between Ba1 and AAA rated bonds can include some impact due to illiquidity, so we can interpret

following a mass shooting, is largely insignificant in the fourth year and reverts by the fifth year. In the primary market, the effect is very similar, with the differential increase in yields at issuance averaging 5.2 bps in the two years following a mass shooting.

Why do mass shootings, arguably isolated and random events, raise municipal borrowing costs? Municipal bond spreads over treasuries consist of three components - a price premium for their favorable tax treatment component and price discounts for the liquidity and default risk components.[5] Hence a change in muni spreads over time must reflect variation in one or more of these components. It is unlikely that mass shootings could have any effect on the tax treatment of municipal bonds.[6] This leaves us with either the liquidity channel or the default risk channel. Indeed, expected returns (bond yield spreads in our case) could increase if bonds of affected issuers become more illiquid in the secondary market (Amihud and Mendelson (1986)). Yet, there is no change in the price dispersion metric (a measure of liquidity in bond markets, Jankowitsch et al. (2011)) or in the average trading volume and quantity issued in the two year period after the mass shootings suggesting that illiquidity is unlikely to drive the observed increase in yield spreads in the primary or the secondary market.

The results above suggest that expectations of an increase in default risk is the likely driver of the rise in muni yield spreads. An increase in the default risk component of the yield spread could be due to a perceived deterioration in the credit quality of issuers in the affected county, or due to an increase in the risk aversion of the marginal investors, or both. We investigate both these channels in greater detail. An increase in risk aversion alone should manifest in higher risk prices for all assets for which the affected investors are marginal. This suggests that bond yield spreads should increase for issuers in neighbouring counties as well, as they are likely to share the same marginal investors.[7] Using a matched sample of control counties for each neighbouring county of the affected county, we find no effect on the tax-adjusted or the raw yield spreads of issuers in neighbouring counties, suggesting that changes in risk aversion do not explain the results.

We next focus on the role of investor expectations of changes in issuer credit quality. This could happen because - a) investors perceive a large economic impact of mass shootings, which impairs local governments' balance sheets, b) investors perceive a higher probability of crime and

the 8.3% increase as a lower bound.

[5]See Ang et al. (2014) and Schwert (2017) for a breakdown of muni yield spreads into credit, liquidity and tax components. While Schwert (2017) finds that default risk explains about 70% of the variation in yield spreads, Ang et al. (2014) find that liquidity matters the most in driving muni bond spreads.

[6]We verify this in a separate test (table not reported) and confirm that the composition of taxable versus non-taxable bonds as a fraction of total issuance doesn't change for the affected counties.

[7]This assumption is supported by the fact that the municipal bond market is highly segmented owing to state specific tax exemptions, with a concentrated local ownership structure (Babina et al. (2021)) dominated by wealthy (high tax) retail investors.

future mass shootings in the affected county, c) even though mass shootings by themselves have no material impact on investor expectations, they draw attention to the the underlying fundamentals of the borrowing entities, which hitherto had gone unnoticed. We first discuss the latter two channels. If investors expected a future increase in the incidence of mass shootings in the affected counties, one implication would be that longer maturity bonds are more likely to be affected than shorter maturity bonds, since it is the expectations of long-run fundamentals that changes.[8] However, the effects are very similar across bonds with maturities less than or greater than 5 years. We also find that the probability of an additional mass shooting occurring in the same county within 10 years of the initial shooting is statistically insignificant. This weakens the case for the second channel to explain our results. Similarly, if the third channel (attention effect) were at play, an implication should be that the effect on yield spreads shouldn't vary much by the number of victims or media attention, since mass shootings in this channel are all but an attention shock. Instead, we find that the effect varies strongly by media coverage and the number of victims. Almost all shootings in our sample receive at least some prime time coverage on national news, as measured in minutes of coverage and number of news stories on ABC, CBS and NBC. So it is unlikely that increased attention to the county finances is the driving mechanism. An equally relevant criticism of the last two channels is that they cannot explain why the yield spread differentials rise initially and then disappear after two years.

Next we test the "economic impact" channel. Indeed, mass shootings could potentially decrease the credit quality of the issuer located in the county of shooting due to an increase in direct costs such as additional law enforcement, judicial and healthcare expenditures,[9] and indirect costs which include forgone income tax revenues (if business conditions worsen) and foregone property tax revenues (if property values decline) after the shooting. To test whether a "rational" expectation of financial deterioration is at work, we examine the effects of mass shootings on different issuer types; we group local governments into three types based on their taxing authority and revenue sources - the first category includes cities, counties and townships, the second and third categories are school districts and special districts respectively. We find that following a mass shooting, raw yield of school districts and special districts go up by almost similar magnitudes (5.4 bps), whereas

[8] In this case, investors realize that certain future states of the world become more likely in the light of recent events (which is rational, as it is based on new information), but this "kernel of truth" makes those future states more salient, which then leads to inflated probabilities of future states. (Bordalo et al. (2012), Bergquist et al. (2019), Dessaint and Matray (2017))

[9] Aside from the loss of life and the pain these events inflict on a community, mass shootings also have financial costs that can be burdensome for governments, especially small or struggling counties. San Bernardino had already filed for bankruptcy when it had to spend $4 million due to the terrorist attack at the Inland Regional Center. Connecticut gave the city of Newtown $50 million just for the costs of rebuilding Sandy Hook Elementary School. The total costs from the 1999 shooting at Columbine High School also amounted to roughly $50 million.

the bond yields of cities, towns and counties are barely affected. This suggests that investors perceive special district and school district bonds to be particularly riskier post mass shootings. But is this rise in yields accompanied by a commensurate deterioration of issuer fundamentals? Contrary to the increase in bond yields, there is no effect on either revenue growth or expenditure growth in the one to three years after the mass shooting; for either municipal governments (cities, counties & towns) or special districts. There is a 1% decrease in the revenue growth (driven by restrained growth in property tax revenues) for school districts in the immediate one year after the shooting, but no impact on finances in the later years. These results are surprising, and even more so, given the additional evidence that the treatment effect is similar across school districts and special district bonds. Our results are reminiscent of findings in the seminal paper by Kaplanski and Levy (2010), who show that losses associated with the negative stock market reaction following an aviation disaster, are sixty times the actual losses from the disaster. Analogously, they also find that the effect is concentrated in ex-ante riskier securities.

The finding that the yield spread differential weakens substantially after the third year could be consistent with an investor "learning" channel; i.e., investors react rationally to negative cash flow shocks following the shooting and local governments recuperate their costs eventually, and thus with arrival of new information investors update their beliefs, driving the yield spreads differentials to their pre-event levels. But the fact that we find no financial impact, even in the short run, for special districts, while at the same time observing an increase in their yield spreads, is evidence against an explanation based on rational learning. Similarly, in terms of county-level economic conditions, we find no change in house prices, violent crime or earnings after the mass shootings. We do observe an approximately 2% drop in employment and establishments per capita,[10] yet these effects do not translate into any meaningful changes in local government finances. We conclude that the results from the analysis of local government finances and economic conditions point to a mechanism driven by investors' behavioral biases coloring their perception of the true credit risk of issuers. The yield spread changes we observe seem to be too high compared to what they would be if expectations of default risk were rational.

Our results seem most consistent with retail investors overestimating the financial impact of mass shootings, and then eventually updating their beliefs as the memory of the event fades with time. The results obtain only for the sample of trades executed by retail investors, and are much stronger for bonds with low institutional ownership. Interestingly, even though mass shootings with the most media coverage move yield spreads by about 10 bps, we find no effect when we regress bond yield spreads on county-level violent crime rates. This provides support for our main hypothesis -

[10]Our results are consistent with Brodeur and Yousaf (2019), who find similar effects.

that the saliency of the shock plays an important role in driving investor risk perceptions. Even for cash-flow shocks that affect fundamentals equally, the change in an asset's price need not be the same, due to differences in tastes, preferences, behavioral biases or even regulatory constraints of the marginal investor. In the case of the municipal debt market, financial frictions like scarce liquidity & high trading costs and thereby the lack of bond arbitrageurs (Harris and Piwowar (2006); Green et al. (2007b); Green et al. (2007a)), could further explain why it takes about two years for erroneous beliefs to be corrected by market forces.

Given the evidence that investors misperceive the real costs of public mass shootings on local government finances, it is imperative to ask why? First, what risk factors do retail investors care about when choosing their portfolio of securities, and is it plausible that the incidence of mass shooting may belong to that set of relevant factors? Second, why is it that retail investors care specifically about these factors and not others (such as the covariance with consumption growth, which theoretically might be equally or more relevant for deciding asset prices)? Also, if they care about the costs of mass shootings, what could possibly lead to their biased beliefs?

Speaking to the first point, surveys and evidence from individual portfolios suggest that advice from financial advisers, personal experiences and beliefs about rare disasters may have first-order effects on asset prices (Giglio et al. (2021); Choi and Robertson (2020); Bender et al. (2021)), over and above that of the covariance of asset returns with consumption growth (Chinco et al. (2021)). The investor profile for municipal bonds is increasingly concentrated amongst high net worth households in the top 1% of the wealth distribution in the US (Bergstresser and Cohen (2016)), which closely matches the investors surveyed in Bender et al. (2021)[11]. To the extent that muni bond investors perceive a mass shooting as a rare disaster event (rare disasters by definition have an out-sized effect on cash-flows) specific to the county, it is plausible to imagine that they require compensation for any perceived increase in default risk.

This brings us to the second point - Why do investors perceive the costs from mass shootings to be much larger than they actually are? Media coverage renders mass shooting events as highly salient, and the salience of a stimulus can distort agents' decisions (Bordalo et al. (2021)). Garmaise et al. (2020) show that household consumption displays excess sensitivity to salient macro-economic news, even when the news is not real. While it seems plausible to believe that heightened media coverage of mass shootings can affect decisions like the support for gun policy (Luca et al. (2020)), it is not obvious that this should lead to biased perceptions of the affected local governments' debt servicing capacity. This motivates us to explore possible psychological underpinnings that could potentially explain the bias.

[11]They survey a sample of high net worth individuals, with atleast 1 million USD of investable assets

A possible hypothesis could be that investors do not differentiate between the non-pecuniary costs from mass shootings (emotional and health toll on the affected community) and the pecuniary costs (which is not really that much, since balance sheets are barely affected and economic activity doesn't deteriorate much). Such behavior could trace its psychological foundation to the coarse thinking mechanism proposed in Mullainathan et al. (2008). What this means is that the value of the debt owed by the affected community doesn't get eroded in reality, but mass shootings do erode the value of the community as an asset for the equity holders - i.e., the renters & homeowners, by affecting their current and future quality of life.[12] Yet, we see that on average, there is no significant effect on house prices in the county. Even though the stakeholders in the affected community have experienced losses, the local governments' balance sheets are barely affected. Investors should be pricing this reduction in the "personal well-being" of residents, only if the local governments transfer those losses to the debt-holders.

From 1966 to 2020, there have been approximately 188 deadly mass shootings in the U.S. leading to more than 1,316 fatalities and thousands of injuries. We conclude that increase in mass shootings in the US are a source of risk for local governments borrowing in the municipal debt market, due to their extreme saliency. In primary markets, in dollar terms, an additional 5.2 basis points increases the cost of an average issue by about $222,000, as the average issue size and duration are about $39 million and 11 years in our sample. The impact on yield spreads seems to fade away three years after the mass shooting.

2. Related Literature

Our findings contribute to the well established literature in behavioral finance that considers biased expectations as central to explaining asset price fluctuations. Studies in this literature either provide evidence inferred from market price dynamics or from survey data on expectations, or a combination of both the methods. A number of papers establish that expectations have significant explanatory power for asset prices and economic decisions - e.g., predictability of equity market returns (Greenwood and Shleifer (2014)), portfolio allocations (Andonov and Rauh (2021); Giglio et al. (2021)), analyst forecast errors (Bordalo et al. (2018)), firm level investment decisions (Gennaioli et al. (2016)). Nagel and Xu (2022) contend that volatile asset prices are better explained through time-varying subjective expectations of fundamentals growth rather than time-varying risk

[12]Fatal school shootings increase youth antidepressant use (Rossin-Slater et al. (2020)); have long-term negative impacts on the likelihood of high school graduation, college enrollment and graduation, as well as employment and earnings (Cabral et al. (2021)); Ang (2021) finds that exposure to police violence leads to persistent decreases in GPA, increased incidence of emotional disturbance, and lower rates of high school completion and college enrollment.

aversion or time-varying perceptions of risk. Our paper extends this literature to the municipal bond market, focusing on saliency of cash flow news as the driver of biased expectations of expected credit losses. Dougal et al. (2015) show that path of credit spreads since a firm's last loan influences its current borrowing rates, thus showing that anchoring heuristics play an important role in the syndicated loan market. In this paper, we link the documented investor bias to its psychological genesis through the lens of saliency and coarse thinking behavior on part of the (mainly retail) muni bond investors.

We also add to the literature on the economic costs of crime, which had burgeoned since the seminal work of Becker (1968). The impact of crime on local property prices is a well researched topic, studied in papers like Cullen and Levitt (1999) who find that each additional reported crime is associated with a roughly one-person decline in city population, and in Linden and Rockoff (2008), who find that house prices within 0.1 miles of a sex offender fall by 4% on average, upon publication of sex offender registries. While the direct costs have been adequately investigated, little research exists on the capital market consequences of crime. In parallel, recent studies have focused on the role of news media in public perception of crime (Mastrorocco and Minale (2018)), and its real consequences; Philippe and Ouss (2018) find that the influence of TV coverage of crime influences jurors' harshness. In highlighting the "media driven sensationalism" of mass shootings and its effect on the municipal bond market, our work contributes to this literature on the important role played by the media in influencing financial market outcomes.

Finally, we contribute to the nascent but growing literature on municipal finance. Schwert (2017) finds that default risk is the most important driver of yield spreads. Gao et al. (2020) find that newspaper closures lead to higher municipal borrowing costs in the long-run through the government inefficiency channel. Similarly, Butler et al. (2009) finds that higher state corruption is associated with greater credit risk and higher bond yields. Cornaggia et al. (2021) provide county-level evidence on the impact of opioid abuse on U.S. municipalities' tax revenues, law enforcement costs, credit risk, and access to finance. Goldsmith-Pinkham et al. (2021) find that climate risk (measured as total exposure to sea level rise) is priced in the muni bond market, especially as the saliency of the risk increases with more public attention. Their findings bear some resemblance to our study, in the sense that it is not the changes in current asset levels, but the expectations of future damages caused by climate change that drive the increase in yield spreads for at-risk school districts. By providing evidence for the credit risk channel, our paper adds to the literature on cross-sectional determinants of municipal bond yield spread. Our study differs from the earlier papers on credit risk determinants in the muni debt market, as our credit risk determinant is a "perceived" rather than a true default risk factor.

3. Data and Summary Statistics

3.1. Mass Shootings

We compile a list of public mass shootings starting from 1999, using the regularly updated data from the Washington Post. Their final data repository is assembled from three sources- Grant Duwe (Duwe (2020)), Mother Jones database, Washington Post's own research, as well as from FBI Supplementary Homicide Reports (SHR) and the Gun Violence archives. The dataset gives a detailed account of the mass shootings - the location, the number of victims (injured or killed), shooter and victim profiles, and the weapons used. We focus on public mass shootings, which even if they account for a tiny fraction of the country's gun deaths, are uniquely terrifying because they occur without warning in random, everyday, public locations. There is no universally accepted definition of a public mass shooting, and the Washington Post list uses the FBI definition of a mass shooting, i.e., four or more people excluding the perpetrator(s) killed in a shooting incident, usually by a lone shooter. It does not include shootings tied to robberies that went awry, and it does not include domestic shootings that took place exclusively in private homes.

From 1999 to 2019, there were 108 mass shootings (110 mass shootings if a shooting in a city located at the border of two counties is treated as two independent shootings) in 88 counties. **Figure 1** displays the histogram of mass shootings from 1999 to 2019 in the United States. On average, 5.2 mass shootings occur annually and the frequency of the shootings appears to increase over time. The map in **Figure 2** illustrates that the shootings are concentrated in big cities in the East and the West, but take place across the United States.

3.1.1. Identification Strategy

In this section, we check whether mass shootings are predictable using local area characteristics. **Table 1** compares the mean of key county variables for mass shooting counties vs the universe of non-mass shooting counties. We see that counties with a mass shooting differ systematically from counties without a mass shooting, in that they have a higher income per capita, significantly larger population, a lower poverty rate, and greater racial diversity. **Table IA.1** captures the impact of the same variables in a logistic regression which predicts the probability of a mass shooting. We see that the only variables that are significant in predicting the incidence of a mass shooting are the unemployment rate, the proportion of people without a high-school diploma, and the population of the county. The coefficient on the variable *Post Shooting* predicts the probability of a mass shooting in the same county in the next ten years, given that a shooting happened in the present year. The insignificant coefficient on *Post Shooting* is not surprising as mass shootings typically do not occur in the same location, unlike criminal activity, which often targets the same areas.

Intuitively, mass shootings are more likely to occur around urban centres and metropolitan areas. The correlation might purely be a case of a higher number of potential perpetrators in counties with larger populations. Yet, it might also be the case that economic or demographic factors predict the incidence of mass shooting. If the treatment is non-random but differs on characteristics that are observable, then we can use the observable characteristics as a controls to restore randomness. But if unobservable time-varying shocks differentially affect treated vs non-treated samples, it confounds the effect of the treatment. If unobservable shocks (e.g., strong housing market growth) affect the outcome (bond yields) for the treated group in the same direction for each unique shooting event, we would not be able to cleanly estimate the effect of the treatment. This necessitates the need to construct a valid control sample for each treatment event.

To make shooting and non-shooting counties more comparable, we adopt a nearest neighbor matching approach based on the propensity score (PSM). Based on predictive variables identified in Table 1B, for each shooting county, we find up to five matched non-shooting counties that (i) are located outside of the state of the shooting county and (ii) are most closely matched on a set of seven county characteristics prior to treatment, including unemployment, population, income per capita, education, racial diversity, poverty, and inequality. To ensure high quality matches, for a shooting county, we require the propensity scores of matched counties to be less than one standard deviation from that of the shooting county. The idea here is to construct a counterfactual set of counties with similar demographics with an equal ex-ante probability of a mass shooting happening, as that of the treated county. Of 108 shootings from 1999 to 2019, the matching process results in 103 shooting counties with 504 matched non-shooting counties.[13] As our secondary market data from MSRB (Municipal Securities Rule-making Board) is available only from March 1998 to June 2020 and the event window of interest is two years before and after a shooting, we restrict our analysis to the shootings from March 2000 to June 2018, which yields 81 events. Of the 81 mass shootings, six are excluded due to insufficient number of observations in the muni market. We present the results of the matching procedure in **Table 2**. As shown in **Panel A**, our final sample consists of 75 shootings in 65 counties and a control sample of 354 non-shootings in 245 counties.

Following Brodeur and Yousaf (2019), we use the Vanderbilt Television News Archive, to measure media coverage of mass shootings as the number of news stories and total duration of news stories dedicated to the shootings on the national networks ABC, CBS, and NBC. For each mass

[13] Five shootings are dropped due to precise PSM matching (within one-standard deviation from the treated county in propensity scores).

shooting, in a week around the event, we construct whether the shooting was covered in the news, the number of different news stories, and the number of minutes dedicated to the shooting. These mass shootings resulted in 8 fatalities and 17 injuries on average. They received attention from the major networks (ABC/CBS/NBC) with an average of 7 news stories and a duration of 38 minutes. As we can see in **Panels B and C**, shooting counties and the set of matched non-shooting counties are similar along all the key county variables, as suggested by the high p-values of the differences. To reiterate, the idea here with the matching is to construct a counterfactual set of counties with similar demographics with an equal ex-ante probability of a mass shooting occurring, as that of the treated county.

3.2. Municipal Bonds

The offering yield and attributes of each bond are collected from the Mergent Municipal Bond Securities database. The attributes of individual bonds include the state of issuance, issue series, issuance date, type of issue sale (negotiated versus competitive), maturity date, coupon rate, bond size, as well as bond ratings from Moody's, Standard and Poor's, and Fitch. Following Cornaggia et al. (2021), we convert character ratings into numeric ratings with 21 corresponding to the highest credit quality and 1 to the lowest. The Mergent database also provides information about whether the bond is general obligation, insured, and callable. We collect the county location of the municipal issuers from Bloomberg and SDC Platinum. This is done by geo-locating each bond to a county using the first six digits of the bond's CUSIP, which uniquely identifies the issuer. We collect from Bloomberg the 6-digit CUSIPs for all issuers that can be linked to a county. These issuers cover various forms of local governments, such as counties, cities, school districts, and special purpose districts. The County FIPS (Federal Information Processing Standards) code is the matching variable we use to merge the municipal bond data with data from local government finances and other county demographics. We also gather the type of municipal issuers from the Electronic Municipal Market Access (EMMA) system to classify issuers into state and local governments.

Municipal bond transaction-level prices and yields are from the Municipal Securities Rule-making Board (MSRB), from March 1998 to June 2020.[14] The data contains all intraday broker-dealer and customer municipal bond trades for the period March 1998 to June 2020. Each observation includes the bond price, yield, par value traded, and transaction type (e.g., customer purchase from a broker-dealer and interdealer trade). We study municipal bond secondary yields around

[14]Our sample begins only from 1998 since the variable "Transaction type" is only available from March 1998 onwards. The data from 1998-2004 were obtained from the MSRB through a special request. Otherwise, the publicly available version of the data from MSRB begins from 2005.

mass shootings at the monthly level. To convert the MSRB database to a monthly frequency, we take the average secondary yield of all customer buy transactions within each bond-month, weighted by the par value traded (Gao et al. (2020)). Restricting the sample to transactions that are sales to customers helps us eliminate the possibility of bid—ask bounce effects, which can be rather large in the municipal bond market (Downing and Zhang (2004)).

Following Gao et al. (2020), we exclude municipal bonds with fewer than ten transactions in our sample period, a maturity of more than 100 years, a coupon rate greater than 20 percentage points, or a variable coupon rate. To mitigate the effect of outliers, we exclude any transaction that (1) occurs less than a year before a maturity, (2) occurs in the first three months after an issuance, (3) has nonpositive yields or yields greater than 50 percentage points, or (4) has dollar prices less than 50% or greater than 150% of par. Additionally, in the secondary market analyses, for each shooting, we only consider bonds issued before the shooting.

As our primary outcome variable, aside from the raw yield, we use the tax-adjusted spread over an identical coupon synthetic treasury bond to proxy for the financing cost of municipal bonds. Since bonds are issued at different times and the offering yields of bonds change with interest rate and other macroeconomic factors, we cannot directly compare the raw yield of bonds. To get the bond yield spread, we first use the yield of a coupon-equivalent synthetic treasury bond by calculating the present value of its future coupon and principal payments using the U.S. Treasury yield curve from Gürkaynak et al. (2007). This present value calculation gives us the price of a synthetic treasury bond with the same payoff structure as the municipal bond, which is then used to calculate the yield-to-maturity on this synthetic treasury bond. Next, to account for the tax effect, we follow 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. We obtain top income tax rates by state and year from the TAXSIM model provided by the NBER. Precisely, we compute the tax-adjustment factor as follows,

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

where τ_t^{fed} is the top federal income tax rate and $\tau_{s,t}^{state}$ is the top income tax rate in state s in year t. After accounting for this tax adjustment factor, we calculate the municipal bond tax-adjusted yield spread as the difference between raw yield of municipal bond and yield-to-maturity of the synthetic risk-free bond.

$$\textit{Tax-adjusted Yield Spread} = \frac{\textit{Raw Yield}}{1 - \tau_{s,t}} - r_t$$

Panel A of Table 3 presents the summary statistics for key variables in the bond-year-month secondary market sample. The sample includes transactions of the municipal bonds issued in the 65 counties (and their matched control counties) that experienced at least one mass shooting in our sample period. The average bond belonging to a treated county trades for 5.5 days (including inter-dealers trades) in a month, has an average trading volume of $0.62 million each month, and has 10.6 years to maturity. The raw yield (tax-adjusted yield spread) of the average bond is 2.68% (1.58%). The panel also provides summary statistics for the municipal bonds issued by the 245 control counties. The average bond belonging to a control county trades for 5.44 days in a month, has an average monthly trading volume of $0.64 million, and has 10.7 years to maturity. The raw yield (tax-adjusted yield spread) of the average bond is 2.94% (1.92%). Overall, the variable means and standard deviations are very similar for treated and control counties.

Panel B of Table 3 presents similar statistics on the primary market variables. There are 56,561 bonds in the mass shooting counties, which represent 4,224 issues. These bonds have an average bond size of $2.77 million, issue size of $40.33 million[15], and maturity of 10.14 years. In the treated sample 34% of the bonds are insured, 58% are general obligation, in that they are backed by the tax revenue of the issuing municipality, and 47% are callable. Finally, 46% of the bonds are sold through competitive bidding. The raw yield (tax-adjusted yield spread) of the average bond is 3.06% (1.92%). For comparison, there are 179,183 bonds in the non-shooting control counties, which represent 11,751 issuances. These bonds have an average bond size of $2.61 million, issue size of $38.06 million, and maturity of 10.16 years. In the control sample, 38% of these bonds are insured, 55% are general obligation, 48% are callable, and 42% are sold through competitive bidding. The raw yield (tax-adjusted yield spread) of the bond in the control sample is 3.22% (2.10%).

Overall, we find that the characteristics of bonds issued by the shooting counties are similar to those issued by the control counties, indicating that there is no self-selection into the treatment group by bond type.

3.3. Local Government Finances, County Demographic and Crime Data

The data on local government finances comes from the annual and quinquennial (once every 5 years) Census of Governments surveys by the U.S. Census from 1970 to 2012 and public-use files on local government finances from 2013 to 2019. This survey provides local government data on debt and assets as well as revenues and expenditures by governmental function of counties, cities, township

[15] In our sample, the average issue has 15 bonds.

governments, special districts and dependent agencies. The quinquennial census surveys all local government units, whereas only larger municipalities are sampled during the intercensal years. To estimate county-level government revenue and expenditure by categories, we follow Cornaggia et al. (2021) and linearly interpolate values for all cities, counties, townships, school-districts and special districts between 5-year census survey dates, preserving data where intercensal data exists. We then sum all variables for cities and townships located in each county along with the county and obtain a balanced geographic issuer-year panel of all local governments over the 1998-2019 period. **Panel C of Table 3** presents the summary statistics for three types of local governments/ issuers that we consider in our analyses - municipalities (includes counties, cities and townships), school districts and special districts. Our three dependent variables are total revenue growth, total expenditure growth and the total outstanding debt growth.

For information on county demographic variables, we gather per capita income from the Bureau of Economic Analysis (BEA), county-level population from Surveillance, Epidemiology, and End Results (SEER) Program.[16] Unemployment rate, local wages, employment and establishments across industry sectors are obtained from the Bureau of Labor Statistics (BLS). Our estimates of county level racial diversity index are calculated based on the data from the Census Bureau's Annual County Resident Population Estimates by Age, Sex, Race, and Hispanic Origin. We use data from the Federal Housing Finance Agency (FHFA) to measure housing price at the county level. FHFA has created single-family housing price indices by county since 1975. The indices are built by using repeat-sales and refinancings for houses whose mortgages have been purchased or securitized by Fannie Mae or Freddie Mac.

Finally, we refer to the FBI UCR (Uniform Crime Reporting) Program Data on Offenses Known and Clearances By Arrest annual data from 1960 to 2020. This data is a compilation of offenses reported to law enforcement agencies in the US. Due to the vast number of categories of crime committed in the United States, the FBI has limited the type of crimes included in this compilation to those crimes which people are most likely to report to police and those crimes which occur frequently enough to be analyzed across time. Crimes included are criminal homicide, forcible rape, robbery, aggravated assault, burglary, larceny-theft, and motor vehicle theft. We aggregate this data across enforcement agencies in a county to come up with the county level violent and property crime index.

Overall, the county level variables are similar across the treated and the control samples.

[16] Although the BEA has population data, for a sample of cities in Virginia the BEA data provides populations for certain combination of cities. The SEER data in comparison gives the estimate for individual cities. Hence we obtain the final population numbers from SEER data. We use the per capita income of the city combinations as the per capita income of the individual cities.

4. Mass Shootings and Local Government Borrowing Costs

4.1. Main Results

To estimate the municipal bond market reactions to a mass shooting, we compare changes in the treated and control counties' existing bonds in the secondary market, in the two years before and after the mass shooting event. Our baseline specification uses the stacked difference-in-differences approach, as implemented in Gormley and Matsa (2011). For each mass shooting, we construct a cohort of treated and control counties using county-bond-year-month level observations for the two years before and after the shooting. The month of the shooting is treated as the first treated month. We then pool the data across cohorts (i.e., across all mass shootings in the sample) and estimate the average treatment effect. Specifically, we estimate the following panel regression,

$$Y_{c,i,j,t} = \beta \cdot Treatment_{c,i} \cdot Post_{c,t} + \text{Bond Controls} + \text{County Controls} + \gamma_{c,i} + \delta_{c,t} + \epsilon_{c,i,j,t}, \quad (1)$$

where c indexes mass shooting cohorts; each cohort includes the treated county and its control counties (so we have 75 cohorts), i indexes counties, j indexes bonds, and t indexes year-month. $Y_{c,i,j,t}$ is either *Raw yield* or *Tax-adjusted yield spread*. $Treatment_{c,i}$ is a dummy variable that equals one if the county i experiences a mass shooting c. $Post_{c,t}$ is an indicator variable that equals one if year-month t is within two-year after the mass shooting c. *BondControl* is a set of bond control variables comprising time to maturity (TTM), inverse-TTM, the natural logarithm of the bond size, and dummies for general obligation bonds, insured bonds, callable, and whether the bond is sold using competitive underwriting. We also supplement the above bond controls with categorical variables for debt types and fixed effects for credit ratings. *CountyControls* are a set of county variables to control for local economic conditions, including the one-year lagged variables of change in population, the change in employment, the natural logarithms of population and income per capita. $\gamma_{c,i}$ are cohort-by-county fixed effects that remove time-invariant county characteristics within cohort c. $\delta_{c,t}$ are cohort-by-year-month fixed effects, as a non-parametric control for any secular time trends. We allow the county and year-month fixed effects to vary by cohort, because this approach is more conservative than including simple fixed effects. Standard errors are double-clustered by issue and year-month.[17]

Table 4, Panel A presents the results from estimating equation (1). The coefficient β of the interaction term, $Treatment_{c,i} \cdot Post_{c,t}$, identifies the differential impact after the mass

[17] Our results hold with an alternate specification too, when we double cluster SEs by issuer and year-month level.

shootings, on average yields of issuers in the affected counties with respect to issuers in the control counties. In Columns (1) and (2), we estimate the regression equation using the raw average yield as the dependent variable. For the regressions using raw yield, we include the benchmark yield as a control, which is the yield to maturity on a synthetic treasury bond with the same payoff structure as our municipal bond. Column (1) denotes the estimates without county controls, which we add in Column (2). We see an increase of 3.9 basis points (bps) in the raw yield using our full set of controls. The effect is statistically significant at the 1% level. Column (4) presents our baseline specification with the tax-adjusted yield spread as our dependent variable. The after-tax yield spread increases by 6.0 bps, after accounting for bond characteristics as well as any changes in credit ratings and county economic conditions.

We next examine the impact on the primary market. **Panel B** presents the results from estimating equation (1) with two changes (to account for the far fewer issuances relative to the number of secondary market transactions), cohort-by-year-month fixed effects are replaced with cohort-by-year fixed effects, and we also include a *Post* variable to account for common variation across all bonds (in both treated and control counties) that are issued in the same year as the mass shooting event, but are issued after the event. For raw yields, the coefficient on the interaction term in Columns (1) and (2) is very similar to the effect we found for the secondary market outcomes. The baseline result for the primary market is also of the similar magnitude as the secondary market, 5.2 bps, which is statistically significant at the 10% level. Even though the primary market has far fewer observations than the secondary market, and we employ a tight set of fixed effects, we are still able to find a significant effect.

To better understand the yield spread dynamics in the secondary market, we plot the difference in the tax-adjusted yield spread (and raw yield) between treated and control counties at a semi-annual frequency in **Figure 2**. Our event window stretches from 2 years before the shooting to 5 years after. We have four semi-annual periods before the mass shooting event and treat the [-6, 0] period or the 6 months just before the event as the benchmark period to evaluate pre-period and the post event dynamics. We treat the month of the shooting as a treatment month and hence as the first month of the first semi-annual period after the shooting event. Specifically, we estimate the following equation:

$$Y_{c,i,j,t} = \alpha + \sum_{s=-4}^{s=10} \beta_s (treatment_{c,i} \times \lambda_s) + \gamma_{ci} + \delta_{ct} + \epsilon_{c,i,j,t} \tag{2}$$

where c indexes mass shooting cohorts, i indexes counties, j indexes bonds, and t indexes year-month, s indexes the half-year relative to the shooting date and λ_s is a dummy that equals 1 in the given half-year relative to the shooting month. $Y_{c,i,j,t}$ is either *Raw yield* or *Tax-adjusted yield spread*. $Treatment_{c,i}$ is a dummy variable that equals one if the county i experiences mass shooting c. γ_{ci} and δ_{ct} are cohort-county and cohort-year-month fixed effects, respectively.

In **Figure 3**, the solid line plots the difference in tax-adjusted average yield spreads over the 2-year window between treated and control counties. The figure reveals no statistical difference between the two groups before the mass shooting event. The treatment and control groups exhibit parallel trends in terms of raw yields and tax adjusted. The data points in the post shooting window reveal some interesting dynamics. After-tax yield spreads (raw yield) are slow to rise in the first few quarters after the mass shooting, eventually reaching a peak at around two years after the shooting. The effect seems to die down gradually after three years, as indicated in the levels and larger confidence intervals in the figure. A possible explanation for the steady increase in spreads could be due to limited liquidity in the market, which leads to slow incorporation of information into prices. The same limited liquidity may explain why yields remain high even two to three years after the shooting, i.e., due to a lack of a price correction mechanism akin to what one would expect in the equity markets.

We next proceed to investigate the muni bond market reactions over different forward windows in the primary market. **Table IA.2** show that the after-tax yield spreads of newly issued bonds do not increase in the first year after the shooting, but show a much stronger rise in the second year that persists up to the third year, and is statically indistinguishable from zero from the fourth year onwards. Multiple bonds are brought to the market in the same issue, and the issue price is usually decided months in advance to the offering date. This staleness could possibly explain why newly issued bonds do not reflect investor expectations as fast as the already trading bonds in the secondary market. For much of the remaining analyses, we focus on the tax adjusted yield spread in the secondary market as our main dependent variable.

4.2. The Cross-sectional Effect

Many of the bonds in our sample have little to no default risk because their cash flows are backed by a third-party guarantee, or the bonds are in a high credit rating category. We can exploit these bond characteristics to sharpen the identification of our baseline results and provide a tighter link between mass shootings and secondary market yield spreads through the default risk channel. The results are reported in **Panel A of Table 5**. First, we examine the mass shooting effect on yield spreads for bonds in the highest credit rating category versus the other bonds. According to Column (1), for bonds in the highest credit rating category, the effect of mass shootings on tax-adjusted yield spreads is a statistically insignificant 4.3 bps. Column (2) shows that for bonds that are not in the highest credit rating category, the effect is a statistically significant 11.3 bps (t-statistic = 3.15), which is significantly higher than the 6.0 basis point effect reported in the baseline result from Column (4) of Table 4. Next, we examine the differential effect between insured and uninsured bonds. Any negative shock to credit quality is unlikely to have a strong effect on the yield spreads of insured bonds because the cash flows from those bonds are still backed by the insurer in the event of default. Uninsured bonds, by contrast, do not have this third-party protection. We stratify our sample into insured and uninsured bonds and examine the impact of mass shootings on after-tax yield spreads for each of these groups. Column (4) shows that the average effect of a mass shooting on the yield spreads of uninsured bonds is 9.9 bps (t-statistic = 2.91). In contrast, Column (3) shows that the effect on insured bonds is a statistically insignificant 0.3 bps, suggesting that mass shootings have a stronger impact on bonds with higher default risk. The results in the bond cross-section are therefore strongly indicative of a default risk channel at work.

We proceed to examining difference in yield spreads across bonds of different maturities. The maturity classification helps us distinguish between two competing hypotheses, viz., (i) conditional on investors' belief that mass shootings affect credit quality to some extent, do investors believe that mass shootings only contribute to additional default risk because of their immediate credit quality fallout, or (ii) do mass shootings lead investors to update their beliefs about the future probability of increased mass shootings in the county? If the second channel were at work, the implication would be that longer maturity bonds should see a higher increase in yield spreads after the shooting. Columns (5) and (6) of **Table 5** show that the effects are rather similar across bonds with maturity less than 5 years (5.2 bps, t-statistic = 2.69) and bonds with maturity greater than 5 years (6.7 bps, t-statistic = 2.55).[18] These results refute the second hypothesis that investors update their beliefs about the probability of future mass shootings in the county. Additionally, we check, based on the logistic regression, whether the probability of another mass shooting happening in the same county within 10 years of the shooting increases. The evidence suggests that it is very unlikely that investors deem the affected county as particularly prone to mass shootings. Even if their beliefs about the probability of future mass shootings go up, it is more likely that it is homogeneous across the U.S. or at least for counties with a similar population size and demographics.

We next test at the individual bond level, whether the documented increases in tax-adjusted yield spreads and offering yields due to a county's exposure to a mass shooting event vary across different types of capital suppliers who buy municipal bonds at issuance. We follow Cornaggia et al.

[18] We confirm in a triple diff-in-diff specification (table not reported), that mass shootings have no differential impact on bonds by their maturity classification

(2021) and focus on two sets of capital suppliers in the municipal bond market: local commercial banks and institutional investors.

4.2.1. Bank-Qualified Bonds

Bank-qualified bonds can be sold directly to banks and provide significant tax incentives to encourage commercial banks to invest in smaller, less-frequent municipal bond issuers. Due to the tax incentives, commercial banks are willing to pay higher prices or accept lower yields for these bonds (Dagostino (2018)). Therefore, we expect the effect of mass shootings on yield spreads to be less dramatic for bank-qualified bonds. To test this hypothesis, we use the same specification as in equation (1) but separate our sample into two sub-samples containing bank and non-bank qualified bonds. The results of this test appear in Columns (7) and (8) of **Table 5**. We see that the number of observations or secondary market trades for bank qualified bonds are far fewer than non-bank qualified bonds. This reiterates the fact that banks are mostly buy-and-hold investors of municipal bonds. The coefficient on *Treatment * Post* is statistically insignificant for bank qualified bonds whereas the coefficient is a statistically significant 6.2 bps for non-bank qualified bonds, suggesting that banks, who are more sophisticated than retail investors, do not react to the mass shooting.

4.2.2. Institutional Ownership

Next, we use a different classification to ask whether the secondary market impact of a mass shooting varies with whether the investors are predominantly institutional. Because institutional investors diversify across locations and assets, they should be less sensitive to a local idiosyncratic shock like mass shootings. Further, institutions are considered to be less susceptible to behavioral biases than retail investors.[19] Accordingly, we expect yield spreads on bonds primarily held by institutions to be less sensitive to mass shooting events, holding all else constant. We collect data on primary market trading activity from the MSRB municipal transaction database, which contains all transaction records to date for each given bond. We categorize trades specifically flagged as when-issued trades or primary/offering take down trades as well as trades within the first two weeks of the offering date as primary market trades. Because the ultimate investors are the primary focus of this test, we include only client trades and discard inter-dealer trades. trades. Traders generally identify the line between retail and institutional trades at $100,000 (Harris and Piwowar (2006)), and so we use a threshold of $100,000 above which a trade is considered institutional. For each bond, we thus obtain the total amount of purchases (net of any sales) separately by institutional

[19] Barber et al. (2009) suggest that institutional investors are more sophisticated than retail investors.

and retail investors at or around issuance. The fraction of institutional clientele is the ratio of institutional net purchases to the sum of institutional and retail net purchases. With the measure of institutional clientele for each bond, we separate our sample into two sub-samples: high and low institutional trading volume samples. The high (low) institutional trading volume sample consists of bonds whose the fraction of institutional clientele is above (below) 0.5. Columns (9) and (10) of **Table 5** show that the results survive only among the low institutional clientele bonds, consistent with our prediction that bonds with a high institutional clientele are significantly less sensitive to mass shootings.

5. Does an Increase in Credit Risk explain the results?

The results thus far suggest that investors "perceive" a decrease in the credit quality of the local governments in the affected county in the immediate aftermath of a mass shooting. Since the perceived increase in credit risk is limited to the effects of this one event, the effects are similar for both shorter maturity and longer maturity bonds and are driven by retail investors. The next question is whether investors are rationally pricing the credit risk deterioration or whether the worsening credit quality is a perceived one?

We test this by exploiting the fact that the credit risk impact of mass shootings on different types of local governments is likely to be different. If county or city governments end up paying for the cost of mass shootings, we should expect to see changes in revenues and expenditures only for such municipalities, and not for utility special districts who earn revenues from fees for their services. If investors rationally price the credit risk deterioration, we should see very different bond market reactions for different issuer types.

We group issuers into three distinct categories as available in the Census of Government Finances data - Municipalities with a FIPS (Federal Information Processing Standards) Place Code (Cities, Counties and Towns). We then group our sample of municipal bonds into three groups as well, based on the issuing authority. For bonds whose issuer name contains words pertaining to school district, we regard them as school district bonds. For bonds whose issuer had never issued general obligation bonds during the sample period, we regard them as special district bonds. For bonds whose issuer name contains specific words pertaining to its function such as hospital and fire department, we regard them as special district bonds, as well. For the remaining bonds, we regard them as county/municipal/township governments, conditional on the fact that they are general obligation bonds.

5.1. Effect on Bond Yields by Issuer Type

We use a triple difference-in-differences design to estimate the differential effect across issuer types based on the following specification:

$$Y_{c,i,j,t} = \beta_0 \cdot Treatment_{c,i} \cdot Post_{c,t} + \beta_1 \cdot Treatment_{c,i} \cdot Post_{c,t} \cdot Municipality + \beta_2 \cdot Treatment_{c,i} \cdot Post_{c,t} \cdot School + \beta_3 \cdot Treatment_{c,i} \cdot Municipality + \beta_4 \cdot Treatment_{c,i} \cdot School + \beta_5 \cdot Post_{c,t,m} + \beta_6 \cdot Post_{c,t,s} + \beta_7 \cdot Municipality + \beta_7 \cdot School + \text{Bond Controls} + \text{County Controls} + \gamma_{c,i} + \delta_{c,t} + \epsilon_{c,i,j,t}, \tag{3}$$

where the dependent variable $Y_{c,i,j,t}$ is either Raw yield or Tax-adjusted yield spread. $Treatment$ is a dummy variable that equals one if the county experiences a mass shooting. *Post* is an indicator variable that equals one if the year is within two years after the shooting. $Municipality$ is a dummy variable that equals one if the issuer is either city, county, municipal, or township government. *School* is a dummy variable that equals one if the issuer is a school district. Bond controls and the County controls are the same as used in equation (1). We include cohort-county fixed effects as well as cohort-year-month fixed effects. Standard errors are double-clustered at issue and year-month.

The benchmark effect is estimated with respect to special district bonds. We present the results of the triple diff-in-diff regression in **Table 6**. In Column 4, the coefficients on *Municipality* and *School* are +7.1 bps and −9.4 bps, respectively. While it may seem surprising that bonds issued by municipalities have higher credit risk than special district bonds, the positive sign on *municipality* arises because we use a dummy for general obligation bonds. The coefficient on *GO* is about -38 bps. Since a majority of bonds issued by school districts and municipalities are general obligation bonds, this implies that bonds of these two types of issuers are much safer than special district bonds, which are generally revenue bonds.

The coefficients of interest are β_1 and β_2, which identify the differential effect of mass shootings on bonds issued by municipalities and school districts relative to bonds issued by special districts. In Columns (2) and (4), β_1 is -5.0 and -8.5 bps while β_2 is a statistically insignificant -1.1 and -1.6 bps. These results provide some interesting insight about investor perceptions of credit risk across issuer types. Mass shootings seem to have a negative impact on general obligation bond yield spreads issued by counties, cities and towns relative to special district bonds, whereas the bonds issued by school districts and special districts are equally affected. It implies that investors perceive a similar credit quality deterioration across school and special districts. This is surprising since the direct costs from mass shootings are borne by the affected cities and counties.

5.2. Impact on Local Government Finances

We now quantify the direct impact of mass shootings on local government revenues and expenditures to establish whether mass shootings impose significant constraints on local government cash flows. Most states and localities have mandates to balance budgets. Nevertheless, we focus on three variables - revenue growth, expenditure growth and the growth in outstanding debt of the local government. Since the data is survey based and not every local government is surveyed each year, local governments like special districts, which are generally smaller in size, might have many data points missing. **Table 7** reports the impact of mass shootings on municipal finances by issuer. Panels A, B, and C compare municipal governments, school district issuers, and special district issuers, respectively, in treated and control counties, for different event windows. The dependent variable is either Revenue growth, Expenditure growth, or Outstanding debt growth. We look at effects in 1- to 3-year after the mass shooting. County controls include one-year lagged values of Change in Population, Change in Employment, Log(Population), and Log(Income per capita).

Panel A presents the results for municipal governments. As shown in Columns (1), (2) and (3), we find no effect on revenue, expenditure, or debt growth in either the first, second or the third year after the mass shooting. Panel B reports that revenue growth in affected school districts declines by 0.01 in the first year after the mass shooting. Given the mean revenue growth rate of 0.04, this represents a 25% drop in the revenue growth rate. This effect disappears after the first year. Similar to municipalities, we find no effect again on either expenditure growth or the growth in total outstanding debt. We run the same tests for special district finances in Panel C. As for municipalities, we find no effect on either revenue growth, expenditure growth or the growth in total outstanding debt, across any time horizon.

Given that the impact of shooting on yield spreads is not driven by the impact on cash flows as proxied by government revenue, expenditure, or debt growth, the results point to a mechanism driven by investor misperception of the true credit risk of issuers. Even for special districts where bond yield spreads go up by 9 bps post the mass shooting, we do not find any effect on their revenue growth. Even if the bond market reaction were fully rational for school districts (factoring in their revenue growth, which falls by 25% in the first year after the shooting), it is hard to explain the yield spread increase for special districts with a rational story. The yield spread changes that we observe seem too high relative to what they would be if expectations of default risk were rational. The evidence rejects an explanation based on investor learning that could explain the reversal in yield spreads three years after the shooting. Learning suggests that investors react rationally to the negative cash flow shocks following a shooting event with local governments recuperating their losses eventually, and thus with arrival of new information investors update their beliefs, driving yield spreads differentials to their pre-event levels. However, the fact that we find no effect even in the short run for both municipal government and special district finances provides evidence against a rational explanation.

5.3. Impact on Local Economic Conditions

We now examine whether mass shootings have an effect on local economic conditions. Following Brodeur and Yousaf (2019), we use a larger window starting from 6 years before the shooting to 4 years after the shooting to estimate the following regression,

$$Y_{c,i,s,t} = \beta \cdot Treatment_{c,i} \cdot Post_{c,t} + \text{County Controls} + \delta_{ct} + \gamma_{cs} + \epsilon_{c,i,s,t}, \tag{4}$$

where the c denotes the cohort, i denotes the county, s denotes the state. In Panel A of **Table IA.3**, the dependent variable is 100 * the natural logarithm of employments per capita or establishments per capita. In Panel B, the dependent variable is 100 * the natural logarithm of salaries per capita, crimes per capita, or house price index. *Treatment* is a dummy variable that equals one if the county in which the issuer is located experiences a mass shooting. *Post* is an indicator variable that equals one if the year is within four years after the shooting. County controls include one-year lagged values of Change in Population, Change in Employment, Log(Population), and Log(Income per capita). We also include cohort-state fixed effects and cohort-year fixed effects to compare outcomes within the same state and same year in the same cohort.

We do find that employment per capita decreases by 2.15% (t-statistic = 2.55) for treated counties. The drop is driven by a commensurate 2.09% drop in the service sector since the service sector accounts for a major proportion of local employment. We also see that employment in establishments owned by the local governments decreases by 4.8%, which is significant at the 10% level. We obtain similar results for establishments per capita, where treated counties see an average drop of 2.18% (t-statistic = 2.0) compared to control counties. This is driven by a relative decrease of 2.5% in service establishments. On average, there is no change in salaries per capita for treated counties relative to control counties. Also, there are no changes in house prices, violent crime per capita, and property crime per capita. Although we see that the number of establishments and employment go down by 2% following the mass shooting, this doesn't necessarily mean that this should lead to a worsening of the debt servicing capacity of local governments. The revenue streams of school districts are tied to property taxes and thereby house prices within the district, and the revenue stream of special districts are tied to the services they provide. This is not directly related to the closing of a few service establishments unless the issuers' revenue stream is impacted in a material way. The evidence in this section suggests that even though economic activity over an extended time period does see some downward adjustment, none of these changes have a material impact on local government finances as examined in the previous section.

5.4. Impact on Credit Ratings

In this section, we examine the impact of mass shootings on credit ratings of bonds issued by local governments in treated counties. Credit rating agencies rely on municipal issuers' financial statements, which lack standardization and are published with significant delay, to provide ratings using a highly mechanical rating process (Cornaggia et al. (2017)). Our dependent variable *Credit ratings* takes values starting from 22 (=AAA) to 1 (=D). We study the effect on credit ratings on a year by year basis upto five years after the shooting. We exclude unrated bonds from our sample. We present our results in **Table 8**. Interestingly, we only find a significant coefficient in the first year after the shooting. We interpret the coefficient of 0.01 as one in a hundred bonds in the affected county having a one notch downgrade. Since we use credit ratings fixed effects in all our main specifications while estimating the effect on tax-adjusted or raw yields, the treatment effect is over and above any change in yield spreads due to a credit rating change. Still, we re-run our main test focusing only on the sample of non-downgraded bonds. Column (6) presents our results, which actually shows a treatment effect of 7.2 bps, a stronger effect than our earlier baseline result suggesting that the yield spread increases more for bonds that are not downgraded versus bonds that are. Thus, credit ratings cannot be driving the impact on yield spreads due to mass shootings.

5.5. Saliency and Biased Expectations of Default Risk

5.5.1. Media Coverage of Mass Shootings

Could the saliency of mass shootings influence investor perceptions of credit risk of bonds issued by the affected counties? To explore the impact of saliency we collect data on the media coverage of the mass shootings from the Vanderbilt Television News Archive. We perform a manual search for the list of mass shootings. We read the detailed description of each news story pertaining to a city in weeks around the mass shooting to measure the news coverage of the shootings. For each mass shooting, we identify whether the shooting was covered in the news, the number of different news stories that covered the shooting, and the number of minutes dedicated to the shooting. We check the characteristics of a shooting that predicts the amount of coverage on national media - ABC, CBS, and NBC. Intuitively, the number of victims should predict higher media coverage of the mass shooting, and the results in the internet appendix table **Table IA.4** confirm this. The age of the shooter also matters for media coverage with higher coverage of younger shooters, suggesting that school shootings receive more media attention, and are therefore more salient.

We now examine the impact of media coverage of mass shootings on municipal bond market outcomes. We estimate a similar triple diff-in-diff specification as in equation (3), but here we use media coverage as the additional conditioning variable. We proxy media coverage through either the number or the duration of news stories. Columns (1) and (2) of **Table 9** show that both the duration and quantity of news coverage has a significant impact on bond yield spreads. Media coverage subsumes any effect on the *Treatment * Post* variable, suggesting that variation in media coverage is sufficient to explain the variation in our post event outcome variable. A one std. dev increase in the duration, amounting to an increase of 67.63 minutes of coverage, would result in an increase of 6.8 bps in tax-adjusted yield spreads of issuers in affected counties. Similarly, a one std. dev increase in the number of news stories, corresponding to an increase of 7.62 news stories, would result in an increase of 4.52 bps in bond yield spreads for affected issuers.

Finally, we test whether it is media attention or other salient drivers of investor reactions to mass shootings, as proxied by the number of fatalities, that have an impact on yield spreads. We implement the same procedure as above, but we instead use the number of fatalities as our conditioning variable. **Column 3** of Table 9 documents our findings. We find that even though the bond yields increase by 4.5 bps post the mass shootings, the interaction term is insignificant, meaning that mass shootings with high fatalities do not necessarily lead to higher yield spreads. This result differs from our findings in column (1) and (2), where the coefficient on the interaction term with media coverage was statistically significant.

One concern is that media coverage is not exogenous, and could be correlated with the number of fatalities. Ideally we would want to estimate the effect of media coverage on muni bond prices and disentangle this effect from the non-media effects of mass shootings, but we are limited by the number of shootings in our sample to implement an instrumental variable approach, such as that employed by Eisensee and Strömberg (2007). In Columns 4 and 5, we include the interaction terms with news duration and number of news stories along with the number of fatalities, and find that the only significant term is the coefficient on the interaction term *Treatment X Post X media coverage*. The evidence suggests that it is media coverage rather than the number of fatalities that drives increased yield spreads. Overall, media attention has an important role to play in driving the saliency of the mass shooting, and hence the biased credit risk perceptions of retail investors.

5.5.2. The Violent Crime rate

We now provide additional evidence that it is the saliency of the mass shootings that drives the yield spreads of muni bonds. The question we ask is whether the mean violent crime rate (other than mass shootings) is a priced credit risk factor in observed bond yield spreads. Unlike all of the analyses in the previous sections of the paper, we perform a panel data analysis to test the effect of violent crime rate, similar to the analysis of the impact of opioid death rates on county bond yields as in Cornaggia et al. (2021). We gather violent crime data from Kaplan (2019), who compiles the data from FBI's Uniform Crime Reporting Program annual publication, *Crime in the United States* which is a detailed report of offense, arrest, and police employment data. Law Enforcement agencies across the US self report this data, as the UCR data are often considered by the federal government in administering law enforcement grants. We aggregate data across enforcement agencies at the county level. Violent crime is composed of four offenses: murder and non-negligent manslaughter, rape, robbery, and aggravated assault. We then estimate the following regression:

$$Y_{j,i,t} = \alpha + \beta \cdot \textit{Violent Crime per capita}_{i,t} + \text{Bond Controls} + \text{County Controls} + \delta_i + \gamma_t + \epsilon_{j,i,t}, \quad (5)$$

where j denotes the bond, i denotes the county, and t denotes the year-month for secondary market yield and year for primary market yield. The dependent variable is either Raw yield or Tax-adjusted yield spread. Violence per capita is one-year lagged value of the number of violent incidents divided by population at the county level. We also include bond controls, county controls, county fixed effects, and year-month or year fixed effects for the primary and secondary market yields.

Table IA.5 reports the results. We are unable to reject the null hypothesis that the violent crime rate has no impact on the yields and the yield spreads in the primary or the secondary market. Even if we were to assume a significant effect in Column (3), the coefficient of 1.084 suggests that a one standard deviation in violent crime per capita (0.005) raises the tax adjusted yield spread by only 0.55 bps. This effect is much smaller when compared to the effect of mass shootings. A one standard deviation increase in the violent crime rate per capita would put significant costs on the municipal governments in terms of the additional increase in police and judicial expenditures, along with lost property tax revenues because of a decline in house prices. Yet muni bond investors do not seem to price violent crime, as opposed to the exogenous but salient shock of a public mass shooting.

One caveat is that a public mass shooting is a type of violent crime, albeit belonging to the rightmost tail of the violent crimes distribution. Thus, it is inherently more salient. However, the previous results on the impact of high versus low media coverage, especially for the low number of fatalities, mitigate the concern that a mass shooting is inherently more salient. The results in this section provide additional support for the impact of saliency on muni bond yield spreads.

6. Other Potential Explanations

6.1. Risk Aversion

An increase in the default risk component of the yield spread could come from an increase in risk aversion of the marginal investors, rather than from a perceived deterioration in the credit quality of issuers in the affected county. In fact, Wang and Young (2020) show that following terrorist attacks, investors reallocate flows from equity mutual funds to government bond funds, driven by a fear-induced increase in aggregate risk aversion. Similarly, in our case, the increase in yield spreads of the relatively riskier special districts and school district issuers, could be driven by an emotional trauma induced rise in risk-aversion. We test for this channel in the following way; we posit that since an increase in risk aversion should manifest in higher risk prices for all assets that share the same marginal investors, this implies that rising risk aversion would lead to an rise in bond yield spreads of neighboring counties within the state, which are likely to share the same marginal investors as the affected counties. Below, we argue why this assumption is merited. The cross-state variation in tax privilege policies leads to concentrated in-state ownership of local municipal bonds and this results in inefficient risk-sharing and a significant valuation discount of municipal bonds (Babina et al. (2021)). This implies that anymore concentration of ownership on part of retail investors[20] would be even more sub-optimal from a risk-sharing perspective. Whereas a strong preference for local ownership in case of equities is justifiable owing to any informational advantage of local investors, the information channel should be much weaker in the case of municipal bonds. Retail muni investors are buy and hold investors and any information induced trading in this market entails substantial transaction costs.

To test our hypothesis, we follow the same procedure as in our main regression, of creating a propensity score matched sample of control counties for each neighboring county of the affected county. We then estimate equation (1) in the matched sample. **Table IA.6** reports that there is no impact on the raw and tax-adjusted yield spreads of issuers located in the neighboring counties. Thus, it is unlikely that the risk aversion of marginal investors explains the impact of mass shootings on yield spreads. This also rules out the possibility that investors perceive any negative credit

[20] say if investors do exhibit a strong local bias and thus mostly hold the bonds of their resident county issuers.

spillover effects of mass shootings on their neighbouring counties' bond issuers.

6.2. Liquidity

The findings thus far suggest that mass shootings increase municipal bond yields through the default risk channel. However, it is possible that mass shootings lead to a deterioration in municipal bond liquidity, thereby increasing the yields in the secondary market (Amihud and Mendelson (1986)). To test this, we construct the following municipal bond liquidity measures, *Price dispersion* and *Trading volume*. *Price dispersion* is the equal- or value-weighted standard deviation of price changes from all transactions (all except interdealer transactions) in a given month. *Trading volume* is the natural logarithm of the number (or amount) of all transactions (all except interdealer transactions) in a given month. **Table IA.7** reports the results from estimating equation (1) for the dependent variables, *Price dispersion* or *Trading volume*. There are no significant changes in the eight different liquidity measures during the two year period after mass shootings suggesting that the impact on yield spreads is not driven by secondary market illiquidity.

Deterioration in liquidity in the secondary market can increase the issuer's cost of capital in the primary market (Goldstein et al. (2019); Brugler et al. (2020)). To study the effect on the liquidity in the primary market, we construct another price dispersion metric, which is calculated by taking the equal- or value-weighted standard deviation of price changes from customer-to-dealer transactions during the 90-day period following the municipal bond offering date. Consistent with the findings in the secondary market, **Table IA.8** shows that even in the primary market there are no significant changes in liquidity following mass shootings. These findings suggest that illiquidity does not drive the observed increases in yield spreads in the primary and secondary markets.

6.3. Issuance Volume

Finally, we check whether our results could potentially be driven by an excess bond supply effect. The reasoning is that investor capital is slow-moving in the municipal bond market due to tax-segmentation and sticky issuer-underwriter-investor relationships.[21] These capital supply frictions could come into play if issuers in affected counties decide to issue larger amounts of bonds after the mass shooting. So even though investors perceive no additional credit risk from these affected issuers, they may require additional compensation to absorb the excess bond supply. The source of this additional compensation could stem from either the market power of the existing investors

[21] Recent studies have indeed documented the effects of capital supply frictions across investor types in the municipal bond market. Dagostino (2018) finds that the local issuance of municipal bonds is sensitive to regulatory constraints of local banks. Similarly, Adelino et al. (2021) show that higher flows into municipal bond funds lead to more municipal bond issuance and larger issues.

of the affected counties' bonds or alternatively their capital constraints. Regardless of the reason, the implication on affected issuers' bond yields should be similar (in sign). To test whether this channel is at work, we construct a county-year panel by aggregating all issuance amounts by issuers in the county each year. We present our results in **Table IA.9**. We see that on average, mass shootings do not impact the average total issuance of either general obligation or revenue bonds in the affected counties. This rules out excess debt supply as an alternative channel that could have potentially explained our findings.

7. Conclusion

The United States has had more mass shootings than any other country. We find that public mass shootings lead to an increase in local government borrowing costs in the municipal debt market. We use propensity score matching and a tight set of fixed effects to argue that the effect on bond market outcomes is causal, and not driven by local economic conditions. We reject explanations based on an increase in risk aversion, an increase in illiquidity of the affected issues, or an increase in investor beliefs about the future probability of more mass shootings in the affected counties. Bond market reactions to mass shootings seem to be driven by misperceptions about the true credit risk impact of mass shootings. It is the media coverage driven saliency of public mass shootings and investors' coarse thinking bias that ultimately drives the increase in relative bond yield spreads.

We attempt to understand this "misperception" based on the investor behavior literature. It seems plausible that a public mass shooting could be (mis)perceived as a high credit risk event by retail investors and their perceptions could impact trades and thus prices. The source of this bias could arise from the coarse thinking behavior documented in prior literature, where investors confuse the non-pecuniary costs (emotional and mental health costs that are ultimately borne by the residents of the affected local community) of mass shootings with the pecuniary costs that affect the debt service capacity of local governments. Such misperceptions could lead to higher borrowing costs for local governments in affected counties, at least in the short run. The findings in our paper document yet another cost of sensationalism in media, in this case, the financial consequences for local governments in counties that borrow in the municipal debt market.

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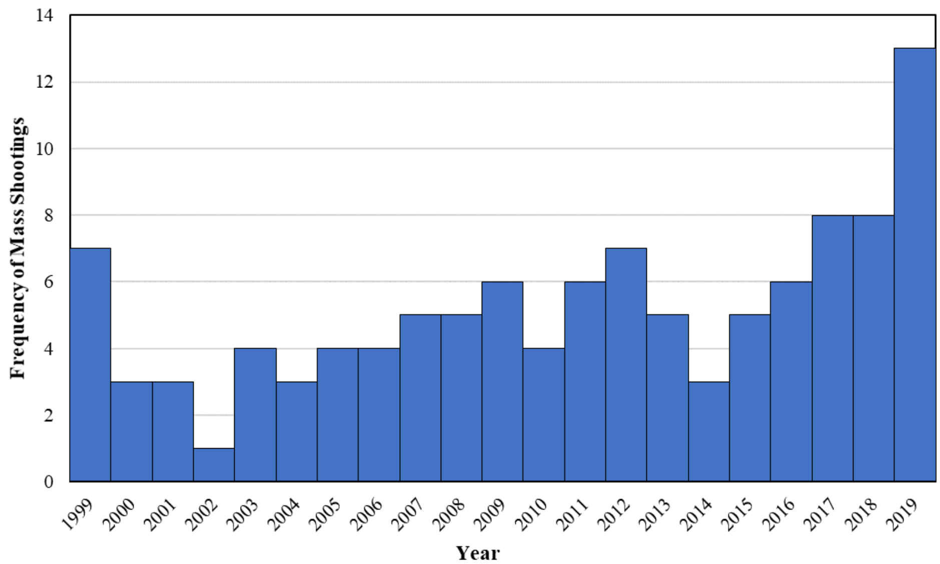
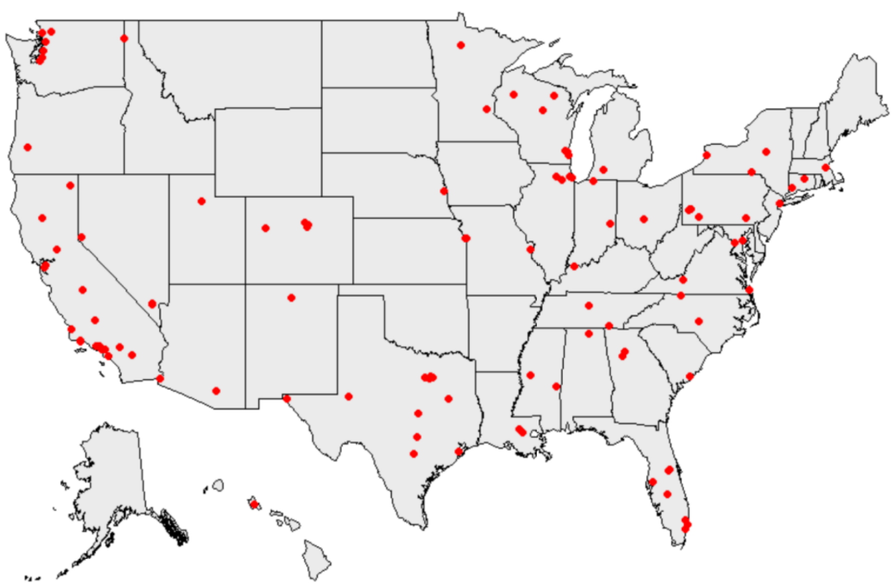
Figure 1: Mass Shootings (1999 – 2019)



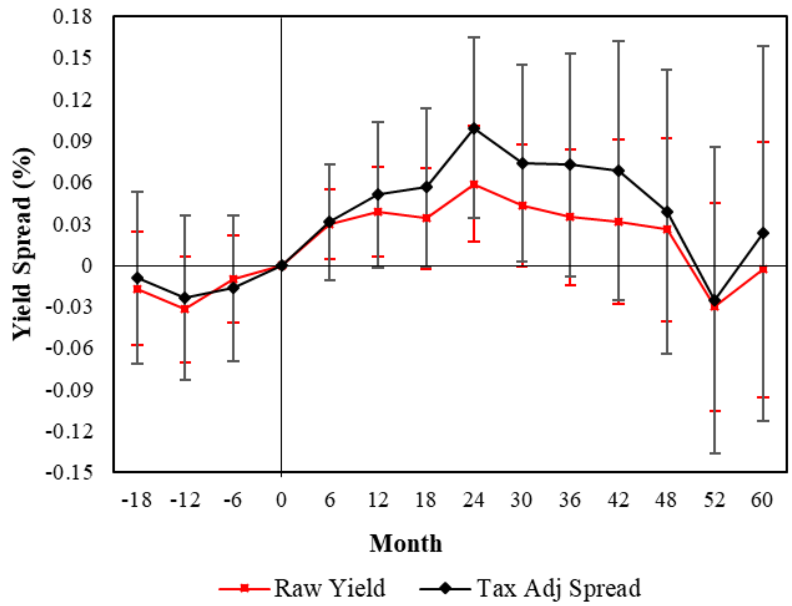
Note: The figure plots the number of mass shootings from 1999 to 2019.

Figure 2: Map of Mass Shooting Counties across the United States



Note: According to the Washington Post, there are 88 counties in which at least one mass shooting occurred during 1999 – 2019. The red dots indicate mass shooting counties.

Figure 3: Dynamic Impact of Mass Shootings



Note: The y-axis represents the difference in yield spread between treated and control counties.

Table 1

Shooting counties versus Non-Shooting counties

This table reports the mean value for key county variables used in analyses in the entire county-year sample, with the test of equality between the shooting and non-shooting counties for each variable. *Number of counties* is the number of county-year observations. *Log(Income per capita)* and *Log(Population)* are the natural logarithms of income per capita and population at the county-level, respectively. *Unemployment*, *Without high school diploma*, and *Poverty* are the proportions of people unemployed, people without high school diploma, and people whose incomes are less than the poverty threshold at the county-level, respectively. *Racial index* and *GINI index* measure county-level ethnic diversity and inequality, respectively. *p<0.10, **p<0.05, ***p<0.01.

	Summary statistics		Test of equality	
Variables	Shooting	Non-Shooting	Diff	p-value
Number of counties	110	65,623	–	–
Log (Income per capita)	10.60	10.34	0.25***	<.0001
Log (Population)	13.03	10.26	2.78***	<.0001
Unemployment (%)	6.12	6.00	0.12	0.65
Without high school diploma (%)	17.17	21.17	-3.99***	<.0001
Racial index	0.34	0.19	0.14***	<.0001
Poverty (%)	13.68	15.01	-1.34**	0.04
GINI index	0.45	0.43	0.02***	<.0001

Table 2

Summary statistics

This table provides summary statistics for the key variables used in analyses. Panel A presents the structure of treatment and control groups (*Number of county-year* and *Number of counties*), and reports the mean value for mass shooting attributes (*Number of shootings, Number of fatalities, Number of injuries*, *Number of news stories*, and *Duration of news stories*). Panel B and C report the mean value for matched county characteristics and other county characteristics, with the test of equalities between the two groups of counties for each variable. *Log(House index)*, *Log(Establishment per capita)*, *Log(Violence per capita)*, and *Log(Property per capita)* are the natural logarithms of house index, establishment per capita, violence per capita, and property crime per capita at the county level, respectively. *p<0.10, **p<0.05, ***p<0.01.

	Summary statistics		Test of equality	
Variables	Treated	Control	Diff	p-value
Panel A: Mass Shootings				
Number of county-year	75	354		
Number of counties	65	245		
Number of shootings	75			
Number of fatalities	8.37			
Number of injuries	17.41			
Number of news stories	6.95			
Duration of news stories (minutes)	37.54			
Panel B: Matched County Characteristics				
Log (Income per capita)	10.59	10.57	0.03	0.47
Log (Population)	12.90	12.87	0.03	0.86
Unemployment (%)	6.35	6.53	-0.18	0.61
Without high school diploma (%)	16.21	16.88	-0.67	0.45
Racial index	0.33	0.33	-0.01	0.75
Poverty (%)	12.98	12.79	0.19	0.76
GINI index	0.45	0.44	0.00	0.45
Panel C: Other County Characteristics				
Log (House Index)	4.90	4.89	0.02	0.49
Log (Establishment per capita)	-3.58	-3.63	0.04	0.22
Log (Violence per capita)	-5.58	-5.64	0.06	0.53
Log (Property per capita)	-3.45	-3.58	0.13	0.23

Table 3

Summary statistics

This table provides summary statistics for the key variables used in analyses. Panel A presents secondary market municipal bond characteristics in the bond-year-month sample. *Raw Yield* is the average secondary yield of all customer buy transactions within each bond-month, weighted by the par value traded. *Tax-adjusted Yield Spread* is the yield after tax adjustments following Schwert (2017). *Benchmark Yield* is the risk-free rate following Gurkaynak, Sack, and Wright (2007). *Price Dispersion* is the standard deviation of price changes from all transactions during the month. *Number of Trading* is the number of transactions in the month and *Trading Volume* is the summation of trading amounts of all transactions in the month. *Time to Maturity* is the years to maturity. *Log(Bond Size)* is the log value of bond size. *General Obligation, Callable, Insured,* and *Competitive* are dummy variables that equal one if the bond is a general obligation bond, callable, insured, or competitive, respectively. Panel B presents primary market municipal bond characteristics in the bond-date sample. *Price Dispersion* is the standard deviation of price changes from all transactions during the 90-day period following the municipal bond offering date. *Maturity* is the year to maturity. *Issue Size* is the size of the issuance. Panel C presents local finance and economy characteristics in the entity-year sample and the county-year sample, respectively. *Revenue Growth, Expenditure Growth, and Outstanding Debt Growth* are the growth rates of total revenue, total expenditure, and total outstanding debt, respectively, for three types of issuers: municipality, school district, and special district. *Log(Employment per capita), Log(Establishment per capita), Log(Salaries per capita), Log(Violence per capita), Log(Property per capita),* and *Log(House Index)* are the natural logarithms of employment per capita, establishment per capita, salaries per capita, violence per capita, and property crime per capita at the county level, respectively.

	All		Treated		Control	
Variables	Mean	SD	Mean	SD	Mean	SD
Panel A: Secondary Market Muni Bond Sample (MSRB)						
Raw Yield (%)	2.88	1.56	2.68	1.54	2.94	1.56
Tax-adjusted Yield Spread (%)	1.84	1.92	1.58	1.76	1.92	1.96
Benchmark Yield (%)	3.00	1.49	2.85	1.50	3.05	1.49
Price Dispersion (Equal)	0.98	0.87	0.91	0.84	1.00	0.88
Price Dispersion (Value)	0.77	0.69	0.72	0.66	0.79	0.70
Number of Trading	5.45	12.41	5.49	10.04	5.44	13.07
Trading Volume ($ Million)	0.64	3.57	0.62	2.81	0.64	3.79
Time to Maturity (Years)	10.68	6.95	10.61	6.87	10.71	6.98
Log (Bond Size)	15.39	1.31	15.50	1.24	15.36	1.33
General Obligation	0.41	0.49	0.40	0.49	0.41	0.49
Callable	0.71	0.46	0.71	0.45	0.71	0.46
Insured	0.53	0.50	0.46	0.50	0.55	0.50
Competitive	0.29	0.45	0.31	0.46	0.29	0.45
Obs (bond × year-month)	1,522,799		367,726		1,155,073	
Panel B: Primary Market Municipal Bond Sample (Mergent)						
Raw Yield (%)	3.18	1.33	3.06	1.37	3.22	1.32
Tax-adjusted Yield Spread (%)	2.05	1.28	1.92	1.17	2.10	1.31
Benchmark Yield (%)	3.32	1.56	3.17	1.63	3.37	1.54
Price Dispersion (Equal)	0.43	1.15	0.42	1.20	0.43	1.13
Price Dispersion (Value)	0.34	0.92	0.32	0.99	0.34	0.89

Maturity (years)	10.14	6.37	10.09	6.32	10.16	6.38
Bond Size ($ Million)	2.65	11.01	2.77	10.76	2.61	11.09
Log (Bond Size)	13.45	1.55	13.57	1.55	13.41	1.54
General Obligation	0.56	0.50	0.58	0.49	0.55	0.50
Callable	0.48	0.50	0.47	0.50	0.48	0.50
Insured	0.37	0.48	0.34	0.47	0.38	0.49
Competitive	0.43	0.49	0.46	0.50	0.42	0.49
Obs (bond × date)	235,744		56,561		179,183	
Issue Size ($ Million)	39.06	133.76	40.33	118.72	38.61	138.78
Number of Issuance	15,975		4,224		11,751	
Panel C: Local Finance and Economy Samples						
Municipal Revenue Growth	0.04	0.10	0.04	0.10	0.03	0.10
Municipal Expenditure Growth	0.04	0.13	0.04	0.13	0.04	0.13
Municipal Outstanding Debt Growth	0.04	0.35	0.04	0.35	0.04	0.35
Obs (entity × year)	38,334		8,122		30,212	
School Revenue Growth	0.04	0.10	0.04	0.10	0.04	0.11
School Expenditure Growth	0.05	0.15	0.05	0.15	0.05	0.14
School Outstanding Debt Growth	0.14	0.93	0.12	0.78	0.15	0.96
Obs (entity × year)	21,732		3,996		17,736	
Special Revenue Growth	0.05	0.23	0.05	0.25	0.05	0.23
Special Expenditure Growth	0.05	0.26	0.05	0.28	0.05	0.26
Special Outstanding Debt Growth	0.02	0.41	0.02	0.42	0.02	0.41
Obs (entity × year)	51,255		12,843		38,412	
Log (Employments per capita)	-0.91	0.32	-0.83	0.33	-0.93	0.32
Log (Establishments per capita)	-3.64	0.27	-3.58	0.29	-3.65	0.27
Log (Salaries per capita)	-1.97	1.25	-2.09	1.30	-1.94	1.24
Log (Violence per capita)	-5.73	1.04	-5.58	0.60	-5.77	1.12
Log (Property per capita)	-3.63	1.06	-3.46	0.42	-3.68	1.16
Log (House Index)	4.87	0.23	4.90	0.22	4.87	0.24
Obs (county × year)	4,360		838		3,522	

Table 4

The effect of mass shootings on municipal bond yields

This table reports the effect of mass shootings on municipal bond yields. Panel A and B compare shooting counties and their matched counties in the secondary and primary markets, respectively, around an equal window of 2 years of the shooting. The dependent variable is either *Raw yield* or *Tax-adjusted yield spread*. *Treatment* is a dummy variable that equals one if the county experiences a mass shooting. *Post* is an indicator variable that equals one if the year is within two-year post the shooting. *Benchmark Yield* is a risk free rate following Gurkaynak, Sack, and Wright (2007). Bond controls include *Time to Maturity* (*TTM*), *Inv-TTM, Log(Bond Size), General Obligation, Callable, Insured, Competitive, Debt type,* and *Credit Ratings*. County controls include one-year lagged values of *Change in Population, Change in Employment, Log(Population),* and *Log(Income per capita).* t-statistics are reported in parentheses and calculated using standard errors double-clustered at issue and year-month. **p*<0.10, ***p*<0.05, ****p*<0.01.

Dependent Variable	Raw Yield		Tax-adjusted Yield Spread	
	(1)	(2)	(3)	(4)
Panel A: Secondary Market				
Treatment × Post	0.037***	0.039***	0.055**	0.060***
	(2.86)	(3.04)	(2.48)	(2.70)
Benchmark Yield	Yes	Yes	–	–
Bond controls	Yes	Yes	Yes	Yes
County controls	–	Yes	–	Yes
Cohort × County FE	Yes	Yes	Yes	Yes
Cohort × Year-Month FE	Yes	Yes	Yes	Yes
Observations	1,522,785	1,522,330	1,522,785	1,522,330
R-squared	0.682	0.682	0.396	0.396
Panel B: Primary Market				
Treatment × Post	0.037**	0.036**	0.052*	0.052*
	(2.28)	(2.22)	(1.88)	(1.89)
Post	0.032	0.032	0.068*	0.067*
	(1.62)	(1.62)	(1.94)	(1.92)
Benchmark Yield	Yes	Yes	–	–
Bond controls	Yes	Yes	Yes	Yes
County controls	–	Yes	–	Yes
Cohort × County FE	Yes	Yes	Yes	Yes
Cohort × Year FE	Yes	Yes	Yes	Yes
Observations	235,743	235,661	235,743	235,661
R-squared	0.879	0.879	0.618	0.618

Table 5

Heterogeneous effects of mass shootings in the Cross-Section

This table reports heterogeneous effects of mass shootings on municipal bond secondary market yields by default risk: credit rating, insurance, and maturity. The dependent variable is *Tax-adjusted yield spread*. *Treatment* is a dummy variable that equals one if the county experiences a mass shooting. *Post* is an indicator variable that equals one if the year is within two-year post the shooting. Bond controls include *Time to Maturity (TTM), Inv-TTM, Log(Bond Size), General Obligation, Callable, Insured, Competitive, Debt type,* and *Credit Ratings.* County controls include one-year lagged values of *Change in Population, Change in Employment, Log(Population), and Log(Income per capita).* Bank Qualified is a dummy variable that equals one if the bond is a bank-qualified bond. Institutional Trading Volume is the ratio of institutional trades (trades in excess of $100,000) to all trades during the first two weeks of issuance. Institutional Trading Volume is High (Low) if the ratio is above (below) 0.5. t-statistics are reported in parentheses and calculated using standard errors double-clustered at issue and year-month. **p*<0.10, ***p*<0.05, ****p*<0.01.

Dependent Variable	Tax-adjusted Yield Spread									
	Default Risk						Capital Supplier			
	Credit Rating		Insurance		Maturities		Bank Qualified		Institutional Trading Vol	
	High	Non-High	Insured	Uninsured	Less 5-Year	More 5-Year	Qualified	Non-Qualified	High	Low
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Treatment × Post	0.043	0.113***	0.003	0.099***	0.052***	0.067**	0.031	0.062***	0.033	0.091***
	(1.61)	(3.15)	(0.09)	(2.91)	(2.69)	(2.55)	(0.39)	(2.75)	(1.41)	(2.70)
Bond controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Cohort × County FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Cohort × Year-Month FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	806,881	715,361	808,916	713,324	369,185	1,153,033	39,956	1,482,085	997,644	524,617
R-squared	0.370	0.411	0.368	0.343	0.427	0.394	0.505	0.398	0.453	0.346

Table 6

Heterogeneous effects of mass shootings by issuer type

This table reports heterogeneous effects of mass shootings on municipal bond secondary market yields by issuer type. The dependent variable is either *Raw yield* or *Tax-adjusted yield spread*. *Treatment* is a dummy variable that equals one if the county experiences a mass shooting. *Post* is an indicator variable that equals one if the year is within two-year post the shooting. *County* is a dummy variable that equals one if the issuer is either county, municipal, or township government. *School* is a dummy variable that equals one if the issuer is a school district. Bond controls include *Time to Maturity (TTM)*, *Inv-TTM*, *Log(Bond Size)*, *General Obligation*, *Callable*, *Insured*, *Competitive*, *Debt type*, and *Credit Ratings*. County controls include one-year lagged values of *Change in Population*, *Change in Employment*, *Log(Population), and Log(Income per capita).* t-statistics are reported in parentheses and calculated using standard errors double-clustered at issue and year-month. **p*<0.10, ***p*<0.05, ****p*<0.01.

Dependent Variable	Raw Yield		Tax-adjusted Yield Spread	
	(1)	(2)	(3)	(4)
Treatment × Post	0.050***	0.054***	0.080**	0.087***
	(2.65)	(2.83)	(2.52)	(2.73)
Treatment × Post × Municipality	-0.045*	-0.050*	-0.078**	-0.085**
	(-1.74)	(-1.89)	(-2.00)	(-2.12)
Treatment × Post × School	-0.008	-0.011	-0.012	-0.016
	(-0.21)	(-0.28)	(-0.18)	(-0.25)
Treatment × Municipality	-0.037	-0.036	-0.043	-0.040
	(-1.20)	(-1.14)	(-0.83)	(-0.78)
Treatment × School	-0.039	-0.037	-0.035	-0.033
	(-1.12)	(-1.08)	(-0.61)	(-0.57)
Post × Municipality	0.026*	0.028*	0.035	0.037
	(1.71)	(1.83)	(1.37)	(1.45)
Post × School	0.018	0.020	0.025	0.027
	(1.07)	(1.15)	(0.84)	(0.91)
Municipality	0.043	0.042	0.072	0.071
	(1.53)	(1.51)	(1.46)	(1.45)
School	-0.048	-0.049	-0.094*	-0.094*
	(-1.59)	(-1.60)	(-1.78)	(-1.79)
GO	-0.231***	-0.231***	-0.382***	-0.382***
	(-8.36)	(-8.37)	(-7.96)	(-7.97)
Benchmark Yield	Yes	Yes	–	–
Bond controls	Yes	Yes	Yes	Yes
County controls	–	Yes	–	Yes
Cohort × County FE	Yes	Yes	Yes	Yes
Cohort × Year-Month FE	Yes	Yes	Yes	Yes
Observations	1,522,785	1,522,330	1,522,785	1,522,330
R-squared	0.682	0.683	0.397	0.397

Table 7

Heterogeneous effect of mass shootings on municipal fundamentals by issuer

This table reports heterogeneous effects of mass shootings on municipal fundamentals by issuer. Panel A, B, and C compare county governments, school district issuers, and special district issuers, respectively, in shooting and their matched counties, for different event window, holding the pre-event window constant at 2 years before the mass shooting. The dependent variable is either *Revenue growth*, *Expenditure growth*, or *Outstanding debt growth*. *Treatment* is a dummy variable that equals one if the county in which the issuer is located experiences a mass shooting. *Post* is an indicator variable that equals one if the year is within 1- to 3-year post the shooting. County controls include one-year lagged values of *Change in Population*, *Change in Employment*, *Log(Population)*, and *Log(Income per capita)*. t-statistics are reported in parentheses and calculated using standard errors clustered at the issuer level. *p<0.10, **p<0.05, ***p<0.01.

Dependent Variable	Revenue Growth			Expenditure Growth			Outstanding Debt Growth		
	[-2,+1]	[-2,+2]	[-2,+3]	[-2,+1]	[-2,+2]	[-2,+3]	[-2,+1]	[-2,+2]	[-2,+3]
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: Municipal Governments									
Treatment × Post	-0.001	-0.000	-0.001	0.000	0.001	-0.001	0.004	0.001	0.001
	(-0.22)	(-0.07)	(-0.45)	(0.05)	(0.32)	(-0.16)	(0.31)	(0.11)	(0.08)
County controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Cohort × County FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Cohort × Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	31,652	38,326	44,534	31,652	38,326	44,534	25,087	30,405	35,379
R-squared	0.098	0.094	0.091	0.084	0.084	0.079	0.076	0.073	0.069
Panel B: School Districts									
Treatment × Post	-0.011**	-0.005	-0.002	-0.001	0.002	0.005	-0.033	-0.011	-0.017
	(-2.48)	(-1.30)	(-0.40)	(-0.16)	(0.38)	(0.85)	(-0.76)	(-0.27)	(-0.47)
County controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Cohort × County FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Cohort × Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	17,691	21,724	25,534	17,691	21,724	25,534	14,886	18,297	21,528
R-squared	0.153	0.143	0.140	0.090	0.081	0.079	0.057	0.052	0.052
Panel C: Special Districts									
Treatment × Post	0.007	0.010	0.008	0.004	0.007	0.008	0.010	0.017	0.021
	(1.32)	(1.64)	(1.44)	(0.70)	(1.02)	(1.27)	(0.74)	(1.29)	(1.62)
County controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Cohort × County FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Cohort × Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	42,172	51,208	59,615	42,172	51,208	59,615	22,647	27,560	32,158
R-squared	0.062	0.062	0.058	0.064	0.063	0.058	0.107	0.106	0.103

Table 8

The dynamic effect of mass shootings on credit rating

This table reports the dynamic effects of mass shootings on municipal bond credit ratings. It compare shooting counties and their matched counties in the secondary markets for different event windows, holding the pre-event window constant at 2 years before the mass shooting. The dependent variable is either *Downgrade* or *Tax-adjusted yield spread*. *Downgrade* is an indicator variable that equals one if the credit rating is downgraded. *Treatment* is a dummy variable that equals one if the county experiences a mass shooting. *Post* is an indicator variable that equals one if the year is within 1- to 5-year post the shooting. In Column (6), *Post* is an indicator variable that equals one if the year is within 2-year post the shooting. *Benchmark yield* is a risk free rate following Gurkaynak, Sack, and Wright (2007). Bond controls include *Time to Maturity (TTM)*, *Inv-TTM*, *Log(Bond Size)*, *General Obligation*, *Callable*, *Insured*, *Competitive*, and *Debt type*. County controls include one-year lagged values of *Change in Population*, *Change in Employment*, *Log(Population)*, and *Log(Income per capita)*. t-statistics are reported in parentheses and calculated using standard errors double-clustered at issue and year-month. **p*<0.10, ***p*<0.05, ****p*<0.01.

Dependent Variable:	Downgrade (= 1 if credit rating is downgraded)					Tax-adjusted Yield Spread
	[-2, +1]	[-2, +2]	[-2, +3]	[-2, +4]	[-2, +5]	Non-Downgrade
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment × Post	0.010*	0.007	0.004	0.002	0.003	0.072***
	(1.92)	(1.39)	(0.74)	(0.46)	(0.47)	(3.09)
Benchmark Yield	Yes	Yes	Yes	Yes	Yes	–
Bond controls	Yes	Yes	Yes	Yes	Yes	Yes
County controls	Yes	Yes	Yes	Yes	Yes	Yes
Cohort × County FE	Yes	Yes	Yes	Yes	Yes	Yes
Cohort × Year-Month FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	910,585	1,176,273	1,392,222	1,573,446	1,716,238	1,226,023
R-squared	0.209	0.201	0.195	0.189	0.185	0.371

Table 9

Heterogeneous effects of mass shootings by salience

This table reports heterogeneous effects of mass shootings on municipal bond secondary market yields by salience. The dependent variable is *Tax-adjusted yield spread*. *Treatment* is a dummy variable that equals one if the county experiences a mass shooting. *Post* is an indicator variable that equals one if the year is within two-year post the shooting. *News Duration* is the duration of news stories in minutes. *News Number* is the number of news stories. *Fatalities* is the number of fatalities. Bond controls include *Time to Maturity (TTM), Inv-TTM, Log(Bond Size), General Obligation, Callable, Insured, Competitive, Debt type,* and *Credit Ratings.* County controls include one-year lagged values of *Change in Population, Change in Employment, Log(Population),* and *Log(Income per capita).* t-statistics are reported in parentheses and calculated using standard errors double-clustered at issue and year-month. *p<0.10, **p<0.05, ***p<0.01.

Dependent Variable	Tax-adjusted Yield Spread				
	(1)	(2)	(3)	(4)	(5)
Treatment × Post	0.018	0.017	0.045*	0.024	0.017
	(0.71)	(0.57)	(1.68)	(0.88)	(0.56)
Treatment × Post × News Duration	0.001***			0.001***	
	(3.46)			(3.37)	
Treatment × Post × News Number		0.006**			0.006**
		(2.21)			(2.04)
Treatment × Post × Fatalities			0.002	-0.001	-0.000
			(0.98)	(-0.53)	(-0.01)
Bond controls	Yes	Yes	Yes	Yes	Yes
County controls	Yes	Yes	Yes	Yes	Yes
Cohort × County FE	Yes	Yes	Yes	Yes	Yes
Cohort × Month FE	Yes	Yes	Yes	Yes	Yes
Cohort × Year-Month FE	Yes	Yes	Yes	Yes	Yes
Observations	1,522,330	1,522,330	1,522,330	1,522,330	1,522,330
R-squared	0.397	0.397	0.397	0.397	0.397

Table IA.1

Logistic regression predicting mass shootings

This table reports results from a logit estimation. The dependent variable is a dummy variable that equals one if the county experiences a mass shooting in the year. The explanatory variables (*Unemployment, Log(Population), Log(Income per capita), Without high school diploma, Racial index, Poverty, GINI index*, and *Post Shooting*) are one-year lagged values to the mass shooting. *Post Shooting* is a dummy variable that equals one if the county experiences a mass shooting for the past 10 years. Standard errors are reported in parentheses. **p*<0.10, ***p*<0.05, ****p*<0.01.

Dependent Variable	Probability of Mass Shooting				
	(1)	(2)	(3)	(4)	(5)
Unemployment	0.150***			0.121**	0.121**
	(0.0496)			(0.0564)	(0.0560)
Log (Population)		1.088***		1.095***	1.072***
		(0.0834)		(0.0848)	(0.0874)
Log (Income per capita)			0.6911	-0.9664	-0.9494
			(0.4845)	(0.7796)	(0.7843)
Without high school diploma	-0.064***	-0.0246	-0.044**	-0.043*	-0.044*
	(0.0189)	(0.0209)	(0.0194)	(0.0227)	(0.0227)
Racial index	5.187***	0.2426	5.036***	0.4917	0.4808
	(0.5751)	(0.7778)	(0.5999)	(0.8027)	(0.8056)
Poverty	-0.117***	0.0292	-0.075**	-0.0231	-0.0203
	(0.0246)	(0.0300)	(0.0295)	(0.0442)	(0.0443)
GINI index	15.369***	1.2056	11.961***	6.9393	6.6655
	(2.4275)	(3.5481)	(3.1587)	(5.2070)	(5.2131)
Post Shooting					(0.3474)
					(0.3050)
Constant	-12.598***	-19.656***	-18.333***	-11.904*	-11.904*
	(1.0418)	(1.3898)	(4.5976)	(6.9694)	(6.9694)
Year FE	Yes	Yes	Yes	Yes	Yes
Observations	65,733	65,733	65,733	65,733	65,733

Table IA.2

Dynamic effects of mass shootings on municipal bond yields

This table reports the dynamic effects of mass shootings on municipal bond yields. It compares shooting counties and their matched counties in the primary markets for different event windows, holding the pre-event window constant at 2 years before the mass shooting. The dependent variable is *Tax-adjusted yield spread*. *Treatment* is a dummy variable that equals one if the county experiences a mass shooting. *Post* is an indicator variable that equals one if the year is within 1- to 5-year post the shooting. Bond controls include *Maturity*, *Inv-Maturity*, *Log(Bond Size)*, *General Obligation*, *Callable*, *Insured*, *Competitive*, *Debt type*, and *Credit Ratings.* County controls include one-year lagged values of *Change in Population*, *Change in Employment*, *Log(Population), and Log(Income per capita).* t-statistics are reported in parentheses and calculated using standard errors double-clustered at issue and year-month. *p<0.10, **p<0.05, ***p<0.01.

Dependent Variable	Tax-adjusted Yield Spread				
	[-2, +1]	[-2, +2]	[-2, +3]	[-2, +4]	[-2, +5]
	(1)	(2)	(3)	(4)	(5)
Treatment × Post	0.008	0.052*	0.047*	0.041	0.032
	(0.26)	(1.89)	(1.79)	(1.59)	(1.29)
Post	0.072**	0.067*	0.067*	0.068*	0.069**
	(2.02)	(1.92)	(1.96)	(1.95)	(1.98)
Bond controls	Yes	Yes	Yes	Yes	Yes
County controls	Yes	Yes	Yes	Yes	Yes
Cohort × County FE	Yes	Yes	Yes	Yes	Yes
Cohort × Year FE	Yes	Yes	Yes	Yes	Yes
Observations	180,503	235,661	285,852	334,426	380,157
R-squared	0.661	0.618	0.623	0.629	0.636

Table IA.3

The effect of mass shootings on local economic outcomes

This table reports the effect of mass shootings on local economic outcomes. Panel A presents the effects of mass shootings on employment and establishment in the event window of (-6, + 4). The dependent variable is 100 X the natural logarithm of either employments per capita or establishments per capita. Panel B presents the effects of mass shootings on salaries, crimes, and house price. The dependent variable is 100 □ the natural logarithm of either salaries per capita, crimes per capita, or house price index. *Treatment* is a dummy variable that equals one if the county in which the issuer is located experiences a mass shooting. *Post* is an indicator variable that equals one if the year is within 4-year post the shooting. County controls include one-year lagged values of *Change in Population*, *Change in Employment*, *Log(Population), and Log(Income per capita)*. t-statistics are reported in parentheses and calculated using standard errors clustered at the county level. *p<0.10, **p<0.05, ***p<0.01.

Panel A: Employment and Establishment

Dependent variable	100 × Log (Y)							
Y	Employments per capita				Establishments per capita			
	Total	Local	Service	Goods	Total	Local	Service	Goods
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treatment × Post	-2.159**	-4.815*	-2.090**	-0.262	-2.181**	-8.983*	-2.496**	-0.772
	(-2.55)	(-1.87)	(-2.38)	(-0.16)	(-2.00)	(-1.77)	(-2.20)	(-0.61)
County controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Cohort × State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Cohort × Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	4,259	3,988	4,259	4,259	4,259	4,203	4,259	4,259
R-squared	0.971	0.894	0.978	0.962	0.960	0.960	0.966	0.949

Panel B: Salary, Crime, and House

Dependent variable	100 × Log (Y)						
Y	Salaries per capita				Crimes per capita		House Price Index
	Total	Local	Service	Goods	Violence	Property	
	(9)	(10)	(11)	(12)	(13)	(14)	(15)
Treatment × Post	0.343	0.667	-0.348	0.231	-5.852	-1.676	1.395
	(0.80)	(1.01)	(-0.46)	(0.53)	(-1.06)	(-0.46)	(0.93)
County controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Cohort × State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Cohort × Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	4,259	3,988	4,259	4,259	4,151	4,189	4,215
R-squared	0.999	0.999	0.998	0.999	0.843	0.833	0.889

Table IA.4

The effect of mass shootings on media coverage

The table reports the effect of mass shootings on media coverage. The dependent variable is either *Log(1+Number of news stories)* or *Log(1+Duration of news stories). Log(Number of victims)* is the natural logarithm of the number of victims comprising fatalities and injuries. *Shooter age* is the age of the perpetrator. *Location* indicates one of locations where the shooting happens: public, workplace, and school. t-statistics are reported in parentheses and calculated using standard errors clustered at the county level. **p*<0.10, ***p*<0.05, ****p*<0.01.

Dependent Variable	Log (1 + Number of news stories)			Log (1 + Duration of news stories)		
	(1)	(2)	(3)	(4)	(5)	(6)
Log (Number of victims)	0.752***	0.769***	0.776***	1.367***	1.390***	1.340***
	(6.33)	(7.54)	(7.87)	(6.90)	(7.19)	(7.96)
Shooter age	-0.015*	-0.008	-0.021**	-0.027**	-0.016	-0.039***
	(-1.95)	(-1.06)	(-2.47)	(2.32)	(1.45)	(-2.94)
Location FE	Yes	Yes	Yes	Yes	Yes	Yes
State FE	Yes	–	Yes	Yes	–	Yes
Year FE	–	Yes	Yes	–	Yes	Yes
Observations	93	107	92	93	107	92
R-squared	0.669	0.635	0.751	0.747	0.684	0.801

Table IA.5

The effect of Violent Crime on municipal bond yields

This table reports the effect of violence on municipal bond yields in the primary and secondary markets. The dependent variable is either *Raw yield* or *Tax-adjusted yield spread*. *Violence per capita* is one-year lagged value of the number of violence divided by population at the county level. *Benchmark Yield* is a risk free rate following Gurkaynak, Sack, and Wright (2007). Bond controls include *Time to Maturity (TTM), Inv-TTM, Log(Bond Size), General Obligation, Callable, Insured, Competitive, Debt type,* and *Credit Ratings.* County controls include one-year lagged values of *Change in Population, Change in Employment, Log(Population), and Log(Income per capita).* t-statistics are reported in parentheses and calculated using standard errors double-clustered at issue and year-month. *p<0.10, **p<0.05, ***p<0.01.

Dependent Variable	Raw Yield		Tax-adjusted Yield Spread	
	Primary	Secondary	Primary	Secondary
	(1)	(2)	(3)	(4)
Violence per capita	0.481	0.675	1.084	0.961
	(0.47)	(0.70)	(0.59)	(0.56)
Benchmark yield control	Yes	Yes	–	–
Bond controls	Yes	Yes	Yes	Yes
County controls	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes
Year FE	Yes	–	Yes	–
Year-Month FE	–	Yes	Yes	Yes
Observations	2,293,739	9,301,180	2,293,739	9,301,180
R-squared	0.804	0.676	0.394	0.361

Table IA.6

The effect of mass shootings on neighboring counties

This table reports the effect of mass shootings on neighboring counties. It compares neighboring counties of shooting counties with their matched counties in the secondary markets around an equal window of 2 years of the shooting. The dependent variable is either *Raw yield* or *Tax-adjusted yield spread*. *Treatment* is a dummy variable that equals one if the county is the neighboring county of shooting counties. *Post* is an indicator variable that equals one if the year is within two-year post the shooting. *Benchmark Yield* is a risk free rate following Gurkaynak, Sack, and Wright (2007). Bond controls include *Time to Maturity (TTM), Inv-TTM, Log(Bond Size), General Obligation, Callable, Insured, Competitive, Debt type,* and *Credit Ratings*. County controls include one-year lagged values of *Change in Population, Change in Employment, Log(Population),* and *Log(Income per capita).* t-statistics are reported in parentheses and calculated using standard errors double-clustered at issue and year-month. *p<0.10, **p<0.05, ***p<0.01.

Dependent Variable	Raw Yield		Tax-adjusted Yield Spread	
	(1)	(2)	(3)	(4)
Treatment × Post	0.007	0.009	-0.005	0.000
	(0.68)	(0.97)	(-0.27)	(0.01)
Benchmark Yield	Yes	Yes	–	–
Bond controls	Yes	Yes	Yes	Yes
County controls	–	Yes	–	Yes
Cohort × County FE	Yes	Yes	Yes	Yes
Cohort × Year-Month FE	Yes	Yes	Yes	Yes
Observations	4,406,712	4,406,712	4,406,712	4,406,712
R-squared	0.637	0.637	0.314	0.314

Table IA.7

The effect of mass shootings on municipal bond secondary market liquidity

This table reports the effect of mass shootings on municipal bond liquidity. It compares shooting counties and their matched counties in the secondary market around an equal window of 2 years of the shooting. The dependent variable is either *Price dispersion* or *Trading volume*. *Treatment* is a dummy variable that equals one if the county experiences a mass shooting. *Post* is an indicator variable that equals one if the year is within two-year post the shooting. *Benchmark Yield* is a risk free rate following Gurkaynak, Sack, and Wright (2007). Bond controls include *Time to Maturity (TTM), Inv-TTM, Log(Bond Size), General Obligation, Callable, Insured, Competitive, Debt type,* and *Credit Ratings.* County controls include one-year lagged values of *Change in Population, Change in Employment, Log(Population),* and *Log(Income per capita).* t-statistics are reported in parentheses and calculated using standard errors double-clustered at issue and year-month. *p<0.10, **p<0.05, ***p<0.01.

Dependent Variable	Price dispersion				Trading volume			
	All Transactions		All except Interdealer		All Transactions		All except Interdealer	
	Equal	Value	Equal	Value	# of trading	$ of trading	# of trading	$ of trading
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treatment × Post	0.002	0.003	-0.000	0.001	0.003	0.007	0.004	0.008
	(0.37)	(0.63)	(-0.01)	(0.18)	(0.63)	(0.68)	(0.99)	(0.85)
Benchmark Yield	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Bond controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Cohort × County FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Cohort × Year-Month FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,522,330	1,522,330	1,522,330	1,522,330	1,522,330	1,522,330	1,522,330	1,522,330
R-squared	0.227	0.245	0.207	0.236	0.259	0.167	0.238	0.160

Table IA.8

The effect of mass shootings on municipal bond primary liquidity

This table reports the effect of mass shootings on municipal bond liquidity. It compares shooting counties and their matched counties in the primary market around an equal window of 2 years of the shooting. The dependent variable is *Price dispersion. Treatment* is a dummy variable that equals one if the county experiences a mass shooting. *Post* is an indicator variable that equals one if the year is within two-year post the shooting. *Benchmark Yield* is a risk free rate following Gurkaynak, Sack, and Wright (2007). Bond controls include *Maturity, Inv-Maturity, Log(Bond Size), General Obligation, Callable, Insured, Competitive, Debt type,* and *Credit Ratings.* County controls include one-year lagged values of *Change in Population, Change in Employment, Log(Population),* and *Log(Income per capita).* t-statistics are reported in parentheses and calculated using standard errors double-clustered at issue and year-month. **p*<0.10, ***p*<0.05, ****p*<0.01.

Dependent Variable	Price dispersion			
	Equal weighted		Value weighted	
	(1)	(2)	(3)	(4)
Treatment × Post	0.040	0.038	0.026	0.027
	(1.63)	(1.65)	(1.46)	(1.51)
Post	0.081***	0.081***	0.059***	0.059***
	(3.52)	(3.54)	(3.40)	(3.39)
Benchmark Yield	Yes	Yes	Yes	Yes
Bond controls	Yes	Yes	Yes	Yes
County controls	–	Yes	–	Yes
Cohort × County FE	Yes	Yes	Yes	Yes
Cohort × Year FE	Yes	Yes	Yes	Yes
Observations	220,256	220,256	220,256	220,256
R-squared	0.058	0.058	0.062	0.062

Table IA.9

The effect of mass shootings on municipal bond issuance

This table reports the effect of mass shootings on municipal bond issuance. It compares shooting counties and their matched counties in the primary market around an equal window of 2 years of the shooting. The dependent variable is *Log(1+Issuance amount)*. *Log(1+Issuance amount)* is the natural logarithm of one plus total issuance amount at the county level. In Columns (2) and (3), the dependent variables measure the total issuance amounts of general obligation bonds and revenue bonds, respectively. *Treatment* is a dummy variable that equals one if the county experiences a mass shooting. *Post* is an indicator variable that equals one if the year is within two-year post the shooting. County controls include one-year lagged values of *Change in Population*, *Change in Employment*, *Log(Population)*, and *Log(Income per capita)*. t-statistics are reported in parentheses and calculated using standard errors clustered at the county level. *p<0.10, **p<0.05, ***p<0.01.

Dependent Variable:	Log (1+Issuance amount)		
	Total	GO	Rev
	(1)	(2)	(3)
Treatment × Post	-0.024	1.290	0.787
	(-0.15)	(1.19)	(0.82)
Post	0.115	-0.327	-0.275
	(0.97)	(-0.54)	(-0.33)
County controls	Yes	Yes	Yes
Cohort × County FE	Yes	Yes	Yes
Cohort × Year FE	Yes	Yes	Yes
Observations	1,358	1,358	1,358
R-squared	0.891	0.763	0.749