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Three Essays in Financial Economics

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Doctor of Philosophy

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An abstract of
A dissertation submitted to the Faculty of the
James T. Laney School of Graduate Studies of Emory University
In partial fulfillment of the requirements for the degree of
Doctor of Philosophy
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Abstract

Three Essays in Financial Economics

By Jinoug Jeung

This dissertation investigates how financial market participants react to socioeconomic changes and how their reactions affect the real economy. In the first essay, I study whether political values shape depositor behavior. Specifically, I examine how depositors respond when they do not agree with their banks' political stances by focusing on divergent political beliefs regarding gun policy, one of the most politically divisive issues in the US. Exploiting a shock drawing public attention to banks' financial relationships with the gun industry, I find that banks that lend to the gun industry experience significant decreases in deposit growth. The effect is stronger in counties with more Democrats or higher support for gun control. Anti-gun depositor movements increase funding costs for gun-lending banks and thus reduce their lending business, which coincides with slower growth in gun establishments. I also find evidence that banks with public anti-gun policies experience reduced deposit growth, specifically in counties with more Republicans or higher support for gun rights. The findings suggest that conflicting political values between banks and depositors lead to depositor movements and pose financial risks to bank operations.

In the second essay, co-authored with Tarun Chordia and Abinash Pati, we document that public mass shootings raise borrowing costs of municipal bond issuers in affected counties. This increase is not driven by any material changes in the issuers' fundamentals or by an increase in illiquidity, risk aversion, or excess supply of debt. In contrast, there is no evidence that the violent crime rate in the county is priced into borrowing costs of municipal bond issuers there. A possible explanation is investors' biased expectations of fundamentals brought about by media-driven salience.

In the third essay, co-authored with Jaemin Lee, we investigate how social connection affects municipal finance. We find that municipal bond mutual funds allocate more capital to counties with stronger social connections, which in turn lowers the municipalities' financing costs in the municipal bond market. Consistent with the familiarity-driven demand channel, the effects are focused on mutual funds with lower institutional resources and opaque bonds facing higher uncertainty. We find no effect for bank-qualified bonds, which mutual funds rarely hold. Fundamental risks, underwriter effects, and large counties with national-level awareness do not drive the results. Overall, we provide a new channel based on social connection that explains the cross-section of municipal bond prices.

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Dedication

To my parents and sister

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Last but not the least, I am immensely indebted to my parents and sister for their sacrifice and support during my Ph.D. journey. Without them, I would not be who I am today.

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Politically Polarized Depositors[*]

Jinoug Jeung[†]

Abstract

Do political values shape depositor behavior? Exploiting a shock drawing public attention to banks' financial relationships with the gun industry, I find that banks that lend to the gun industry experience significant decreases in deposit growth. The effect is stronger in counties with more Democrats or higher support for gun control. Anti-gun depositor movements increase funding costs for gun-lending banks and thus reduce their lending business, which coincides with slower growth in gun establishments. Moreover, I find evidence that banks with public anti-gun policies also experience reduced deposit growth, specifically in counties with more Republicans or higher support for gun rights. The findings suggest that political values shape depositor behavior and pose financial risks to bank operations.

JEL: *G21*, *G41*, *M14*
Keywords: Depositor behavior, Political value, Financial activism, Corporate social responsibility

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1. Introduction

Do political values shape depositor behavior? Answering this question is essential to understanding bank operations in a politically polarized era because deposits finance a large portion of bank assets. Recent studies document the role of political values in consumer behavior and financial markets (e.g., Ke, 2020; Meeuwis et al., 2021; Liaukonyte et al., 2022). Yet, we have a limited understanding of their role in the banking sector, especially in the deposit market. This paper sheds light on this issue by examining how depositors respond when they do not agree with their banks' political stances.

To study this idea, I focus on political beliefs about gun policy, one of the most divisive issues in the US.[1] I identify gun stances of banks based on their financial relationships with the gun industry and those of depositors based on granular socio-political measures. In general, depositors are unaware of banks' asset portfolios (Freixas and Rochet, 2008), making it hard for depositors to identify banks' gun stances. I thus exploit an exogenous shock drawing public attention to the bank's financial relationships with the gun industry, specifically anti-gun financial activism movements following the deadly school shooting at Marjory Stoneman Douglas High School in Parkland, Florida, on February 14, 2018.

In the wake of the shooting, activists urged financial institutions to engage in gun violence prevention by cutting their business ties with the gun industry. On February 26, 2018, ThinkProgress, an American progressive news website, revealed a list of banks financing the gun industry.[2] In early 2019, Guns Down America, a left-of-center advocacy campaign, published a

[1] According to a 2019 Pew Research Center survey, the largest partisan gaps occur with gun policy, followed by racial attitudes and climate/environment. See https://www.pewresearch.org/politics/2019/12/17/in-a-politically-polarized-era-sharp-divides-in-both-partisan-coalitions/.

[2] ThinkProgress. "These are the banks financing the assault weapons industry." See https://archive.thinkprogress.org/banks-financing-guns-c985a46dd4d1/.

widely cited online report card titled "Is Your Bank Loaded?" highlighting financial relationships between the top fifteen US banks and the gun industry.[3] Some banks (e.g., Bank of America, Citibank) responded by implementing anti-gun policies and restricting business with the gun industry. Others (e.g., Wells Fargo) refused to change their business practices.[4] In April 2019, during testimony before the House Committee on Financial Services, Democrats praised bank leaders for their anti-gun policies. In contrast, Republican committee member Sean Duffy (R-Wis) said to Bank of America CEO Brian Moynihan, "There's a lot of Americans who you serve that would greatly disagree with that policy [to stop loaning money to gun makers]. It might play well in the East Coast, it might play well in California, but your bank is not the Bank of New York or California, it's the Bank of America."[5]

Using 2018 anti-gun financial activism movements as a source of exogenous variation in public attention to banks' gun stances, I employ a difference-in-differences approach to examine whether depositors discipline banks that lend to the gun industry ("gun lender"). I use a bank-branch-year deposit growth sample from 2015 through 2019, including granular county-by-year or zip code-by-year fixed effects to compare bank branches operating in similar markets whose holding banks have different gun stances. I find that gun lenders experience 1 percentage point (ppt) decreases in deposit growth, which is a sizable 12.5% relative to the average annual deposit growth of 8%. The estimated economic magnitude is $1.32 billion annual deposit losses per bank, which is 13.2 times larger than the average lending amount of gun lenders to the gun industry. These findings suggest

[3] Guns Down America. "Is Your Bank Loaded?" See https://isyourbankloaded.org/.

[4] The banks' gun stances imposed financial burdens on their business. For example, some states, including Texas and Louisiana, banned Bank of America and Citibank from participating in municipal bond sales. The American Federation of Teachers removed Wells Fargo from its list of approved mortgage lenders.

[5] UPI. "Big banks defend policies on gun manufacturers to Congress" See https://www.upi.com/Top_News/US/2019/04/10/Big-banks-defend-policies-on-gun-manufacturers-to-Congress/4691554916335/.

that anti-gun depositor movements have an economically significant impact on gun lenders' deposits.

According to a 2018 Pew Research Center survey, Republicans are four times more likely than Democrats to say that gun rights are more important than gun control (76% versus 19%).[6] Using cross-sectional variation in bank branch exposure to Democrats, I find that the effect of anti-gun depositor movements is stronger in more Democrat-leaning counties. Specifically, gun lenders experience 3.1 ppt decreases in deposit growth in blue counties while having no differentials from control banks in red counties.[7] In addition, using the Political Action Committee (PAC) donation share to the Republican Party, I find that the effect is stronger for gun lenders that contribute more. These findings suggest that political values affect depositor behavior.

To strengthen the identification and mitigate potential unobserved heterogeneity, I conduct a series of triple-difference-in-differences analyses with the following measures: switching cost, public attitude towards gun control, social movement engagement, and social proximity to Parkland. I find that the effect is more significant in counties with lower switching costs, higher proportions of people supporting gun control, higher proportions of people engaging in social movements, and higher social proximities to Parkland. Most importantly, the effect of political values on depositor behavior remains significant after controlling for these confounding factors.

If anti-gun depositors take disciplinary action by moving their funds away from gun lenders, then a natural question is whether pro-gun depositors similarly discipline banks publicizing anti-gun policies ("anti-gun banks"). I find that anti-gun banks also experience significant decreases in deposit growth, the effect of which is comparable to that of anti-gun depositor movements. In

[6] Pew Research Center. "Gun Policy Remains Divisive, But Several Proposals Still Draw Bipartisan Support" See https://www.pewresearch.org/politics/2018/10/18/gun-policy-remains-divisive-but-several-proposals-still-draw-bipartisan-support/.

[7] Blue (red) is defined as the county with a democrat (republican) share greater than or equal to 70%.

contrast to anti-gun depositor movements, however, the effect of pro-gun depositor movements is stronger in counties with more Republicans or higher support for gun rights. This evidence strengthens the view that conflicting political values between banks and depositors lead to depositor movements.

Given the significant impact of anti-gun depositor movements on gun lenders, I assess their implications for the deposit market and gun industry. I first examine whether gun lenders adjust deposit spreads to attract depositors. I find that gun lenders raise smaller deposit spreads (fed funds rate minus deposit rate) in more Democrat-leaning counties, suggesting that anti-gun depositor movements worsen the market competitiveness of gun lenders, leading them to reduce deposit spreads and thus raise their funding costs. Combined with the loss of deposits, the increased funding costs of gun lenders curtail their lending business. I document that gun lenders decrease the dollar amount of small business loans by 15 percent, and this result is stronger in more Democrat-leaning counties. Then, I examine how this contracted lending business affects the gun industry and find that the number of firearms businesses shrinks more in counties with larger borrowing exposures to gun lenders. The effect is concentrated in Democrat-leaning counties. These findings imply that anti-gun depositor movements lead to a contraction of the gun industry by transferring the increased funding costs of gun lenders to the gun industry.

This paper contributes to different strands of the literature. First, it links political values to depositor behavior. Prior literature on depositor behavior primarily emphasizes banks' fundamentals or depositors' financial interests (Saunders and Wilson, 1996; Martinez Peria and Schmukler, 2001; Maechler and McDill, 2006; Egan et al., 2017; Martin et al., 2018). Yet, few studies evaluate how banks' non-fundamentals and depositors' non-financial interests (e.g., corporate social responsibility, political ideology) affect depositor behavior. This paper adds to the literature by examining how divergent political (i.e., gun policy) beliefs of banks and depositors influence depositor movements. To my best knowledge, this paper is the first to explore the role of political beliefs in the deposit market and the real economic implications.

Second, I contribute to the literature examining the relationship between political values and financial decision making. This literature documents that different political ideologies result in divergent financial decisions among corporations (Hutton et al., 2014; Di Giuli and Kostovetsky, 2014), institutional investors (Hong and Kostovetsky, 2012; Kempf and Tsoutsoura, 2021; Kempf et al., 2022), entrepreneurs (Engelberg et al., 2022), and households (Kaustia and Torstila, 2011; Cookson et al., 2020; Ke, 2020; Meeuwis et al., 2021). This paper relates to the household side. Kaustia and Torstila (2011) and Ke (2020) show that Democrats are less likely to participate in the stock market. Cookson et al. (2020) and Meeuwis et al. (2021) provide evidence of a partisan divide in investor beliefs during the COVID-19 pandemic and 2016 presidential election, respectively. Distinct from these studies focusing on stock market participants, this paper extends the literature to include depositors by documenting divergent depositor movements by political value.

Lastly, this paper fits into the literature focusing on corporate social responsibility. Recent studies show that stakeholders pay attention to corporate social responsibility and try to discipline socially irresponsible firms (Albuquerque et al., 2019; Chen et al., 2019; Naaraayanan et al., 2021; Homanen, 2022). In the banking sector, for example, Chen et al. (2019) document that adverse bank social performance causes significant deposit outflows that lead to a deterioration in bank financial performance. Homanen (2022) shows that depositors disciplined banks financing the 2016 Dakota Access Pipeline project. Different from these studies documenting one-dimensional market discipline on socially irresponsible banks, this paper complement this literature by documenting two-dimensional market disciplines (i.e., anti-gun depositors against gun lenders; pro-gun depositors against anti-gun banks) by political value.

The rest of this paper is organized as follows. Section 2 describes data and variables, and Section 3 presents summary statistics. Section 4 discusses empirical methodologies and results, and Section 5 discusses implications. Section 6 concludes.

2. Data and Variables

2.1 Bank-branch-year deposit growth sample

I collect annual data on deposit holdings from the Federal Deposit Insurance Corporation (FDIC) Summary of Deposits (SOD). The FDIC SOD, the annual survey of branch office deposits as of June 30, provides bank-branch-year-level data on deposit holdings of US branches of all FDIC-insured institutions, including insured US branches of foreign banks. Using the granular SOD bank-branch-year data, I compute the deposit growth for each branch in each year and then construct a bank-branch-year deposit growth sample from 2015 to 2019.[8] For the control variables *Log Bank assets, Log Bank deposits, Bank asset specialization, Bank type,* and *Branch type,* as well as *Scandal,*[9] I use SOD data on financial and business characteristics of banks and branches. Appendix A provides detailed variable definitions.

I restrict the sample to branches with deposits between $100,000 and $1 billion because large branches are funded mainly by large institutions rather than retail depositors, which are the focus of my study, and small branches might exhibit abnormal deposit growth (Homanen, 2022). I also

[8] I use deposit growth as the dependent variable to difference out bank-specific trends (Gilje et al., 2016). My findings are robust to using the log value of deposit holdings as the dependent variable.
[9] I control for the Wells Fargo account fraud scandal with *Scandal*, which is equal to one if the bank is Wells Fargo and the year is 2017. Furthermore, I test the robustness of the findings in a sample excluding Wells Fargo. Table 13 shows that the findings remain statistically and economically significant in the sample excluding Wells Fargo.

exclude acquired, entering, or exiting branches during the sample period to control for the effect of market entries and exits.[10]

2.2 Bank-branch-quarter deposit spread sample

I use RateWatch data to evaluate how banks adjust deposit spreads in response to anti-gun depositor movements. RateWatch collects weekly bank-branch-level deposit rates of multiple products from US depository institutions, including banks and credit unions. The data cover 80% of all US branches of FDIC-insured banks as of 2017 and contain information on whether the branch is an active setter of deposit rates.

I restrict the sample to branches that set their own deposit rates to mitigate duplication of observations (Drechsler et al., 2017). In addition, I focus on four types of certificates of deposits (CDs): 12-month CD with an account size of $10,000 (12MCD10K), which is among the most common deposit products, as well as 12MCD50K, 12MCD100K, and 24MCD10K deposit products.[11] For each deposit product in each branch in each quarter, I compute the deposit spread as the federal funds rate minus the deposit rate. Deposit spread measures the cost of holding deposits. Then, I construct the dependent variable *Δ Spread* as the change in deposit spread over a quarter. The final sample comprises bank-branch-product-quarter deposit spreads from 2017 through 2019 for active branches that offer the four types of CDs.

2.3 County-year firearms business sample

To evaluate the impact of anti-gun depositor movements on the gun industry, I use the federal firearms license data provided by the Bureau of Alcohol, Tobacco, Firearms, and Explosives. The federal firearms license is a legal requirement in the US to engage in a business pertaining to

[10] BB&T and SunTrust completed merger on December 6, 2019. Though my sample ranges from July 2015 to June 2019, I test the robustness of the findings in a sample excluding BB&T. Table 13 shows that the findings remain statistically and economically significant in the sample excluding BB&T.
[11] My findings are robust to using other types of deposit products.

firearms and ammunitions. The data include federal firearms licensees by business activity (e.g., dealer of firearms other than destructive devices, manufacturer of firearms other than destructive devices) and geographic location.

I focus on federal firearms licenses issued for manufacturers and dealers of firearms other than destructive devices. The data present 11,919 manufacturers and 55,659 dealers in 2,076 and 3,037 counties, respectively, in 2017. For each business type, I construct a county-year firearms business sample from 2015 through 2019. Specifically, for each type of business in each county in each year, I compute the number of licensees and construct the dependent variable, *Log # firearms manufacturers* or *Log # firearms dealers*, as the log value of the number of licensees.

2.4 Gun lenders and anti-gun banks

I define gun lenders based on the following three criteria: (1) banks that financed the ten biggest firearms manufacturers in the US at the time of the 2018 Parkland shooting, (2) banks that did not implement anti-gun policies after the shooting, and (3) banks whose gun business received media attention following the shooting.[12] Using DealScan's detailed information on historical loan contracts, I identify 32 banks that financed $3.2 billion in loans and facilities to six major firearms manufacturers at the time of the shooting. Of the 32 banks, four banks implemented anti-gun policies following the shooting. Bank of America, Berkshire Bank, and Fifth Third Bank stopped business with the gun industry, and Capital One restricted transactions pertaining to firearms and ammunitions. Finally, 15 banks out of 28 banks were listed on either ThinkProgress or Guns Down America as banks financing the gun industry. Criterion (3) addresses information asymmetry

[12] According to a 2016 Mother Jones report, the following 10 U.S. firearms manufacturers produced more than 8 million firearms per year for the US market, accounting for more than two-thirds of the total market: Sturm Ruger, Remington Outdoor (formerly Freedom Group), Smith & Wesson, Glock, Sig Sauer, O.F. Mossberg & Sons, Savage Arms (owned by Vista Outdoor), Springfield Armory, Beretta, and Taurus International. In 2014, they produced 95% of all firearms in the US (8.59 million out of 9.05 million). See https://www.motherjones.com/politics/2016/06/fully-loaded-ten-biggest-gun-manufacturers-america/.

between banks and depositors, which hinders depositors from identifying gun stances of banks based on their asset portfolios.[13] Table 1 Panel A lists 15 gun lenders with media attention, which I use in the empirical analysis. Table 1 Panel B lists 13 gun lenders without media attention. Table 1 Panel C lists 7 anti-gun banks whose anti-gun policies were publicized following the shooting, which I use to study pro-gun depositor movements as a backlash against anti-gun depositor movements.

2.5 Political values of depositors

To measure political values of depositors, I collect the 2016 presidential election vote shares for each county from CQ Press. I then construct county-level *Democrat share* as the major percentage of votes for Hillary Clinton. Figure 2 presents a map of county-level *Democrat share* across the US, along with blue (red) counties whose *Democrat* (*Republican*) *share* is greater than or equal to 70%. The blue and red counties are used in sub-sample analyses later.

I construct a more granular zip code-level *Democrat share* to complement the county-level *Democrat share*. Following Meeuwis et al. (2021), I use 2015–2016 individual campaign donation data from the Federal Election Commission, specifically individual donations to Political Action Committees (PACs) associated with the two major parties and with at least $20 million in donations. I first count the number of donors to either party in each zip code. I exclude zip codes with fewer than ten donors to eliminate noise stemming from zip codes with insignificant numbers of donors. Then, for each zip code, I compute *Democrat share*, which equals the number of donors to the Democratic Party divided by the total number of donors.

[13] My findings are robust to the exclusion of criterion (3). However, Table 13 shows that the effect of anti-gun depositor movements is concentrated among gun lenders that receive media attention.

2.6 Political leanings of gun lenders

To measure political leanings of gun lenders, I use the Federal Election Commission's 2015–2016 reported PAC donations to Republican or Democratic politicians by each gun lender. I construct *Rep PAC share*, which equals the amount of donations to Republican politicians divided by the total amount of donations. *Rep PAC share* thus serves as a proxy for gun lenders' political leanings. Gun lenders contributed, on average, $206,210 and $89,296 to Republican and Democratic politicians, respectively. The average *Rep PAC share* is 0.658, suggesting that gun lenders in general lean Republican. Table 12 provides a summary of the results.

2.7 Additional variables

To strengthen the identification and mitigate potential unobserved heterogeneity, I construct several cross-sectional variables that measure (1) switching cost, (2) public attitude towards gun control, (3) social movement engagement, and (4) social proximity to Parkland. First, based on Klemperer (1995) that switching costs make a market less competitive, I use the county-level Herfindahl-Hirschman Index (*HHI*) as a proxy for switching costs. *HHI* is the sum of the squared deposit market shares of all bank branches operating in the county in 2017.

Second, based on Luca et al. (2020) that people who experience mass shootings are more likely to support gun control, I construct *Mass shooting*, which equals one for counties where at least one public mass shooting occurred during 1999–2018. I find 78 such counties in the Washington Post database. Figure 3 plots a map of public mass shooting counties across the US. Furthermore, I construct variables to measure state-level variations in public attitudes towards the National Rifle Association (NRA) and towards a political action committee led by 20 surviving students of the 2018 shooting at Marjory Stoneman Douglas (MSD) high school. *Boycott NRA* and *Never again*

MSD indicate state-level intensities of Google searches for "Boycott NRA" and "Never Again MSD" in 2018. Figure 4 illustrates those state-level variations.

Third, as Campbell (2006) finds that educated people are more likely to engage in social movements, I construct county-level *Education*, which equals the proportion of people with a bachelor's degree or higher. In addition, anecdotal evidence shows that young adults played significant roles in spreading anti-gun movements across the US following the 2018 Parkland shooting.[14] Thus, I construct county-level *Young*, which equals the proportion of people under age 65. The data for *Education* and *Young* are from the U.S. Census Bureau.

Fourth, given that anti-gun movements rapidly spread through social media such as Facebook and Twitter, I use Facebook's Social Connectedness Index (*SCI*) to measure each county's social proximity to Parkland in Broward County, FL.[15,16] *SCI* measures the probability of randomly selected Facebook users being Facebook friends with a Facebook user in Broward County as of 2019. To control for the physical distance effect, I compute each county's distance to Broward County, based on their centroids. Figure 5 illustrates a map of *SCI* with Broward County.

Lastly, to control for local economic conditions, I collect county-level data on population, per capita income, and unemployment rate from the U.S. Bureau of Economic Analysis and the Economic Research Service of U.S. Department of Agriculture.

3. Summary Statistics

[14] NeverAgain.com See https://www.neveragain.com/gun-control/.

[15] For example, "Never Again MSD" gained 35,000 followers on Facebook, and the Twitter hashtag "#NeverAgain" went viral, with tweets generating between 2,000 and 6,000 likes and being retweeted from 300 to 2,000 times over the next three days after the shooting. See https://www.diggitmagazine.com/articles/neveragain-msd-outrage-movement-gun-control/.

[16] Roughly 70% of the adult population in the US use Facebook, and users' demographic characteristics closely resemble those of the overall population (Kuchler et al., 2022).

Table 2 reports summary statistics for the main variables used in the empirical analysis. Panel A presents the numbers of branches and their operating counties for each group of gun lenders and control banks. The control banks, a benchmark to evaluate the impact of anti-gun depositor movements, are defined as banks either not financing the ten biggest firearms manufacturers in the US at the time of the shooting or implementing anti-gun policies after the shooting. They comprise all FDIC-insured banks except the 28 banks that meet both criteria (1) and (2) in Section 2.4.[17] In particular, I restrict attention to branches in counties where gun lenders operate. Control banks have 38,059 branches in 1,730 counties (22 branches per county), and gun lenders have 20,673 branches in 1,783 counties (12 branches per county).

Panel B provides summary statistics for key variables of the bank-branch-year deposit growth sample, described in Section 2.1. Gun lenders are large relative to control banks, with average branch deposit holdings of $91.6 million versus $76.9 million. [18] In addition, gun lenders run more businesses in counties with higher *Democrat share*, higher proportions of people supporting gun control, and higher social proximities to Parkland. These findings suggest that gun lenders do not cater to depositors in terms of gun stances, thus mitigating potential endogeneity issues caused by their business decisions catering to depositors.

Panel C presents summary statistics of the bank-branch-quarter deposit spread sample for 12MCD10K, described in Section 2.2. The average change in deposit spread for the sample period is positive 5 basis points because the federal funds rate increased during the sample period and the deposit rate lagged behind this increase. In addition, consistent with Driscoll and Judson (2013)

[17] My findings are robust to using different sets of control banks in Table 22: matched control banks based on key bank characteristics, control banks excluding anti-gun banks, control banks excluding community banks, control banks excluding both anti-gun banks and community banks.

[18] To mitigate potential endogeneity issues caused by fundamental differences (e.g., size, performance) between gun lenders and control banks, I test the robustness of my findings in a matched sample constructed using a 1-to-3 nearest neighbor matching approach with key bank characteristics (bank assets, capital asset ratio, profitability, number of branches, and political exposure). Tables 18 through 20 show that my findings hold in the matched sample.

that larger banks adjust deposit spread more slowly when the federal fund rate rises, gun lenders, which are relatively large compared to control banks, have larger changes in deposit spread than control banks.

Panel D summarizes two county-year firearms business samples for manufacturers and dealers, described in Section 2.3. The average numbers of firearms manufacturers and dealers are 5.65 and 18.11 per county, respectively. Both are primarily located in Republican-leaning counties, with average *Democrat share* values of 35% and 33%. Gun lenders finance, on average, 26%–27% of total small business loans in these counties.

4. Empirical Methodologies and Results

4.1 Anti-gun depositor movements

Using 2018 anti-gun financial activism movements as a source of exogenous variation in public attention to gun lenders, I examine whether depositors discipline gun lenders. Specifically, I run the following difference-in-differences regression using the bank-branch-year deposit growth sample:

$$Branch\ deposit\ growth_{i,j,c,t} = \beta \times Gun\ lender_i \times Post_t + Control\ Variables + \gamma_{i,j} + \delta_{c,t} + \varepsilon_{i,j,c,t} \tag{1}$$

where $Branch\ deposit\ growth_{i,j,c,t}$ refers to the deposit growth of branch j of bank i in county (or zip code) c in year t. $Gun\ lender_i$ is an indicator equal to one if bank i is the gun lender, as defined in Section 2.4. $Post_t$ is an indicator equal to one if year t is either 2018 or 2019. $Control\ Variables$ is a set of bank- and branch-level control variables, including *Log Bank assets*, *Log Bank deposits, Bank asset specialization, Bank type, Branch type,* and *Scandal*. $\gamma_{i,j}$ are branch fixed effects that remove time-invariant branch characteristics. $\delta_{c,t}$ are county-by-year or zip-by-year fixed effects that capture time-varying local economic conditions that affect local deposit demands. They mitigate the possibility that local deposit demands drive my results. Standard errors are clustered at the branch level.[19]

Table 3 shows that depositors discipline gun lenders. The most stringent specification in column (4) reports that the coefficient estimate on $Gun\ lender_i \times Post_t$ is -0.01 and statistically significant at the 1% level, suggesting that gun lenders experience 1 ppt decreases in deposit growth relative to control banks. The result remains statistically and economically similar when I run different specifications in columns (1) through (3). These reductions account for 12.5% of the average annual deposit growth of 8%.

The estimated economic magnitude of anti-gun depositor movements is sizable. The average branch deposit holdings of gun lenders is \$91.6 million, and a 1 ppt decrease in annual deposit growth is equivalent to \$0.92 million annual losses per branch (\$91.6 million × 1%). The average number of gun lender branches is 1,438, yielding total annual deposit losses of \$1.32 billion per bank (\$0.92 million × 1,438). These losses are 13.2 times larger than the average lending amount of gun lenders to the gun industry of \$100 million.[20] These findings suggest that anti-gun depositor movements have an economically significant impact on gun lenders' deposits.

Figure 1 and columns (5) through (8) show the dynamic impact of anti-gun depositor movements on gun lenders. These results validate the assumption of parallel trends underlying the difference-in-differences approach. I find that gun lenders had no differentials from control banks

[19] My findings are robust to using different clustered standard errors in Table 22: state, county, state-by-bank, county-by-bank, state and year.
[20] As my empirical setting captures depositor movements within the same market, the estimate might double-count the effect by taking the difference between gun lenders and control banks. The conservative interpretation is, therefore, half the effect (i.e., 0.5 ppt decreases and \$660 million annual losses per bank).

prior to the anti-gun financial activism movements, but they lost 1.1 ppt and 1 ppt in deposit growth in 2018 and 2019, respectively.

4.2 Politically polarized movements

4.2.1 Political values of depositors. According to a 2018 Pew Research Center survey, Republicans are four times more likely than Democrats to say that gun rights are more important than gun control (76% versus 19%).[21] Using cross-sectional variation in bank branch exposure to Democrats, I test whether Democrats engage more in anti-gun depositor movements than Republicans. Specifically, by matching the deposit growth sample with the county-level and zip code-level *Democrat share*, as defined in Section 2.5, I run the following triple-difference-in-differences regression:

$$\begin{aligned} Branch\ deposit\ growth_{i,j,c,t} = {} & \beta_1 \times Gun\ lender_i \times Post_t \\ & + \beta_2 \times Gun\ lender_i \times Democrat\ share_{i,j,c} \times Post_t \\ & + \gamma_{i,j} + \delta_{c,t} + \tau_{i,t} + \varepsilon_{i,j,c,z,t} \end{aligned} \qquad (2)$$

where *i* indexes bank, *j* indexes branch, *c* indexes county or zip code, and *t* indexes year. $Democrat\ share_{i,j,c}$ is the proportion of Democrats in county (or zip code) *c*, where branch *j* of bank *i* is located. $\tau_{i,t}$ are bank-by-year fixed effects that control for time-varying bank characteristics and generate within-bank variation in deposit growth. Other variables and fixed effects are the same as those in equation (1). Standard errors are clustered at the branch level.

Table 4 shows that Democrats primarily drive anti-gun depositor movements. In column (1), I find that a one-standard-deviation increase in county-level *Democrat share* (0.18) decreases the deposit growth of gun lenders by 0.7 ppt. The result is more apparent when I run equation (1) using

[21] Pew Research Center. "Gun Policy Remains Divisive, But Several Proposals Still Draw Bipartisan Support" See https://www.pewresearch.org/politics/2018/10/18/gun-policy-remains-divisive-but-several-proposals-still-draw-bipartisan-support/.

subsamples with respect to county-level *Democrat share* in columns (2) through (4). In blue counties whose *Democrat share* is greater than or equal to 70%, gun lenders experience 3.1 ppt decreases in deposit growth relative to control banks. In moderate counties with *Democrat share* between 30% and 70%, gun lenders also see significant 0.7 ppt decreases, but the magnitude is far less than that in blue counties. In red counties with less than or equal to 30% *Democrat share*, gun lenders have no differentials from control banks. The results are robust to using zip code-level *Democrat share* with zip-by-year fixed effects in Table 14. These findings suggest that political values affect depositor behavior.

4.2.2 Political leanings of gun lenders. To complement the above findings, I test whether the effect of anti-gun depositor movements differs with respect to political leanings of gun lenders. I partition gun lenders into two groups based on *Rep PAC share*, as defined in Section 2.6: *High gun lender* and *Low gun lender*. I then run the following regression:

$$\begin{aligned} Branch\ deposit\ growth_{i,j,c,t} = {} & \beta_1 \times High\ gun\ lender_i \times Post_t \\ & +\beta_2 \times Low\ gun\ lender_i \times Post_t \\ & + Control\ Variables + \gamma_{i,j} + \delta_{c,t} + \varepsilon_{i,j,c,t} \end{aligned} \tag{3}$$

where *i* indexes bank, *j* indexes branch, *c* indexes county or zip code, and *t* indexes year. $High\ gun\ lender_i$ ($Low\ gun\ lender_i$) is an indicator equal to one if bank *i* is the gun lender and its *Rep PAC share* is above (below) the median *Rep PAC share* of 0.637. Other variables and fixed effects are the same as those in equation (1). Standard errors are clustered at the branch level.

Table 5 shows that the effect of anti-gun depositor movements is stronger for gun lenders that contribute more to the Republican Party. Column (1) reports that highly Republican-leaning gun lenders experience 2.1 ppt decreases in deposit growth, whereas less Republican-leaning gun lenders experience 0.3 ppt decreases. The difference between them is significant at the 1% level.

Furthermore, subsample analyses with respect to county-level *Democrat share* in columns (2) through (4) show that the effect is strongest for highly Republican-leaning gun lenders in blue counties, where they lose 5.6 ppt in deposit growth compared to 1.2 ppt for less Republican-leaning gun lenders. In moderate counties, the effect is muted for less Republican-leaning gun lenders, but highly Republican-leaning gun lenders still see significant 1.6 ppt decreases. In red counties, the effect is muted for both types of gun lenders. The results are robust to using zip code-level *Democrat share* with zip-by-year fixed effects in Table 15. These findings corroborate the view that conflicting political values between banks and depositors lead to depositor movements.

4.3 Cross-sectional tests

To strengthen the identification and mitigate potential unobserved heterogeneity, I conduct a series of triple-difference-in-differences analyses with the following measures: (1) switching cost, (2) public attitude towards gun control, (3) social movement engagement, and (4) social proximity to Parkland. Specifically, I run the regression of equation (2) but replace *Democrat share* with *HHI*, *Mass shooting*, *Boycott NRA*, *Never Again MSD*, *Education*, *Young*, or *Log SCI*, as defined in Section 2.7. These variables serve as proxies for the measures above.

4.3.1 Switching cost. Kiser (2002) documents that households perceive a switching cost as a significant determinant in shifting between banks. I thus test whether the effect of anti-gun depositor movements is smaller in counties with higher switching costs. I use the county-level Herfindahl-Hirschman Index (*HHI*) as a proxy for switching costs, as described in Section 2.7. A lower *HHI* indicates lower switching costs.

Table 6 provides evidence consistent with Kiser (2002). Column (1) reports that a one-standard-deviation decrease in *HHI* (0.08) reduces the deposit growth of gun lenders by 0.5 ppt. The result is more apparent when I run equation (1) using the tercile subsamples sorted on *HHI* in columns (2) through (4). In counties with low switching costs, gun lenders experience 2 ppt decreases in deposit growth. In counties with moderate switching costs, gun lenders still see significant 0.8 ppt decreases, but the magnitude is less than half that in counties with low switching costs. The effect is muted in counties with high switching costs. These findings indicate that switching costs impede anti-gun depositor movements.

4.3.2 Public attitude towards gun control. Though public stances on gun control are highly politically polarized, other factors independent of political beliefs also contribute. For example, Luca et al. (2020) document that people who experience public mass shootings are more likely to support gun control. I thus test whether the effect of anti-gun depositor movements differs with respect to public attitude towards gun control.

Table 7 shows that the effect is more significant in counties where at least one public mass shooting occurred during 1999–2018, where people are more likely to stand against the NRA, and where people pay more attention to the "Never again MSD" gun control movement. Specifically, column (1) reports that gun lenders lose 1.3 ppt more in mass shooting counties than in other counties. Columns (2) and (3) show that a one-standard-deviation increase in *Boycott NRA* (1.62) or *Never Again MSD* (2.1) is associated with 0.6–0.8 ppt decreases in deposit growth for gun lenders. In particular, when I control for *Democrat share* in column (4), the effects of *Mass shooting* and *Never Again MSD* remain significant. These findings imply that other social factors also affect public stances on gun control.

4.3.3 Social movement engagement. I evaluate whether the effect of anti-gun depositor movements is more pronounced in counties with higher proportions of people engaging in social movements. Campbell (2006) documents that educated people are more likely to engage in social movements. In addition, anecdotal evidence indicates that young adults played significant roles in spreading anti-gun movements across the US following the 2018 Parkland shooting. I thus use *Education* and *Young* to capture cross-sectional variation in county-level social movement engagement.

Table 8 shows that the effect is larger in counties with higher proportions of college degree holders or people under age 65. Specifically, column (1) reports that a one-standard-deviation increase in *Education* (0.11) decreases the deposit growth of gun lenders by 0.5 ppt. In column (2), a one-standard-deviation increase in *Young* (0.04) is associated with 0.4 ppt decreases in deposit growth for gun lenders. These findings suggest that anti-gun depositor reactions are stronger in counties with higher proportions of people engaging in social movements.

4.3.4 Social proximity to Parkland. Following the 2018 Parkland shooting, anti-gun movements rapidly spread through social media. The online group "Never Again MSD" gained 35,000 followers on Facebook over the next three days after the shooting, and the Twitter hashtag "#NeverAgain" went viral. Thus, I test whether the effect of anti-gun depositor movements is larger in counties with higher social proximities to Parkland. I use Social Connectedness Index (*SCI*) data from Facebook to capture cross-sectional variation in county-level social proximity to Parkland.

Columns (3) through (5) in Table 8 show that the effect is more significant in counties with higher social proximities to Parkland. Column (3) reports that a one-standard-deviation increase in *Log SCI* (0.94) is associated with 0.6 ppt decreases in deposit growth for gun lenders. When I control for the effect of physical distance with *Log Phy Distance*, the effect becomes stronger. A one-standard-deviation increase in *Log SCI* (0.94) is associated with 1.7 ppt decreases in deposit growth for gun lenders.[22] These findings suggest that social connection facilitates anti-gun depositor movements.

All of the findings in Sections 4.3.1 through 4.3.4 align with evidence from the extant literature, thus strengthening the identification. Most importantly, when I compare *Democrat share* with these confounding factors, the effect of *Democrat share* on gun lenders remains both statistically and economically significant. These findings confirm that political values affect depositor behavior.

4.4 Pro-gun depositor movements

Previous sections demonstrate that anti-gun depositors take disciplinary action by moving their funds away from gun lenders. To determine whether pro-gun depositors similarly discipline anti-gun banks, I run the regressions of equations (1) and (2) but replace *Gun lender* with *Anti-gun*.

Table 9 provides evidence of pro-gun depositor movements. Column (1) reports that anti-gun banks experience 0.8 ppt decreases in deposit growth, which is comparable to the 1 ppt decreases in anti-gun depositor movements. However, in contrast to anti-gun depositor movements, pro-gun depositor movements are primarily driven by Republicans. Column (2) reports that a one-standard-deviation decrease in *Democrat share* (0.18) reduces the deposit growth of anti-gun banks by 1 ppt. In addition, columns (3) through (5) show that the effect of pro-gun depositor movements is more pronounced in localities with higher support for gun rights. In column (3), anti-gun banks lose 1.8 ppt less in mass shooting counties than in other counties. Similarly, in columns (4) and (5), a one-standard-deviation decrease in *Boycott NRA* (1.62) or *Never Again MSD* (2.1) is associated with 1.1–1.3 ppt decreases in deposit growth for anti-gun banks. The results are robust

[22] The insignificant estimate of *Log Phy Distance* in column (4) and the significant but negative sign of *Log Phy Distance* in column (5) are consistent with Jeung and Lee (2022) and Kuchler et al. (2022). They provide evidence that social proximity dominates physical distance in shaping investors' decisions, and social proximity is one potential driver of the effect of physical distance.

to controlling for the effect of anti-gun depositor movements in Table 16. These findings suggest that pro-gun depositors also discipline anti-gun banks. Furthermore, the evidence of opposite drivers behind those divergent movements strengthens the view that conflicting political values between banks and depositors lead to depositor movements.

5. Implications of Anti-gun Depositor Movements

This section assesses the implications of anti-gun depositor movements for the deposit market and gun industry. Given the significant deposit losses of gun lenders, I first evaluate how gun lenders respond to anti-gun depositor movements. Specifically, in Section 5.1, I test whether gun lenders adjust deposit spread to attract depositors. Then, Section 5.2 examines how the increased funding costs of gun lenders affect the gun industry.

5.1 Deposit market

If anti-gun depositor movements deteriorated the market competitiveness of gun lenders, they would be more likely to decrease deposit spread to attract depositors, particularly in more Democrat-leaning counties. To test this idea, I run the regression of equation (4) using the bank-branch-quarter deposit spread samples described in Section 2.2. In particular, I include *HHI* and its interaction terms to control for the effect of market concentration on deposit spreads (Drechsler et al., 2017). *HHI* is the county-level Herfindahl-Hirschman Index in 2017.

$$\begin{aligned}\Delta\, Spread_{i,j,c,t} = {} & \beta_1 \times Gun\ lender_i \times Post_t \\ & + \beta_2 \times Gun\ lender_i \times Democrat\ share_{i,j,c} \times Post_t \\ & + \beta_3 \times Gun\ lender_i \times HHI_{i,j,c} \times Post_t \\ & + Control\ Variables + \gamma_{i,j} + \delta_{c,t} + +\varepsilon_{i,j,c,t} \qquad (4)\end{aligned}$$

where $\Delta\, Spread_{i,j,c,t}$ refers to the change in deposit spread of branch j of bank i in county c in year-quarter t. Deposit spread is the cost of holding deposits, computed as the federal funds rate minus the deposit rate. $Gun\ lender_i$ is an indicator equal to one if bank *i* is the gun lender. $Post_t$ is an indicator equal to one if year *t* is either 2018 or 2019. $Democrat\ share_{i,j,c}$ is the proportion of Democrats in county *c*, where branch *j* of bank *i* is located. $HHI_{i,j,c}$ is the Herfindahl-Hirschman index in county *c*, where branch j of bank i is located. $Control\ Variables$ is a set of bank- and branch-level control variables, including *Log Bank assets, Log Bank deposits, Bank asset specialization*, *Bank type, Branch type,* and *Scandal*. $\gamma_{i,j}$ are branch fixed effects that remove time-invariant branch characteristics. $\delta_{c,t}$ are county-by-quarter fixed effects that capture time-varying local economic conditions that affect local deposit demands, thus mitigating the possibility for local deposit demands driving my results. [23] Standard errors are clustered at the branch level.

Table 10 shows that gun lenders raise lower deposit spread in more Democrat-leaning counties.[24] In column (1), for 12-month certificates of deposit with an account size of $10,000 (12MCD10K), a one-standard-deviation increase in *Democrat share* (0.18) decreases 1 basis point in *Δ Spread* for gun lenders, which is a sizable 20% relative to the average deposit spread change of 5 basis points. The result remains statistically and economically similar for other deposit products in columns (2) through (4). Overall, the declines in *Δ Spread* with the increasing extent of anti-gun depositor movements indicate that anti-gun depositor movements worsen the market competitiveness of gun lenders and thus lead them to cut deposit spreads in favor of depositors. Furthermore, these findings suggest that anti-gun depositor movements impose additional costs on gun lenders beyond their deposit losses.

[23] As most banks have a few number of active branches that set deposit rates (average 1.3 active branch per bank in the sample), the power of statistical tests is largely sacrificed with bank-by-quarter fixed effects. The results with bank-by-quarter fixed effects are statistically insignificant (t-statistics range from -0.6 to -1.1), but the direction and magnitude of coefficients are consistent with the findings in Table 10.

[24] On average, *Δ Spread* of gun lenders was higher than that of control banks during the sample period, which is consistent with Driscoll and Judson (2013) that larger banks adjust deposit spreads more slowly when the federal funds rate rises. During the sample period from 2017 to 2019, the federal funds rate steadily increased, and gun lenders are large relative to control banks, as described in Section 3.

5.2 Gun industry

If the sluggish deposit growth and decreased deposit spreads of gun lenders elevated their funding costs, especially in Democrat-leaning counties, the gun industry would face higher financing costs that would disrupt its business (financial constraint channel).[25] To test this idea, I first construct *Gun lender loan share* that captures cross-sectional variation in county-level borrowing exposure to gun lenders. Specifically, using data on small business lending from the Community Reinvestment Act, I compute the share for each county, which equals the amount of small business loans made by gun lenders divided by the total amount of small business loans in 2017. I then run the regression of equation (5) using the county-year firearms business samples described in Section 2.3. In particular, I include *Democrat share* and its interaction terms with time dimension (i.e., × year) to control for the effect of local political factors.

$$\begin{aligned} Log\ \#\ firearms\ business_{c,t} = & \sum_{s\neq 2017} \beta_s \times Gun\ lender\ loan\ share_c \times 1_{t=s} \\ & + \sum_{u\neq 2017} \beta_u \times Democrat\ share_c \times 1_{t=u} \\ & + Control\ Variables + \gamma_c + \delta_t + \varepsilon_{c,t} \qquad (5) \end{aligned}$$

where $Log\ \#\ firearms\ business_{c,t}$ refers to the log value of the number of firearms manufacturers or dealers in county c in year *t*. $Gun\ lender\ loan\ share_c$ is the share of small business loans made by gun lenders in county *c*. $Democrat\ share_c$ is the proportion of Democrats in county *c*. $Control\ Variables$ is a set of county-level control variables, including *Log Population, Log Per capita income*, *Change in population*, and *Unemployment rate*. γ_c are county

[25] Table 17 shows that the dollar amount of small business loans made by gun lenders decreases by 15 percent, and the effect is stronger in more Democrat-leaning counties. A one-standard-deviation increase in *Democrat share* (0.18) decreases gun lenders' small business loans by 13 percent.

fixed effects that remove time-invariant county characteristics. δ_t are year fixed effects that control for time-varying macro conditions. Standard errors are clustered at the county level.

Table 11 shows that anti-gun depositor movements disrupt the gun industry through the financial constraint channel. The full-sample analysis for firearms manufacturers in column (1) presents an insignificant effect on the manufacture business. However, the effect manifests when I partition counties into two groups based on *Democrat share* in columns (2) and (3). This partition studies the heterogeneous effect by the extent of deposit losses of gun lenders. Specifically, in counties with *Democrat share* greater than or equal to 50%, column (2) reports that, relative to 2017, a one-standard-deviation increase in *Gun lender loan share* (0.16) reduces the number of firearms manufacturers by 3 percent in 2018. In contrast, the effect is muted in counties with *Democrat share* less than 50%, as shown in column (3). To mitigate concerns that local political factors drive the results (e.g., local government law enforcement), I include county-level *Democrat share* and its interaction terms with time dimension in columns (4) through (6). The results still remain statistically and economically similar. These findings also hold for firearms dealers in Table 21, implying that anti-gun depositor movements contract the gun industry by transferring the increased funding costs of gun lenders to the gun industry.

6. Conclusion

A growing literature explores the role of political values in the financial market. This paper extends the literature to the deposit market by investigating how depositors respond when they do not agree with their banks' political stances. Focusing on political beliefs about gun policy, one of the most divisive issues in the US, I find that Democrats discipline gun lenders by moving their funds away from these banks, and Republicans similarly discipline anti-gun banks. As a result of anti-gun depositor movements, the increased funding costs of gun lenders reduce their lending business. These costs are then transferred to the gun industry, leading to a contraction of its business.

This paper concludes that conflicting political values between banks and depositors lead to depositor movements and pose financial risks to bank operations. The evidence hints at the potential risk of segmentation in the deposit and lending markets. An interesting direction for future research would be to explore how the banking sector can be segmented by political ideology of banks and depositors and the implications for bank operations and, more broadly, industry formation through politically polarized lending channels. This area is worth exploring in depth in such a politically polarized era.

Appendix: Variable Definitions

A.1 Bank and Branch Variables

- *Gun lender* – indicator equal to one if the bank is the gun lender, as defined in Section 2.4
- *Anti-gun* – indicator equal to one if the bank is anti-gun, as defined in Section 2.4
- *Post* – indicator equal to one if the year is either 2018 or 2019
- *Log Bank assets* – log value of bank assets in $ thousands [Source: FDIC SDI]
- *Log Bank deposits* – log value of bank deposits in $ thousands [Source: FDIC SDI]
- *Bank asset specialization* – categorical variable for primary asset specialization (e.g., commercial lending, mortgage lending) [Source: FDIC SDI]
- *Bank type* – categorical variable for institution type (e.g., national member, state member) [Source: FDIC SDI]
- *Branch deposit* – amount of branch deposits in $ millions [Source: FDIC SDI]
- *Branch deposit growth* – ($\textit{Branch deposit}_t$ - $\textit{Branch deposit}_{t-1}$) / $\textit{Branch deposit}_{t-1}$, winsorized at the 1st and 99th percentiles
- *Branch type* – categorical variable for branch service type (e.g., brick and mortar office, retail office) [Source: FDIC SDI]
- *Scandal* – indicator equal to one if the bank is Wells Fargo and the year is 2017
- *Rep PAC share* – the share of the PAC donation to the Republican Party [Source: Federal Election Commission]
- *Deposit spread* – *Fed funds target rate* - *Deposit rate* [Source: FRED & RateWatch]
- Δ *Spread* – $\textit{Deposit spread}_t$ - $\textit{Deposit spread}_{t-1}$
- *Gun lender loan share* – amount of small business loans made by gun lenders divided by the total amount of small business loans at the county level in 2017 [Source: Community Reinvestment Act]

A.2 Demographic Variables

- *Democrat share (county)* – proportion of Democrats at the county level based on the 2016 presidential election vote shares [Source: CQ Press]
- *Democrat share (zip)* – proportion of Democrats at the zip code-level based on the individual campaign donations during the 2015–2016 election cycle [Source: Federal Election Commission]
- *HHI* – sum of the squared deposit market shares at the county level in 2017
- *Mass shooting* – indicator equal to one if the county experienced at least one mass shooting from 1999 to 2018 [Source: Washington Post]
- *Boycott NRA* – intensity of Google search "Boycott NRA" at the state-level for 2018 [Source: Google Trends]
- *Never Again MSD* – intensity of Google search "Never Again MSD" at the state-level for 2018 [Source: Google Trends]
- *Education* – proportion of people with a bachelor's degree or higher at the county level [Source: U.S. Census Bureau]
- *Young* – proportion of people younger than age 65 at the county level [Source: U.S. Census Bureau]

- *Log SCI* – log value of social proximity to Parkland in Broward County, FL at the county level [Source: Facebook]
- *Log Population* – log value of population at the county level [Source: U.S. Bureau of Economic Analysis]
- *Log Per capita income* – log value of per capita income at the county level [Source: U.S. Bureau of Economic Analysis]
- *Change in population* – ($Population_t$ - $Population_{t-1}$) / $Population_{t-1}$
- *Unemployment rate* – unemployment rate at the county level [Source: U.S. Department of Agriculture]

A.3 Gun Business Variables

- *Log # firearms manufacturers* – log value of the number of firearms manufacturer business licensees at the county level [Source: Bureau of Alcohol, Tobacco, Firearms, and Explosives]
- *Log # firearms dealers* – log value of the number of firearms dealer business licensees at the county level [Source: Bureau of Alcohol, Tobacco, Firearms, and Explosives]

A.4 Additional Variables in Internet Appendix

- *Log $ Loans* – log value of the amount of small business loans in $ thousands at the county level [Source: Community Reinvestment Act]
- *Log Bank Assets* – log value of bank assets in $ thousands [Source: FDIC SDI]
- *Log # Branches* – log value of the number of bank branches [Source: FDIC SDI]
- *Capital-asset Ratio – Total equity capital / Total assets* [Source: FDIC SDI]
- *Deposit-asset Ratio – Total deposits / Total assets* [Source: FDIC SDI]
- *Mortgage-asset Ratio – Mortgage loans / Total assets* [Source: FDIC SDI]
- *Business Loan-asset Ratio – Business loans / Total assets* [Source: FDIC SDI]
- *ROA – Net income / Total assets* [Source: FDIC SDI]
- *NPL – Non-performing loans / Total loans* [Source: FDIC SDI]
- *Cost-to-income – Operating expenses / Operating incomes* [Source: FDIC SDI]
- *(Non)Media* – indicator equal to one if the bank is the gun lender with (without) media attention
- *Exposure to democrats* – deposit-weighted average of county-level democrat share

Biased Expectations and Default Risk in the Municipal Bond Market*

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Abstract

Public mass shootings raise borrowing costs of issuers in affected counties by an average of six (five) 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 illiquidity, risk aversion, or excess supply of debt. In contrast, there is no evidence that the violent crime rate in the county is priced into yield spreads. A possible explanation is investors' biased expectations of fundamentals brought about by media driven salience.

Keywords: Biased beliefs, Violent crime, Municipal debt, Salience

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1. Introduction

An important driver of credit spreads is investors' expectations of borrower's default risk and, expected returns on the debt security. Both expected default and expected returns cannot be observed. Thus the understanding of how investors form expectations of issuer's fundamentals, or how they perceive risk is critical to pricing. 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] This raises a number of questions. 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 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? 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.

[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 and 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), newspaper closures (Gao et al., 2020), and the opioid epidemic (Cornaggia et al., 2022).

The data on public mass shootings is obtained from the Washington Post. Since mass shootings are more likely to occur in urban and 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.[4] 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.[5] The yield spread differential persists for around three years 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.[6]

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, price discounts for illiquidity and default risk.[7] Hence a change in muni spreads over time must reflect variation in one or more of these components. It is unlikely that

[4] Given the issues raised by Baker et al. (2022) about staggered difference-in-differences, we use the stacked difference-in-differences methodology as in Gormley and Matsa (2011). See also Cengiz et al. (2019).

[5] The difference between Ba1 and AAA rated bonds can include some impact due to illiquidity, so we can interpret the 8.3% increase as a lower bound.

[6] To provide anecdotal evidence for the effect, we check the secondary market yields for affected bonds for two prominent mass shootings in our sample - the Santa Fe High School Shooting, and Sandy Hook Elementary School Shooting. We find that for bonds issued by the Santa Fe School district, yields rise by 17.2 bps in the one week after the shooting when compared to the week before. Similarly, in the case of Sandy Hook, general obligation (GO) bonds issued by Newtown, Connecticut see a rise of 30 bps in yields in the 2 weeks after the shooting.

[7] 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.

mass shootings could have any effect on the tax treatment of municipal bonds.[8] 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; Bao et al., 2011). Yet, there is no change 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 neighboring counties as well, as they are likely to share the same marginal investors.[9] Using a matched sample of control counties for each neighboring county of the affected county, we find no effect on the tax-adjusted or the raw yield spreads of issuers in neighboring counties, suggesting that changes in risk aversion do not explain the results.

We 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 future

[8] 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.

[9] 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.

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 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.[10] 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. 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,[11] and

[10] 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).

[11] 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.

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, and 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 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 after 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 municipal governments (cities, counties, and towns), school districts, or special districts. These 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. Then with arrival of new information, investors update their beliefs, driving the yield spread differentials to their pre-event levels. But the fact that we find no financial impact, even in the short run, for school districts and 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,[12] yet these effects do not translate into any meaningful changes in local government finances. Moreover, the drop in establishments and employment should impact only cities, counties, and townships, i.e., municipalities and school districts, which receive funding from property taxes, or special districts, which receive revenues from utilities, hospitals, etc. But the yield spreads of municipalities do not change while those of school districts and special districts increase. 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 (other than mass shootings). This provides support for our main hypothesis 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 of the marginal investor or even regulatory constraints. In the case of the municipal debt

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

market, financial frictions like scarce liquidity, high trading costs, and thereby the lack of bond arbitrageurs (Harris and Piwowar, 2006; Green et al., 2007a, Green et al., 2007b), could further explain why it takes about two years for erroneous beliefs to be corrected by market forces. We explore the potential psychological mechanism that underpins our findings in more detail in section 7.

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 $200,000, as the average issue size is about $39 million and the average maturity is 10 years. 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, 2022; 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 by time-varying subjective expectations of growth in fundamentals rather than time-varying risk 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 the path of credit spreads since a firm's last loan influences its current borrowing rates. Thus, 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 contribute to the literature on the economic costs of crime, which had burgeoned since the seminal work of Becker (2000). 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) find that higher state corruption is associated with greater credit risk and higher bond yields. Cornaggia et al. (2022) 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. (2022) 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 conditions, 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), the Mother Jones database, the 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 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 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 6 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 7 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 23 compares the mean of key county variables for mass shooting counties versus 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 32 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 centers and metropolitan areas. The correlation might purely be due to 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 versus non-treated samples, it confounds the effect of the treatment. If unobservable shocks (e.g., 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 32, for each shooting county, we find up to five matched non-shooting counties that (a) are located outside of the state of the shooting county and (b) 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

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

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 24. As shown in Panel A of Table 24, 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 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 of Table 24, 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. (2022), 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 Rulemaking Board (MSRB), from March 1998 to June 2020. 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).[14] We study municipal bond secondary yields around 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).

[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.

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 non-positive 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 the raw yield of the municipal bond (divided by $1-\tau_{s,t}^{state}$) and the yield-to-maturity of the synthetic risk-free bond.

$$Tax\ Adjusted\ Yield\ Spread = \frac{Raw\ Yield}{1-\tau_{s,t}} - Yield\ on\ Synthetic\ Treasury$$

Panel A of Table 25 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 25 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

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

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 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. (2022) 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 25 presents the summary statistics for three types of local government 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.

[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.

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

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:

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

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*. *Bond Controls* 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. *County Controls* 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]

Panel A of Table 26 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 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.[18]

[17] Our results hold with an alternate specification too, when we double cluster SEs by issuer and year-month level.
[18] As robustness checks (in unreported tables), we use different matching variables and ratios and find that our results are robust to different matching specifications. Specifically, in the first set of robustness checks, we use just three matching variables - log (Population), log (per capita income) and unemployment, and in the second set just the log (Population) and unemployment. The size of coefficient doesn't vary by much when using different matching variables or ratio (we obtain larger coefficients than what we document in the main analysis).

We next examine the impact on the primary market. Panel B of Table 26 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 8. 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:

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

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 one 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. $\gamma_{c,i}$ and $\delta_{c,t}$ are cohort-county and cohort-year-month fixed effects, respectively.

In Figure 8, the solid black 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 period after the shooting 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.

We next proceed to investigate the muni bond market reactions over different forward windows in the primary market. Table 33 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.

4.1.1 Understanding the yield spread dynamics in the secondary market. From Figure 8, we see that yield spreads in the secondary market keep rising up until 2 years after the event, before eventually coming back to pre-event levels. A possible explanation for the steady increase in spreads could be owing to the limited liquidity in the market, which leads to slow incorporation of information into prices. In Table 40, we examine the various transaction types in the secondary bond market to better understand this issue. *Trading volume* is the natural logarithm of the number (or amount) of all transactions (all except interdealer transactions) in a given month. In Panel A, in the first quarter of the after the shooting, for the treated bonds, we see an increase in customer sell transactions by 1% and an increase in the trading amount by 10%. In contrast, there is no statistically or economically significant impact on customer buy transactions for the treated bonds. When we extend the analysis window to 2 years before and after the shooting, we still see a significant increase of 5.4% in the volume of customer initiated sell transactions. This implies that in the immediate aftermath of a mass shooting event, certain panic driven investors rush to the muni bond dealers to offload their holdings of treated bonds. Faced with increased inventory risk, dealers price the negative information about treated bonds and adjust their prices downward over time such that the number of customer sell transactions go down over time. Thus, the reason why prices keep rising over such a long period is that dealers take time to learn about order imbalance from customer trades after the shootings. 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. In Table 39, we see that over the two year period after the mass shooting, there are no meaningful changes in either the retail, institutional customers or inter-dealer transaction volumes and amount. This suggests that overall liquidity for the treated bonds does not change in the period after the shooting.

4.2 Cross-sectional tests

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 27. 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 26. 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 27 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).[19] 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. (2022) 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

[19] 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.

municipal bond issuers. Due to the tax incentives, commercial banks are willing to pay higher prices or accept lower yields for these bonds (Dagostino, 2019). 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 27. 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 $Treatment_{c,i} \cdot Post_{c,t}$ 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. Furthermore, institutions are considered to be less susceptible to behavioral biases than retail investors.[20] 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

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

inter-dealer 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 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 27 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:

$$\begin{aligned} Y_{c,i,j,t} = {} & \beta_0 \cdot Treatment_{c,i} \cdot Post_{c,t} + \beta_1 \cdot Treatment_{c,i} \cdot Post_{c,t} \cdot Municipality \\ & + \beta_2 \cdot Treatment_{c,i} \cdot Post_{c,t} \cdot School + \beta_3 \cdot Treatment_{c,i} \cdot Municipality \\ & + \beta_4 \cdot Treatment_{c,i} \cdot School + \beta_5 \cdot Post_{c,t,m} + \beta_6 \cdot Post_{c,t,s} \\ & + \beta_7 \cdot Municipality + \beta_8 \cdot School + Bond\ Controls + County\ Controls \\ & + \gamma_{c,i} + \delta_{c,t} + \epsilon_{c,i,j,t} \end{aligned} \quad (3)$$

where the dependent variable $Y_{c,i,j,t}$ is either *Raw yield* or *Tax-adjusted yield spread*. $Treatment_{c,i}$ is a dummy variable that equals one if the county experiences a mass shooting. $Post_{c,t}$ 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 *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 28. 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 29 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* includes 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. A potential concern with the finances data from the Census of Government Surveys is that the data for the smaller local governments is collected once every five years. We fill the missing years through linear interpolation between the reported years. Since our set of mass shootings occur mostly in larger cities and counties, our finance data for most cities and counties in our sample is reported every year. However, this is not the case for the school and special districts in our sample. To circumvent this issue, we gather better data on school finances from the National Center for Education Statistics (NCES), which reports annual school district level collection of revenues and expenditure for every public school district in the United States.[21] Table 34 reports our findings. We see that in the NCES sample there is no effect of mass shootings on either revenues or expenditure in the one to three years after the event. We see a $\approx$ 25% reduction (although statistically insignificant) in the outstanding debt growth in the one year after the event.

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 after 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

[21] Revenues and expenditures are audited after the close of the fiscal year and are then submitted to NCES by each state education agency. Beginning with fiscal year 1990, detailed fiscal data on revenues and expenditures for all school districts providing public education to pre-kindergarten to grade 12 students have been collected.

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:

$$Y_{c,i,s,t} = \beta \cdot Treatment_{c,i} \cdot Post_{c,t} + County\ Controls + \gamma_{c,s} + \delta_{c,t} + \epsilon_{c,i,s,t} \qquad (4)$$

where the *c* denotes the cohort, *i* denotes the county, *s* denotes the state. In Panel A of Table 35, the dependent variable is 100 × the natural logarithm of employment 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. $Treatment_{c,i}$ is a dummy variable that equals one if the county in which the issuer is located experiences a mass shooting. $Post_{c,t}$ is an indicator variable that equals one if the year is within four years after the shooting. *County Controls* includes 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 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., 2018). 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 up to five years after the shooting. We exclude unrated bonds from our sample. We present our results in Table 30. 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?[22] 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 Table 36 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

[22] Hirshleifer and Sheng (2022) find that shootings impact investor attention. Specifically, investors pay less attention to earnings announcements when there is news about shootings.

the number or the duration of news stories. Columns (1) and (2) of Table 31 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 $Treatment_{c,i} \cdot Post_{c,t}$ variable, suggesting that variation in media coverage is sufficient to explain the variation in our post event outcome variable. A one standard deviation 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 standard deviation 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, which 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 31 documents our findings. We find that even though the bond yields increase by 4.5 bps after 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 Columns (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, $Treatment \cdot Post \cdot Media\ Coverage$. 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 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 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. (2022). 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:

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

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*. *Violent Crime 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 37 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. 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[23] 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 38 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 spillover effects of mass shootings on their neighboring counties' bond issuers.

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

6.2 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.[24] 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 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 41. 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. Discussion of Results

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,

[24] Recent studies have indeed documented the effects of capital supply frictions across investor types in the municipal bond market. Dagostino (2019) 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.

which theoretically should be more relevant for 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., (2022)), over and above that of the covariance of asset returns with consumption growth (Chinco et al., (2022)). 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. (2022).[25] 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., 2022). 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.

[25] They survey a sample of high net worth individuals, with at least $1 million 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 are small, 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 and homeowners, by affecting their current and future quality of life.[26] 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.

8. 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

[26] 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.

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.

Social Connection and Financing Cost of Municipal Governments*

Jinoug Jeung† and Jaemin Lee‡

Abstract

We investigate how social connection affects municipal finance. Municipal Bond Mutual Funds allocate more capital to counties with stronger social connection, which in turn lowers the municipalities' financing costs in the municipal bond market. Consistent with the familiarity-driven demand channel, the effects are focused on mutual funds with lower institutional resources and opaque bonds facing higher uncertainty; we find no effect for bank-qualified bonds, which mutual funds rarely hold. Fundamental risks, underwriter effects, and large counties with national-level awareness do not drive the results. Overall, we provide a new channel based on social connection that explains the cross-section of municipal bond prices.

JEL: *G1, G11, G12, G23, G41, H7, H74*
Keywords: Social connection, Municipal bonds, Mutual funds

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1. Introduction

Municipal bonds are the primary source of financing for municipalities in the U.S., with a market size reaching $4.1 trillion as of March 2022.[1] Despite their historically low default rate,[2] the literature provides limited understanding of the drivers underlying the heterogeneous issuance cost of municipal bonds. In this paper, we propose a new channel based on municipalities' social connection to large institutional investors. In particular, we argue that Municipal Bond Mutual Funds (MBMFs) allocate greater capital to counties with stronger social connection, which in turn lowers the financing cost of these municipalities.

To measure the intensity of social connection between municipal bonds and mutual funds, we rely on the Social Connectedness Index (SCI), which is widely used in recent studies on the effect of social connection (e.g., Bailey et al., 2018b; Kuchler et al., 2022). For each pair of counties in the United States, the SCI provides the probability of two randomly selected people, one in each county, being Facebook friends. We use the county location of a municipal bond's issuer and that of a mutual fund's investment adviser and measure the social connection between the two counties. In the first part of our paper, we show that MBMFs are more likely to hold bonds issued by socially closer municipalities, controlling for physical proximity and cohorts of fixed effects. Specifically, we compare the propensity to hold municipal bonds for a given county across funds with different social connections, while accounting for a mutual fund's potential home bias and its investment mandate in target states. We find that the effect is not only statistically significant, but also economically sizable. For example, in our most stringent specification, a 10% increase in the social

[1]According to Municipal Securities Rulemaking Board. See https://www.msrb.org/sites/default/files/Trends-in-Municipal-Securities-Ownership.pdf

[2] According to Moody's investors service report, the average five-year default rate since 2011 is 0.12%, compared with a global corporate default rate of 7.4%. See https://www.fidelity.com/bin-public/060_www_fidelity_com/documents/fixed-income/moodys-investors-service-data-report-us-municipal-bond.pdf

connection measure is associated with a 0.94% increase in the holdings of municipal bonds in the focal county, which corresponds to roughly two-thirds of the average non-zero holdings (1.54%) in our sample.

Next, we examine whether the relationship between the SCI and mutual fund holding is heterogeneous across the characteristics of mutual funds and municipal bonds. We first show that our findings do not hold for passively managed funds, whose managers have little discretion on the capital allocation decision. Second, we show that mutual funds under smaller families, which possibly have less resources for investment analysis, are more likely to overweight municipal bonds with stronger social connection. Lastly, with regard to municipal bond characteristics, we find that the SCI effect is focused on municipal bonds that are opaque and face greater uncertainty (i.e., revenue bonds and long-term maturity bonds).

One potential concern about our analyses is that an unobservable link between fund advisers and municipal bonds may exist and be correlated with our county-level SCI measure. To alleviate this concern, we turn to panel data of MBMF holdings from 2010 to 2019, which allows us to include fund-by-bond county fixed effects. Because the SCI measure is time-invariant for a fund advisor-bond county pair, all variations in this analysis rely on changes in the advisers' county location, driven by either turnover or relocation. Despite losing over 80% of our sample, we continue to find a significantly positive relationship between the SCI and mutual funds' holdings.

If social connection plays a role in MBMFs' capital allocation, a natural question is whether social connection can also affect the market price of municipal bonds. Kuchler et al. (2022) suggest that social connection broadens the investor base of an asset, which facilitates risk sharing and augments the value of the asset. We thus hypothesize that municipalities with higher social proximity to mutual fund capital experience lower bond issuance cost. We first construct a measure of social proximity to MBMF capital for each county. Specifically, following Kuchler et al. (2022), we compute the SCI-weighted average of MBMF capital in the other counties. According to this measure, a county with stronger social connection to the counties where large MBMFs are located has greater access to their capital and therefore experiences lower cost during its municipal bond issuance.

We first validate this argument by showing that municipal bonds with higher social proximity to capital have larger mutual fund ownership, controlling for cohorts of fixed effects as well as county and municipal bond characteristics. We find that a one-standard-deviation increase in social proximity to capital is associated with an increase of 0.46 percentage points in mutual fund ownership on average, which corresponds to 13% of the average mutual fund holdings during the sample period. Furthermore, we find that the relationship between social proximity to capital and mutual fund holdings is concentrated among non-bank-qualified bonds, which are more likely to be owned by mutual funds. In addition, consistent with our prior results, the effect is stronger for bonds that are more opaque and uncertain.

Based on the positive relationship between social proximity to capital and mutual fund ownership, we examine whether the increase in mutual fund holdings through social connection reduces the issuance cost of municipal bonds. We find a strong negative relationship between social proximity to capital and municipal bond issuance cost. A one-standard-deviation increase in social proximity to capital is associated with a statistically significant decrease of 7 basis points in offering spread. This reduction accounts for 16.7% of the average offering spread during our sample period, which suggests that the impact on issuance cost is economically significant as well. In addition, consistent with the preceding findings, we observe heterogeneous effects of social proximity to capital on offering spread across municipal bond characteristics. We first find that the effect does not hold for bank-qualified bonds, which are rarely held by mutual funds.[3] We also find that the effect is stronger for revenue bonds and long-term maturity bonds, which are more opaque and uncertain.

We argue that social connection expands the potential investor base of municipal bonds, which in turn decreases their issuance cost. However, municipalities with greater social proximity to capital may also have (1) lower fundamental risk, (2) stronger relationships with competitive underwriters, and (3) stronger national recognition by investors. All of these factors lower the municipalities' issuance cost but are not necessarily driven by social connection. To address these concerns, we conduct several tests. First, we test whether our findings are driven by fundamental risk. We consider the two types of fundamental risk, default risk and liquidity risk, which are the major pricing determinants of municipal bonds (Ang et al., 2014; Schwert, 2017). Specifically, we include rating-by-year fixed effects, using a subset of municipal bonds with valid credit rating information at the time of their issuances. Even when comparing bonds with the same default risk within the same year, we continue to find that social proximity to capital lowers the issuance cost. In addition, we find that social proximity to capital is not associated with liquidity risk.[4]

Second, we test whether our results are driven by the underwriters' ability to search for investors or by greater bidding competitions (Kessel, 1971; Benson, 1979). We find that our findings are robust to controlling for underwriter fixed effects, which isolates the effect of social proximity to capital from that of the underwriters' ability to search for investors. We also find a similar result when we restrict our attention to a subset of negotiable bonds that do not involve the bidding process during their issuances, which therefore minimizes the role of underwriters. Lastly,

[3] In our mutual fund holdings sample, only 2% of bonds held by mutual funds are bank-qualified municipal bonds.
[4] Following prior literature (Jankowitsch et al., 2011; Gao et al., 2020), we measure liquidity risk as the standard deviation of price changes from customer-to-dealer transactions during the 90-day period following the municipal bond offering date.

we test whether high national-level recognition by investors, which is measured by the size of the county population, drives our results. We find that the effect of social proximity to capital on offering spread is significant in all tercile subgroups sorted on county population. Indeed, we do not find significant differences of the effect across the subgroups, which suggests that social connection enlarges the investor base of municipal bonds beyond the national-level recognition.

We next investigate the specific channel through which social connection increases the investors' demand for municipal bonds. We examine two channels: information advantage and familiarity. Kuchler et al. (2022) document that social connection does not provide an information edge in the stock market. Consistent with their findings, we find no evidence that mutual fund managers receive information advantages through social connection in the municipal bond market. Compared with bonds that have low social proximity to capital, those with high social proximity do not experience either decreases in secondary market spread or upgrades in credit ratings. This finding suggests that social connection expands investors' demand by increasing their familiarity with the municipalities.

In the last part of our paper, we examine the relationship between social proximity to capital and search cost of underwriters. If stronger social connection provides greater investor pools for municipal bonds, then the enlarged demands may facilitate underwriters to resell their bonds quickly, allowing them to set a lower search cost. We use gross spread to measure the search cost of underwriters (Dougal et al., 2019). Consistent with our conjecture, we find that municipalities with greater social proximity to capital experience lower gross spread. On average, a one-standard-deviation increase in social proximity to capital is associated with an 8-basis-point decrease in gross spread. In addition, the effect is concentrated within the subset of non-bank-qualified bonds, confirming that the result is indeed driven by social proximity to mutual fund capital.

This paper contributes to three strands of literature. First, it contributes to a growing literature in social finance (Kuchler and Stroebel, 2021; Hirshleifer, 2020). Most of the existing studies in this area focus on how social connection can influence individuals' economic behaviors,[5] and few explore how social effects on individual behavior can extend to aggregate market outcomes. An exception is the study by Kuchler et al. (2022), which show that firms with greater proximity to institutional investors exhibit higher institutional ownership, liquidity, and valuation. Our paper expands this sparse literature by documenting a strong positive relationship between social connection and mutual funds' municipal bond holdings that leads to lower financing cost for the connected municipalities.

Second, our findings add to the literature on the pricing of municipal bonds. Most of the existing studies in this literature focus on fundamental risks or government policies as key determinants of municipal bond prices (Ang et al., 2010; Longstaff, 2011; Novy-Marx and Rauh, 2012; Ang et al., 2014; Schwert, 2017; Gao et al., 2019; Babina et al., 2021; Li et al., 2021).[6] Recent studies explain a cross-section of municipal bond prices based on behavioral channels, such as attention, saliency, or non-standard preference (Goldsmith-Pinkham et al., 2022; Chordia et al., 2022; Bergstresser et al., 2013; Dougal et al., 2019).[7] Our study provides a new channel of social

[5] Prior literature shows that social connection can affect individuals' stock market decisions (Shiller and Pound, 1989; Hong et al., 2004; Brown et al., 2008; Kaustia and Knüpfer, 2012; Ouimet and Tate, 2020), housing market beliefs (Bailey et al., 2018a; Bayer et al., 2021), consumption (Grinblatt et al., 2008; De Giorgi et al., 2020), savings (Duflo and Saez, 2003), and loan repayment (Breza, 2012).
[6] For example, prior literature introduces major fundamental risks—illiquidity and default—that shape municipal bond prices (Ang et al., 2014; Schwert, 2017). Based on these risks, subsequent empirical work demonstrates how government policies or state-wide shocks such as state pension investments (Novy-Marx and Rauh, 2012), tax exemptions (Babina et al., 2020), and the US Affordable Care Act affect municipal bond prices. In addition, Li et al. (2021) shows that the market's perception of mutual fund fragility risks during the COVID-19 pandemic imposes a liquidity risk on municipal bonds, thus discounting their prices.
[7] For example, Goldsmith-Pinkham et al. (2022) show a significant sea-level-rise premium in municipal bond prices as investors start paying attention to climate change around 2011, while Chordia et al. (2022) document a temporary spike in municipal bond spreads that are issued by municipalities experiencing mass-shooting incidents. In addition, Bergstresser et al. (2013) and Dougal et al. (2019) find that the municipal bond market can be segmented by investors' preferences based on ethnicity and religion, which deter the underwriting process to search for investors and thus increases municipal borrowing cost.

connection that increases institutional investor's familiarity with municipalities, which in turn decreases municipal borrowing cost. Importantly, with respect to recent studies showing that the segmentation of the municipal bond market leads to higher borrowing cost (Babina et al., 2021; Bergstresser et al., 2013; Dougal et al., 2019), we provide evidence that social connection can alleviate such cost by expanding potential investor pools.

Lastly, this study contributes to the literature on the investment choices of institutional investors. While a number of studies show that these professional investors, as retail investors, also invest in familiar assets (Coval and Moskowitz, 1999; Bernile et al., 2015; Sialm et al., 2020), the findings are mostly attributed to home bias, which may weaken as the importance of physical distance decreases over time (Da et al., 2021). We provide an additional channel, based on social connection, through which institutional managers may exhibit familiarity bias toward their connected municipalities (Kuchler et al., 2022).

The remainder of this paper is organized as follows. Section 2 describes the data. In Section 3, we examine the relationship between social connection and municipal bond holdings. In Section 4, we examine the price implication of such relationship. Section 5 concludes.

2. Data

2.1 Social Connectedness Index

To measure the intensity of social connection across mutual funds and municipal bonds, we use the SCI first used in Bailey et al. (2018b). The measure is based on the number of Facebook friends between paired counties in the United States, and it is provided as a snapshot in each year.[8] As described in detail by Kuchler et al. (2022), roughly 70% of the adult population in the United States use Facebook, and the users' demographic characteristics closely resemble those of the

[8] The data are available at: https://dataforgood.facebook.com/dfg/tools/social-connectedness-index#methodology. We use snapshot of the measure provided in year 2019.

overall population. They also argue that friendship on Facebook is more likely to mirror real-world friendship compared with other online platforms such as Twitter.[9] Specifically, the SCI (*Social_Connectedness_Index*) between a pair of counties *i* and *j* is computed as follows:

$$Social\_Connectedness\_Index_{i,j} = \frac{Facebook\_Connections_{i,j}}{Facebook\_Users_i \times Facebook\_Users_j}, \tag{1}$$

where $Facebook\_Users$ is the number of Facebook users in a county (i or *j*), and $Facebook\_Connections_{i,j}$ is the number of Facebook friends between county i and j.[10] This measure can be interpreted as the probability of randomly selected Facebook users in the two counties being Facebook friends.[11]

Figure 9 illustrates the heat map of SCI for two counties where a large number of MBMF advisers are located in our sample: Fulton County, Georgia (Atlanta) and Cook County, Illinois (Chicago). The darker green in each figure corresponds to counties with a stronger SCI. One noticeable pattern is that distance plays a key role in determining social connection. Thus, we tightly control for the distance by using distance percentile fixed effects in our specifications. However, physical distance is not the sole determinant of social connection. For example, Fulton County in Georgia has strong social ties with other southern states, which possibly reflects the historical legacy from the Confederate era. Similarly, as also pointed out by Kuchler et al. (2022), Cook County has strong social ties with counties along the Mississippi River, which is likely owing

[9] Other sources provide evidence that Facebook friendship is a good proxy for real-world social connections in the United States. See Rehbein and Rother (2020), Bailey et al. (2018b; 2019; 2020a,b; 2021), and Chetty et al. (2022a,b) for specifics.

[10] Note that this measure slightly differs from that used in Kuchler et al. (2022), which uses product of population instead of that of Facebook users. We find that the SCI heatmap of Cook County, IL in Kuchler et al. (2022) and that in our study (Figure 9) are very similar, which suggests that a different denominator does not seem to make material changes to the SCI measure.

[11] Detailed methodology regarding the SCI measure is available at: https://dataforgood.facebook.com/dfg/docs/methodology-social-connectedness-index

to the Great Migration (1916–1970) during which African Americans migrated from southern to northern cities. As explained in more detail in Section 3, we exploit these variations in SCI by comparing the holdings of the same focal county by MBMF advisers located in different locations, after heavily controlling for the distance effect.

2.2 Municipal Bond Mutual Funds (MBMFs) data

We collect MBMFs' municipal bond holdings from CRSP Mutual Fund Holdings Database (CRSP Holdings). Unlike other sources of mutual fund holdings that focus on equities, CRSP Holdings provides mutual fund holdings of other securities including municipal bonds. For each security held by a mutual fund, the database provides the security's percentage of the fund's total net assets (TNAs), as well as the date on which the fund reports its holdings. We identify municipal bonds by matching *cusip* of CRSP Holdings and Mergent Municipal Bond Securities Database, which is described in Section 2.3.

We define MBMFs as mutual funds that ever held municipal bonds greater or equal to 50% of their TNAs on average during a given year.[12] We cross-check the validity of this threshold by matching CRSP with Morningstar Direct, following the approach of Berk and van Binsbergen (2015). We find that over 97% of matched mutual funds are identified as municipal bond funds according to the Morningstar category.[13] We extract the mutual fund holdings of these funds

[12] The 50% threshold is rationalized by the distribution of the percentage of municipal bond holdings of all mutual funds in CRSP Holdings that ever held at least one municipal bond, which resembles bimodal distribution as shown in Figure 12. Specifically, the percentage of municipal bond holdings starts to increase from 50%, and peaks around 96%. In Appendix B, we also restrict our attention to MBMFs that hold municipal bonds greater than 90% of their TNAs as a robustness test and we find similar results.
[13] The remaining 3% of funds are identified as high-yield bonds, government bonds, and intermediate core-bonds.

closest to the end of each year,[14] starting from 2010.[15] We also extract the characteristics of the MBMFs from CRSP Mutual Fund Database (MFDB).

Our primary measure of mutual fund *i*'s municipal bond holdings in county *j* is measured as:

$$\%Holdings_{i,j,t} = \frac{Municipal\ Bond\ Holdings_{i,j,t}}{Municipal\ Bond\ Holdings_{i,t}}, \tag{2}$$

where $Municipal\ Bond\ Holdings_{i,j,t}$ is the total dollar value of all bonds issued by municipalities located in county *j* held by fund *i* in year *t*. We normalize this value by the total dollar value of all municipal bonds held by fund *i* in year *t*, because the maximum proportion relative to its TNAs that MBMFs can invest in municipal bonds differs across funds owing to their investment mandates.[16] In Appendix B, we find that our results are robust to using alternative denominators, such as TNAs.

To identify the location of the MBMF's investment adviser, we rely on two sources. First, we download Form ADV from the EDGAR website,[17] which is available from October 2000 and submitted annually by all investment advisers registered with the Securities and Exchange Commission according to the Investment Advisers Act of 1940. The dataset provides the names, filing identifications, and office location addresses of registered investment advisers. Based on the

[14] We find that the frequency and reporting month differ across funds. As most funds report close to year-end, we use holdings that are reported closest to the end of each year for our holdings analysis.
[15] Although the CRSP Holdings sample starts from 2006, they are less reliable prior to 2010, as also reported by Wharton Research Data Services (https://wrds-www.wharton.upenn.edu/documents/1414/WRDS_Ownership_Data.pdf). Consistent with this, we find that funds tend to report different percentage holdings relative to TNAs for the same municipal bond prior to year 2010, but such tendency is effectively eliminated starting from 2010. In addition, restricting our sample period from 2010 allows us to avoid the global financial crisis period, during which there may be structural break or data errors (e.g., CRSP fails to track merged funds, for which we find evidence in our data).
[16] Still, we find that over 90% of funds in our sample hold over 90% of their TNAs in municipal bonds.
[17] This dataset was uploaded in response to Freedom of Information Act (FOIA) request to the Securities and Exchange Commission by Williams Beggs in 2015. See Beggs (2022) for detailed information on this data.

investment advisers' names, we match this dataset to CRSP MFDB, which provides the names of each mutual fund's investment advisers starting from 1993.

We complement these data using Form N-SAR and N-CEN, which are submitted by investment management companies regarding their financial information such as sale of their fund shares, portfolio turnover rate, and other inputs from their shareholder reports. In June 2018, N-SAR was replaced by N-CEN, which provides relatively similar data but in a different format. Importantly for this study, the dataset provides the name, address, and filing identifications of investment advisers who manage the management company's funds. We again match this information with CRSP MFDB, using investment company CIK code and fund names. While the two sources provide very similar information on advisors' county locations, we use the location provided by N-SAR and N-CEN whenever there is a discrepancy.[18]

2.3 Municipal bonds data

We collect the offering yield and attributes of each municipal bond from Mergent Municipal Bond Securities Database. The attributes include state of issuance, issuance date, type of debts (e.g., certificates of obligation and collateralized notes), type of issue sale (negotiated versus competitive), type of source (general obligation versus revenue), insurance, callability, maturity date, coupon rate, bond size, and underwriter, as well as credit rating information from Standard and Poor's, Moody's, and Fitch. We complement the geo-location of each municipal bond by collecting the county location of its issuers from Bloomberg and SDC Platinum.

Following prior literature (Schwert, 2017; Gao et al., 2020), we restrict our attention to fixed-coupon tax-exempt bonds, excluding those with maturities more than 100 years, coupon rates

[18] For a few cases in which the investment advisor is located in multiple different counties in a given year according to N-SAR or N-CEN, we use the new address if the change is permanent (according to time-series of advisor's address). Otherwise when the change is temporary (less than six months), we keep the old address.

greater than 20 percentage points, negative offering yields, or dollar price less than 50% or greater than 150% of the par. To compute credit spread of municipal bonds, we use Municipal Market Advisors AAA-rated curve ("MMA curve") from Bloomberg as our benchmark for tax-exempt bonds. We compute credit spread as the difference between an offering yield and the maturity-matched par yield from the MMA curve.

We also collect gross spread information from the Securities Data Company (SDC) Global Public Finance Database. Gross spread is reported in basis points as a fraction of the bond's par value and is used to proxy for search cost of bond issuances (Dougal et al., 2019). Because the SDC provides bond characteristics at the issuance level, which is a group of tranches, we adjust maturity and bond size at the issuance level in our gross spread analysis. Specifically, for a group of tranches that are issued together, we define maturity as the longest maturity in the tranches and bond size as the total amount of issuance (Dougal et al., 2019).

3. Social Connectedness and Municipal Bond Holdings of Mutual Funds

3.1 Baseline specification

We start our analysis by testing whether the social connection between mutual fund advisor location *i* and municipality location *j* is positively associated with the fund's holding of bonds issued by municipalities in county *j*. We first construct a sample of county-fund-year pairs by creating a cartesian product of MBMFs with reported holdings in a given year and counties whose municipal bonds that ever appeared in CRSP Holdings.[19] For the sake of computing power, we drop all state-fund-year observations that are never held by the fund; these observations are dropped owing to our fixed effects approach anyway.[20] Panel A of Table 42 shows the summary

[19] We find that 2,331 counties had at least one municipal bond that appeared in CRPS holdings database.
[20] For similar reason, we also drop county-year groups and distance percentile-year groups that are not owned by any funds, although the number of such cases is extremely small.

statistics of this sample. As shown in the first row (*% Holdings*), MBMFs hold 0.15% of their TNAs in each county on average. In addition, most funds do not own a county up to the 90th percentile, which suggests that mutual funds own relatively concentrated portfolios. When we condition on non-zero holding county-fund pairs, as shown in the second row, the average *% Holdings* increases to 1.58%.

Following prior literature (Kuchler et al., 2022; Bailey et al., 2021), we use a regression model with the following functional form to test our hypothesis:

$$\%Holdings_{i.j,t} = \exp\left[\beta Log\ Social\ Connectedness_{i,j} + \gamma X_{i,j,t} + \theta_{j,t} + \delta_{i,s(j),t}\right] \cdot \epsilon_{i,j,t}, \tag{3}$$

which includes various fixed effects, as well as time-varying control variables between fund *i* and county *j*. $X_{i,j,t}$ includes a dummy variable for whether fund advisor *i* and county *j* are located in the same county. It also includes the physical distance between the fund advisor and the county, which we control either linearly or non-parametrically using percentile fixed effects. The functional form in equation (3) is motivated by the binned scatter plot in Figure 10, which suggests a log-linear relationship between $\%Holdings_{i.j,t}$ and $Social\ Connectedness_{i,j}$. Following Kuchler et al. (2022) and Bailey et al. (2021), we estimate this regression using Poisson Pseudo Maximum Likelihood (PPML). As discussed in detail by Silva and Tenreyro (2006), PPML is an adequate approach to dealing with zero values for the dependent variable, and it is widely used in trade literature that is also prone to left-censoring of trades between countries.[21] For our baseline regression, we include municipal county fixed effects, which allows us to compare holdings in the same focal county across funds with different SCIs. We also include fund-by-municipal state fixed effects to control for mutual funds' heterogeneous mandate regarding their target investment states.

[21] In relation with equation (3), all subscript *t* is replaced with 2019.

We report our baseline results in Table 43. Because our SCI measure is published in 2019, we first start by using a cross-sectional sample of holdings as of 2019 as in Kuchler et al. (2022), although we repeat the analysis using panel data in Table 46. In the first column of Table 43, the coefficient estimate on *Log Social Connectedness* is 0.0486, which is statistically significant at the 5% level. However, such relationship may be driven by home bias, which is extensively documented in the literature (e.g., Coval and Moskowitz, 1999; Baik et al., 2010; Bernile et al., 2015). For example, a fund advisor may be located in the same focal county or a nearby county and therefore be better aware of the economic condition of municipalities in that area. Because SCI tends to be stronger in nearby areas, as shown in Figure 9, the estimate on *Log Social Connectedness* in column (1) may capture the home bias effect.

To rule out this possibility, we first include same county fixed effects in column (2). We find that the coefficient estimate on *Log Social Connectedness* increases by about 60% and is statistically significant at the 1% level. Next, when we include *Log Physical Distance* in lieu of *Log Social Connectedness* in column (3), consistent with the literature on home bias, we find a negative relationship between the distance and municipal bond holdings, although the estimate is not statistically significant. When we run a horserace using both *Log Physical Distance* and *Log Social Connectedness* in column (4), we find that only the latter is statistically significant at the 1% level, with the coefficient estimate increasing by roughly 23%. As in Kuchler et al. (2022), the effect of geographical distance changes the sign, which suggests that social connection is one potential driver of the local bias documented in prior literature (Coval and Moskowitz, 1999).

In column (5), we control for the effect of physical distance non-parametrically by using percentile fixed effects. We find a similar economic effect of social connection on holdings, with greater *t*-stat (2.90). Even in our most stringent specification, we find strong economic significance;

a 10% increase in *Log Social Connectedness* corresponds to an increase of roughly 0.94 percentage points in municipal bond holdings of the focal county, which corresponds to roughly two-thirds ($\approx 0.94/1.58$) of the average holdings among our non-zero holding pairs.[22]

3.2 Heterogeneity analysis

Next, we examine whether the effect of social connection differs with respect to cross-sectional fund and bond characteristics. We begin by examining the effect of social connection on bond holdings for actively and passively managed funds. Because passive funds aim to track a predetermined municipal bond index instead of actively selecting bonds, they are likely to be less affected by social connection. We interact *Log Social Connectedness* with partitioning variables for each subgroup of active and passive funds. As in Kuchler et al. (2022), we also split fixed effects within each subgroup. We provide the result in Panel A of Table 44. Column (1) shows that *Log Social Connectedness* is statistically significant for active funds, but not for passive funds. Column (2) shows that the result remains similar when we non-parametrically control for physical distance.

Prior literature suggests that funds from smaller families have relatively less resources allocated for investment analysis, and therefore, are more likely to rely on the manager's own ideas (Pool et al., 2012). Similarly, we examine whether funds with limited resources rely more on social connection. Following the literature, we proxy for fund family's resources based on its TNAs. We partition funds into two groups based on their fund family's TNAs. Column (3) shows that social

[22] In Panel A of Table 52 of Appendix B, we run several robustness tests of Table 43 results. First, we run several subsample analyses. We exclude tri-state (New York, Massachusetts, and Connecticut) and California area municipal bonds, which account for a large number of municipal bonds in our sample. In the next column, we exclude mutual funds whose advisors are located in three counties (i.e., the cities New York, Chicago, and Boston) that account for the location of 50% advisors. In the last column, we exclude municipal bonds whose percentage of municipal bond holdings relative to their TNAs is less than 90%. In Panel B, we winsorize the dependent and main explanatory variables, or use alternative denominator for our *%Holdings* measure. The results remain both economically and statistically similar to results in Table 43.

connection is associated with holdings of funds that belong to small families, but not those that belong to large families.[23]

Next, we repeat our analyses along different municipal bond characteristics. Following our prior results on institutional resources, we would expect funds to rely more on social connection when investing in municipal bonds whose information is less well known (i.e., opaque) and/or those that have greater uncertainty. We thus focus on two bond characteristics that are related to opaqueness and uncertainty: bond types (general obligation versus revenue) and maturities.

General obligation bonds are mostly issued by local governments and school districts that regularly report their financial conditions, while revenue bonds are largely issued by small municipalities, including special districts that do not regularly report their financial conditions (Amornsiripanitch, 2022). Even for revenue bonds issued by local governments and school districts, identifying the exact revenue sources used to pay bond coupon and its principal is still difficult. A maturity is the period during which the bond holder receives interest payments on the investment. The longer the maturity is, the more uncertain the bond is. Following Chordia et al. (2022), we define bonds with maturities less than or equal to five years as short-term (ST) and with maturities greater than five years as long-term (LT). Given the attributes of revenue bonds and LT-maturity bonds, we thus expect the impact of social connection on bond holdings to be stronger for revenue bonds and LT-maturity bonds.

Using a subset of municipal bonds based on these characteristics, we create a similar county-fund level observations based on the cartesian approach explained in Section 3.1. Panel B of Table 44 reports the effect of social connection based on the following attributes of municipal bonds: (1)

[23] We find similar pattern for physical distance and holdings: the positive association holds only for funds with small families, which is consistent with Pool et al. (2012) documenting stronger home bias for funds that belong to small families.

general obligation, (2) revenue, (3) ST maturity, and (4) LT maturity. In column (1), we do not find the effect of social connection on bond holdings for general obligation bonds, but we do find a significant effect for revenue bonds. Similarly, we find a statistically stronger and economically larger social connection effect for LT-maturity bonds relative to ST-maturity bonds.

3.3 Panel data analysis

One possible concern regarding our prior results is an omitted variable bias, that is, the presence of an unobservable variable that is associated with holdings in the focal county and may be correlated with *Log Social Connectedness* measure. For example, if a fund county and a municipal bond county have similar race constitution, the mutual fund manager may be more inclined to invest in municipal bonds of that county. If these two counties also have strong social connection, it is possible that our findings are not necessarily driven by social connection, but rather by demographic familiarity.[24]

To alleviate such concern, we turn to a panel regression analysis. This analysis allows us to include fund-by-municipal county fixed effects, which control for unobservable connection between funds and municipal bond counties that may affect the funds' holdings. Because Facebook provides only a snapshot of SCI (reported as of August 2019).[25] Our *Log Social Connectedness* measure is time invariant as well. Therefore, when including fund-by-municipal county fixed effects, we exploit variation from a subset of funds whose advisors change their locations during the sample period. This change may be driven either by relocations of existing advisers or by

[24] Consistent with this argument, Bailey et al. (2018b) find that difference in demographic characteristics across counties, such as income, education, political stance, and religion, are important determinants of social connection.
[25] Owing to the nature of the measure, even the snapshots reported in other years are expected to be very similar.

mutual funds replacing their advisors with those in new locations.[26] We find that about 20% of funds' advisors change location during the sample period.

We report the results in Table 45. In columns (1) through (4), we run regressions with specifications similar to those used in the cross-sectional analyses of Table 43. Because we are running a panel regression, we further interact fixed effects with time dimension (i.e., × year). The results confirm that our results in Table 43 remain unchanged overall. Our focus is column (5), in which we include fund-by-municipal county fixed effects to absorb unobservable relationships between mutual funds and municipal bond counties. Because we are using the subset of funds whose advisors changed their location, it reduces our sample size by more than 80%. Despite losing substantial variation, we continue to find a statistically significant effect of *Log Social Connectedness*, while producing a similar economic significance; a 10% increase in social connectedness is associated with an increase of 0.72 percentage points in municipal bond holdings of the focal county.

4. Capital Market Implication for Municipalities

4.1 Measurement: social proximity to capital

Having established a robust relationship between social connection and municipal bond holdings, we ask whether such a relationship can affect aggregate market outcomes. Specifically, natural questions are whether and how municipalities' social proximity to capital affects their financing costs. Prior literature shows that social connection broadens an investor base (Kuchler et al., 2022), which facilitates risk sharing and thus augments the value of an asset (Merton, 1987; Hong et al., 2008; Lehavy and Sloan, 2008; Fang and Peress, 2009; Babina et al., 2021). Importantly, given the segmented nature of the municipal bond market driven by state-level

[26] Kuchler et al. (2022) similarly exploit relocation of firms' headquarters. This is infeasible in our setting because a county's location is fixed, which is why we exploit the relocation of fund advisors instead.

policies (e.g., tax exemption) that adds another layer of home bias (Babina et al., 2021), we argue that the effect of social connection is particularly important in alleviating such bias.

To answer these questions, we first create a measure of each county's social proximity to capital of MBMFs, which are one of the major investors in the municipal bond market, especially in the primary market (Azarmsa, 2021). Specifically, for each county *j* in quarter *t*, we compute *Social Proximity to Capital* as follows:

$$Social\ Proximity\ to\ Capital_{j,t}$$
$$= \sum_{f \in i} Social\ Connectedness\ Index_{i,j} \times Municipal\ Bond\ Holdings_{f,t}$$
$$where\ Municipal\ Bond\ Holdings_{f,t} = 0$$
$$if\ fund\ f\ has\ mandate\ for\ state\ s\ and\ state(j) \neq s \tag{4}$$

where $Social\ Connectedness\ Index_{i,j}$ is a measure of social connection between county *i* and *j* (equation (1)), and $Municipal\ Bond\ Holdings_{f,t}$ is a dollar amount of municipal bonds held by fund *f* in quarter *t* (equation (2)). This measure is similar to that used in Kuchler et al. (2022), but with slight modification in that we treat $Municipal\ Bond\ Holdings_{f,t} = 0$ if fund *f* has a specific mandate for a target investment state *s* (i.e., fund *f* is a state MBMF), and county *j* is not located in state *s*. In other words, we assume that a mutual fund with a state mandate invests exclusively in that mandated state.[27] Having a greater value of this measure can be interpreted as a county having greater social connection to its potential mutual fund investors.

[27] We classify funds into state and national funds based on their reported holdings. Specifically, a fund is classified as a state fund if it invests over 50% of its municipal bonds in a single state. All other funds are classified as national funds. We impose a 50% threshold because state funds are allowed to invest in bonds outside the mandated state (Babina et al., 2020). We cross-check the validity of this approach by matching CRSP MFDB with Morningstar Direct following the approach of Berk and van Binsbergen (2015). We find that roughly 99% of mutual funds classified based on our threshold match the classification based on Morningstar category.

Because physical proximity has a close relationship with social connection (Figure 9), we control for physical proximity to capital, using a similar measure computed as follows:

$$\begin{gathered}
Physical\ Proximity\ to\ Capital_{j,t} \\
= \sum_{f \in i} \frac{1}{Physical\ Distance_{i,j}} \times Municipal\ Bond\ Holdings_{f,t} \\
where\ Municipal\ Bond\ Holdings_{f,t} = 0 \\
\textit{if fund } f \textit{ has mandate for state } s \textit{ and state(j)} \neq s
\end{gathered} \tag{5}$$

where $Physical\ Distance_{i,j}$ is a distance (miles) between county *i* and j, which is used to control physical distance in the previous section. Figure 11 shows the heatmap of the two measures, social and physical proximity to capital, across U.S. counties as of the third quarter of 2019. A few important patterns emerge. First, similar to SCI in Figure 9, physical proximity to capital is positively correlated with social proximity to capital. Both physical and social proximity to capital are stronger in northeastern and Chicago regions, where a lot of mutual fund advisors are located. However, this is not always the case. Unlike physical proximity to capital, which is primarily focused in these regions, many other regions have high social proximity to capital. These areas include areas along the Mississippi River (high social connection with Chicago) and Florida (strong connection with New York).

We first validate this measure by testing whether a county's social proximity to capital is positively associated with actual MBMFs' aggregate holdings of the county's municipal bonds. Although an ideal setting would be to examine the MBMFs' holdings in the *primary* market, such data are not available. We therefore focus on MBMFs' holdings in the *secondary* market, based on their reported holdings of municipal bonds.[28] Specifically, we run the following panel regression:

$$\%Holdings_{i,c,t} = \beta Log\ Social\ proximity\ to\ Capital_{c,t-1}$$
$$+\gamma_1 X_{i,t-1} + \gamma_2 \Gamma_{c,t-1} + \psi_{state(c),t} + \Theta_{c,t-1} + \varepsilon_{i,c,t}, \tag{6}$$

where $\%Holdings_{i,c,t}$ is the percentage of the total dollar volume of all MBMFs' holdings of bond *i*, out of total dollar volume outstanding of bond *i* in year *t*. $X_{i,t-1}$ and $\Gamma_{c,t-1}$ are characteristics vectors of bond *i* and county *c*, respectively, at the end of previous year $t-1$. Specifically, $X_{i,t-1}$ includes time to maturity, inverse maturity, log of bond size, indicator variables for whether the bond is general obligation, callable, competitive, and insured, and dummy variables for credit ratings and debt types. $\Gamma_{c,t-1}$ includes log of population, log of per capita income, change in population, and unemployment rate.[29] To tightly control for the effect of physical proximity to capital, we also include percentile fixed effects of physical proximity to capital of county *c*, denoted as $\Theta_{c,t-1}$. We also include state × year fixed effects, denoted as $\psi_{state(c),t}$, to control for time-varying state-level characteristics.

Table 46 provides the result, with state-by-year and municipal bond credit rating fixed effects included for all specifications. In the first column of Panel A, we control for county-level characteristics. We find a strong positive relationship between social proximity to capital and municipal bond holdings, which is statistically significant at the 1% level. We find a smaller

[28] We argue that our results may underestimate the effect of social proximity to capital on mutual fund holdings in the primary market. As argued by Azarmsa (2021), MBMFs buy municipal bonds in the primary market and resell part of them in the secondary market. If our results based on the secondary market show a fraction of what mutual funds actually buy in the primary market, it is possible that the effect social proximity to capital may be greater in the primary market than what we find.

[29] We collect county-level data on population, per capita income, and unemployment rate from the U.S. Bureau of Economic Analysis and the U.S. Bureau of Labor Statistics.

coefficient estimate of social proximity to capital when we include physical proximity to capital percentile fixed effects in column (2), but the economic effect remains sufficiently large and statistically significant at the 5% level. In columns (3) and (4), we repeat the same analysis while controlling for municipal bond characteristics instead of county characteristics. We continue to find a positive association between social proximity to capital and MBMFs' aggregate holdings with a similar economic magnitude in column (2). In the last two columns, we include both county and municipal bond controls, and our result remains unchanged. In the most stringent specification in column (6), a one-standard-deviation increase in *Social Proximity to Capital* is associated with an increase of 0.46 percentage points (1.04×0.448) in mutual fund holdings, which corresponds to roughly 13% (0.46/3.52) of the average mutual fund holdings during the sample period. In Panel B, we provide further evidence by repeating our analyses using subsamples focusing on the following municipal bond characteristics: (1) bank qualified, (2) general obligation, and (3) maturity. Bank-qualified bonds can be sold directly to banks and provide significant tax incentives to encourage commercial banks to invest in municipal bonds. Because of such tax incentives, commercial banks are willing to pay higher prices (i.e., accept lower yields) for those bonds (Bergstresser and Orr, 2014; Dagostino, 2019). Therefore, they are rarely owned by mutual funds who do not face such incentives.[30] We thus expect the impact of social proximity to capital of mutual funds to be focused on non-bank-qualified bonds. In the first two columns, this is exactly what we find. The effect of social proximity to capital exists only for the subset of non-bank-qualified bonds, for which one of the marginal investors is a mutual fund.

Next, we focus on two characteristics that are related to the opaqueness and uncertainty of municipal bonds. Following the similar logic in Section 3.2, we argue that social proximity to

[30] Consistent with this argument, only 2% of bonds held by mutual funds are bank-qualified municipal bonds in our county-fund pair sample.

capital would matter more for municipal bonds with more opaque and uncertain information. Consistent with our finding in Section 3.2, we find that the positive association between social proximity to capital and MBMFs' holdings is focused only on revenue bonds (column (4)), which are more opaque, but not on general obligation bonds (column (3)). Similarly, the effect is roughly two times stronger for longer-term bonds than short-term bonds (columns (5) and (6)).

4.2 Social proximity and municipal financing cost

Based on the positive relationship between social proximity to capital and mutual fund ownerships in Section 4.1, we next hypothesize that municipalities with greater social proximity to capital experience lower issuance cost in the municipal bond market. To test this hypothesis, we estimate the following regression:

$$Spread_{i,c,t} = \beta Log\ Social\ proximity\ to\ Capital_{c,t-1} + \gamma_1 X_i$$
$$+\gamma_2 \Gamma_{c,t-1} + \eta_c + \psi_{state(c),t} + \Theta_{c,t-1} + \varepsilon_{i,c,t} \tag{7}$$

where $Spread_{i,c,t}$ is the offering spread of municipal bond *i* of county *c* in quarter *t*. X_i and $\Gamma_{c,t-1}$ are vectors of characteristics of bond *i* and county *c*, respectively, which are the same as those used in Section 4.1. We also include county fixed effects, η_c, to control for unobservable time-invariant county characteristics; state-by-quarter fixed effects, $\psi_{state(c),t}$, to control for time-varying state-level characteristics; and physical proximity percentile fixed effects, $\Theta_{c,t-1}$, to control for the effect of physical proximity to capital.

Table 47 provides the result. In the first column, we find a strong negative relationship between social proximity to capital and offering spread. A one-standard-deviation increase in *Social Proximity to Capital* is associated with a statistically significant 7-basis-point decrease in offering spread. This reduction accounts for 16.7% of the average offering spread (0.41) during the sample period, suggesting that the effect on issuance cost is economically significant. Including municipal bond control variables, county control variables, or physical distance fixed effects from columns (2) through (8) does not change this point estimate in a significant way, which suggests that a municipality's social proximity to capital reduces its bond issuance cost, independent of municipal bond and county characteristics.

4.3 Heterogeneity analysis

Following the ownership heterogeneity analysis in Section 4.1, we investigate whether the relationship between *Social Proximity to Capital* and municipal bond issuance cost is also heterogeneous by the following bond characteristics: (1) bank qualified, (2) general obligation, and (3) maturity. Based on the similar argument in Section 4.1 that most of bank-qualified bonds are not owned by mutual funds, we expect the effect of social proximity to *mutual fund* capital on municipal bond issuance cost to be smaller for bank-qualified bonds. In columns (1) and (2) of Table 48, we find that this is indeed the case; the effect of the social connection to mutual fund capital is significant only for non-bank-qualified bonds. A one-standard-deviation increase in *Social Proximity to Capital* is associated with a statistically significant 11-basis-point increase in offering spread, which is roughly 50% larger than its average effect in Table 47. We, however, find an insignificant effect for bank-qualified bonds, which is consistent with our findings in Section 4.1.

Furthermore, we investigate whether social proximity to capital matters more for municipal bonds that are more opaque and uncertain. If mutual fund managers rely on social connection for municipal bonds that have less well known or more uncertain information, as shown in Sections 3 and 4.1, we should also expect the effect of *Social Proximity to Capital* to be stronger for more opaque and uncertain municipal bonds. In columns (3) through (6), consistent with this argument, we find that the impact is concentrated among revenue bonds and stronger for LT-maturity bonds.

4.4 Alternative channels

Our central argument is that mutual fund managers have a greater propensity to invest more in socially connected counties because they are more likely to become aware of those municipalities through their social networks. This in turn allows municipalities with higher social proximity to capital to experience lower financing cost. However, because social connection is largely time-invariant within a short horizon (Kuchler et al., 2022), other pricing determinants that may be correlated with *Social Proximity to Capital* might drive our results. In this section, we discuss and rule out potential competing interpretations: (1) lower municipal fundamental risk, (2) stronger relationship with competitive underwriters, and (3) national-level recognition, all of which may lower municipal financing cost but are not necessarily through social connection.

We first test whether our results are driven by municipal fundamentals—default risk and liquidity risk—which are the major pricing determinants of municipal bonds (Ang et al., 2014; Schwert, 2017). If municipalities with lower default risk and liquidity risk are concentrated in counties with greater social proximity to capital, then the negative relationship between *Social Proximity to Capital* and municipal financing cost would be mechanical. Table 49 shows that this is not the case. In column (1), in order to control for default risk more closely, we include rating-by-year fixed effects by using a subset of municipal bonds with valid credit rating information at the time of issuance. Even when comparing bonds with the same default risk in within the same year, we continue to find that social proximity to capital is negatively associated with issuance cost. With respect to liquidity risk, in column (2), we run the regression of Equation (7), using the liquidity measure of municipal bond *i* in quarter t, $Price\ Dispersion_{i,c,t}$, as the dependent variable. We find that social connection is not associated with municipal liquidity risk.

Second, one might argue that counties with higher social proximity to capital have stronger relationships with competent underwriters or have more competitive bid underwritings, and thus have lower municipal bond issuance cost, independent of their social connection to mutual funds. Prior literature suggests that the better ability of underwriters to search for investors and the more intensive bidding competitions decrease municipality interest cost (Kessel, 1971; Benson, 1979). To isolate the impact of social connection from those of underwriters, we add underwriter fixed effects in column (3), and focus on a subsample of negotiable bonds in column (4). These two settings allow us to control the capability of underwriters in searching investors and the effect of bidding competition. As reported in column (3), our findings are robust within underwriter, implying that underwriters would attract a larger group of investors through municipal social connection and thus offer lower offering spreads. Also, the subsample analysis in column (4) shows that the effect of social connections is significant among negotiable bonds, suggesting that our findings are independent from the bidding competition.

Lastly, we test whether our results are driven by national-level county recognition by investors. For example, most of large counties (e.g., Cook County [Chicago] and New York County) are nationally well known to most investors and therefore are likely to enjoy lower financing cost owing to their diverse investor base. If these counties also exhibit strong social connection to counties with large mutual fund capital, then the negative relationship between *Social Proximity to Capital* and municipal financing cost may be mechanical. Of course, we believe such a scenario is unlikely for two reasons. First, we strictly control for both time-varying and time-invariant county characteristics in Table 48, and therefore, our results are based on a within-county effect.

Second, our measure of social connection, SCI, is standardized by the number of Facebook users and therefore constructed independent of the county size (see Equations (1) and (4)). Column (5) corroborates this argument. We find that the effect of social proximity to capital on offering spread is statistically significant in all tercile subgroups sorted on county population, and we fail to reject that a significant difference exists in the magnitude of the effect across the subgroups. This finding suggests that social connection enlarges the investor base of municipal bonds beyond the national recognition.

4.5 Information advantage versus familiarity

In this section, we examine the potential channel through which social connection with mutual fund capital lowers municipalities' bond issuance cost. Specifically, we examine two competing hypotheses: information advantage channel and familiarity channel. If fund managers acquire informational advantages via social connection, all else equal, bonds issued by municipalities with higher social proximity to capital would perform better post issuances. To test this hypothesis, we measure the performance of municipal bonds based on their changes in yield spreads and credit ratings post issuances. We use these two measures as a dependent variable in equation (7).

Table 50 shows that mutual fund managers do not receive informational advantages through social connection. In columns (1) to (3), bonds issued by municipalities with higher social proximity to capital do not perform better in terms of yield spreads. There are no significant changes in yield spread for the period of one to three years post issuances. In addition, columns (4) to (6) corroborate the findings with their credit rating changes post issuances. During the period of five years post issuances, bonds with larger exposures to mutual fund capital through social connection do not experience credit rating upgrades. This outcome suggests that social connection enlarges mutual funds' demand through mere familiarity rather than informational advantage, which is consistent with institutional investors' investment on firms (Kuchler et al., 2022).

4.6 Social proximity and search cost

Our findings so far suggest that municipalities with greater social proximity to mutual funds with large capital experience higher demands for their municipal bonds. If these enlarged demands also facilitate underwriters to resell bonds more easily in the secondary market, the underwriters may set a lower search cost to these bonds. To test this hypothesis, we run the following regression:

$$Gross\ Spread_{i,c,t} = \beta Log\ Social\ proximity\ to\ Capital_{c,t-1} + \gamma_1 X_i$$
$$+\gamma_2 \Gamma_{c,t-1} + \psi_{state(c),t} + \eta_c + \delta_{underwriter(i)} + \varepsilon_{i,c,t} \tag{8}$$

where $Gross\ Spread_{i,c,t}$ is the gross spread of municipal bond i of county c in quarter t, which proxies for the search cost during the underwriting process (Dougal et al., 2019). X_i is a vector of municipal bond characteristics at the issuance level, as explained in Section 2.3. A set of county characteristics, state-by-quarter fixed effects, and county fixed effects, denoted as $\Gamma_{c,t-1}$, $\psi_{state(c),t}$, and η_c, respectively, are the same as those used in Equation (7), while we also add underwriter fixed effects, $\delta_{underwriter(i)}$ to control for the inherent ability of underwriters to search for investors.

Table 51 shows that municipalities with greater social proximity to capital experience a lower search cost. Columns (1) and (2) show that a one-standard-deviation increase in *Social Proximity to Capital* is associated with a 7- to 9-basis-point decrease in gross spread, although the impact is marginally significant at the 10% level. The evidence, however, is more manifested when we compare bank-qualified versus non-bank-qualified bonds. Columns (3) to (6) show that the effect of social proximity to capital is concentrated among non-bank-qualified bonds, while it is muted for bank-qualified bonds. Specifically, columns (5) and (6) show that a one-standard-deviation increase in *Social Proximity to Capital* is associated with an 11- to 13-basis-point decrease in gross spread, which is both statistically and economically significant. Combined with our findings in Section 4.2 that social proximity to capital reduces municipal offering spreads, this finding suggests that municipalities with greater social proximity to capital can even have extra cost savings in issuing bonds by bearing less amount of underwriting fees.

5. Conclusion

Recent studies show that the segmentation of the municipal bond market deteriorates the municipal borrowing cost. We provide evidence that social connection can alleviate such cost by expanding potential investor pools across the United States. Specifically, we investigate how the geographic structure of social networks shapes the allocation of capital to municipalities and thereby contributes to heterogeneous issuance cost of municipal bonds. We find that MBMFs allocate more capital to municipalities with stronger social connection. This familiarity-driven demand broadens investor bases, facilitates risk sharing, and thus lowers the offering spread of municipal bonds. In addition, by facilitating underwriters to resell bonds more easily in the secondary market, the municipalities bear less search cost, which is part of underwriting fees. We thus conclude that municipalities with greater social proximity to capital experience lower financing cost.

Appendix: Variable Definitions

A.1 Mutual fund

- *Social Connectedness Index* – Number of Facebook friends between a pair of two counties, divided by the product of Facebook users in the two counties; see equation (1) [Source: Facebook]
- *Log Social Connectedness* – the log value of *Social Connectedness Index*
- *Log Physical Distance* – the log value 1 + distance between a pair of two counties
- *%Holding* – the percentage of municipal bonds issued by county j, held by the fund i, relative to its total municipal bond holdings; see equation (2) [Source: CRSP Holdings Database]
- *%Holding | Non-zero holdings* – *%Holding* conditional on *%Holding*>0
- *Passive fund* – an indicator equal to one if the fund is classified as index fund [Source: CRSP Mutual Fund Database]
- *TNAs (in $ millions)* – total dollar value (in millions) of a fund's total net assets, aggregated across fund classes [Source: CRSP Mutual Fund Database]
- *Institutional TNAs (in $ millions)* – total dollar value (in millions) of a fund family's total net assets, aggregated across funds in the same family [Source: CRSP Mutual Fund Database]

A.2 Municipal bond variables at tranche-level

- *Benchmark Yield* – the maturity-matched par yield from Municipal Market Advisors AAA-rated curve [Source: Bloomberg]
- *Spread* – the difference between *Offering Yield* and *Benchmark Yield*
- *Change in Yield Spread* – the change in *Spread* during the 1-, 2-, and 3-year period following the bond offering date
- *Change in Credit Rating* – the change in credit rating (AAA = 22, D = 1) during the 5-year period following the bond offering date
- *Upgrade (Downgrade)* – an indicator equal to one if the credit rating is upgraded (downgraded) during the 5-year period following the bond offering date
- *Maturity* – the number of years until maturity [Source: Mergent]
- *Inverse Maturity* – the inverse number of years until maturity
- *Log Size* – the log value of the principal amount of the maturity's original offering [Source: Mergent]
- *GO* – an indicator equal to one if the bond is a general obligation bond [Source: Mergent]
- *Callable* – an indicator equal to one if the bond is callable [Source: Mergent]
- *Insured* – an indicator equal to one if the bond is insured [Source: Mergent]
- *Competitive* – an indicator equal to one if the type of issue sale is competitive [Source:

Mergent]

- *Bank Qualified* – an indicator equal to one if the bond is bank qualified [Source: Mergent]
- *Price Dispersion* – the trade-size value-weighted standard deviation of price changes from customer-to-dealer transactions during the 90-day period following the bond offering date

A.3 Municipal bond variables at issuance-level

- *Gross Spread* – the gross spread in basis points as a fraction of the bond's par value [Source: SDC]
- *Maturity* – the longest maturity in the tranches [Source: SDC]
- *Log Size* – the log value of the total amount of issuance [Source: SDC]

A.4 Demographic variables

- *Log Population* – the log value of the county population [Source: US Bureau of Economic Analysis]
- *Change in Population* – the yearly change in the county population
- *Log Income* – the log value of the county per capita income [Source: US Bureau of Economic Analysis]
- *Unemployment* – the unemployment rate [Source: US Bureau of Labor Statistics]

Appendices

Table 1. Lists of gun lenders and anti-gun banks

This table lists gun lenders and anti-gun banks used in the empirical analysis. Panel A (Panel B) reports the list of gun lenders that received media (no media) attention following the 2018 Parkland shooting. Panel C reports the list of anti-gun banks that implemented anti-gun policies following the 2018 Parkland shooting. See Section 2.4 for detailed definitions of each bank type.

Panel A: Gun Lenders with Media Attention		
Wells Fargo & Co	JPMorgan Chase	U.S. Bancorp
PNC	BB&T	Regions Bank
TD Bank	Citizens Financial Group	Bank of Montreal
Zions First National Bank	People's United Bank	MUFG Bank
Northern Trust Company	Stifel Bank & Trust	Morgan Stanley
Panel B: Gun Lenders without Media Attention		
American Bank & Trust Co	Associated Bank	Bank of the West
Bear State Bank	Busey Bank	Deutsche Bank
First Bank	First Federal Bank	First Guaranty Bank
Midland States Bank	Raymond James Bank	Royal Bank of Canada
Woodforest National Bank		
Panel C: Anti-gun Banks		
Amalgamated Bank	Bank of America	Berkshire Bank
Citibank	Capital One	Fifth Third Bank
First National Bank of Omaha		

Table 2. Summary statistics

This table provides the summary statistics of the main variables used in the empirical analysis. Panel A reports the structure of treatment (gun lenders) and control groups. *# Branches* and *# Counties* are the numbers of branches and their operating counties. Panels B, C, and D report statistics of the bank-branch-year deposit growth sample, the bank-branch-quarter deposit spread sample for 12MCD10K, and two county-year firearms business samples for manufacturers and dealers, respectively, as described in Section 2. Variable definitions are provided in Appendix A.

	All		Treatment (Gun lenders)		Control	
Variable	Mean	Std	Mean	Std	Mean	Std
Panel A: Treatment and Control Groups						
# Branches	58,732		20,673		38,059	
# Counties	1,783		1,783		1,730	
# Branches / # Counties	33		12		22	
Panel B: Bank-Branch-Year Deposit Growth Sample						
Branch deposit growth	0.08	0.18	0.09	0.17	0.08	0.19
Branch deposit (*in $ millions*)	82.03	92.49	91.59	94.02	76.84	91.23
Bank assets (*in $ trillions*)	0.52	0.76	1.04	0.83	0.23	0.54
Bank deposits (*in $ trillions*)	0.34	0.50	0.68	0.51	0.16	0.39
Democrat share (*county*)	0.51	0.18	0.54	0.17	0.49	0.18
Democrat share (*zip*)	0.61	0.20	0.63	0.19	0.60	0.20
HHI	0.17	0.08	0.17	0.08	0.18	0.08
Mass shooting	0.18	0.39	0.23	0.42	0.16	0.37
Boycott NRA	3.38	1.62	3.61	1.67	3.26	1.59
Never Again MSD	3.30	2.10	3.64	2.13	3.12	2.05
Education	0.33	0.11	0.34	0.11	0.32	0.11
Young	0.84	0.04	0.84	0.04	0.84	0.04
Log SCI	8.13	0.94	8.25	1.01	8.06	0.90
Obs. (branch × year)	293,660		103,365		190,295	
Panel C: Bank-Branch-Quarter Deposit Spread Sample						
ΔSpread (*12MCD10K*)	0.05	0.25	0.08	0.25	0.04	0.25
Deposit spread	1.05	0.59	1.58	0.51	1.00	0.57
Democrat share (*county*)	0.39	0.18	0.46	0.17	0.38	0.18
Obs. (branch × quarter)	62,604		6,048		56,556	
Panel D: County-Year Firearms Business Samples (Left: Manufacturer, Right: Dealer)						
# licensees	5.65	11.94	18.11	25.19		
Gun lender loan share	0.27	0.16	0.26	0.17		
Democrat share (*county*)	0.35	0.16	0.33	0.16		
Obs. (county × year)	10,421		15,192			

Table 3. Anti-gun depositor movements and deposit growth of gun lenders

This table tests the effect of anti-gun depositor movements on gun lenders using a difference-in-differences regression with 2018 anti-gun financial activism movements. Specifically, the table reports estimates for the regression specification of equation (1). The dependent variable is *Branch deposit growth*. *Gun lender* is an indicator equal to one if the bank is the gun lender, defined in Section 2.4. *Post* is an indicator equal to one if the year is either 2018 or 2019. Bank controls include *Log Bank assets, Log Bank deposits, Bank asset specialization, Bank type, Branch type,* and *Scandal*. Columns (1) through (4) report the average effect of anti-gun depositor movements. Columns (5) through (8) report the dynamic effect of anti-gun depositor movements by interacting *Gun lender* with time dimension (i.e., × year). Detailed variable definitions are provided in Appendix A. The t-statistics, computed from standard errors clustered at the branch level, are shown in parentheses. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

Dependent Variable	*Branch deposit growth*							
	Average Effect				Dynamic Effect			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Gun lender × Post	-0.009***	-0.009***	-0.010***	-0.010***				
	(-5.78)	(-5.82)	(-6.67)	(-6.74)				
Gun lender × 2015					0.002	0.002	0.003	0.003
					(1.08)	(1.08)	(1.08)	(1.11)
Gun lender × 2016					-0.001	-0.001	-0.003	-0.003
					(-0.33)	(-0.34)	(-1.14)	(-1.15)
Gun lender × 2017					Omitted	Omitted	Omitted	Omitted
Gun lender × 2018					-0.007***	-0.007***	-0.011***	-0.011***
					(-3.29)	(-3.32)	(-4.51)	(-4.53)
Gun lender × 2019					-0.008***	-0.008***	-0.010***	-0.010***
					(-3.76)	(-3.78)	(-4.08)	(-4.12)
Bank controls	No	No	Yes	Yes	No	No	Yes	Yes
Bank FE	Yes	No	Yes	No	Yes	No	Yes	No
Branch FE	No	Yes	No	Yes	No	Yes	No	Yes
County × Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	293,520	293,520	293,520	293,520	293,520	293,520	293,520	293,520
Adj R-Squared	0.051	0.091	0.055	0.093	0.051	0.091	0.055	0.093

Table 4. Anti-gun depositor movements by political value of depositors

This table tests the heterogeneous effect of anti-gun depositor movements by political value of depositors. Specifically, the table reports estimates for the regression specification of equation (2). The dependent variable is *Branch deposit growth*. *Gun lender* is an indicator equal to one if the bank is the gun lender, defined in Section 2.4. *Post* is an indicator equal to one if the year is either 2018 or 2019. *Democrat share* is the proportion of Democrats at the county level, defined in Section 2.5. Bank controls include *Log Bank assets*, *Log Bank deposits*, *Bank asset specialization*, *Bank type*, *Branch type*, and *Scandal*. Column (1) present the result of the full-sample analysis including bank-by-year fixed effects. Columns (2) through (4) present results based on subsamples with respect to *Democrat share*. Blue (red) includes counties whose *Democrat* (*Republican*) *share* is greater than or equal to 70%. Moderate includes all counties except blue and red. Detailed variable definitions are provided in Appendix A. The t-statistics, computed from standard errors clustered at the branch level, are shown in parentheses. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

Dependent Variable	*Branch deposit growth*			
	Full Sample	Sub Sample by Democrat Share		
		Blue	Moderate	Red
	(1)	(2)	(3)	(4)
Gun lender × Democrat share × Post	-0.037***			
	(-3.45)			
Gun lender × Post		-0.031***	-0.007***	0.003
		(-8.44)	(-3.93)	(0.57)
Bank controls	No	Yes	Yes	Yes
Branch FE	Yes	Yes	Yes	Yes
Bank × Year FE	Yes	No	No	No
County × Year FE	Yes	Yes	Yes	Yes
Observations	288,095	44,985	210,200	38,335
Adj R-Squared	0.105	0.161	0.079	0.081

Table 5. Anti-gun depositor movements by political leaning of gun lenders

This table tests the heterogeneous effect of anti-gun depositor movements by political leaning of gun lenders. Specifically, the table reports estimates for the regression specification of equation (3). The dependent variable is *Branch deposit growth*. *High* (*Low*) *gun lender* is an indicator equal to one if the bank is the gun lender and its *Rep PAC share* is above or equal to (below) the median value of 0.637, defined in Section 2.6. *Post* is an indicator equal to one if the year is either 2018 or 2019. Bank controls include *Log Bank assets*, *Log Bank deposits*, *Bank asset specialization, Bank type, Branch type,* and *Scandal*. Column (1) presents the result of the full-sample analysis. Similar to Table 4, columns (2) through (4) present results based on subsamples with respect to *Democrat share*. Difference (*High-Low)* reports the difference of coefficients between *High gun lender* × *Post* and *Low gun lender* × *Post* with its statistical significance. Detailed variable definitions are provided in Appendix A. The t-statistics, computed from standard errors clustered at the branch level, are shown in parentheses. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

Dependent Variable	*Branch deposit growth*			
	Full Sample	Sub Sample by Democrat Share		
		Blue	Moderate	Red
	(1)	(2)	(3)	(4)
High gun lender × *Post*	-0.021***	-0.056***	-0.016***	0.005
	(-9.28)	(-10.73)	(-5.67)	(0.74)
Low gun lender × *Post*	-0.003*	-0.012***	-0.002	0.000
	(-1.79)	(-2.73)	(-0.99)	(0.08)
Difference (*High* - *Low*)	-0.018***	-0.044***	-0.014***	0.005
Bank controls	Yes	Yes	Yes	Yes
Branch FE	Yes	Yes	Yes	Yes
County × Year FE	Yes	Yes	Yes	Yes
Observations	293,520	44,985	210,200	38,335
Adj R-Squared	0.093	0.161	0.079	0.081

Table 6. Anti-gun depositor movements by switching cost

This table tests the heterogeneous effect of anti-gun depositor movements by switching cost. I run a similar regression as in Table 4 but replace *Democrat share* with *HHI*. The dependent variable is *Branch deposit growth*. *Gun lender* is an indicator equal to one if the bank is the gun lender, defined in Section 2.4. *Post* is an indicator equal to one if the year is either 2018 or 2019. *HHI* is county-level Herfindahl-Hirschman Index used as a proxy for switching cost, defined in Section 2.7. *Democrat share* is the proportion of Democrats at the county level, defined in Section 2.5. Bank controls include *Log Bank assets, Log Bank deposits, Bank asset specialization, Bank type, Branch type,* and *Scandal*. Column (1) presents the result of the full-sample analysis including bank-by-year fixed effects. Columns (2) through (4) present results based on subsamples with respect to *HHI*. Low, Moderate, and High are the tercile groups by *HHI*. Detailed variable definitions are provided in Appendix A. The t-statistics, computed from standard errors clustered at the branch level, are shown in parentheses. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

Dependent Variable	*Branch deposit growth*			
	Full Sample	Sub Sample by HHI		
		Low	Moderate	High
	(1)	(2)	(3)	(4)
Gun lender × HHI × Post	0.063***			
	(2.85)			
Gun lender × Post		-0.020***	-0.008***	-0.001
		(-7.73)	(-3.09)	(-0.42)
Bank controls	No	Yes	Yes	Yes
Branch FE	Yes	Yes	Yes	Yes
Bank × Year FE	Yes	No	No	No
County × Year FE	Yes	Yes	Yes	Yes
Observations	288,095	98,555	97,250	97,715
Adj R-Squared	0.105	0.103	0.100	0.074

Table 7. Anti-gun depositor movements by public attitude towards gun controls

This table tests the heterogeneous effect of anti-gun depositor movements by public attitude towards gun controls. I run a similar regression as in Table 4 but replace *Democrat share* with *Mass shooting, Boycott NRA,* or *Never Again MSD*. The dependent variable is *Branch deposit growth*. *Gun lender* is an indicator equal to one if the bank is the gun lender, defined in Section 2.4. *Post* is an indicator equal to one if the year is either 2018 or 2019. *Mass shooting* is an indicator equal to one for counties where at least one public mass shooting occurred during 1999–2018. *Boycott NRA* and *Never Again MSD* are state-level intensities of Google searches "Boycott NRA" and "Never Again MSD" in 2018. *Democrat share* is the proportion of Democrats at the county level, defined in Section 2.5. Bank controls include *Log Bank assets, Log Bank deposits, Bank asset specialization, Bank type, Branch type,* and *Scandal*. Columns (1) through (3) report the heterogeneous effects by *Mass shooting*, *Boycott NRA*, and *Never again MSD*. In column (4), I compare *Mass shooting, Boycott NRA, Never again MSD,* and *Democrat share*. Detailed variable definitions are provided in Appendix A. The t-statistics, computed from standard errors clustered at the branch level, are shown in parentheses. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

Dependent Variable	*Branch deposit growth*			
	MS	NRA	MSD	Comparison
	(1)	(2)	(3)	(4)
Gun lender × Mass shooting × Post	-0.013***			-0.009**
	(-3.28)			(-2.14)
Gun lender × Boycott NRA × Post		-0.004***		-0.002
		(-3.70)		(-1.61)
Gun lender × Never Again MSD × Post			-0.004***	-0.002**
			(-3.78)	(-2.09)
Gun lender × Democrat share × Post				-0.025**
				(-2.23)
Branch FE	Yes	Yes	Yes	Yes
Bank × Year FE	Yes	Yes	Yes	Yes
County × Year FE	Yes	Yes	Yes	Yes
Observations	288,095	288,095	288,095	288,095
Adj R-Squared	0.105	0.105	0.105	0.105

Table 8. Anti-gun depositor movements by social movement engagement and social proximity to Parkland

This table tests the heterogeneous effects of anti-gun depositor movements by social movement engagement and social proximity to Parkland. I run a similar regression as in Table 4 but replace *Democrat share* with *Education*, *Young*, *Log SCI*, or *Log Phy Distance*. The dependent variable is *Branch deposit growth*. *Gun lender* is an indicator equal to one if the bank is the gun lender, defined in Section 2.4. *Post* is an indicator equal to one if the year is either 2018 or 2019. *Education* (*Young*) is the proportion of people with a bachelor's degree or higher (people under age 65) at the county level, defined in Section 2.7. *Log SCI* (*Log Phy Distance*) is the log value of social proximity (physical distance) to Parkland at the county level, defined in Section 2.7. Columns (1) and (2) report the heterogeneous effects by *Education* and *Young*. Columns (3) through (5) report the heterogeneous effect by social proximity to Parkland. Detailed variable definitions are provided in Appendix A. The t-statistics, computed from standard errors clustered at the branch level, are shown in parentheses. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

Dependent Variable	*Branch deposit growth*				
	Education	Young	Social Proximity to Parkland		
	(1)	(2)	(3)	(4)	(5)
Gun lender × *Education* × *Post*	-0.049***				
	(-3.20)				
Gun lender × *Young* × *Post*		-0.107**			
		(-2.48)			
Gun lender × *Log SCI* × *Post*			-0.006***		-0.018***
			(-3.17)		(-4.87)
Gun lender × *Log Phy Distance* × *Post*				0.001	-0.013***
				(0.60)	(-3.66)
Branch FE	Yes	Yes	Yes	Yes	Yes
Bank × Year FE	Yes	Yes	Yes	Yes	Yes
County × Year FE	Yes	Yes	Yes	Yes	Yes
Observations	288,095	288,095	288,095	288,095	288,095
Adj R-Squared	0.105	0.105	0.105	0.105	0.105

Table 9. Pro-gun depositor movements and deposit growth of anti-gun banks

This table tests the effect of pro-gun depositor movements on anti-gun banks. I run similar regressions as in Tables 3, 4, and 7 but replace *Gun lender* with *Anti-gun*. The dependent variable is *Branch deposit growth*. *Anti-gun* is an indicator equal to one if the bank is anti-gun, defined in Section 2.4. *Post* is an indicator equal to one if the year is either 2018 or 2019. *Democrat share* is the proportion of Democrats at the county level, defined in Section 2.5. *Mass shooting* is an indicator equal to one for counties where at least one public mass shooting occurred during 1999–2018. *Boycott NRA* and *Never again MSD* are state-level intensities of Google searches "Boycott NRA" and "Never Again MSD" in 2018. Bank controls include *Log Bank assets, Log Bank deposits, Bank asset specialization, Bank type, Branch type,* and *Scandal*. Column (1) reports the average effect of pro-gun depositor movements on anti-gun banks. Columns (2) through (5) report the heterogeneous effects of pro-gun depositor movements by cross-sectional variables, as specified in each column. Detailed variable definitions are provided in Appendix A. The t-statistics, computed from standard errors clustered at the branch level, are shown in parentheses. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

Dependent Variable	*Branch deposit growth*				
	Baseline	Democrat Share	Public Attitude		
			MS	NRA	MSD
	(1)	(2)	(3)	(4)	(5)
Anti-gun × Post	-0.008***				
	(-3.54)				
Anti-gun × Democrat share × Post		0.053***			
		(3.24)			
Anti-gun × Public Attitude × Post			0.018***	0.008***	0.005***
			(3.55)	(4.50)	(3.87)
Bank controls	Yes	No	No	No	No
Branch FE	Yes	Yes	Yes	Yes	Yes
Bank × Year FE	No	Yes	Yes	Yes	Yes
County × Year FE	Yes	Yes	Yes	Yes	Yes
Observations	293,520	288,095	288,095	288,095	288,095
Adj R-Squared	0.093	0.105	0.105	0.105	0.105

Table 10. Anti-gun depositor movements and deposit spread of gun lender

This table tests whether gun lenders adjust deposit spread to attract depositors. Specifically, the table reports estimates for the regression specification of equation (4). The dependent variable is *Δ Spread*, defined in Section 2.2. Deposit spread is the cost of holding deposits, computed as the federal funds rate minus the deposit rate. *Gun lender* is an indicator equal to one if the bank is the gun lender, defined in Section 2.4. *Post* is an indicator equal to one if the year is either 2018 or 2019. *Democrat share* is the proportion of Democrats at the county level, defined in Section 2.5. *HHI* is county-level Herfindahl-Hirschman Index. Bank controls include *Log Bank assets*, *Log Bank deposits*, *Bank asset specialization*, *Bank type*, *Branch type*, and *Scandal*. Each column reports the effect of anti-gun depositor movements on *Δ Spread* of the deposit product, as specified in the column. 12MCD10K is a 12-month certificate of deposit with an account size of $10,000. Detailed variable definitions are provided in Appendix A. The t-statistics, computed from standard errors clustered at the branch level, are shown in parentheses. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

Dependent Variable	*Δ Spread*			
	12MCD10K	12MCD50K	12MCD100K	24MCD10K
	(1)	(2)	(3)	(4)
Gun lender × Democrat share × Post	-0.053***	-0.061**	-0.052***	-0.061***
	(-2.83)	(-2.19)	(-2.59)	(-2.79)
Gun lender × HHI × Post	0.006	-0.040	-0.012	0.013
	(0.17)	(-0.64)	(-0.29)	(0.31)
Gun lender × Post	0.036***	0.046**	0.039***	0.023
	(3.04)	(2.51)	(2.95)	(1.64)
Bank controls	Yes	Yes	Yes	Yes
Branch FE	Yes	Yes	Yes	Yes
County × Quarter FE	Yes	Yes	Yes	Yes
Observations	52,515	45,832	49,515	50,356
Adj R-Squared	0.545	0.558	0.515	0.439

Table 11. Anti-gun depositor movements and gun industry

This table tests whether the increased funding costs of gun lenders affect the gun industry. Specifically, the table reports estimates for the regression specification of equation (5). The dependent variable is *Log # firearms manufacturers*, defined in Section 2.3. *Gun lender loan share* is the share of small business loans made by gun lenders at the county level in 2017. Democrat share controls include county-level *Democrat share* and its interaction terms with time dimension. County controls include *Log Population*, *Log Per capita income*, *Change in population*, and *Unemployment rate*. Columns (1) and (4) report results of the full-sample analyses. Columns (2), (3), (5), and (6) report results based on subsamples with respect to *Democrat share*. Detailed variable definitions are provided in Appendix A. The t-statistics, computed from standard errors clustered at the county level, are shown in parentheses. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

Dependent Variable	*Log # firearms manufacturers*					
	All	Dem ≥ 50	Dem < 50	All	Dem ≥ 50	Dem < 50
	(1)	(2)	(3)	(4)	(5)	(6)
Gun lender loan share × 2015	-0.031	0.088	-0.044	-0.028	0.091	-0.040
	(-0.54)	(0.60)	(-0.71)	(-0.49)	(0.63)	(-0.65)
Gun lender loan share × 2016	-0.009	0.073	-0.019	-0.011	0.075	-0.019
	(-0.20)	(0.69)	(-0.42)	(-0.27)	(0.72)	(-0.41)
Gun lender loan share × 2017	Omitted	Omitted	Omitted	Omitted	Omitted	Omitted
Gun lender loan share × 2018	-0.029	-0.205**	-0.008	-0.027	-0.195*	-0.007
	(-0.87)	(-2.01)	(-0.23)	(-0.78)	(-1.94)	(-0.20)
Gun lender loan share × 2019	-0.019	-0.025	-0.011	-0.004	-0.017	-0.004
	(-0.37)	(-0.15)	(-0.20)	(-0.08)	(-0.11)	(-0.07)
Democrat share controls	No	No	No	Yes	Yes	Yes
County controls	Yes	Yes	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	10,267	1,830	8,437	10,267	1,830	8,437
Adj R-Squared	0.933	0.958	0.920	0.933	0.958	0.920

Table 12. PAC share of gun lenders

This table reports the donation amounts of political action committees of gun lenders during the 2015–2016 election cycle. *$ Republican* (*Democrat*) is the donation amount to Republican (Democratic) politicians. *Rep PAC share* is the share of donations to Republican politicians.

	$ Republican	*$ Democrat*	*Rep PAC share*
Wells Fargo	539,000	306,700	0.637
JPMorgan Chase	647,750	311,500	0.675
U.S. Bancorp	278,700	166,000	0.627
PNC	304,525	55,700	0.845
BB&T	355,500	20,000	0.947
Regions Bank	293,700	135,500	0.684
TD Bank	61,750	47,500	0.565
Citizens Financial Group	64,500	57,750	0.528
Bank of Montreal	27,000	29,200	0.480
Zions First National Bank	69,300	4,500	0.939
People's United Bank	10,000	6,500	0.606
MUFG Bank	3,500	6,000	0.368
Northern Trust Company	25,500	14,000	0.646
Stifel Bank & Trust	13,300	10,800	0.552
Morgan Stanley	545,000	282,000	0.659
Mean	206,210	89,296	0.658
Median	69,300	47,500	0.637

Table 13. Robustness tests in different samples

This table tests the robustness of the findings in different samples. I run similar regressions as in Table 3 and 4 using four different samples: sample including gun lenders without media attention (columns (1) through (3)), sample excluding Wells Fargo (columns (4) and (5)), sample excluding BB&T (columns (6) and (7)), sample excluding both Wells Fargo and BB&T (columns (8) and (9)). The dependent variable is *Branch deposit growth*. *Gun lender* is an indicator equal to one if the bank is the gun lender, defined in Section 2.4 (regardless of media attention in columns (1) through (3)). *Post* is an indicator equal to one if the year is either 2018 or 2019. (*Non*)*Media* is an indicator equal to one if the bank is the gun lender with (without) media attention. *Democrat share* is the proportion of Democrats at the county level, defined in Section 2.5. Bank controls include *Log Bank assets, Log Bank deposits*, *Bank asset specialization*, *Bank type*, *Branch type,* and *Scandal*. Columns (1), (4), (6), and (8) report the average effect of anti-gun depositor movements on gun lenders. In columns (2), I interact *Gun lender* × *Post* with *Media* and *NonMedia* to estimate the heterogeneous effect by media attention. Columns (3), (5), (7), and (9) report the heterogeneous effect by political value of depositors. Difference (*Media-NonMedia)* reports the difference of coefficients between *Gun lender* × *Media* × *Post* and *Gun lender* × *NonMedia* × *Post* with its statistical significance. Detailed variable definitions are provided in Appendix A. The t-statistics, computed from standard errors clustered at the branch level, are shown in parentheses. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

Dependent Variable	*Branch deposit growth*								
	Sample including Gun lenders without Media Attention			Sample excluding Wells Fargo		Sample excluding BB&T		Sample excluding Wells Fargo and BB&T	
	Baseline	Media	Democrat	Baseline	Democrat	Baseline	Democrat	Baseline	Democrat
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Gun lender × *Post*	-0.010***			-0.009***		-0.016***		-0.016***	
	(-6.45)			(-5.65)		(-10.05)		(-9.54)	
Gun lender × *Media* × *Post*		-0.010***							
		(-6.78)							
Gun lender × *NonMedia* × *Post*		0.005							
		(0.72)							
Gun lender × *Democrat share* × *Post*			-0.038***		-0.023*		-0.040***		-0.026**
			(-3.60)		(-1.95)		(-3.74)		(-2.10)
Difference (*Media* − *NonMedia*)	–	-0.015**	–	–	–	–	–	–	–
Bank controls	Yes	Yes	No	Yes	No	Yes	No	Yes	No
Branch FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Bank × Year FE	No	No	Yes	No	Yes	No	Yes	No	Yes
County × Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	297,450	297,450	292,020	267,695	262,280	286,590	281,160	260,760	255,340
Adj R-Squared	0.092	0.092	0.105	0.092	0.103	0.097	0.110	0.097	0.108

Table 14. Anti-gun depositor movements by political value of depositors (zip code)

This table tests the heterogeneous effect of anti-gun depositor movements by political value of depositors. Specifically, the table reports estimates for the regression specification of equation (2). The dependent variable is *Branch deposit growth*. *Gun lender* is an indicator equal to one if the bank is the gun lender, defined in Section 2.4. *Post* is an indicator equal to one if the year is either 2018 or 2019. *Democrat share* is the proportion of Democrats at the zip code level, defined in Section 2.5. Bank controls include *Log Bank assets*, *Log Bank deposits*, *Bank asset specialization*, *Bank type, Branch type,* and *Scandal*. Column (1) reports the average effect of anti-gun depositor movements on gun lenders with zip-by-year fixed effects. Column (2) presents the result of the full-sample analysis including bank-by-year fixed effects. Columns (3) through (5) present results based on subsamples with respect to *Democrat share*. Blue (red) includes zip codes whose *Democrat (Republican) share* is greater than or equal to 70%. Moderate includes all zip codes except blue and red. Detailed variable definitions are provided in Appendix A. The t-statistics, computed from standard errors clustered at the branch level, are shown in parentheses. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

Dependent Variable	*Branch deposit growth*				
	Full Sample		Sub Sample by Democrat Share		
			Blue	Moderate	Red
	(1)	(2)	(3)	(4)	(5)
Gun lender × *Post*	-0.010***		-0.018***	-0.007***	0.013*
	(-5.62)		(-6.31)	(-2.92)	(1.79)
Gun lender × *Democrat share* × *Post*		-0.028***			
		(-2.77)			
Bank controls	Yes	No	Yes	Yes	Yes
Branch FE	Yes	Yes	Yes	Yes	Yes
Bank × Year FE	No	Yes	No	No	No
Zip × Year FE	Yes	Yes	Yes	Yes	Yes
Observations	253,070	247,300	86,555	149,620	16,895
Adj R-Squared	0.041	0.058	0.060	0.033	-0.006

Table 15. Anti-gun depositor movements by political leaning of gun lenders (zip code)

This table tests the heterogeneous effect of anti-gun depositor movements by political leaning of gun lenders. Specifically, the table reports estimates for the regression specification of equation (3). The dependent variable is *Branch deposit growth*. *High* (*Low*) *gun lender* is an indicator equal to one if the bank is the gun lender and its *Rep PAC share* is above or equal to (below) the median value of 0.637, defined in Section 2.6. *Post* is an indicator equal to one if the year is either 2018 or 2019. Bank controls include *Log Bank assets*, *Log Bank deposits*, *Bank asset specialization, Bank type, Branch type,* and *Scandal*. Column (1) presents the result of the full-sample analysis with zip-by-year fixed effects. Similar to Table 14, columns (2) through (4) present results based on subsamples with respect to *Democrat share*. Difference (*High-Low)* reports the difference of coefficients between *High gun lender* × *Post* and *Low gun lender* × *Post* with its statistical significance. Detailed variable definitions are provided in Appendix A. The t-statistics, computed from standard errors clustered at the branch level, are shown in parentheses. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

Dependent Variable	*Branch deposit growth*			
	Full Sample	Sub Sample by Democrat Share		
		Blue	Moderate	Red
	(1)	(2)	(3)	(4)
High gun lender × *Post*	-0.022***	-0.033***	-0.019***	0.017*
	(-8.54)	(-7.89)	(-5.72)	(1.78)
Low gun lender × *Post*	-0.002	-0.010***	0.002	0.009
	(-0.89)	(-2.87)	(0.83)	(0.86)
Difference (*High* - *Low*)	-0.020***	-0.023***	-0.021***	0.008
Bank controls	Yes	Yes	Yes	Yes
Branch FE	Yes	Yes	Yes	Yes
Zip × Year FE	Yes	Yes	Yes	Yes
Observations	253,070	86,555	149,620	16,895
Adj R-Squared	0.041	0.061	0.034	-0.006

Table 16. Pro-gun depositor movements and deposit growth of anti-gun banks

This table tests the effect of pro-gun depositor movements on anti-gun banks after controlling for the effect of anti-gun depositor movements. I run a similar regressions as in Tables 3, 4, and 7 but add *Anti-gun* and its interaction terms. The dependent variable is *Branch deposit growth*. *Anti-gun* (*Gun lender*) is an indicator equal to one if the bank is anti-gun (the gun lender), defined in Section 2.4. *Post* is an indicator equal to one if the year is either 2018 or 2019. *Democrat share* is the proportion of Democrats at the county level, defined in Section 2.5. *Mass shooting* is an indicator equal to one for counties where at least one public mass shooting occurred during 1999–2018. *Boycott NRA* and *Never again MSD* are state-level intensities of Google searches "Boycott NRA" and "Never Again MSD" in 2018. Bank controls include *Log Bank assets, Log Bank deposits, Bank asset specialization, Bank type, Branch type,* and *Scandal*. Column (1) reports the average effect of pro-gun depositor movements on anti-gun banks. Columns (2) through (5) report the heterogeneous effects of pro-gun depositor movements by cross-sectional variables, as specified in each column. Detailed variable definitions are provided in Appendix A. The t-statistics, computed from standard errors clustered at the branch level, are shown in parentheses. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

Dependent Variable	*Branch deposit growth*				
	Baseline	Democrat Share	Public Attitude		
			MS	NRA	MSD
	(1)	(2)	(3)	(4)	(5)
Anti-gun × *Post*	-0.016***				
	(-6.26)				
Gun lender × *Post*	-0.014***				
	(-8.43)				
Anti-gun × *Democrat share* × *Post*		0.036**			
		(1.98)			
Gun lender × *Democrat share* × *Post*		-0.026**			
		(-2.16)			
Anti-gun × *Public Attitude* × *Post*			0.012*	0.006***	0.003**
			(1.87)	(2.73)	(1.99)
Gun lender × *Public Attitude* × *Post*			-0.009*	-0.002	-0.002*
			(-1.73)	(-1.40)	(-1.94)
Bank controls	Yes	No	No	No	No
Branch FE	Yes	Yes	Yes	Yes	Yes
Bank × Year FE	No	Yes	Yes	Yes	Yes
County × Year FE	Yes	Yes	Yes	Yes	Yes
Observations	293,520	288,095	288,095	288,095	288,095
Adj R-Squared	0.093	0.105	0.105	0.105	0.105

Table 17. Anti-gun depositor movements and small business loans of gun lenders

This table tests the effect of anti-gun depositor movements on gun lenders' small business loans. I run the similar regressions as in Table 3 and 4 but replace the dependent variable *Branch deposit growth* with *Log $ Loans*. *Log $ Loans* is the log value of the amount of small business loans at the county level. *Gun lender* (*Anti-gun*) is an indicator equal to one if the bank is the gun lender (anti-gun), defined in Section 2.4. *Post* is an indicator equal to one if the year is either 2018 or 2019. *Democrat share* is the proportion of Democrats at the county, defined in Section 2.5. Bank controls include *Log Bank Assets, Log # Branches, Capital-asset Ratio, Deposit-asset Ratio, Mortgage-asset Ratio, Business Loan-asset Ratio, ROA, NPL,* and *Scandal*. The control variables are one-year lagged. Detailed variable definitions are provided in Appendix A. The t-statistics, computed from standard errors clustered at the county level, are shown in parentheses. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

Dependent Variable	*Log $ Loans*			
	(1)	(2)	(3)	(4)
Gun lender × Post	-0.153***		-0.159***	
	(-12.57)		(-12.46)	
Anti-gun × Post			-0.031**	
			(-2.51)	
Gun lender × Democrat share × Post		-0.745***		-0.725***
		(-11.65)		(-11.13)
Anti-gun × Democrat share × Post				0.157**
				(2.49)
Bank controls	Yes	Yes	Yes	Yes
Bank × County FE	Yes	Yes	Yes	Yes
Bank × Year FE	No	Yes	No	Yes
County × Year FE	Yes	Yes	Yes	Yes
Observations	822,284	822,274	822,284	822,274
Adj R-Squared	0.524	0.619	0.524	0.619

Table 18. Test of equality (1-to-3 nearest neighbor matching)

This table reports the mean values of matched bank characteristics (*Log # Branches, Log Bank assets, Capital-asset ratio, ROA,* and *Exposure to democrats*) and other bank characteristics (*Cost-to-income, Deposit-asset Ratio, Business Loans-asset Ratio, Mortgage-asset Ratio,* and *NPL*) with the test of equality between treatment and control groups for each variable. Detailed variable definitions are provided in Appendix A. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

	Summary statistics		Test of equality	
Variables	Treatment	Control	Diff	*t*-stat
Matched Bank Characteristics as of 2017				
Log # Branches	6.966	6.486	0.480	0.85
Log Bank assets	19.216	18.580	0.636	1.34
Capital-asset Ratio	0.117	0.119	-0.002	-0.25
ROA	0.905	1.075	-0.170	-1.33
Exposure to democrats	0.571	0.559	0.012	0.38
Other Bank Characteristics as of 2017				
Cost-to-income	1.990	2.790	-0.800	-0.63
Deposit-asset Ratio	0.772	0.785	-0.013	-0.55
Business Loan-asset Ratio	0.150	0.181	-0.031	-0.94
Mortgage-asset Ratio	0.394	0.377	0.017	0.18
NPL	0.011	0.010	0.001	0.39

Table 19. Anti-gun depositor movements in the matched sample – Part I

This table tests the robustness of the findings in the matched sample. I run similar regressions as in Table 3, 4, and 6 using the matched sample. The dependent variable is *Branch deposit growth*. *Gun lender* is an indicator equal to one if the bank is the gun lender, defined in Section 2.4. *Post* is an indicator equal to one if the year is either 2018 or 2019. *Democrat share* is the proportion of Democrats at the county or zip code level, defined in Section 2.5. *HHI* is county-level Herfindahl-Hirschman Index used as a proxy for switching cost, defined in Section 2.7. Bank controls include *Log Bank assets, Log Bank deposits, Bank asset specialization, Bank type, Branch type,* and *Scandal*. Columns (1) and (2) report the average effect of anti-gun depositor movements on gun lenders. Columns (3) through (5) report the heterogeneous effects by political value of depositors and switching cost. Detailed variable definitions are provided in Appendix A. The t-statistics, computed from standard errors clustered at the branch level, are shown in parentheses. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

Dependent Variable	*Branch deposit growth*				
			Democrat Share		
	Baseline		County	Zip	HHI
	(1)	(2)	(3)	(4)	(5)
Gun lender × Post	-0.018***	-0.018***			
	(-8.37)	(-7.07)			
Gun lender × Democrat share × Post			-0.059***	-0.042***	
			(-4.27)	(-3.10)	
Gun lender × HHI × Post					0.178***
					(4.26)
Bank controls	Yes	Yes	No	No	No
Branch FE	Yes	Yes	Yes	Yes	Yes
Bank × Year FE	No	No	Yes	Yes	Yes
County × Year FE	Yes	No	Yes	No	Yes
Zip × Year FE	No	Yes	No	Yes	No
Observations	152,270	134,895	152,270	134,895	152,270
Adj R-Squared	0.081	0.007	0.100	0.031	0.100

Table 20. Anti-gun depositor movements in the matched sample – Part II

This table tests the robustness of the findings in the matched sample. I run similar regressions as in Tables 7 and 8 using the matched sample. The dependent variable is *Branch deposit growth*. *Gun lender* is an indicator equal to one if the bank is the gun lender, defined in Section 2.4. *Post* is an indicator equal to one if the year is either 2018 or 2019. *Mass shooting* is an indicator equal to one for counties where at least one public mass shooting occurred during 1999–2018. *Boycott NRA* and *Never again MSD* are state-level intensities of Google searches "Boycott NRA" and "Never Again MSD" in 2018. *Education* (*Young*) is the proportion of people with a bachelor's degree or higher (people under age 65) at the county level, defined in Section 2.7. *Log SCI* (*Log Phy Distance*) is the log value of social proximity (physical distance) to Parkland at the county level, defined in Section 2.7. Each column reports the heterogeneous effect of anti-gun depositor movements by cross-sectional variable, as specified in the column. Detailed variable definitions are provided in Appendix A. The t-statistics, computed from standard errors clustered at the branch level, are shown in parentheses. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

Dependent Variable	*Branch deposit growth*					
	Public Attitude			Social Engagement		
	MS	NRA	MSD	Education	Young	SCI
	(1)	(2)	(3)	(4)	(5)	(6)
Gun lender × *Mass shooting* × *Post*	-0.020***					
	(-4.14)					
Gun lender × *Boycott NRA* × *Post*		-0.006***				
		(-4.21)				
Gun lender × *Never Again MSD* × *Post*			-0.004***			
			(-3.79)			
Gun lender × *Education* × *Post*				-0.077***		
				(-3.72)		
Gun lender × *Young* × *Post*					-0.175***	
					(-3.36)	
Gun lender × *Log SCI* × *Post*						-0.027***
						(-5.61)
Gun lender × *Log Phy Distance* × *Post*						-0.021***
						(-4.99)
Branch FE	Yes	Yes	Yes	Yes	Yes	Yes
Bank × Year FE	Yes	Yes	Yes	Yes	Yes	Yes
County × Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	152,270	152,270	152,270	152,270	152,270	152,270
Adj R-Squared	0.100	0.100	0.100	0.100	0.100	0.100

Table 21. Anti-gun depositor movements and gun industry

This table tests whether the increased funding costs of gun lenders affect the gun industry. Specifically, the table reports estimates for the regression specification of equation (5). The dependent variable is *Log # firearms dealers*, defined in Section 2.3. *Gun lender loan share* is the share of small business loans made by gun lenders at the county level in 2017. Democrat share controls include county-level *Democrat share* and its interaction terms with time dimension. County controls include *Log Population*, *Log Per capita income*, *Change in population*, and *Unemployment rate*. Columns (1) and (4) report results of the full-sample analyses. Columns (2), (3), (5), and (6) report results based on subsamples with respect to *Democrat share*. Detailed variable definitions are provided in Appendix A. The t-statistics, computed from standard errors clustered at the county level, are shown in parentheses. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

Dependent Variable	*Log # firearms dealers*					
	All	Dem ≥ 50	Dem < 50	All	Dem ≥ 50	Dem < 50
	(1)	(2)	(3)	(4)	(5)	(6)
Gun lender loan share × 2015	0.004	0.067	-0.003	0.001	0.062	-0.007
	(0.19)	(1.14)	(-0.11)	(0.05)	(1.05)	(-0.26)
Gun lender loan share × 2016	0.002	0.052	-0.002	0.002	0.050	-0.005
	(0.12)	(1.15)	(-0.12)	(0.12)	(1.10)	(-0.25)
Gun lender loan share × 2017	Omitted	Omitted	Omitted	Omitted	Omitted	Omitted
Gun lender loan share × 2018	-0.017	-0.098**	-0.009	-0.017	-0.096*	-0.009
	(-1.12)	(-1.99)	(-0.52)	(-1.08)	(-1.95)	(-0.55)
Gun lender loan share × 2019	0.037	-0.157*	0.059**	0.039	-0.154*	0.059**
	(1.37)	(-1.91)	(2.08)	(1.45)	(-1.88)	(2.08)
Democrat share controls	No	No	No	Yes	Yes	Yes
County controls	Yes	Yes	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	15,177	2,348	12,829	15,177	2,348	12,829
Adj R-Squared	0.979	0.990	0.975	0.979	0.990	0.975

Table 22. Robustness to different control banks and clustered standard errors

This table tests the robustness of the result in Table 3 to different control banks and clustered standard errors. The dependent variable is *Branch deposit growth*. *Gun lender* is an indicator equal to one if the bank is the gun lender, defined in Section 2.4. *Post* is an indicator equal to one if the year is either 2018 or 2019. Bank controls include *Log Bank assets, Log Bank deposits, Bank asset specialization, Bank type, Branch type,* and *Scandal*. Panel A presents results using different control banks, as specified in the columns. Panel B presents results using different clustered standard errors, as specified in the columns. In Panel A, the t-statistics, computed from standard errors clustered at the branch level, are shown in parentheses. In Panel B, the t-statistics, computed from standard errors clustered at the specified level, are shown in parentheses. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

Panel A: Different Control Banks					
Dependent Variable	*Branch deposit growth*				
	Baseline	Matching	Excluding Anti-gun Banks	Excluding Community Banks	Excluding (3) & (4)
	(1)	(2)	(3)	(4)	(5)
Gun lender × Post	-0.010***	-0.018***	-0.014***	-0.011***	-0.016***
	(-6.74)	(-8.37)	(-8.10)	(-6.03)	(-7.70)
Controls & Fixed Effects	Bank controls, Branch FE, County × Year FE				
Observations	293,520	152,270	262,040	209,705	178,175
Adj R-Squared	0.093	0.081	0.101	0.075	0.086

Panel B: Different Clustered Standard Errors					
Dependent Variable	*Branch deposit growth*				
	State	County	State-by-Bank	County-by-Bank	State and Year
	(1)	(2)	(3)	(4)	(5)
Gun lender × Post	-0.010***	-0.010***	-0.010**	-0.010***	-0.010**
	(-2.94)	(-4.23)	(-2.14)	(-4.51)	(-4.42)
Controls & Fixed Effects	Bank controls, Branch FE, County × Year FE				
Observations	293,520	293,520	293,520	293,520	293,520
Adj R-Squared	0.093	0.093	0.093	0.093	0.093

Table 23. 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	
Variable	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 24. 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	
Variable	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 25. 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). *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. *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	
Variable	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
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
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 Issuances	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 26. 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	No	No
Bond controls	Yes	Yes	Yes	Yes
County controls	No	Yes	No	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	No	No
Bond controls	Yes	Yes	Yes	Yes
County controls	No	Yes	No	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 27. 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 28. 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	No	No

Bond controls	Yes	Yes	Yes	Yes
County controls	No	Yes	No	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 29. 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 30. 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	No
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 31. 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 32. 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 33. 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 34. The effect of mass shootings on school district fundamentals

This table reports the effect of mass shootings on school district fundamentals. It compares school district issuers 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)
Treatment × *Post*	0.004	0.007	0.007	0.011	0.009	0.007	-0.035	-0.045	-0.045
	(0.72)	(1.32)	(1.41)	(1.44)	(1.40)	(1.19)	(-1.07)	(-1.54)	(-1.59)
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	24,050	29,305	34,246	24,050	29,305	34,246	24,050	29,305	34,246
R-squared	0.109	0.103	0.101	0.095	0.089	0.084	0.050	0.051	0.050

Table 35. 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 36. 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	No	Yes	Yes	No	Yes
Year FE	No	Yes	Yes	No	Yes	Yes
Observations	93	107	92	93	107	92
R-squared	0.669	0.635	0.751	0.747	0.684	0.801

Table 37. 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	No	No
Bond controls	Yes	Yes	Yes	Yes
County controls	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes
Year FE	Yes	No	Yes	No
Year-Month FE	No	Yes	No	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 38. 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	No	No
Bond controls	Yes	Yes	Yes	Yes
County controls	No	Yes	No	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 39. 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 *Log (the number of trading)* or *Log (the amount of trading)*. Column (1) and (2) report the results based on all types of transactions including customer-to-dealer and interdealers. Column (3) and (4) focus on customer-to-dealer transactions, and Column (5) and (6) are based on retail customer-to-dealer transactions. *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	*Trading (Log)*					
	All Transactions		Customer-to-Dealer		Retail Customer-to-Dealer	
	# of trading	$ of trading	# of trading	$ of trading	# of trading	$ of trading
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment × Post	0.003	0.007	0.004	0.008	0.006	0.014
	(0.63)	(0.68)	(0.99)	(0.85)	(1.26)	(0.86)
Benchmark Yield	Yes	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	1,522,330	1,522,330	1,522,330	1,522,330	1,522,330	1,522,330
R-squared	0.259	0.167	0.238	0.160	0.209	0.031

Table 40. The effect of mass shootings on municipal bond secondary market yields and liquidity

This table reports the effect of mass shootings on municipal bond secondary market yields and liquidity. It compares shooting counties and their matched counties in the secondary market around an equal window of 1 quarter (Panel A) or 2 years (Panel B) of the shooting. The dependent variable is either bond yield (*Raw Yield or Tax-adjusted Yield Spread)* or liquidity measure (*Log (the number of trading) or Log (the amount of trading)*. Column (1), (2), (9) and (10) report the results on bond yields. Other columns report the results on liquidity measures as specified in each column. *Treatment* is a dummy variable that equals one if the county experience a mass shooting. *Post* is an indicator variable that equals one if the time is within one-quarter (Panel A) or two-year (Panel B) 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*.

	Panel A: -1 Quarter to +1 Quarter							
Dependent Variable	*Yield*		*Trading (Log)*					
			Investor Buy		Investor Sell		Retail Investor Sell	
	Raw	Tax-adjusted	# of trading	$ of trading	# of trading	$ of trading	# of trading	$ of trading
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treatment × Post	0.023***	0.031***	-0.002	0.003	0.010**	0.113**	0.009**	0.108**
	(4.08)	(2.99)	(-0.29)	(0.18)	(2.17)	(2.36)	(2.23)	(2.07)
Benchmark Yield	Yes	No	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	234,306	234,306	234,306	234,306	234,306	234,306	234,306	234,306
R-squared	0.718	0.411	0.211	0.148	0.246	0.072	0.207	0.046
	Panel B: -2 Years to +2 Years							
	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
Treatment × Post	0.039***	0.060***	0.000	0.006	0.006*	0.029	0.006*	0.054*
	(3.04)	(2.70)	(0.10)	(0.69)	(1.76)	(1.14)	(1.79)	(1.83)
Benchmark Yield	Yes	No	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	0.682	0.396	0.213	0.150	0.252	0.070	0.213	0.043

Table 41. 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

Table 42. Summary statistics

This table presents the summary statistics of the main variables used in our empirical analysis. Panel A reports statistics of county-fund-year pair observations, which are constructed by creating a cartesian product of Municipal Bond Mutual Funds (MBMFs) with reported holdings in a given year, and counties whose municipal bonds that ever appeared in CRSP Mutual Fund Holdings Database. *%Holding* computed as defined in equation (2). Panel B reports statistics of fund-year observations used in Panel A. Panel C reports statistics of municipal bonds used in our analysis, as described in Section 2.3. Variable definitions are provided in Appendix A.

Panel A. Fund-county-year observations										
Variable	N	Mean	Std Dev	5th	10th	25th	50th	75th	90th	95th
% Holding	3,779,994	0.15	1.49	0	0	0	0	0	0	0.47
% Holding | Non-zero holdings	368,277	1.58	4.54	0.03	0.06	0.17	0.49	1.34	3.26	5.64
Log Social Connectedness	3,779,994	7.55	1.05	6.17	6.38	6.81	7.40	8.09	8.83	9.40
Log Physical Distance	3,779,994	6.68	0.88	5.05	5.52	6.27	6.79	7.32	7.68	7.80
Panel B. Fund-year observations										
Variable	N	Mean	Std Dev	5th	10th	25th	50th	75th	90th	95th
Passive Fund	5,826	0.05	0.21	0	0	0	0	0	0	0
TNAs (in $ millions)	5,823	962	2,597	17	32	88	239	797	2,257	4,211
Institutional TNAs (in $ millions)	5,816	15,704	23,274	91	266	1,480	9,132	18,344	37,117	64,882
Panel C. Primary market municipal bond observations										
Variable	N	Mean	Std Dev	5th	10th	25th	50th	75th	90th	95th
Spread (%)	1,277,900	0.41	0.48	−0.12	−0.02	0.13	0.33	0.61	0.93	1.21
Maturity	1,277,900	9.32	6.37	1.04	1.97	4.22	8.21	13.28	18.17	20.32
Bond Size (in $ millions)	1,274,579	2.71	27.55	0.06	0.11	0.24	0.57	1.63	4.88	9.84
General Obligation (GO)	1,277,900	0.60	0.49	0.00	0.00	0.00	1.00	1.00	1.00	1.00
Bank Qualified	1,277,900	0.44	0.50	0.00	0.00	0.00	0.00	1.00	1.00	1.00
Log Social Proximity to Capital	1,277,900	21.18	1.06	19.68	19.93	20.42	21.03	21.84	22.57	23.08
Log Physical Proximity to Capital	1,277,900	6.69	1.21	5.35	5.56	5.86	6.34	7.26	8.26	9.22

Table 43. Social connectedness and mutual funds' municipal bond holdings

This table tests whether mutual funds are more likely to hold bonds of a municipality with which they have a stronger social connection. Specifically, the table reports estimate for the following cross-sectional regression as of year 2019:

$$\%Holdings_{i.j,2019} = \exp\left[\beta Log\ Social\ Connectedness_{i,j} + \gamma X_{i,j,2019} + \theta_{j,2019} + \delta_{i,s(j),2019}\right] \cdot \epsilon_{i,j,2019},$$

where $\%Holdings_{i.j,t}$ is mutual fund i's municipal bond holdings in county j, as defined in equation (2), and $Log\ Social\ Connectedness_{i,j}$ is the log-normalized social connectedness index between fund *i* and county *j*, as defined in equation (1). $X_{i,j,2019}$ is a vector of control variables that include a dummy variable for whether fund advisor *i* and county *j* are located in the same county and for the physical distance between the fund advisor and the county. $\theta_{j,2019}$ and $\delta_{i,s(j),2019}$ are county and fund × state fixed effects, respectively. Parameters are estimates using the PPML approach, as described in Section 3.1. More detailed variable definitions are provided in Appendix A. The *t*-statistics, computed from standard errors double clustered at the fund and county level, are shown in parentheses. *** 1%, ** 5%, * 10% significance.

	(1)	(2)	(3)	(4)	(5)
Log Social Connectedness	0.0486**	0.0773***		0.0951***	0.0940***
	(2.02)	(3.08)		(2.58)	(2.90)
Log Physical Distance			−0.0330	0.0297	
			(−1.40)	(0.83)	
Municipal County FE	Yes	Yes	Yes	Yes	Yes
Fund × Municipal State FE	Yes	Yes	Yes	Yes	Yes
Same County FE	No	Yes	Yes	Yes	Yes
Distance Percentile FE	No	No	No	No	Yes
N Obs	480,717	480,717	481,376	480,861	480,717
Pseudo-R2	0.734	0.734	0.734	0.734	0.735

Table 44. Social connectedness and mutual funds' municipal bond holdings—Cross-sectional sort

This table tests whether the effect of social connection on mutual fund's municipal bond holdings differs with respect to cross-sectional fund and bond characteristics. In Panel A, we run the similar regression as in Table 43, while interacting *Log Social Connectedness* with two fund characteristics: active vs. passive funds (Columns (1) and (2)) and fund institution size (Columns (3) and (4). In Panel B, we run the similar regression as in Table 43 using a subset of municipal bonds based on its characteristics: the dependent variable, $\%Holdings$, is computed using GO bonds and revenue bonds (Column (1) and (2)), and using bonds with a time to maturity of less than or greater than 5 years (Columns (3) and (4)). Parameters are estimates using the PPML approach, as described in Section 3.1. More detailed variable definitions are provided in Appendix A. The *t*-statistics, computed from standard errors double clustered at the fund and county level, are shown in parentheses. *** 1%, ** 5%, * 10% significance.

Panel A. Fund characteristics

	(1)	(2)	(3)	(4)
	Active vs passive funds		Fund institution size	
Log Social Connectedness				
×*1[Active Fund]*	0.0906**	0.0911***		
	(2.39)	(2.77)		
×*1[Passive Fund]*	0.0542	0.0852		
	(0.46)	(0.82)		
×*1[Small Institution]*			0.178***	0.175***
			(3.74)	(4.14)
×*1[Large Institution]*			−0.0288	−0.0189
			(−0.61)	(−0.41)
Log Physical Distance				
×*1[Active Fund]*	0.0222			
	(0.60)			
×*1[Passive Fund]*	−0.0722			
	(−0.75)			
×*1[Small Institution]*			0.0885*	
			(1.93)	
×*1[Large Institution]*			−0.0387	
			(−0.82)	
Municipal County FE	Yes × Split	Yes × Split	Yes × Split	Yes × Split
Fund × Municipal State FE	Yes	Yes	Yes	Yes
Distance Percentile FE	No	Yes × Split	No	Yes × Split
Same County FE	Yes × Split	Yes × Split	Yes × Split	Yes × Split
N Obs	451,500	451,500	385,772	385,792
Pseudo-R2	0.733	0.734	0.731	0.732

Panel B. Bond characteristics

	(1)	(2)	(3)	(4)
	GO	Revenue	Maturity ≤ 5yr	Maturity > 5yr
Log Social Connectedness	0.0503	0.0952***	0.0860*	0.102***
	(0.83)	(2.83)	(1.69)	(2.99)
Fixed Effects	Municipal County, Fund × Municipal State, Distance Percentile, Same County			
N Obs	171,473	357,755	207,438	392,565

Pseudo-R2	0.643	0.721	0.614	0.733

Table 45. Social connectedness and mutual funds' municipal bond holdings—Panel evidence

This table tests the effect of social connection on mutual fund's municipal bond holdings using a panel regression from 2010 to 2019. Specifically, the table reports estimate for the regression specification of equation (3). Columns (1) through (4) report results with similar specifications used in cross-sectional analysis of Table 43, except that all fixed effects are further interacted with time dimension (i.e., × year). In Column (5), we include fund × municipal bond county fixed effects, in which we exploit variation from a subset of funds whose advisors change their locations during the sample period. Parameters are estimates using OLS. More detailed variable definitions are provided in Appendix A. The *t*-statistics, computed from standard errors double clustered at the fund and county level, are shown in parentheses. *** 1%, ** 5%, * 10% significance.

	(1)	(2)	(3)	(4)	(5)
Log Social Connectedness	0.0508***	0.0706***	0.0709***	0.0724***	0.0720*
	(2.70)	(2.84)	(3.17)	(3.12)	(1.84)
Log Physical Distance		0.0315			
		(1.29)			
Municipal County × Year FE	Yes	Yes	Yes	Yes	Yes
Fund × Year FE	Yes	Yes	Yes	No	Yes
Fund × Municipal State FE	Yes	Yes	Yes	No	No
Fund × Municipal State × Year FE	No	No	No	Yes	No
Fund × Municipal County FE	No	No	No	No	Yes
Distance Percentile FE	No	No	Yes	Yes	Yes
Same County FE	Yes	Yes	Yes	Yes	Yes
N Obs	3,779,938	3,779,938	3,779,938	3,777,732	595,801
Pseudo-R2	0.732	0.732	0.733	0.741	0.697

Table 46. Social proximity to capital and mutual fund ownership

This table tests whether a county's social proximity to capital is positively associated with actual MBMFs' aggregate holdings of the county's municipal bonds. Specifically, the table reports estimate for the regression specification of equation (6). The dependent variable is $\%Holdings_{i,c,t}$, which is the percentage of total dollar volume of all MBMFs' holdings of bond *i*, out of total dollar volume outstanding of bond *i* in year *t*. Municipal bond controls include time to maturity, inverse maturity, log of bond size, indicator variables for whether the bond is general obligation, callable, competitive, and insured, and dummy variables for credit ratings and debt types. County controls include log of population, log of per capita income, change in population, and unemployment rate. We include percentile rank fixed effects of physical proximity to capital fixed effects, and state × year fixed effects. Panel B reports results based on subsamples with respect to bond characteristics, as specified in each column. Detailed variable definitions are provided in Appendix A. The *t*-statistics, computed from standard errors double clustered at year and county level, are shown in parentheses. *** 1%, ** 5%, * 10% significance.

Panel A. Full sample analysis

	(1)	(2)	(3)	(4)	(5)	(6)
Log Social Proximity to Capital	1.361***	0.774**	0.863***	0.867***	0.650***	0.448**
	(4.05)	(2.40)	(5.56)	(5.94)	(3.30)	(2.19)
County Controls	Yes	Yes	No	No	Yes	Yes
Municipal Bond Controls	No	No	Yes	Yes	Yes	Yes
State × Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Bond Rating FE	Yes	Yes	Yes	Yes	Yes	Yes
Proximity to Physical Capital Percentile FE	No	Yes	No	Yes	No	Yes
N Obs	5,762,470	5,762,470	5,762,569	5,762,569	5,762,336	5,762,336
Adjusted-R2	0.0701	0.0747	0.126	0.128	0.127	0.129

Panel B. Subsample analysis

	(1)	(2)	(3)	(4)	(5)	(6)
	Bank	Non-Bank	GO	Revenue	Maturity ≤ 5yr	Maturity > 5yr
Log Social Proximity to Capital	0.0248	0.569**	-0.114	0.947***	0.276**	0.550**
	(0.40)	(2.09)	(-0.78)	(2.87)	(2.09)	(2.12)
Municipal Bond & County Controls	Yes	Yes	Yes	Yes	Yes	Yes
Fixed Effects	State × Year, Bond Rating, Proximity to Physical Capital Percentile					
N Obs	2,426,934	3,335,393	3,386,001	2,376,333	2,287,620	3,474,715
Adjusted-R2	0.0459	0.128	0.0637	0.151	0.105	0.142

Table 47. Social proximity to capital and municipal bond yield spread

This table tests whether a county's social proximity to capital is negatively associated with the offering spread of municipal bonds. Specifically, the table reports estimate for the following regression:

$$Spread_{i,c,t} = \beta Log\ Social\ proximity\ to\ Capital_{c,t-1} + \gamma_1 X_i + \gamma_2 \Gamma_{c,t-1} + \psi_{state(c),t} + \eta_c + \varepsilon_{i,c,t}$$

where $Spread_{i,c,t}$ is the offering spread of municipal bond *i* in quarter *t*. X_i and $\Gamma_{c,t-1}$ are characteristics vectors of bond *i* and county *c* respectively, at the end of previous quarter *t-1*. Specifically, X_i includes maturity, inverse maturity, log of bond size, indicator variables for whether the bond is general obligation, callable, competitive, and insured, and dummy variables for credit ratings and debt types. $\Gamma_{c,t-1}$ includes log of population, log of per capita income, change in population, and unemployment rate. We include percentile rank fixed effects of physical proximity to capital, county fixed effects, and state × quarter fixed effects. Detailed variable definitions are provided in Appendix A. The *t*-statistics, computed from standard errors double clustered at year-month and issuance level, are shown in parentheses. *** 1%, ** 5%, * 10% significance.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Log Social Proximity to Capital	−0.0656***	−0.0664**	−0.0607**	−0.0607**	−0.0657**	−0.0762***	−0.0598**	−0.0686**
	(−2.62)	(−2.36)	(−2.43)	(−2.13)	(−2.61)	(−2.65)	(−2.36)	(−2.37)
Bond controls	No	No	No	No	Yes	Yes	Yes	Yes
Municipal County controls	No	No	Yes	Yes	No	No	Yes	Yes
Credit Rating controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Quarter × Municipal State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Municipal County FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Proximity to Physical Capital Percentile FE	No	Yes	No	Yes	No	Yes	No	Yes
N Obs	1,277,879	1,277,879	1,277,838	1,277,838	1,274,557	1,274,557	1,274,516	1,274,516
Adjusted-R2	0.363	0.364	0.363	0.364	0.428	0.428	0.428	0.429

Table 48. Heterogeneous effect of social proximity to capital by ownership and uncertainty

This table tests whether the effect of social proximity to capital on the offering spread of municipal bonds differs with respect to cross-sectional bond characteristics. We run the similar regression as in Table 47 using a subset of municipal bonds based on its characteristics, as specified in each column. Detailed variable definitions are provided in Appendix A. The *t*-statistics, computed from standard errors double clustered at year-month and issuance level, are shown in parentheses. *** 1%, ** 5%, * 10% significance.

	(1)	(2)	(3)	(4)	(5)	(6)
	Bank	Non-bank	GO	Revenue	Maturity ≤ 5yr	Maturity > 5yr
Log Social Proximity to Capital	−0.0169	−0.103**	0.00435	−0.162***	−0.0534**	−0.0864**
	(−0.63)	(−2.09)	(0.16)	(−3.05)	(−2.39)	(−2.47)
Bond controls	Yes	Yes	Yes	Yes	Yes	Yes
Municipal County controls	Yes	Yes	Yes	Yes	Yes	Yes
Credit Rating controls	No	Yes	Yes	Yes	Yes	Yes
Quarter × Municipal State FE	Yes	Yes	Yes	Yes	Yes	Yes
Municipal County FE	Yes	Yes	Yes	Yes	Yes	Yes
Proximity to Physical Capital Percentile FE	Yes	Yes	Yes	Yes	Yes	Yes
N Obs	567,268	707,172	767,931	506,532	377,592	896,904
Adjusted-R2	0.551	0.441	0.452	0.449	0.464	0.433

Table 49. Alternative channels

This table tests whether the effect of social connection on the offering spread of municipal bonds is endogenous, being subject to fundamental risk (Columns (1) and (2)), underwriters (Columns (3) and (4)), or national recognition by investors (Column (5)). In Column (1), we run the similar regression as in Table 47, while adding rating-by-year fixed effects in the subset of municipal bonds with valid credit rating information at the time of their issuances. In Column (2), we use the liquidity measure of municipal bonds, $Price\ Dispersion$, as the dependent variable. In Column (3), we add underwriters fixed effects and in Column (4), we run the regression in the subset of negotiable bonds. In Column (5), we interact *Log Social Proximity to Capital* with the tercile groups by county size. More detailed variable definitions are provided in Appendix A. The t-statistics, computed from standard errors double clustered at year-month and issuance level, are shown in parentheses. *** 1%, ** 5%, * 10% significance.

	Fundamental Risk		Underwriter		County Size
	(1)	(2)	(3)	(4)	(5)
	Spread	Price Dispersion	Spread	Spread	Spread
Log Social Proximity to Capital	−0.0470**	−0.0121	−0.0608**	−0.0894*	
	(−2.09)	(−0.57)	(−2.31)	(−1.72)	
x 1[Small County]					−0.0698**
					(−2.48)
x 1[Medium County]					−0.0768***
					(−2.70)
x 1[Large County]					−0.0662**
					(−2.28)
Bond controls	Yes	Yes	Yes	Yes	Yes
Municipal County controls	Yes	Yes	Yes	Yes	Yes
Credit Rating controls	No	Yes	Yes	Yes	Yes
Credit Rating Available Only	Yes	No	No	No	No
Credit Rating x Year FE	Yes	No	No	No	No
Negotiable Bonds Only	No	No	No	Yes	No
Underwriter FE	No	No	Yes	Yes	No
Fixed Effects	Quarter x Municipal State FE, Municipal County FE, Proximity to Physical Capital Percentile FE				
N Obs	1,060,648	611,633	1,274,433	479,271	1,274,516
Adjusted-R2	0.540	0.187	0.505	0.552	0.429

Table 50. Information Advantage versus Familiarity

This table tests whether social connection provides an information advantage. We run the similar regression as in Table 47, while using a set of different dependent variables: change in yield spread for the period of 1- to 3-year post issuances (Columns (1) through (3)) and change in credit rating for the period of 5-year post issuances (Columns (4) through (6)). Specifically, in Column (4), we use the numeric change in credit rating (AAA = 22; D = 1), and in Columns (5) and (6), we use dummy variables that equal to one if the rating is upgraded and downgraded, respectively. More detailed variable definitions are provided in Appendix A. The *t*-statistics, computed from standard errors double clustered at year-month and issuance level, are shown in parentheses. *** 1%, ** 5%, * 10% significance.

	Change in Yield Spread			Change in Credit Rating		
	(1)	(2)	(3)	(4)	(5)	(6)
	< 1-year	< 2-year	< 3-year	Change	Upgrade	Downgrade
Log Social Proximity to Capital	−0.0232	−0.00315	0.0134	−0.00743	0.00333	−0.0265
	(−0.66)	(−0.10)	(0.40)	(−0.10)	(0.18)	(−1.13)
Bond controls	Yes	Yes	Yes	Yes	Yes	Yes
Municipal County controls	Yes	Yes	Yes	Yes	Yes	Yes
Credit Rating controls	Yes	Yes	Yes	Yes	Yes	Yes
Quarter x Municipal State FE	Yes	Yes	Yes	Yes	Yes	Yes
Municipal County FE	Yes	Yes	Yes	Yes	Yes	Yes
Proximity to Physical Capital Percentile FE	Yes	Yes	Yes	Yes	Yes	Yes
N Obs	104,875	218,613	236,706	1,274,516	1,274,516	1,274,516
Adjusted-R2	0.325	0.270	0.263	0.186	0.217	0.179

Table 51. Social proximity to capital and municipal bond gross spread, with ownership

This table tests whether underwriters set lower search cost for bond issued by more socially connected municipalities. Specifically, the table reports estimate for the following regression:

$$Gross\ Spread_{i,c,t} = \beta Log\ Social\ proximity\ to\ Capital_{c,t-1} + \gamma_1 X_i + \gamma_2 \Gamma_{c,t-1} + \psi_{state(c),t} + \eta_c + \delta_{underwriter(i)} + \varepsilon_{i,c,t}$$

where $Gross\ Spread_{i,c,t}$ is the gross spread of municipal bond *i* of county c in quarter t. X_i is a vector of municipal bond *i*'s characteristics grouped at the issuance-level, including maturity (longest maturity among tranches), inverse maturity, log of bond issuance size, indicator variables for whether the bond is general obligation, callable, competitive, and insured, and dummy variables for credit ratings. $\Gamma_{c,t-1}$ is characteristics vector of county c at the end of previous quarter *t-1*, including log of population, log of per capita income, change in population, and unemployment rate. We include percentile rank fixed effects of physical proximity to capital, county fixed effects, state × quarter fixed effects, and underwriters fixed effects. Columns (1) and (2) report results based on the full sample, while Columns (3) through (6) report results based on subsamples by whether the bond is bank qualified. Detailed variable definitions are provided in Appendix A. The *t*-statistics, computed from standard errors double clustered at sale date and county level, are shown in parentheses. *** 1%, ** 5%, * 10% significance.

	(1)	(2)	(3)	(4)	(5)	(6)
	All		Bank		Non-bank	
Log Social Proximity to Capital	−8.730*	−6.779	0.735	0.288	−12.25**	−10.37*
	(−1.86)	(−1.50)	(0.11)	(0.05)	(−2.05)	(−1.70)
Bond controls	Yes	Yes	Yes	Yes	Yes	Yes
Municipal County controls	Yes	Yes	Yes	Yes	Yes	Yes
Credit Rating controls	Yes	Yes	Yes	Yes	Yes	Yes
Underwriter FE	No	Yes	No	Yes	No	Yes
Quarter × Municipal State FE	Yes	Yes	Yes	Yes	Yes	Yes
Municipal County FE	Yes	Yes	Yes	Yes	Yes	Yes
Proximity to Physical Capital Percentile FE	No	Yes	No	Yes	Yes	Yes
N Obs	56,927	56,882	21,626	21,586	34,530	34,485
Adj-R2	0.444	0.524	0.418	0.485	0.395	0.501

Table 52. Robustness of social connectedness and mutual funds' municipal bond holdings

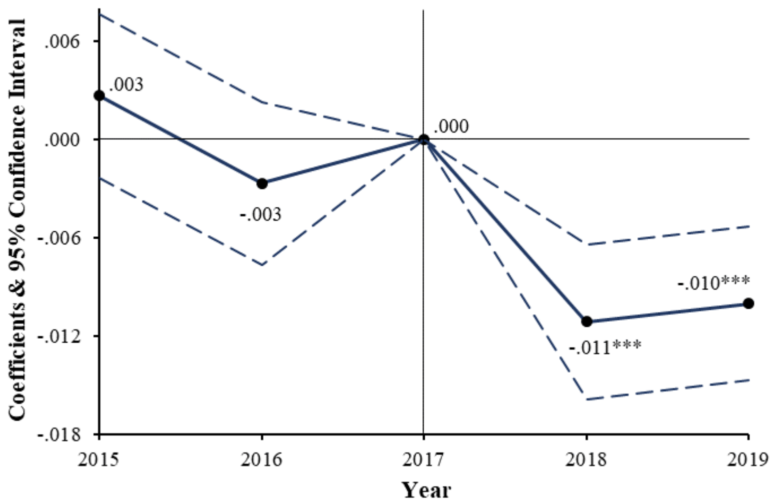
This table shows result of robustness tests of Table 43. Panel A shows results of subsample analyses. Column (1) reports the result from the last column in Table 43 for reference. In column (2), we exclude tri-state (New York, Boston, and Connecticut) and California area municipal bonds, which account for a large number of municipal bonds in our sample. In column (3), we exclude mutual funds whose advisors are located in three counties (New York, Chicago, Boston) that account for the location of 50% advisors in our sample. In the last column, we exclude municipal bonds whose percentage of municipal bond holdings relative to its TNA is less than 90%. In Panel B, we winsorize the dependent and main explanatory variables, or use alternative denominator when computing our *%Holdings* measure. Specifically, *%Holding(TNA)* uses a fund's total net asset (TNA) as a denominator, and *%Holding(no location)* uses a fund's holding of municipal bonds including those without location information. The *t*-statistics, computed from standard errors double clustered at the fund and county level, are shown in parentheses. *** 1%, ** 5%, * 10% significance.

Panel A. Subsample analysis				
	(1)	(2)	(3)	(4)
		Subsample		
	Original specification	Exclude tri-state municipal bonds	Exclude top 3 advisor location	Exclude municipal holding < 90%
Log Social Connectedness	0.0940***	0.105***	0.107**	0.0955***
	(2.90)	(2.65)	(2.36)	(3.03)
Fixed effects	Municipal County, Fund × Municipal State, Distance Percentile, Same County			
N Obs	480,717	431,685	226,135	444,072
Pseudo-R2	0.735	0.712	0.717	0.741

Panel B. Winsorization and alternative holding measures					
	(4)	(5)	(6)	(7)	(8)
	Winsorization			Alternative holding measures	
	%Holding	*Log Social Conn.*	*%Holding Log Social Conn.*	*%Holding (TNA)*	*%Holding (no location)*
Log Social Connectedness	0.105***	0.0809**	0.129***	0.0759**	0.0839**
	(3.14)	(2.14)	(3.60)	(2.25)	(2.50)
Municipal County FE	Municipal County, Fund × Municipal State, Distance Percentile, Same County				
N Obs	480,717	480,717	480,717	480,717	480,717
Pseudo-R2	0.542	0.735	0.542	0.730	0.728

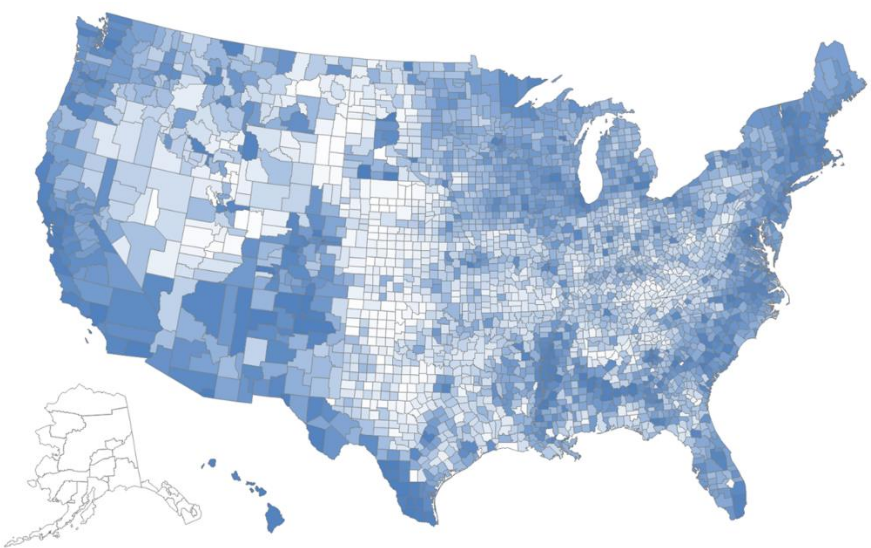
Figures

Figure 1: Dynamic Impact of Anti-gun Depositor Movements



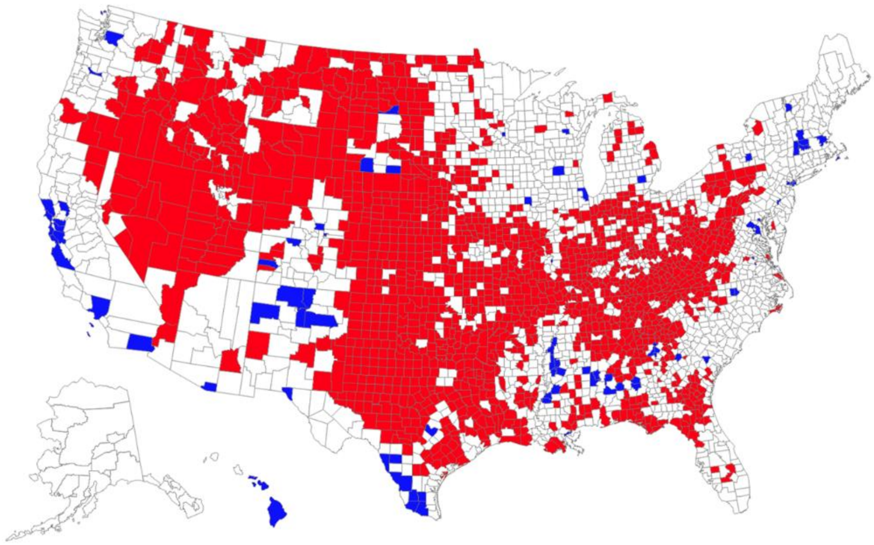
Note: The y-axis represents the difference in deposit growth between gun lenders and control banks.

Figure 2.A: 2016 Presidential Election Results by County



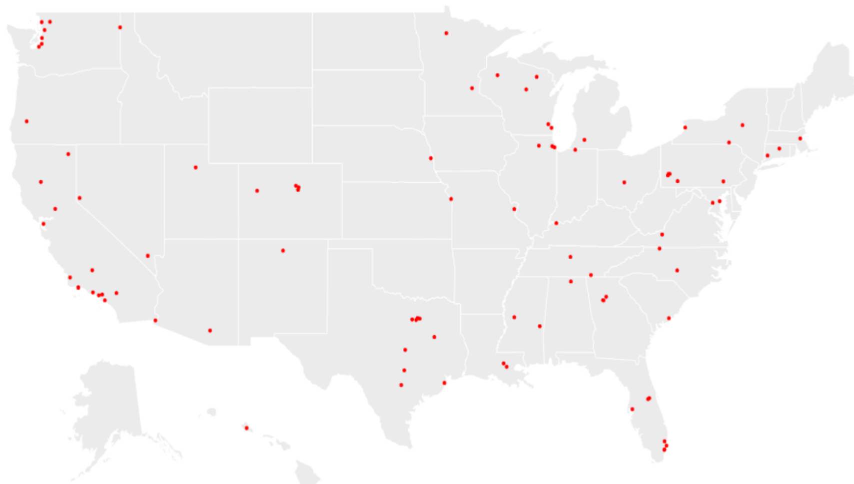
Note: This figure illustrates the county-level vote shares for Hillary Clinton. Darker blue indicates higher share. No information is available for Alaska at the county level.

Figure 2.B: Blue and Red Counties



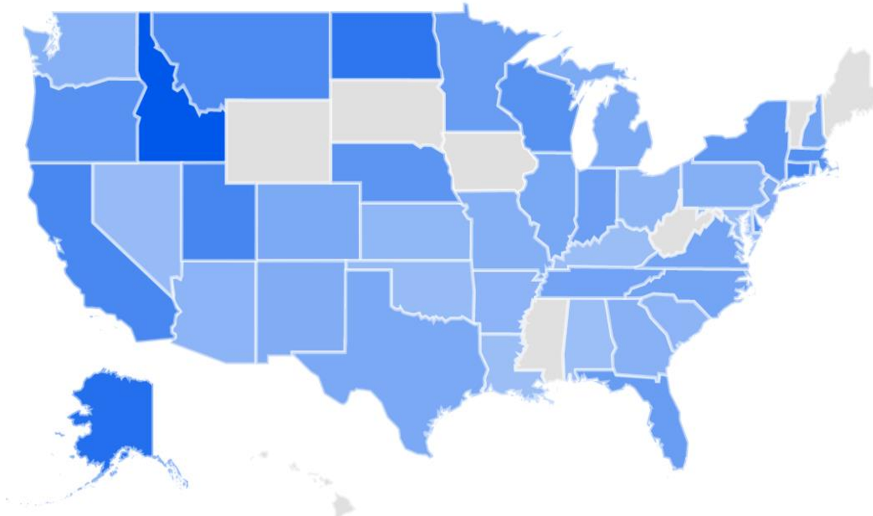
Note: This figure illustrates Democrat- and Republican-leaning counties. Blue indicates a Democrat share greater than or equal to 70%; red indicates a Democrat share less than or equal to 30%. No information is available for Alaska at the county level.

Figure 3: Map of Public Mass Shootings



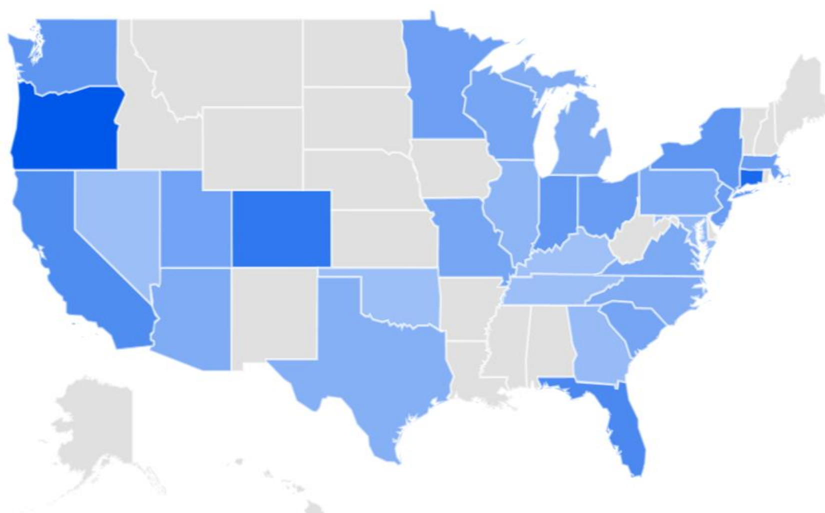
Note: Red dots indicate a US county with at least one public mass shooting during 1999–2018.

Figure 4.A: Google Trends: "Boycott NRA"



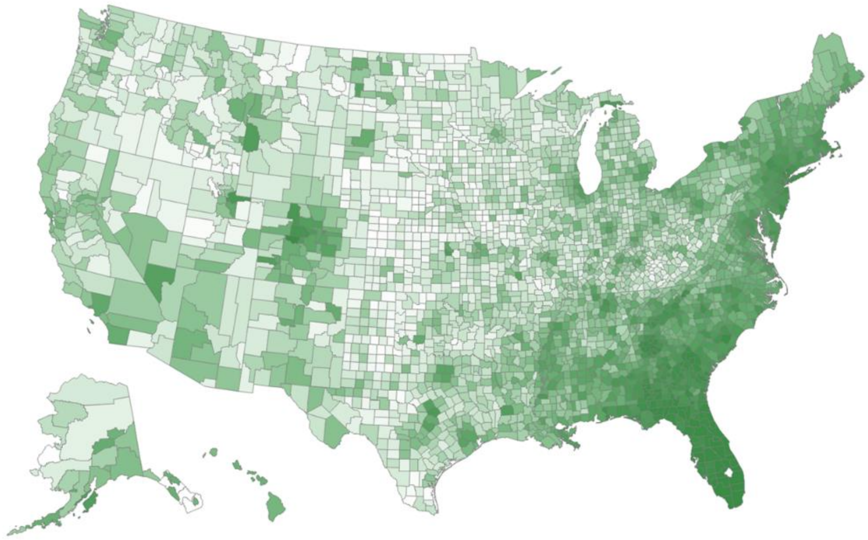
Note: This figure illustrates state-level intensity of Google searches for "Boycott NRA" in 2018. Darker blue indicates higher intensity.

Figure 4.B: Google Trends: "Never Again MSD"



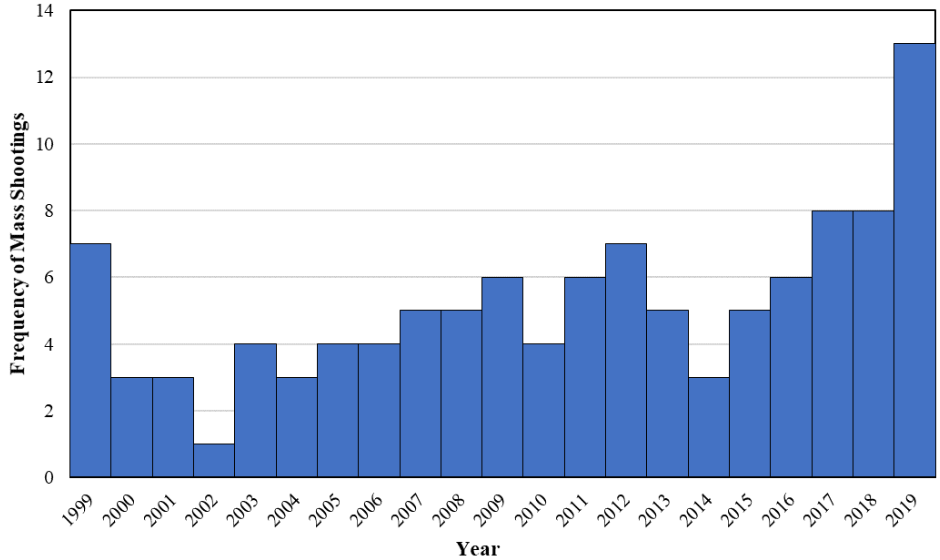
Note: This figure illustrates state-level intensity of Google searches for "Never Again MSD" in 2018. Darker blue indicates higher intensity.

Figure 5: Social Proximity to Parkland in Broward County, FL



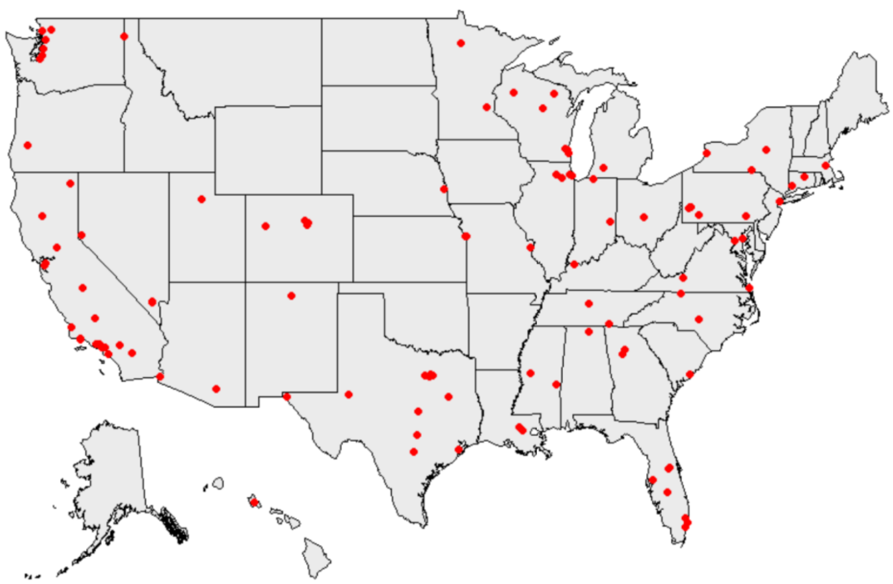
Note: This figure shows a heat map of U.S. counties' social proximity to Parkland in Broward County, FL. Darker green indicates stronger connectedness. [*Source: Facebook*]

Figure 6: Mass Shootings (1999 – 2019)



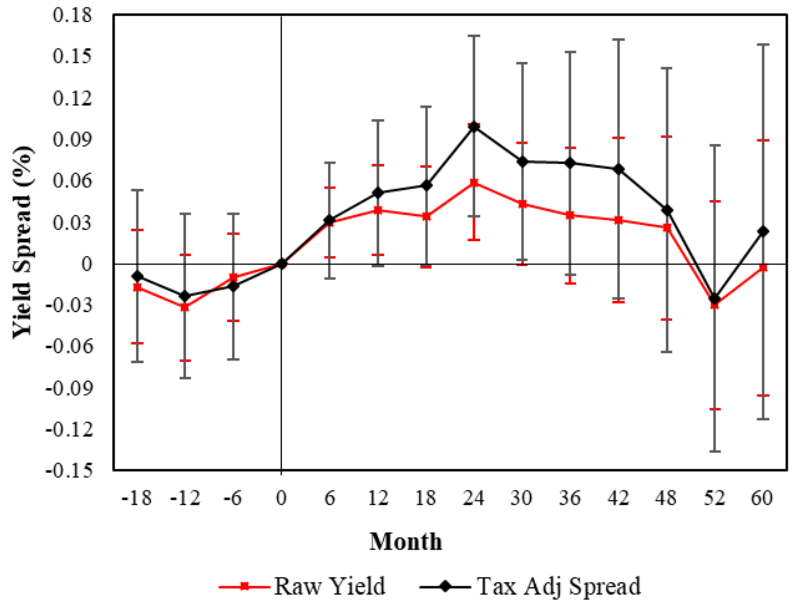
Note: The figure plots the number of mass shootings from 1999 to 2019.

Figure 7: Map of Mass Shooting Counties across the United States



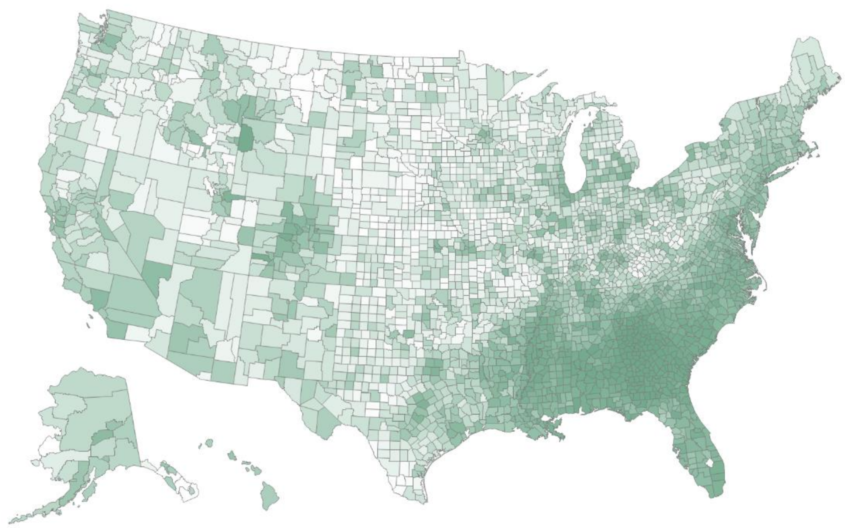
Note: According to the Washington Post, there are 88 counties in which at least one mass shooting occurred during 1999 – 2019. The red dots indicate mass shooting counties.

Figure 8: Dynamic Impact of Mass Shootings

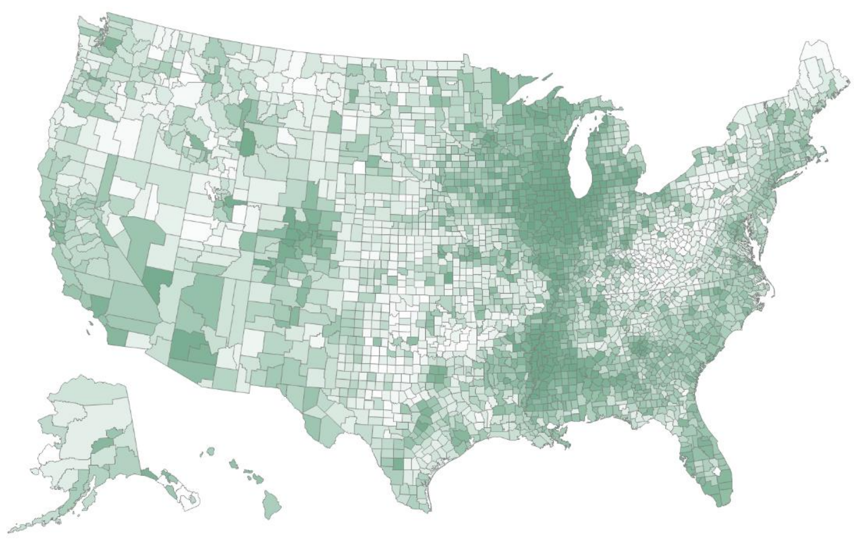


Note: The y-axis represents the difference in yield spread between treated and control counties.

Figure 9. Heatmap of the social connectedness index



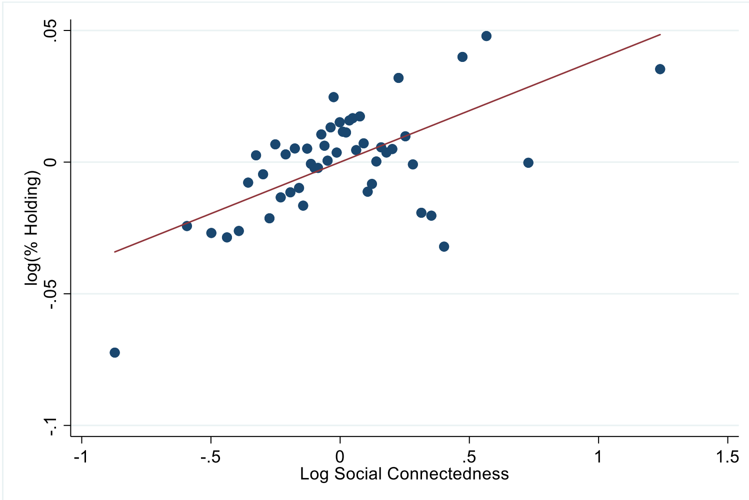
Panel A. Fulton County, Georgia (Atlanta)



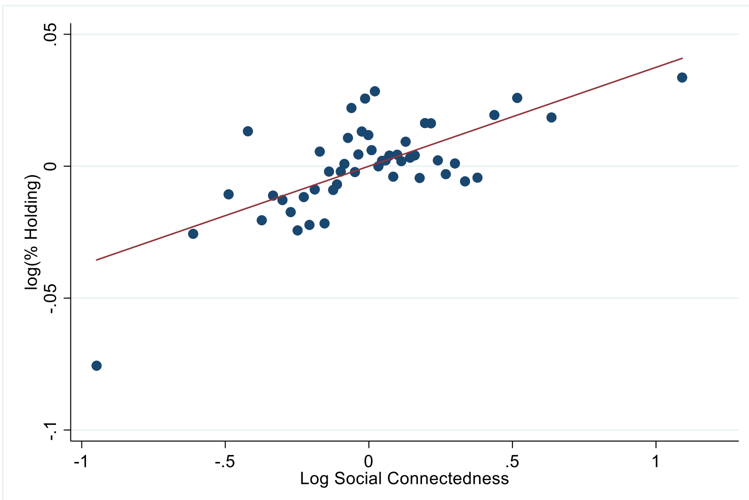
Panel B. Cook County, Illinois (Chicago)

Note: This figure shows heat maps of U.S. counties' social connectedness index (SCI) to Fulton County, Georgia (Panel A) and Cook County, Illinois (Panel B). SCI is provided by Facebook and computed as shown in equation (1). Darker green counties indicate higher SCI with the focal county.

Figure 10. Linear relationship between *log(% Holding)* and *Log Social Connectedness*



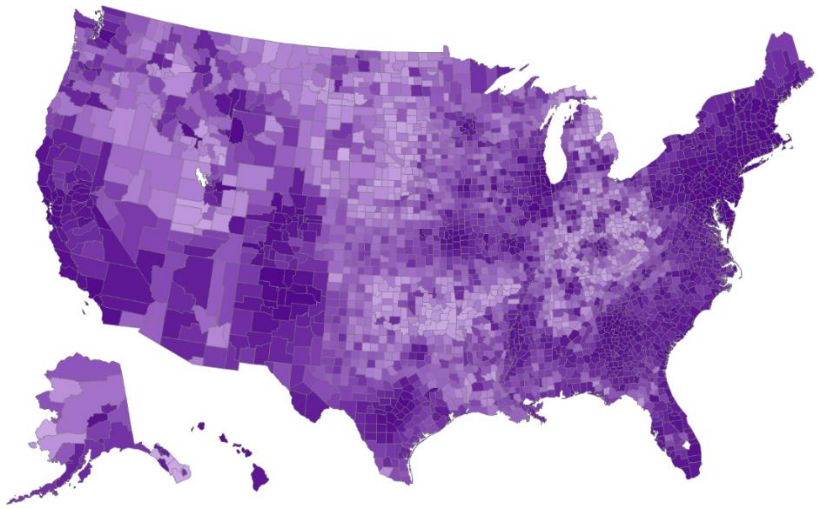
Panel A. Without distance percentile fixed effect



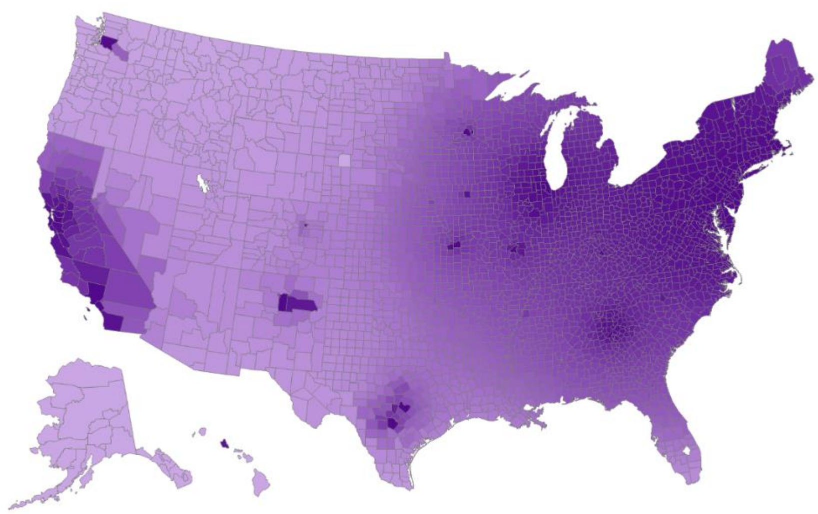
Panel B. With distance percentile fixed effect

Note: We first standardize log(% Holding) and Log Social Connectedness to have a mean of 0 and standard deviation of 1. County-fund-year observations with non-zero holdings are then sorted into 50 bins based on the standardized Log Social Connectedness. Within each bin, we plot the conditional mean of standardized log(% Holding) and Log Social Connectedness. We condition on fund × state × year fixed effect, county × year fixed effect, and same county fixed effect. In Panel B, we further condition on distance (between fund county i and bond county j) percentile fixed effect.

Figure 11. Heatmap of proximity to capital



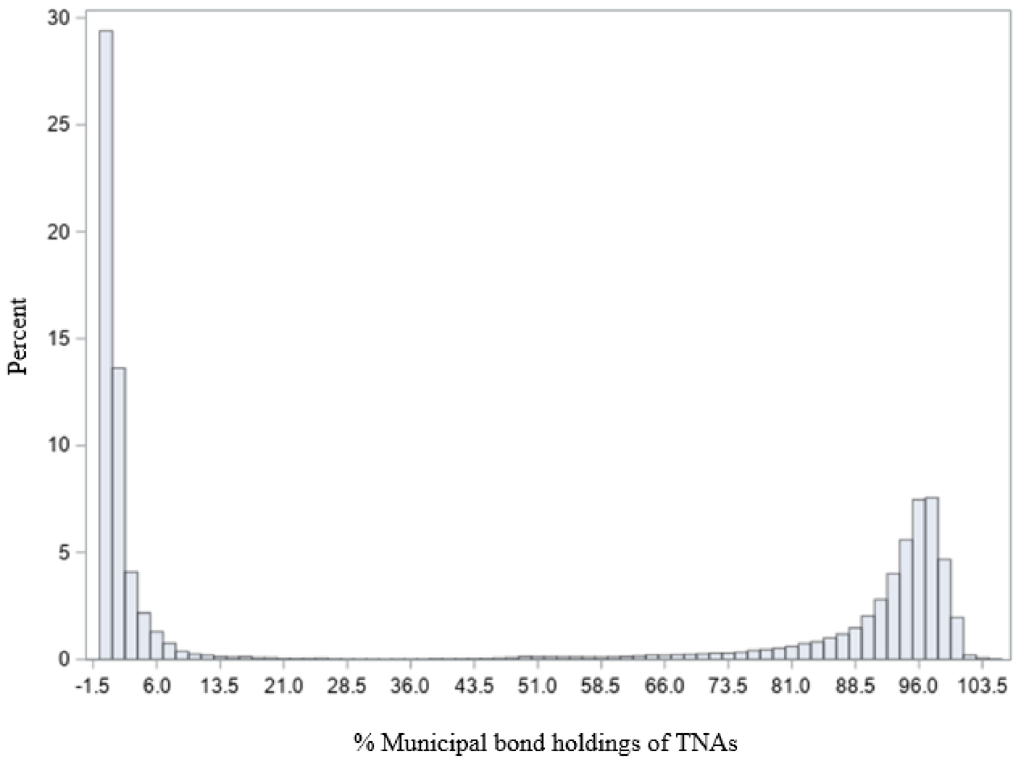
Panel A. Social proximity to capital



Panel B. Physical proximity to capital

Note: This figure shows heat maps of U.S. counties' social proximity to capital (Panel A) and physical proximity to capital (Panel B). The two measures are computed as shown in equation (4) and (5), respectively. Darker purple counties indicate a greater value for each measure.

Figure 12. Distribution of percentage of municipal bond holdings



Note: This figure shows the distribution of percentage of municipal bond holdings of all mutual funds in CRSP Holdings that ever held at least one municipal bonds. The horizontal axis shows percentage of municipal bond holdings relative to the fund's TNA.

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