Two Essays on Corporate Finance, Banking, and Political Economy

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Abstract

The dissertation consists of two essays on corporate finance, banking, and political economy. The first chapter studies how partisan-driven views about climate change affect institutional investors' investment in assets that are exposed to climate risk. The second chapter examines how unexpected political chaos can affect politically active companies in a negative way.

In "Climate Change, the Partisan Divide, and Exposure to Climate Risk", I study how partisan-driven beliefs about climate change affect the distribution of climate risk across mortgage lenders. Using wildfires to capture climate exposure, I find that Republican-leaning lenders are more likely to approve mortgage applications in high wildfire risk areas than Democratic-leaning lenders. This difference in approval rates is only evident among second-lien and jumbo mortgage applications, highlighting how securitization affects risk-taking incentives. Lastly, Republican-leaning lenders

originate more climate-exposed second-lien and jumbo loans and thus hold more wildfire risk. The findings suggest that dispersion over climate change beliefs affects how institutional investors hold climate risks, potentially affecting financial stability.

In "Downsides of Corporate Political Spending: Evidence from Mass Shootings", I study the negative impacts of corporate political spending on firm outcomes. Using data from 20 years of mass shootings, I find that when mass shootings take place, companies that primarily donate to pro-gun-rights politicians experience negative stock price reactions and worse operating performance. The negative impacts on companies' bottom line are stronger when incidents are deadlier. The decline in operating performance reverses within a couple of years. The findings are not driven by firms contributing to Republican politicians. Similarly, using Summary of Deposits data from FDIC, I find that banks primarily donating to pro-gun-rights politicians also experience higher deposit outflows around mass shootings. After incidents, firms significantly reduce corporate political donations to pro-gun-rights politicians. Overall, my findings highlight negative impacts on companies resulting from their political spending being disapproved by stakeholders.

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Chapter One

Climate Change, the Partisan Divide, and Exposure to Climate Risk

1.1 Introduction

Expectations about climate risk can affect investment decisions. Investors who are more concerned about climate change may view climate-change-exposed assets, such as those exposed to wildfire risk, as expensive and thus negative NPV. However, more optimistic investors can view the same assets as profitable. Disagreement over climate risks can thus lead to self-sorting among institutional investors in holding climate risk. While many papers study how climate risk is priced in the financial market, there is little evidence on the distribution of climate risk in the financial system, which has important implications for understanding how climate change could affect the stability of the financial system (Giglio et al., 2021).[1] This paper attempts to fill this gap.

I study the mortgage market and find that the partisan divide over climate risk expectations affects lenders' investment decisions. Partisan identity has been shown to correlate with many dimensions of investor beliefs, including those about climate change.[2] In fact, studies show that Republicans are more optimistic and less concerned about climate change than Democrats (Dunlap et al., 2016; Baldauf et al.,

[1]Swiss Re Group, the world's largest reinsurance company, estimates that by 2050, climate change will cost approximately 10% of the world's total economic value if it stays on the currently-anticipated trajectory.

[2]See, e.g., Gerber and Huber (2009), Cookson et al. (2020), and Allcott et al. (2020).

2020). Similarly, surveys find an increasing partisan divide on the climate change issue (see Figure 1.1). Thus, I use mortgage lenders' political preferences — as measured by the fraction of political contributions from their political action committees (PACs) to Republican politicians (*REP Donation%* henceforth) — to capture their optimism about climate risk.[3] I find that Republican-leaning lenders are more likely to approve mortgage applications in high wildfire risk areas. These effects only exist among high-risk second-lien mortgages and hard-to-securitize jumbo mortgage applications. Republican-leaning lenders also receive more mortgage applications and originate more mortgage loans than Democratic-leaning lenders in high fire risk areas, suggesting that financial institutions with more optimistic views about climate change hold more climate-change-exposed assets in their investment portfolios.

The primary empirical challenge is to measure climate risk. Some studies rely on historical natural disasters to make inferences about future climate risks. Historical disasters, however, do not necessarily predict future risks.[4] Moreover, not all adverse weather events can be directly attributed to climate change. In this paper, I use the Wildfire Hazard Potential (WHP) map, obtained from the U.S. Forest Service, to construct a forward-looking wildfire hazard measurement. The WHP map depicts the potential for future wildfires that would be difficult to contain in the continental United States. When combined with the locations of mortgages' underlying properties, the map provides direct estimations on mortgages' exposure to wildfire risk.

Several features make wildfire risk an appealing representation of climate risk. First, the relationship between climate change and wildfires is straightforward: global warming increases heat and creates drier conditions, making it easier for wildfires to spread and harder for them to be contained. Figure 1.2 shows that the correlation between wildfires and global temperature anomaly reaches approximately 60%.[5]

[3]To validate the *REP Donation%* measurement, I rely on the firm-level climate change exposure constructed by Sautner et al. (2020) and find that Republican lenders are indeed less likely to mention climate risk and express more positive sentiment about climate change on conference calls.

[4]Ramsay (2017) shows that a remarkably consistent number of tropical cyclones (both hurricanes and tornadoes) are formed each year, indicating a less-decisive relationship between climate change and the formation of tropical cyclones. For similar discussions, see "What We Know About Climate Change and Hurricanes," *The New York Times*, August 29, 2021.

[5]The data on wildfire acres is obtained from the National Interagency Fire Center, and the data on global temperature anomaly is taken from the National Oceanic and Atmospheric Administration.

Second, both the threat of wildfires and the losses incurred by those wildfires have continued to grow.[6] As shown in Figure 1.2, the number of acres burned in the United States has increased fivefold over the last 40 years, reaching 10 million acres in 2020 (twice the land area of Massachusetts).[7] CoreLogic, a property intelligence company, estimates that, nationwide, more than $638 billion worth of single-family residences are at risk from wildfires. Third, wildfires represents one of the least insured natural disasters - insurance companies sometimes cancel existing policies and charge higher premiums in fire-prone regions.[8] In addition, Issler et al. (2020) show that mortgage delinquency rate increases significantly after exposures to wildfires. Fourth, wildfires pose more immediate threats to the economy. Conversely, the impacts of other types of climate change, such as sea-level rise, will take decades to be fully realized.

In the main analysis, I estimate regressions by interacting *REP Donation%* with mortgage applications' exposure to wildfire risk. Lenders' mortgage issuance decisions depend on a variety of factors, such as borrowers' credit risk, local economic conditions, etc.[9] For identification, I first control for time-varying local economic conditions (e.g., employment rate, real estate price, mortgage demand) by including county-year fixed effects. Moreover, the time-invariant unobserved heterogeneities (e.g., loan officer leniency, local enrollments in higher education) across lender branches can also impact lenders' mortgage issuances decisions. Therefore, I further control for lender-county fixed effects. Lastly, I control for a battery of loan and borrower characteristics. The empirical strategy thus compares mortgage issuance decisions by lenders with different climate-risk beliefs in the same county and year, while at the same time controlling for borrower characteristics, loan characteristics, and fixed differences between lender-county pairs. Furthermore, I conduct alternative analyses based on staggered difference-in-differences tests using historical wildfires as natural experiments.

[6]The estimated total loss from the 2018 California wildfires alone reached approximately $150 billion, Wang et al. (2021). As a result of the 2018 California wildfires, Merced Property & Casualty Co, an insurance company, and Pacific Gas & Electric Corp., California's largest utility company, both filed for bankruptcy

[7]Internationally, we also saw more wildfires burning in Australia, Canada, Greece, Turkey, the Amazon rainforest, and even Siberia.

[8]See "“Insurers dropped nearly 350,000 California homeowners with wildfire risk", *The Sacramento Bee*, August 20, 2019; "Many Californians Being Left Without Homeowners Insurance Due to Wildfire Risk", *Insurance Journal*, December 4, 2020.

[9]Studies also find that local real estate markets depend on residents' beliefs about climate change (Baldauf et al., 2020; Bernstein et al., 2020).

My first key finding is that Republican-leaning lenders have a significantly higher approval rate of the mortgage applications than Democratic-leaning lenders in high wildfire risk areas. Importantly, the difference in mortgage approval rates only exists among mortgage applications that are less securitizable after originations (second-lien or jumbo), indicating that lenders have little incentive to price climate risk for mortgages that won't stay in their portfolios. The effects are also economically sizable. Compared with low fire risk areas (25th percentile), a one-standard-deviation increase in *REP Donation%* in high fire risk areas leads to an approximately 3.44% (0.47%) higher approval rate of second-lien (jumbo) mortgage applications in high wildfire risk areas (75th percentile). On the contrary, in low-risk areas, a one-standard-deviation increase in *REP Donation%* is associated with statistically insignificant changes in mortgage approval rates. Moreover, the effects are larger for areas exposed to higher fire risk (i.e., those in the 80th or 90th percentile).

The findings help to rule out several alternative explanations. First, mortgage lenders' mortgage approval decisions might depend on their partisan perception of economic outlooks (Kempf and Tsoutsoura, 2021; Dagostino et al., 2020). This explanation is unlikely because it does not explain the statistically insignificant results in low wildfire risk areas. The second alternative explanation is a political favor story: Republican-leaning lenders might be connected with Republican politicians, and these lenders might approve more mortgage applications in Republican-leaning electoral areas to help Republican incumbents get re-elected, as found in Bertrand et al. (2018). This is also unlikely, because wildfire risk is not distributed in a partisan way. Moreover, I split the sample based on counties' Republican vote share in the 2012 presidential election and show that the effects exist in both "red" and "blue" regions. The third alternative story is related to lenders' general risk tolerance. Lenders may have exactly the same climate risk perceptions, but Republican-leaning lenders may have a higher risk tolerance and thus are more willing to invest in risky assets. I examine lenders' general risk tolerance, and the findings suggest that this explanation does not hold. To further support the climate risk belief interpretation, I provide two additional pieces of evidence. Republican-leaning lenders are more likely to hold mortgage loans originated in fire-prone areas, and they are also less likely to deny mortgage applications for collateral-related reasons, indicating that Republican-leaning lenders are more optimistic towards mortgage underlying properties exposed to fire risk.

I proceed to show that Republican-leaning lenders' more optimistic lending policies bring them more mortgage applications in high wildfire risk areas. Even if mortgage applications exposed to high fire risks are rejected by Democratic-leaning lenders, borrowers can still file mortgage applications with Republican-leaning lenders.[10] As a result, Republican-leaning lenders receive more mortgage applications in high fire risk areas. Indeed, I find that although lenders charge similar interest rates, Republican-leaning lenders receive a higher number of mortgage applications in high fire risk areas than Democratic-leaning lenders. Compared with low-risk areas (25th percentile), a one-standard-deviation increase in *REP Donation%* is associated with an approximately 5.72% (2.15%) increase in the number of second-lien (jumbo) mortgage applications from high wildfire risk areas (75th percentile).

These findings show that in high wildfire risk areas, optimistic Republican-leaning lenders not only have a higher approval rate but also receive more mortgage applications than pessimistic Democratic-leaning lenders. Taken together, these two findings suggest that Republican-leaning lenders originate more mortgages in the high wildfire risk areas. Further tests confirm this point. Relative to low wildfire risk areas (25th percentile), a one-standard-deviation increase in *REP Donation%* is associated with a 9.3% (2.9%) higher total amount of originated second-lien (jumbo) mortgages in high wildfire risk counties (75th percentile), highlighting that optimistic lenders hold more wildfire risks in their portfolio. The effects remain insignificant in low fire risk areas. In dollar terms, a one-standard-deviation increase in lenders' *REP Donation%* is associated with a $178 million approximate increase in their nationwide originations and holdings of second-lien and jumbo mortgages.

Given that the true risk parameters of wildfires are unknown, it's almost impossible for one to find the ex-ante optimal lending policies. The lack of data on loan performance of second-lien and jumbo loans represents another limitation. In this paper, I rely on the single-family loan performance data from Fannie Mae and Freddie Mac to examine the delinquency rates of mortgages after wildfires. Since the data

[10]More optimistic Republican-leaning lenders may also advertise more in high wildfire risk areas.

only covers conforming loans, it's unlikely that any difference will be found in delinquency rates between mortgages originated by Republican-leaning and Democratic-leaning lenders after wildfire incidents. Indeed, the findings using single-family loan performance data confirm this point. Similar to Issler et al. (2020), the delinquency test does show that mortgage delinquency rates increase significantly after wildfires. Therefore, it can be inferred that Republican-leaning lenders bear more losses after wildfires since they originate more second-lien and jumbo loans in high wildfire risk areas. Again, due to data limitations, the short-term trade-off between benefits from a higher market share and the costs from higher mortgage defaults is unclear. Lastly, large-scale wildfires represent tail risks, and wildfires are expected to accelerate with greater severity in the future. The likelihood of tail events also changes depending on how climate change evolves. In other words, the current optimal lending policy won't be the same as the long-term optimal lending policy.

This study is, to the best of my knowledge, the first to identify how the disagreement over climate risk beliefs lead to self-sorting among institutional investors in holding climate-exposed assets. Many empirical studies in the climate finance literature focus on documenting how climate risk is priced in the financial market while treating financial investors with homogenous climate risk concerns.[11] However, survey evidence (Ilhan et al., 2020; Stroebel and Wurgler, 2021; Bresnahan et al., 2021) shows sizable dispersions among financial investors. My study explores the heterogeneity of financial investors' climate risk beliefs and identifies who are more likely to hold climate risks. As pointed out by Giglio et al. (2021), understanding the distribution of climate risks among market participants is "an important and valuable research agenda" because it not only helps institutions to manage and hedge their own climate risk exposure but also helps regulators to ensure that climate change will not become a systematic threat to the financial stability.

This paper also adds to the recent literature studying how partisan divide in climate change belief affects individual economic decisions. Baldauf et al. (2020) study the role of climate change beliefs in real estate markets, finding that houses exposed to sea-level rise in Democratic-leaning areas sell for

[11] See, e.g., Goldsmith-Pinkham et al. (2019), Correa et al. (2020), Duan and Li (2021), and Painter (2020).

less money than similar houses in Republican-leaning areas. Relatively, Bernstein et al. (2020) show that partisan beliefs in climate change are reflected in households' residential choices, with Republicans more likely to own coastal properties. Ratnadiwakara and Venugopal (2020) find that partisan climate risk perception also influences individuals' demand for flood insurance. I contribute to the literature by focusing on financial institutions and the supply side of the mortgage market and extending the role of partisan climate risk perceptions from household decision making to institutional decision making.

Finally, my paper provides methodological contributions to the climate finance literature. One popular measurement of climate risk in the literature is sea-level rise. The wildfire risk measure based on the Wildfire Hazard Map has a couple of advantages over the sea-level rise risk measure. As discussed earlier, the wildfire risk represents a more immediate threat from climate change than sea-level rise. Many short maturity asset classes that are hardly impacted by sea-level rise, such as business loans, are subject to wildfire threats. Moreover, sea-level rise, by nature, mostly impacts coastal areas and the corresponding real estate and mortgage markets. On the contrary, wildfire risks exist in both coastal and inland regions. Thus, one can measure wildfire risks for businesses, municipalities, and agriculture, etc.

The rest of the paper proceeds as follows. Section 2 describes the institutional background and develops hypotheses. Section 3 describes the data. Section 4 presents empirical estimations on mortgage approval rates. Section 5 studies whether more optimistic lenders hold more climate risk. Section 6 provides further analysis on real effects, other types of climate risks, and a battery of robustness tests. Section 7 concludes the paper.

1.2 Hypothesis Development

The mortgage market is an ideal setting to study how physical climate risk is distributed in the financial system. First, mortgage loans are standardized financial investments that are highly comparable across mortgage lenders. In fact, sometimes one mortgage applicant sends the same mortgage application to multiple lenders. Second, lenders originate mortgage loans in all 50 states. Locations of the underlying properties provide a direct estimation of mortgages' exposure to physical climate risks. Third, the long maturity of mortgage loans (typically 30 years) is another advantage because the long-term effects of climate change (i.e., sea-level rise) take time to be fully realized. Lastly, in the mortgage application process, mortgage lenders' underwriting departments typically make the final decisions on whether to approve mortgage applications.[12] In other words, the final mortgage approval decisions are centralized at the firm level, making it legitimate to measure corporate climate change beliefs at the mortgage lender level.

Many lenders have been increasingly taking climate change into account as a risk factor (see Table 1.A.3 for anecdotal evidence from the SEC filings).[13] Moreover, studies (e.g., Dunlap et al., 2016; Baldauf et al., 2020) and survey evidence (see Figure 1.1) show that Republican-leaning individuals are more optimistic about climate change. Taken together, I argue that Republican-leaning lenders and Democratic-leaning lenders have diverse perceptions of climate change and potentially price climate risk differently when reviewing mortgage applications. Importantly, I do not assume that Republican-leaning and Democratic-leaning lenders behave binarily on incorporating climate risk in their lending process, which is unlikely to be the case. Instead, I only assume that lenders apply different weights of climate risk in their internal risk models. Given these analyses, I reach my first hypothesis.

Hypothesis 1: *In areas exposed to high climate risk, Republican-leaning lenders are more likely to approve mortgage applications than Democratic-leaning lenders.*

It is well understood that securitization reduces mortgage lenders' incentive to screen borrowers (Keys et al., 2010). Through the same reasoning, securitization can also reduce lenders' incentive to consider climate risk. Two main factors—the loan amount and lien status—influence the securitization process. Jumbo loans, which are loans that exceed the conforming loan limits, are not eligible to be purchased by Fannie Mae or Freddie Mac, making these loans hard to securitize. Second-lien mortgages are also less likely to be securitized due to their high-risk nature (Lee et al., 2013). As a result, both second-lien and jumbo mortgage loans are highly likely to remain in lenders' investment portfolios after origination.

[12]See "The Mortgage Underwriting Process Explained," Merchants Bank.

[13]Recent studies also show the impacts of climate change on lenders' mortgage business (see, e.g., Nguyen et al., 2020; Ouazad and Kahn, 2019; Duan and Li, 2021).

Therefore, it's expected that mortgage lenders are more likely to consider climate risk among second-lien and jumbo mortgage applications but not among the conforming mortgage applications, making the effects less significant among conforming mortgage applications.

Hypothesis 2: *The difference in the approval rates of mortgage applications between Republican-leaning and Democrat-leaning lenders decreases with the securitibility of mortgages: (1), whether the mortgage is a section-lien secured; (2), whether the mortgage is a jumbo loan.*

The difference in mortgage approval rates can also have an impact on the demand side. From a borrower's prospective, their demand for mortgage loans won't disappear if their initial mortgage application is denied by one bank. If Democratic-leaning lenders are less likely to approve mortgage applications in high climate risk areas, the mortgage borrowers in these areas can submit new applications and borrow from more climate-optimistic Republican-leaning lenders. Moreover, relative to climate-pessimistic mortgage lenders, optimistic lenders may advertise more in high climate risk regions. Consequently, thanks to their higher tolerance of climate risks, Republican-leaning lenders can receive a higher number of mortgage applications in high climate risk areas. Given the higher approval rate and higher number of applications, it's expected that Republican-leaning lenders issue more mortgages in high risk areas.

Hypothesis 3: *Thanks to their high tolerance for climate risk, Republican-leaning lenders receive a higher number of mortgage applications and also originate more mortgage loans in high climate risk areas.*

The effects of climate change take various forms, including sea-level rise, flooding, wildfire, abnormal temperature, etc. However, not all climate change effects will be priced in the same way by mortgage lenders. In the context of mortgage lending, wildfire risk potentially represents the most prominent climate risk. The primary reason is the lack of insurance coverage. Insurance for wildfires is mostly provided by commercial insurance companies. In fact, while wildfires are becoming increasingly severe, the insurance market for wildfires has shrunk.[14] Insurance providers often charge high insurance premiums and refuse to renew insurance policies in high wildfire risk regions.[15] Along with high repair costs after wildfires, the

[14]See "As US wildfire threat grows, insurance capacity shrinks," *S&P Global Market Intelligence*, 21 July 2021.

[15]See "As wildfire risk increases in Colorado and the West, home insurance grows harder to find," *The Denver Post*, January

mortgage default rate after wildfires is very high (Issler et al., 2020).[16] In comparison, the National Flood Insurance Program provides insurance protections for flooding in most areas of the United States, even after major hurricanes (Kousky et al., 2020). As for sea-level rise, the long-term effects take decades to be realized. The short-term effects of sea-level rise, such as floods, are also protected by insurance programs such as the National Flood Insurance Program.

Hypothesis 4: *Compared to climate risks such as flood and sea level rise, the difference in the probability of approving mortgage applications is stronger with wildfire risk.*

1.3 Data and Sample Construction

This paper combines data from various sources, including: (1) Home Mortgage Disclosure Act (HMDA) mortgage application data; (2) the Wildfire Hazard Potential map from the United States Forest Service (USFS); (3) campaign finance data from the Federal Election Commission (FEC); (4) the single-family loan performance data from Fannie Mae and Freddie Mac; (5) Historic Fire Perimeters data from the National Interagency Coordination Center (NIFC); (6) national flood hazard layer data from the Federal Emergency Management Agency (FEMA); (7) sea-level rise data from the National Oceanic and Atmospheric Administration (NOAA); (8) real estate price data from the Zillow Home Value Index (ZHVI); and (9) regional economic accounts data from the U.S. Bureau of Economic Analysis (BEA). The following subsections describe several main data in detail.

1.3.1. Mortgage Related Data

I obtain mortgage application information from the publicly-available HMDA data. The public version of the HMDA data provides an annual summary of mortgage applications. The data provides various types of information, including borrower, loan, lender, and property characteristics. Borrower characteristics

7, 2019; "Many Californians Being Left Without Homeowners Insurance Due to Wildfire Risk," *Insurance Journal*, December 4, 2020.

[16] According to American Family Insurance, while a small in-house fire costs between $3,000 and $5,000 to repair, repairing the damage from large fires can cost $50,000 or more. In comparison, according to the HomeAdvisor, the typical range of repairing water-damaged houses is between $1,200 and $5,000.

include ethnicity, race, gender, and gross income. Loan characteristics include the amount of the loan, loan type, lien status, approval decision, denial reason, and whether an originated loan is securitized in the secondary market. Property characteristics mainly cover information about the property's location, such as state, county, and census tract.

I apply two filters during the sample construction. First, I only include conventional mortgage applications. Thus, loan applications insured by government agencies, such as the Federal Housing Administration and Veterans Administration, are excluded from the sample.[17] Second, I include loan applications with the type of action from 1 to 3. By applying this filter, I exclude mortgage applications that are either incomplete or withdrawn by applicants. This filter also excludes mortgages purchased by financial institutions to avoid the double counting of mortgage loans (where *type_of_action* equals 6). A small number of preapproval mortgage applications are also not included in the sample. This is because lenders only voluntarily report preapproval mortgage applications.[18]

Two changes in the HMDA 2018 reporting policies impact my analysis. First, under the new reporting policy, most lenders are required to report home equity lines of credit (HELOCs).[19] The change has a significant impact on the number of second-lien mortgage applications reported by lenders. Under the pre-2018 regulation, some lenders choose not to report second-lien mortgage applications that are HELOCs. For example, JPMorgan Chase reported 43 second-lien mortgage applications in 2017 and 112,845 second-lien mortgage applications in 2018. For Bank of America, the numbers are 32 and 194,279, respectively. To ensure that lenders consistently report the second-lien mortgage applications, I drop lenders from the second-lien mortgage application sample if a lender has fewer than 1,000 applications in any year from 2012 to 2019.[20] Second, a new variable classifying whether a mortgage application passes conforming loan limits was added in the 2018 and 2019 HMDA data. Following the literature, I rely on the

[17]The mortgage applications that are insured by government agencies account for approximately 25% of all mortgage applications.

[18]The second filter drops about 30% of the remaining mortgage applications (among the 30%, about 20% of mortgages are purchased by financial institutions, about 8%-9% of mortgage applications are incomplete or withdrawn, and the remaining 1% of them are preapproval-related applications).

[19]See "CFPB finalizes temporary increase of HMDA HELOC reporting threshold and other minor HMDA amendments," *Ballard CFS Group*, August 25, 2017.

[20]The results are similar without this filter.

loan amount and county-level conforming loan limits to identify jumbo loans for mortgage applications before 2018. For the 2018 and 2019 mortgage applications, I rely on the new variable to identify jumbo loans.[21]

In addition to the HMDA mortgage application data, I obtain the mortgage performance and the mortgage interest rate data of conforming loans from the Fannie Mae and Freddie Mac Single-Family loan performance database. The data track the loan performance over the lifecycle of conforming loans and provide a variety of borrower and loan characteristics. Loan characteristics include origination month, origination interest rate, original loan to value, and seller information. Borrower characteristics provide information on borrowers' credit scores and debt-to-income ratios. The loan performance information also includes monthly delinquent information.

1.3.2. Measuring Climate Risk Beliefs Using Lenders' Political Donations

I capture lenders' climate risk beliefs based on their political preferences. Following the literature, I measure lenders' political preferences using political donations made by their corporate Political Action Committees (PACs) to federal candidates.[22] The federal political donation data are taken from the Federal Election Commission (FEC).

It is worthwhile to describe how corporate PACs make political donations based on the behavior of corporations. Under the current Federal Campaign Finance Law (2 U.S.C. § 441b), corporate PACs can only solicit voluntary political contributions from employees, shareholders, and family members of these two groups. Corporations often create internal committees chaired by senior managers to oversee PAC activities.[23] In sum, corporate PACs collect funds from employees and related stakeholders and then make political donations with corporate leaders' influence.

To identify lenders' PAC contributions, I match all PACs of *corporation* organization type (FEC

[21]The distributions of jumbo loans are similar before 2018 and after 2018.

[22]Corporate PAC contributions are widely used in the literature to identify corporate political connections and political preference (see, e.g., Cooper et al., 2010; Akey, 2015; Di Giuli and Kostovetsky, 2014; and Kempf et al., 2021).

[23]"The 2019 CPA-Zicklin Index Corporate Political Disclosure and Accountability" report from the Center for Political Accountability shows that nearly half of S&P 500 companies have board oversight of corporate PAC activities.

ORG_TP: C) with mortgage lenders from the HMDA data. In total, there are over 8000 mortgage lenders in the HMDA data. However, the largest 500 mortgage lenders receive over 80% of all mortgage applications. I match the top 500 mortgage lenders with corporate PACs from the FEC data. Not all 500 lenders have corporate PACs, and the final matched sample includes 84 mortgage lenders. The 84 mortgage lenders include banks, retail mortgage lenders (i.e., Quicken loans), and federal credit unions. The 84 lenders are also geographically dispersed and headquartered in about 30 states.

I calculate the fraction of corporate political contributions donated to Republican politicians over the last two election cycles (4 years) to measure firms' political preferences. On the one hand, a short time window, such as one or two years, is likely to represent lenders' policy preferences, which introduce irrelevant noise. On the other hand, measurements based on a long time window, such as 6 to 10 years, do not consider changes in lenders' political preferences.[24] Specifically, I first calculate the total direct contribution made by a lender's corporate PAC to all federal candidates in the last four years. Then, I calculate the mortgage lender's PAC contribution to all Republican candidates in the last four years. Finally, I calculate the main explanatory variable *REP Donation%* as follows. Figure 1.A.1 presents the sample distribution of the *REP Donation%*.

$$REPDonation\% = \frac{\textit{Total Donations to REP Candidates}}{\textit{Total Donations to ALL Candidates}} \tag{1}$$

One potential concern is about the extreme value of the *REP Donation%* due to less active PAC donations. For example, if a company donates only $200 in a year, it is likely that it donated all $200 to one candidate. Thus, the company will be identified as 100% Republican or 100% Democrat with the *REP Donation%* equaling either 0 or 1. However, it may be that the company is simply less active in making political contributions. To alleviate this issue, I drop lender-year pairs if a lender has made donations worth less than $10,000 over the last four years.[25]

There are several advantages to measuring mortgage lenders' political preferences using corporate

[24]The results hold robust to alternative specifications of years, such as three years or five years. I provide robustness tests in the robustness section.

[25]The findings are not sensitive to this filter, and I provide robustness analysis in the robustness section.

PAC campaign contributions. Most corporate PACs make political donations consistently over time, while contributions made by CEOs are relatively sparse. On the contrary, employee contributions can be more populated than PAC contributions but do not reflect the structure of decision making within companies as PAC contributions do. Moreover, comparing the PAC contributions with voter registration data, which classifies individuals into Republican and Democrat politicians, enables me to identify mortgage lenders' political preference on the political spectrum from fully conservative to fully liberal.

1.3.3. Wildfire Hazard

I rely on the Wildfire Hazard Potential (WHP) map from the U.S. Forest Service (USFS) to measure wildfire risk; see Figure 1.3 for the 2018 WHP map (Dillon, 2015). The WHP map quantifies wildfire risk based on various types of information, including weather, historical fire occurrence, terrain, spatial fuel, and vegetation coverage.[26] The map has two forms: continuous integer values and classified values. The evaluation of wildfire risk in the continuous WHP map takes integer values from 0 to 100,000. In the classified WHP map, wildfire risk is classified into five categories, including very low, low, moderate, high, and very high.

In my analysis, I construct county-level wildfire risk measurements based on both the continuous and the classified WHP map. Based on the continuous version of the WHP map, I construct the continuous wildfire risk measurement as the log of the average value of wildfire hazard within a county, *Log(WFH)*. Additionally, I construct two classified county-level wildfire risk measurements: *High Risk* and *VHigh Risk*. *High Risk* measures the fraction of lands that are assigned as high risk or very high risk within a county, and *VHigh Risk* measures the fraction of lands that are assigned as very high within a county.

There are a couple of limitations related to the WHP map. USFS has published several WHP maps, including the 2012 version, the 2014 version, and the 2018 version. For each year without the WHP map, I assume it has the same value as the last available map. To illustrate, the wildfire risk in 2013 is the same as the risk in 2012. Since the earliest WHP map available is from 2012, my sample period starts from 2012.

[26] See "FSim-Wildfire Risk Simulation Software," U.S. Forest Service; "Wildfire Hazard Potential," U.S. Forest Service.

In addition, the WHP maps only cover the continental United States. States like Alaska and Hawaii are not included in the analysis.

1.3.4. Sea-Level Rise and Flood Risk

To measure county-level sea-level rise (SLR) risk, I obtain sea-level rise data from the National Oceanic and Atmospheric Administration (NOAA). NOAA's SLR data estimate areas that will be submerged into the sea if the sea level rises by 0 to 10 feet. To capture sea-level rise exposure, I calculate county-level sea-level rise risk measurements based on the fraction of areas that are impacted if the sea level rises by 5 feet – *SLR 5 Feet%*.

I measure flood risk based on the flood hazard map obtained from the Federal Emergency Management Agency (FEMA). Consistent with the measurement of wildfire risk, I calculate a county-level flooding risk based on the percentage of high flooding areas within a county, *High Risk (Flood)*. The FEMA flood hazard map assigns local communities with different designations of flood hazards, including Zone A, Zone AE, Zone B, Zone V, etc. For example, Zone A represents areas with a 1% annual chance of flooding and a 26% chance of flooding over the life of a 30-year mortgage. Following FEMA's classification, I attribute flood zones that begin with the letters A and V as high-risk areas and attribute flood zones that begin with the letters B, C, and X as moderate and low-risk areas.[27]

1.3.5. Summary Statistics

Table 1.1 presents the summary statistics of the full HMDA mortgage application sample. The sample period is from 2012 to 2019. The sample period starts from 2012 because the first wildfire hazard map was published in 2012. Starting from 2020, the Covid-19 pandemic caused both a global health crisis and a global economic recession. Thus, my sample stops at 2019 to avoid the potential impact from the pandemic. In total, the sample includes 24,771,654 mortgage applications to 84 lenders from 3,108 counties. While 84 lenders seems to be a small number, the lenders' 24,771,654 mortgage applications

[27]See the definitions of FEMA Flood Zone Designations on the FEMA website.

represent 36.44% of all mortgage applications during the sample period, indicating a large sample. Panel A presents the summary statistics of the second-lien and non-jumbo sample. Panel B presents the summary statistics of the jumbo and first-lien sample. Panel C presents the summary statistics of the non-jumbo and first-lien sample. In the paper, I conduct estimations based on multiple samples, and I provide summary stats of these samples in the online appendix.

1.4 Mortgage Approval Rate

In this section, I present empirical results on whether lenders' climate-risk beliefs impact their mortgage approval decisions. The first subsection describes the empirical strategy in detail. I next present the key findings based on the empirical strategy. Then, I present further evidence to separate the climate change belief channel with alternative explanations. Lastly, I conduct a "staggered difference-in-differences" analysis using historical wildfire incidents as natural experiments.

1.4.1. Empirical Strategy on Mortgage Approval Decisions

In the baseline empirical analysis, I test *Hypothesis 1* on whether the partisan divide in climate-risk beliefs affects lenders' mortgage approval decisions in high climate risk areas. Formally, I estimate the following interaction model:

$$\begin{aligned} Approval_{i,b,c,t} = \alpha_{c,t} + \lambda_{b,c} + \beta_1 \times \textit{REP Donation\%}_{b,t} + \beta_2 \times \textit{REP Donation\%}_{b,t} \\ \times \textit{Climate Risk}_{c,t} + \theta' Controls_{i,b,c,t} + \epsilon_{i,b,c,t} \end{aligned} \tag{2}$$

The sample is at the application level. *Approval* is an indicator variable on the final approval decision of mortgage application i received by lender *b* in county *c* in year *t*. Two main explanatory variables include the measurement of lenders' political preference, *REP Donation%*, and measurement of climate risk, *Climate Risk*. As described in the previous section, *REP Donation%* represents the fraction of total corporate PAC donations to Republican politicians from *t-5* to *t-1*. Climate Risk measurements include *Log(WFH)*, *High Risk*, and *VHigh Risk*. *Log(WFH)* is the wildfire risk measurement based on the continuous version of the WHP map. *High Risk* (*VHigh Risk*) measures the fraction of lands that are identified as high or very high (very high) risk in the classified version of the WHP map. The *Controls* variable represents control variables, including Log(Loan Amount), Income, Gender, Race, and Log(#Tot Lender Applications).[28] Appendix Table 1.A.1 describes all the variables in detail.

The coefficient of interest is β_2, which captures how partisan preference impacts lenders' mortgage approval decisions in high climate risk areas. If lenders' political preference does not impact lenders' mortgage issuance decisions, β_2 would be statistically indifferent from zero. If Republican-leaning lenders are more optimistic (pessimistic) over climate risk, we would expect β_2 to be significantly positive (negative).

My primary empirical strategy relies on two sets of high dimensional fixed effects, including county-year fixed effects, denoted by $\alpha_{c,t}$, and lender-county fixed effects, denoted by $\lambda_{b,c}$. The county-year fixed effects absorb shocks common to each county, such as changes in local economic conditions (i.e., GDP and unemployment), changes in local mortgage demands, and local real estate price fluctuations. Furthermore, I include lender-county fixed effects to control for unobserved heterogeneities between different lender-county pairs. For example, bank A's clients may be more educated clients in an area than bank B's clients in the same region, which will impact lenders' approval rates. Finally, I double cluster standard errors by lenders and by states of mortgage applications' underlying properties.

One may wonder whether the REP Donation% variable captures lenders' views on climate risk. To address this point, I rely on the firm-level climate risk exposure data constructed by Sautner et al. (2020), which measures firm-level climate risk exposure based on the transcripts of conference calls held by companies. Table 1.2 presents the empirical findings, and the sample is at the lender-quarter level. There are three dependent variables: *Climate Change Exposure*, *Climate Change Risk*, and *Climate Change*

[28]In the baseline analysis, I only include the Log(Total Applications) as the bank-year level control. In the appendix, I present the estimation results with ROA, bank size, etc. as additional control variables. Since some lenders are not banks (i.e., retail mortgage lenders), they don't have call report data. The number of observations drops by about 20% (from 24,771,654 to 19,583,258) if including other bank-year controls. For this reason, I only include Log(#Total Applications) in the main analysis.

Sentiment.[29] The first two measure the relative frequency of companies mentioning climate risk, and the last one measures the sentiment when companies talk about climate change. I control for heterogeneous characteristics between lenders (i.e., lenders' headquarters locations and lenders' fixed exposure to the fossil fuel and renewable industries) by including lender fixed effects. I also include time fixed effects to control for changes in the general attention to climate change. The results show that *REP Donation%* does capture lenders' beliefs in climate risk: Republican-leaning lenders are less likely to mention climate change and are more likely to express more optimistic sentiments about climate change than Democratic-leaning ones.[30]

1.4.2. Baseline Results: Lenders' Approval Decisions

Table 1.3 tests the first two hypotheses. Panel A presents the estimation results based on the second-lien mortgage application sample.[31] In Column 1, I estimate whether lenders' political preferences impact their mortgage issuance decisions. The coefficient on *REP Donation%* is statistically indifferent from zero, suggesting no evidence that *REP Donation%* affects lenders' mortgage approval decisions. In columns 2 to 4, I estimate the interaction regression as described in Equation 2. The coefficients on the interaction term, $REPDonation\% \times ClimateRisk$, are positive and significant, suggesting that Republican-leaning lenders are more likely to approve mortgage applications from high wildfire risk regions. In columns 5 to 7, I additionally control for similar application fixed effects, which group mortgage applications that are potentially sent by one mortgage applicant to different lenders.[32] These results show that in high wildfire risk areas, even conditional on the same mortgage application, Republican-leaning lenders still have a higher approval rate than Democratic-leaning lenders. Moreover, although similar application fixed effects explain a large amount of variation (the R-squared increases from 0.192 to 0.786), my findings remain

[29] See Sautner et al. (2020) for a detailed description of all three variables.

[30] See the online appendix for summary statistics of this sample.

[31] To isolate the effects of second-lien mortgages from jumbo ones, the sample of Panel A only includes second-lien mortgage applications that are also non-jumbo.

[32] I construct a quasi-similar application identifier by grouping mortgage applications with the same loan amounts, the same gender, the same race, the same income level, and from the same census tract. Presumably, mortgage applications with the same identifier allows me to group mortgage applications from the same mortgage applicants. Additionally, within each grouped mortgage applications, I require the first mortgage application been rejected by lenders.

robust. Lastly, in Panel B, the findings based the jumbo applications show similar effects as Panel A, again confirming *Hypothesis 1*.

The findings in Table 1.3 also highlight how climate risk beliefs affect mortgage approval rates differently in high and low wildfire risk areas. Based on Column 3 of Panel A, changes in the *REP Donation%* in low-risk areas (25th percentile, High Risk = 0) do not have statistically significant effects on mortgage approval rates.[33] However, relative to low wildfire risk areas, a one-standard-deviation increase in the *REP Donation%* (0.139) in high wildfire risk areas (75th percentile, *High Risk* = 0.142) is associated with a 3.4% (= $0.139 \times 0.142 \times 1.741$) higher increase in the approval probability of mortgage applications, which is 8.1% higher relative to the average approval rate among second-lien mortgage applications (42.5%). Moreover, in areas exposed to more severe wildfire threats (i.e., those in the 90th percentile of *High Risk*), the economic magnitude will be even larger. For jumbo mortgage applications, the economic magnitude is smaller. Relative to low wildfire risk areas, a one-standard-deviation increase in the *REP Donation%* in high wildfire risk areas is associated with a 0.47% increase in the approval probability of jumbo mortgage applications. The smaller magnitude among jumbo loans might be due to the high-risk nature of second-lien mortgages.

I proceed to examine *Hypothesis 2* on whether securitization affects the lenders' incentive to consider climate risk. In Panel C of Table 1.3, I conduct the same mortgage approval analysis based on both first-lien and non-jumbo mortgage applications, which are mortgages that are easy to securitize. Interestingly, with a much larger sample, the coefficients on the interaction terms are neither statistically significant nor economically large. The sharp comparison between Panel A/B and Panel C indicates that lenders have little incentive to consider climate risk when evaluating applications for mortgages that are less likely to stay in lenders' portfolios, confirming *Hypothesis 2*.

[33] *High Risk* is a better variable than the other two to make inferences about economic magnitudes. *Log(WFH)* is calculated based on within-county average value of the continuous wildfire hazard (ranging from 0 to 100,000), making the variable more sensitive to large wildfire hazard values. *VHigh Risk* does not capture the land areas that are exposed to high risk.

1.4.3. Alternative Explanations

Other than the climate risk perceptions explanation, several alternative stories exist. The first one is the partisan perception of economic outlook. Depending on lenders' political alignment with the president in office, Republican-leaning and Democratic-leaning lenders can have different economic outlooks, leading to a potential difference in mortgage approval rates (Kempf and Tsoutsoura, 2021; Dagostino et al., 2020). The second alternative explanation is relative to the locations that are exposed to wildfire risks. For example, rural areas are more Republican-leaning and might also be more likely to be impacted by wildfires. Republican-leaning lenders, who are connected with Republican incumbents from the rural areas, might originate more mortgages in these areas to help these Republican politicians get re-elected (Bertrand et al., 2018; Duchin and Hackney, 2020). The third alternative explanation is about lenders' risk appetite. Republican-leaning and Democratic-leaning lenders might have exactly the same climate risk perceptions. However, Republican-leaning lenders might be generally more risk-seeking than Democratic-leaning lenders, making them more willing to invest in riskier mortgage loans from high wildfire risk areas.

The comparison between low and high wildfire risk areas suggests that the first alternative explanation does not drive my findings. If Republican and Democratic-leaning lenders have different economic outlooks, the difference in approval rates would also be observable among mortgage applications from low wildfire risk areas. In the second alternative explanation, if wildfires are more likely to happen in Republican-leaning areas (rural areas), Republican-leaning lenders might help Republican incumbents get re-elected by approving more mortgages. In Table 1.4, I split the sample into Republican-leaning and Democratic-leaning regions depending on how counties voted in the 2012 presidential election.[34] The findings show that the effects do not depend on whether locals are more supportive of Republicans or Democrats, suggesting that the second alternative explanation is not plausible. To examine the third alternative story, I test whether Republican-leaning lenders are more likely to approve mortgage applications from borrowers with higher loan-to-income ratios. Table 1.5 shows that the lenders are not more likely to

[34]The data on county-level presidential election results are taken from the MIT election lab. I find similar results if I classify counties as "red" or "blue" based on the 2016 presidential election.

originate more risky mortgages, suggesting that the risk appetite story is also not a likely explanation for the findings.

I provide two additional pieces of evidence to further support the climate risk perception explanation. First, if Republican-leaning lenders are more optimistic about wildfire risks, they are less likely to securitize mortgage loans in the secondary market after origination. To test this point, I use accepted first-lien and non-jumbo loans which are the most liquid/sellable loans in the secondary market. Table 1.6 presents the findings, showing that after mortgage originations, Republican-leaning lenders are indeed more likely to hold the first-lien and non-jumbo loans that are exposed to high wildfire risks in their portfolios. Second, if lenders are more concerned about climate risk, it's expected that they are more likely to deny mortgage applications for reasons related to collaterals which are endangered by climate risks (Duan and Li, 2021). Table 1.7 shows that among all denied loans in high wildfire risk areas, Republican-leaning lenders are less likely to deny mortgage applications for collateral-related reasons. In addition, Table 1.7 extends Duan and Li (2021) by highlighting how disagreement over lenders' climate risk beliefs affects their mortgage denial decisions. Thus, both Table 1.6 (based on accepted loans) and Table 1.7 (based on denied applications) support the interpretation of the climate risk perception.

1.4.4. Alternative Specification: Staggered Difference-in-Differences

My baseline empirical analysis is based on an interaction regression, as described in Equation 2. To estimate future climate risk, the interaction regression relies on a forward-looking wildfire hazard measurement covering the continental United States. However, one might be interested in how historical wildfire incidents affect lenders' mortgage approval decisions. Specifically, I estimate the following model using historical wildfire perimeters data from NIFC.

$$\begin{aligned} Approval_{i,b,c,t} = \alpha_{c,t} + \lambda_{b,c} + \beta_1 \times \textit{REP Donation\%}_{b,t} + \beta_2 \times \textit{REP Donation\%}_{b,t} \\ \times \textit{Wildfire Happened}_{c,t} + \theta' Controls_{i,b,c,t} + \epsilon_{i,b,c,t} \end{aligned} \tag{3}$$

The model is the same as Equation 2, except for the *Wildfire Happened* variable, which is a county-year level indicator variable on whether the county has been exposed to large-scale wildfires. More specifically, the *Wildfire Happened* variable stays at 0 before a large-scale wildfire happens and remains at 1 after it takes place in the county.

Importantly, two factors play a role in lenders' expectation of wildfire risks and thus their mortgage approval decisions: (1) the historical frequency of wildfires; (2) the severity of wildfires. First, lenders' wildfire risk expectations toward an area depend on the frequency of wildfires in the region. If wildfires occur in a county on a regular basis, it's common knowledge for both Republican-leaning and Democratic-leaning lenders that mortgages from this county are exposed to high wildfire hazard. Consequently, we won't expect a significant difference in mortgage approval rates between the optimistic and pessimistic lenders. For this reason, I restrict my analysis to counties that have experienced 3 or fewer wildfire incidents and use counties that have had more than 3 wildfire incidents as a placebo test. Second, it's straightforward to understand how wildfire severity impacts lenders' future wildfire expectations: small-scale wildfires are less likely to be noticed by lenders, thus not really affecting their mortgage approval decisions. Thus, I further restrict wildfire incidents to wildfires that consumed at least 1% of counties' land areas. The average county land area is approximately 3,050 km², making 1% of the land area about 30.5 km².

Table 1.8 presents the empirical findings. Panel A includes mortgage applications from counties that comply with both two requirements. As a comparison, Panel B presents estimation results for counties with more than 3 wildfire incidents. In total, there are 46 (31) counties in Panel A for second-lien (jumbo) mortgage applications. For Panel B, there are 191 (163) counties. As shown in Panel A, I find that Republican-leaning lenders are more likely than Democratic-leaning lenders to approve second-lien mortgage applications after unexpected large-scale wildfires. The effects do not survive among the jumbo mortgage applications. As for Panel B, we don't observe the effects in both second-lien sample and the jumbo sample, confirming that both Republican-leaning and Democratic-leaning lenders consider wildfire hazards if a county constantly experiences wildfires. Figure 1.4 plots the parallel trend of the second-lien sample in Panel A, showing that the assumption is satisfied. Given the small number of counties included in the staggered difference-in-differences sample, I continue to use the interaction regression in most of my analyses.

1.5 Do Optimistic Lenders Hold More Wildfire Risk?

After establishing robust evidence on how climate risk beliefs affect lenders' mortgage-approval decisions, in this section, I study whether optimistic Republican-leaning lenders are more likely to hold wildfire risks in their portfolios, leading to climate risk concentration in the financial sector. Specifically, I look at: (1) the total number of mortgage applications received by lenders in each county; (2) the total loan amount originated and the market share occupied by lenders in each county; (3) the mortgage interest rates charged by lenders; and (4) whether Republican-leaning lenders benefit from their optimistic lending policies in both the short and long term.

Figure 1.5 illustrates how changes in the local wildfire risk are associated with changes in the local market share of Republican-leaning lenders. The horizontal axis represents changes in *High Risk* from the beginning of the sample (2012 and 2013) to the end of the sample (2018 and 2019), and the vertical axis represents changes in the market share of Republican-leaning lenders, which is the average of lenders' *REP Donation%* weighted by lenders' market share within each county (only approved loans). Each dot represents counties with similar changes in wildfire hazards (grouped at the 0.001 scale), and the line fits across all the dots. While the fitted line has a slightly negative slope in Panel C (conforming), we observe a significantly positive slope in both Panel A (second-lien) and Panel B (jumbo), indicating that higher wildfire risks in a county are associated with a higher presence of Republican-leaning lenders in second-lien and jumbo mortgage loans. Since both second-lien and jumbo mortgage loans are likely to remain in lenders' portfolios, Figure 1.5 provides intuitive evidence on the increasing climate risk concentration among optimistic Republican-leaning mortgage lenders.

1.5.1. Number of Mortgage Applications

As detailed in *Hypothesis 3*, if borrowers' mortgage applications in high wildfire risk areas are denied by Democratic-leaning lenders, the borrowers can still shop around and file new mortgage applications with other lenders. Thus, we expect that in high wildfire risk areas, Republican-leaning lenders would receive a relatively higher number of mortgage applications than Democratic-leaning lenders. Specifically, I estimate the following empirical model:

$$\begin{aligned} Log(\#App.)_{b,c,t} = \alpha_{c,t} + \lambda_{b,c} + \beta_1 \times \textit{REP Donation\%}_{b,t} + \beta_2 \times \textit{REP Donation\%} \\ \times \textit{Climate Risk}_{c,t} + \theta' Controls_{b,c,t} + \epsilon_{b,c,t} \end{aligned} \tag{4}$$

Unlike in Equation 2, the sample is at the lender-county-year level. The dependent variable, *Log(#App.)*, is the log of the number of mortgage applications received by lender b in county c in year t. *Controls* represents control variables, including the average value of Log(Loan Amount) among all mortgage applications, average Income Level, average Gender, average Race, and Log(#Tot Lender Applications). The fixed effect specification is the same as in Equation 2. Again, we are interested in the β_2 on the interaction term, $REPDonation\% \times ClimateRisk$.

Table 1.9 presents the estimation results. The first four columns include the sample of second-lien mortgage applications, and the last four columns include the sample of jumbo mortgage applications. I find positive and significant coefficients on all interaction terms, $REPDonation\% \times ClimateRisk$, indicating that Republican-leaning lenders receive more mortgage applications in high wildfire risk areas than Democratic-leaning lenders. In terms of economic magnitude relative to low wildfire risk areas (25th percentile *High Risk*), a one-standard-deviation increase in the REP Donation% in high wildfire risk areas (75th percentile of *High Risk*) is associated with a 5.72% (2.15%) higher increase in the number of second-lien (jumbo) mortgage applications. Therefore, the findings confirm that Republican-leaning lenders benefit from their higher tolerance of climate risk by receiving a greater number of mortgage applications in high wildfire risk areas.[35]

1.5.2. The Holding of Wildfire Exposed Mortgages

Given higher approval rates and the higher number of mortgage applications, it's obvious that Republican-leaning lenders originate more mortgages in high wildfire risk areas. In Table 1.10, I estimate whether Republican-leaning lenders originate a greater number of mortgage loans in high wildfire risk areas.[36] The sample is at the lender-county-year level.

The findings confirm this intuition. The magnitude is also economically sizable. Relative to low fire risk areas (25th percentile), a one-standard-deviation increase in *REP Donation%* in high fire risk areas (75th percentile) is associated with a 9.3% (2.9%) increase in the total number of second-lien (jumbo) mortgage loans. The average value for the total number of originated second-lien (jumbo) at lender-county-year level is $841,313$(15,984,184). Therefore, at the bank-county-year level, the magnitude is approximately $78,040 ($466,274) for second-lien (jumbo) loans. After counting the number of counties with *High Risk* that is greater than the 75th percentile, the effects at the lender-year level are about $48 million ($130 million) for second-lien (jumbo) loans.[37] Considering that *High Risk* at the 75th percentile represents the minimal value of *High Risk* among the top quartile counties, the $48 million ($130 million) represents the lower bound of the estimated effects.

1.5.3. Mortgage Interest Rate Charged

The findings have documented the quantity effects: optimistic Republican-leaning lenders are more likely to invest in mortgage loans exposed to high wildfire risks. I next examine the price effects: whether Republican-leaning lenders and Democratic-leaning lenders charge different mortgage interest rates in

[35]Given that the number of applications is a count variable, Table 1.A.8 presents the alternative Poisson estimation (Cohn et al., 2021).

[36]I conduct similar tests based on the number of approved mortgage applications, as well as the market share of each lender. The findings are similar.

[37]For the second-lien sample, there are on average 610 counties that are exposed to high wildfire risks each year. The number is 280 for the jumbo mortgage sample.

high wildfire risk areas. However, the interest rate effects are subject to important data limitations. The HMDA data only started including the interest rate variable in 2018.[38] Although the single-family loan performance data from Fannie Mae and Freddie Mac provide mortgage interest rate information, they only cover conforming loans. As shown earlier, Republican-leaning and Democratic-leaning lenders are indifferent in approving conforming mortgage applications and thus unlikely to charge different interest rates for conforming loans.

Table 1.11 estimates the interest rate effects based on the single-family loan performance data. The samples are at mortgage loan level. The table relies on the single-family loan performance data. The most granular geographic information in the single-family loan performance data is at the 3-digit zip code level.[39] Thus, I re-calculate the wildfire risk at the 3-digit zip code level. As expected, I find that Republican and Democratic-leaning lenders are statistically indifferent in charging mortgage interest rates for mortgages from high wildfire risk areas.

1.5.4. Optimal Lending Policies

One important question left unanswered is whether Republican-leaning lenders benefit from their optimistic lending policies in high wildfire risk areas both in the short term and in the long term. Answering this question has similar data limitations as the interest rate effects do. In this paper, I only observe the performance of conforming loans and don't observe the performance of second-lien or jumbo mortgages. Following the same reasoning of mortgage securitization, it's unlikely that differences would be observed in the mortgage delinquency rate of conforming loans at the mortgage level, and therefore at the mortgage-lender level. More importantly, as an econometrician, I don't know the true parameter on the likelihood of future wildfire incidents, making it almost impossible to conduct an ex-ante cost benefits analysis for lenders. Given these limitations, I make attempts to address this question using the limited single-family loan performance data, as well some economic reasoning that supports my conclusion.

[38]Almost 95% of approved mortgage applications from before 2018 in the HMDA data don't have interest rate information (the rate spread variable).

[39]There are approximately 900 different 3-digit zip codes in the United States.

In Table 1.12, I use the single-family loan performance data to estimate whether mortgages originated by Republican-leaning and Democratic-leaning lenders have different delinquency rates after exposure to wildfire incidents. *REP Donation%* is estimated at the mortgage origination, and *Wildfire Impacted Area%* is the fraction of land areas that are exposed to wildfires in the 3-digit zip code in a year. The results in Column 1 confirm Issler et al. (2020), who find that mortgage delinquency rates increase significantly after wildfire incidents. As expected, I don't observe a significant difference in mortgage delinquency rates between Republican-leaning and Democratic-leaning lenders. In Column 2, the coefficient on the interaction term is significant at 10%, and the effect is economically very small. For the largest wildfire incidents,(those in the 99th percentile by *Wildfire Impacted Area%*), a one-standard-deviation increase in *REP Donation%* is associated with only a 2.3 basis point increase in mortgage default probability. Therefore, the results suggest that we cannot conduct a cost-benefit analysis using conforming loan data.

However, based on existing evidence, one can infer that optimistic Republican-leaning lenders experience losses around large fire incidents. We know: (1) mortgage delinquency rates increase after large-scale wildfires, as seen in Column 1 of Table 1.12 and Issler et al. (2020); and (2) optimistic Republican-leaning lenders hold more second-lien and jumbo mortgages in high wildfire risk areas (Table 1.10). Therefore, it's obvious that Republican-leaning lenders will experience a higher number of mortgage defaults after wildfires, thus bearing losses from mortgage delinquencies. If both the interest rates and the loan performance of second-lien and jumbo mortgages are available, it's possible to make an ex-post analysis on whether Republican-leaning lenders on average experience losses after wildfires. Again, without knowing the true risk parameter of wildfires, I won't be able to make an inference on the ex-ante optimal policy. Moreover, the true parameter of wildfire probability is also evolving over time depending on how global warming proceeds, making it even harder to estimate the long-term optimal lending policy.

1.6 Further Analysis

In this section, I study other types of climate risk, examine real effects, and also provide a battery of robustness tests.

1.6.1. Other Types of Climate Risk

The effects of climate change take various forms. Mortgage lenders can have different priorities for different types of climate risk. In this section, I study the relative importance of different climate risks, including wildfires, sea-level rise, and floods. The findings are generally consistent with *Hypothesis 4*, suggesting that wildfire risk plays a more important role in mortgage lenders' mortgage issuance decisions than does sea-level rise or flooding.

Table 1.13 presents the empirical results. As defined in Section 3.4 and Table 1.A.1, *SLR 5 Feet* estimates the fraction of land that will be underwater if the sea level rises by 5 feet, and *High Risk (Flood)* measure the fraction of county areas that are exposed to high flood risk. The findings suggest that lenders consider the sea-level rise and flood risk for second-lien mortgage applications (Column 1 and Column 2) but not for jumbo applications (Column 5 and Column 6). The difference might be due to the high-risk nature of second-lien mortgages, making lenders more cautious when approving such mortgages. The other columns compare wildfire risk with the other two types of climate risks. Due to the potential collinearity between *High Risk (Flood)* and *SLR 5 Feet*, I separately estimate the effects. The findings indeed show that wildfire risk represents a more severe threat than the other two risks. As described in *Hypothesis 4*, the reason is likely because of insurance coverage and the more immediate threat of wildfires.

1.6.2. Real Effects

The last question is whether the increasing presence of Republican-leaning lenders has real effects on the local economy. To examine this issue, I rely on several data sources, including real estate price data from Zillow, local GDP, FDIC bank deposit branch information, and employment from BEA. To capture the presence of Republican-leaning lenders in local areas, I calculate the weighted average of *REP Donation%* (weighted by the number of branches in the region). Table 1.14 presents the estimation results on real effects. The findings suggest no real effects in high-risk areas.

1.6.3. Robustness

In this subsection, I present robustness tests. The results are generally consistent and robust. First, one may be concerned about the large sample size and corresponding large sample bias. To alleviate this concern, I collapse the sample from the mortgage application level to the lender-county-year level. For each lender-county-year pair, I construct a new dependent variable on the percentage of applications that are ultimately approved. To avoid extreme values, such as 0 or 1, I only include lender-county-year pairs that have at least 3 mortgage applications. Table 1.A.5 presents the findings based on the lender-county-year level sample. The results are robust to the lender-county-year level sample. Second, Table 1.A.6 provides estimation results based on *REP Donation%* with 3 and 5 years. The findings remain the same. Third, large mortgage lenders receive many more applications. It will be informative to see if the findings hold after removing the largest lenders. Table 1.A.7 provides the estimation results after removing the top 10 largest lenders. While the results become weaker for the jumbo mortgage applications, they remain strong for the second-lien mortgage applications. Finally, I provide estimation results with census tract-year and lender-census tract fixed effects in Appendix Table 1.10, and the findings are similar.

1.7 Conclusion

Using the mortgage market as a laboratory, I document that disagreement over climate risk beliefs leads to self-sorting among institutional investors in holding climate risk. Building on the literature and survey evidence, I use mortgage lenders' political preferences to capture their perceptions on climate change. I find that Republican-leaning lenders are more likely to approve mortgage applications that are exposed to high wildfire risks. Importantly, the effects only exist among hard-to-securitize second-lien and jumbo mortgages, highlighting how securitization reduces lenders' incentive to consider climate risk. I also show that Republican-leaning lenders also originate more second-lien and jumbo loans in high wildfire areas and thus hold more wildfire risks in their portfolios.

The findings have implications on the financial stability and also for future research. As many have argued, climate change is generally recognized as one of the defining challenges of our time. Over the last several years, we have also witnessed an acceleration of weather-related adverse events, such as wildfires, droughts, floods, etc. Although we don't know the exact impacts of climate change, there is a non-trivial probability that climate change will severely impact the economy in a systematic way. From the perceptive of financial stability, it's better to have a large scale of risk-sharing across financial institutions. However, my findings show that without intervention, it's more likely to have risk concentration instead of risk-sharing, which creates policy implications around disclosing climate risk exposure, stress testing, etc. Finally, while my paper uses the mortgage market as a setting to study how disparate climate risk beliefs lead to climate risk concentration, the general mechanism can also take place in other markets and different settings, which can be interesting for future research.

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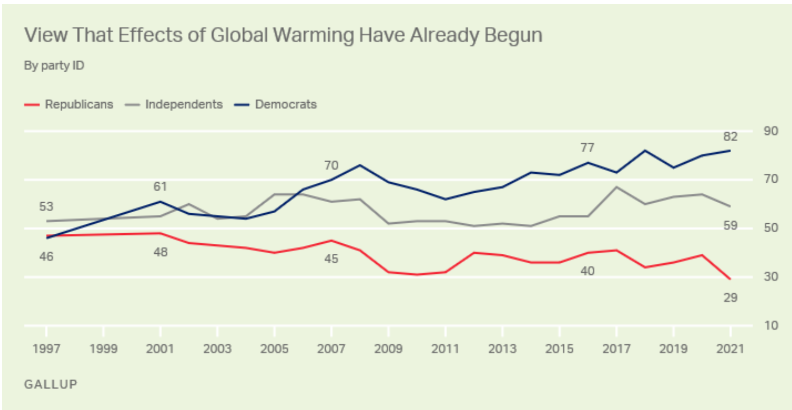
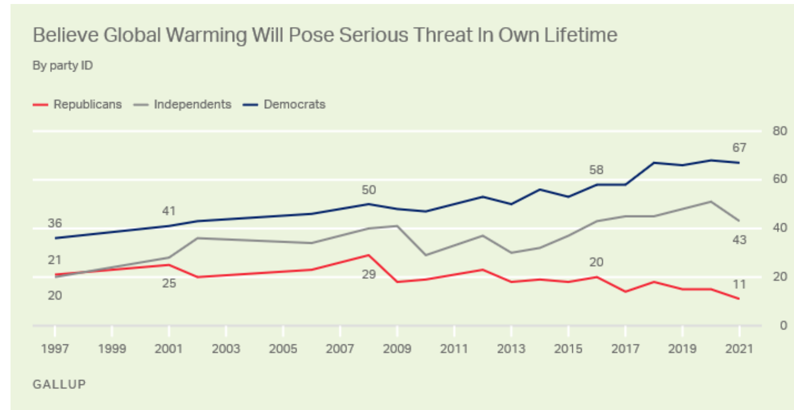
Figure 1.1: Survey Evidence on the Partisan Divide over Climate Change Beliefs

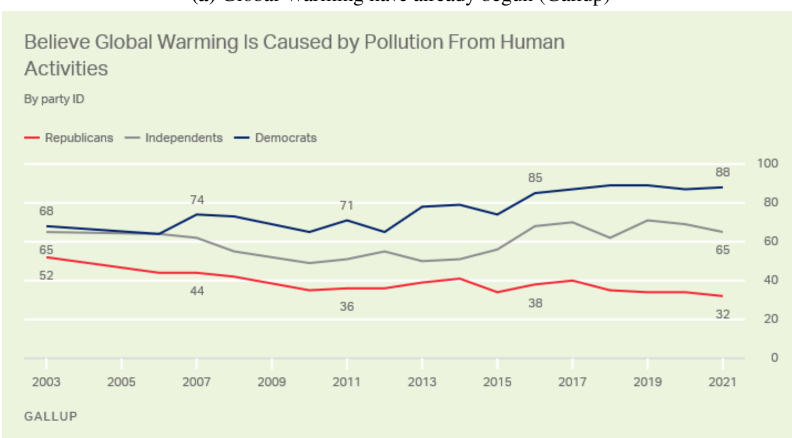
This Figure presents survey evidence on the partisan divide over climate change beliefs from both Gallup (Panel A to Panel C) and Pew Research Center (Panel D). For Panel A to Panel C, see "Global Warming Attitudes Frozen Since 2016", *Gallup*, April 5, 2021. For Panel D, see "U.S. concern about climate change is rising, but mainly among Democrats", *Pew Research Center*, April 16, 2020.



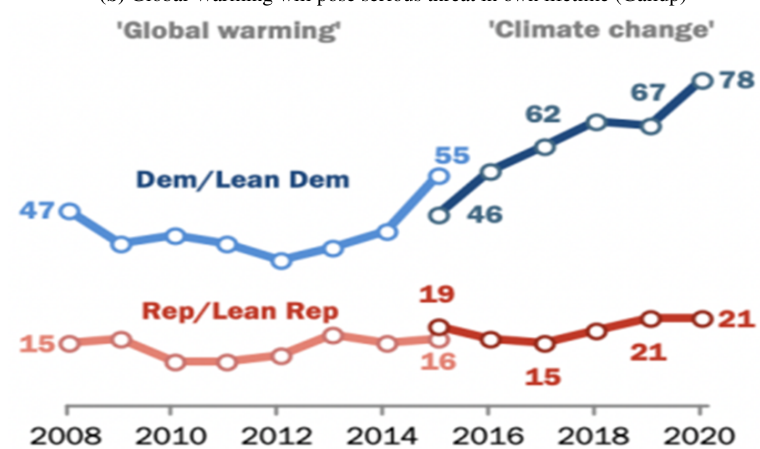
(a) Global-Warming have already begun (Gallup)



(b) Global-Warming will pose serious threat in own lifetime (Gallup)



(c) Global-Warming is caused by human activities (Gallup)



(d) Global-Warming should be a top priority for the government (Pew)

Figure 1.2: Temperature Anomaly and Wildfires

The data on wildfire acres are obtained from the National Interagency Fire Center (NIFC), and the data on global temperature anomaly are from the National Oceanic and Atmospheric Administration. Temperature anomaly is measured relative to the average temperature in the 20th century. NIFC doesn't track wildfire information before 1983.

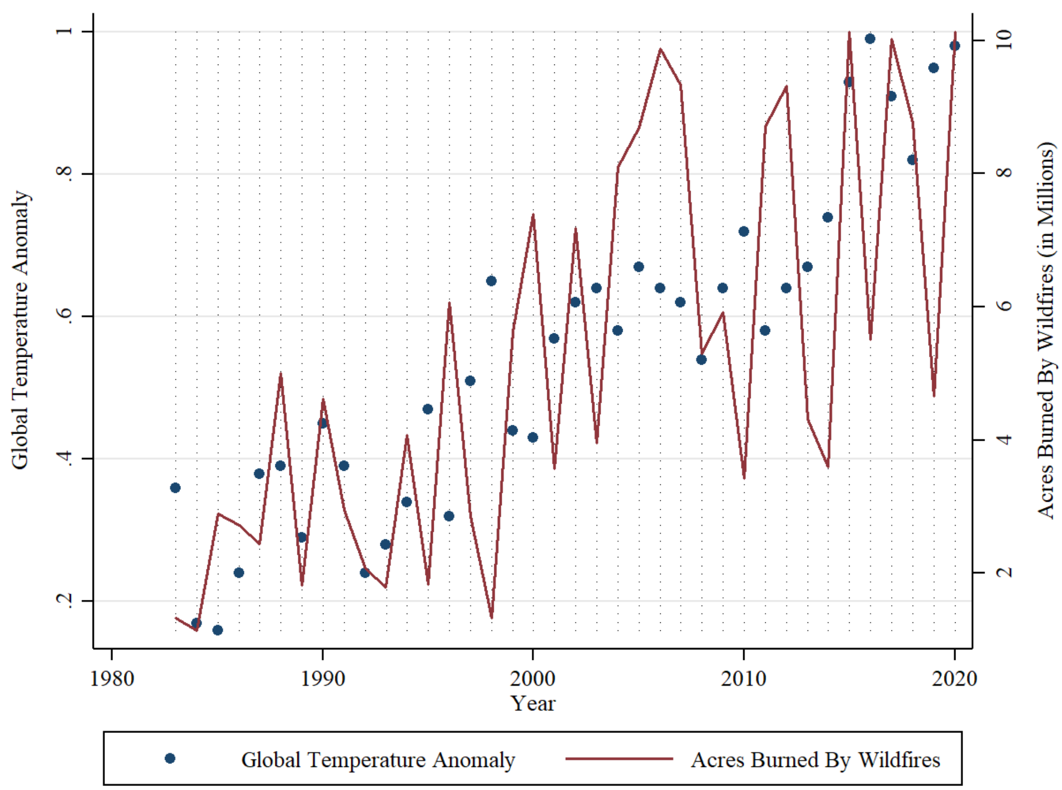


Figure 1.3: The 2018 Wildfire Hazard Potential map

The figure presents the 2018 version of Wildfire Hazard Potential map developed by USDA Forest Service. The goal of the map is *"to depict the relative potential for wildfire that would be difficult for suppression resources to contain"*. Areas are classified into five classes of wildfire risks, including very low risk (green), low risk (light green), moderate risk (yellow), high risk (orange), and very high risk (red). Alaska and Hawaii are not included in the map. The full description of the Wildfire Hazard Potential map is available at the USDA Forest Service website.

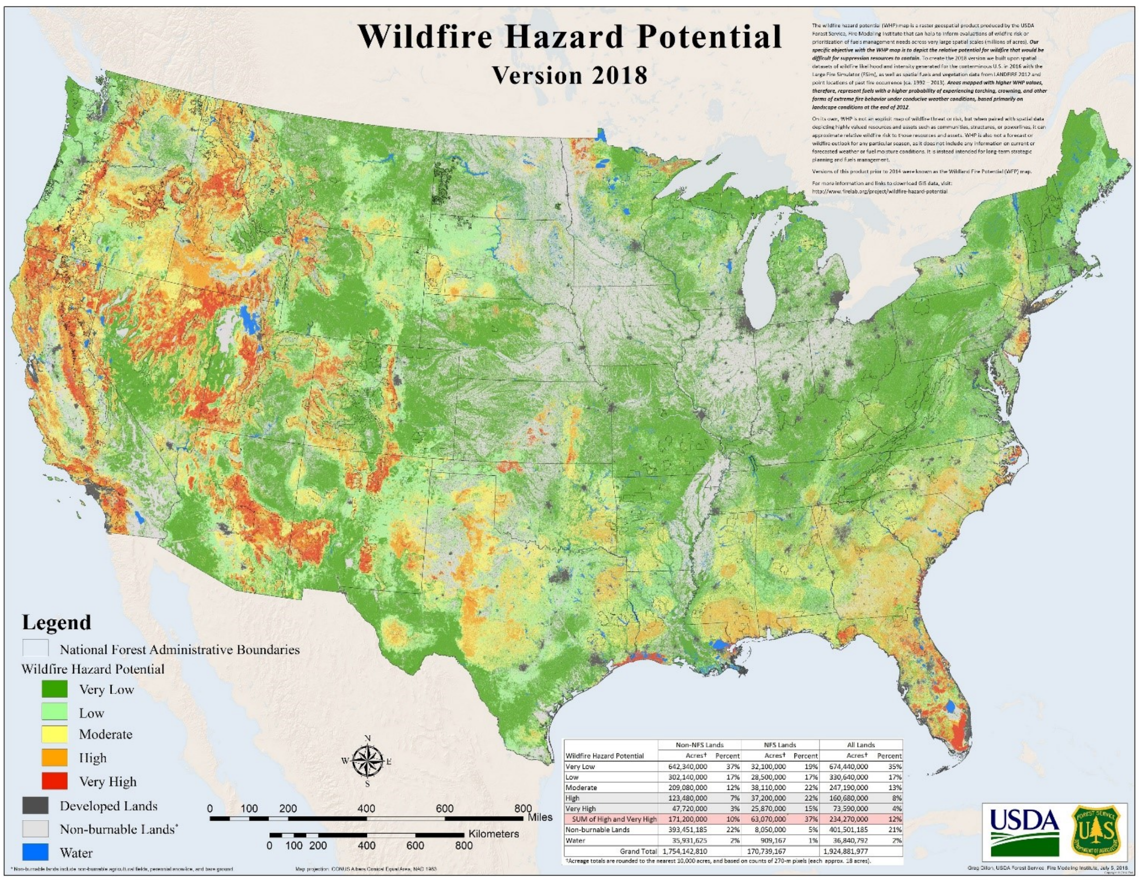


Figure 1.4: Parallel Trends around Wildfire Incidents

This figure plots the parallel trends around large fire incidents. The figure corresponds to column 1 of the Table 8. The baseline is years before *t-3*.

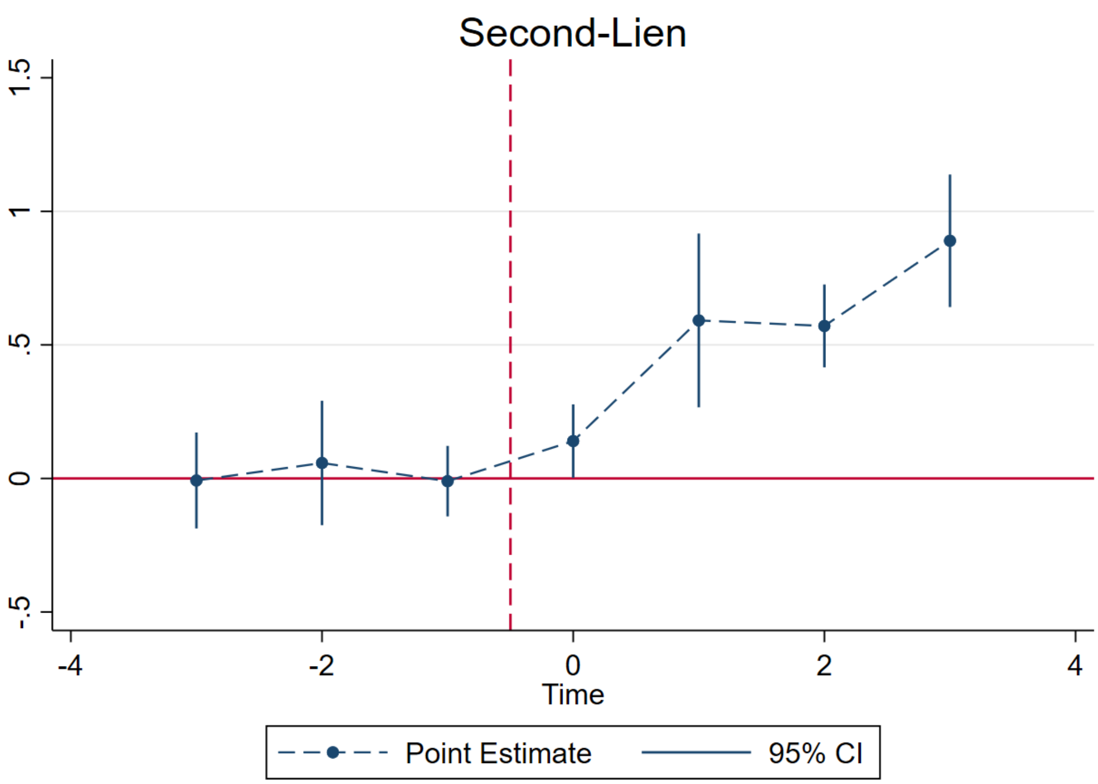


Figure 1.5: Local Wildfire Hazard and Republican-Lean Lenders' Market Share

The figure provides non-parametric analysis on the effects of wildfire hazard and Republican-lenders' market share. The horizontal axis represents county-level changes of wildfire risks between the first two years (2012-2013) and the last two years (2018-2019) in the sample. Similarly, the vertical axis represents the changes in the market share of Republican-leaning lenders, which is the *Rep Donation%* weighted by lenders' market share within each county.

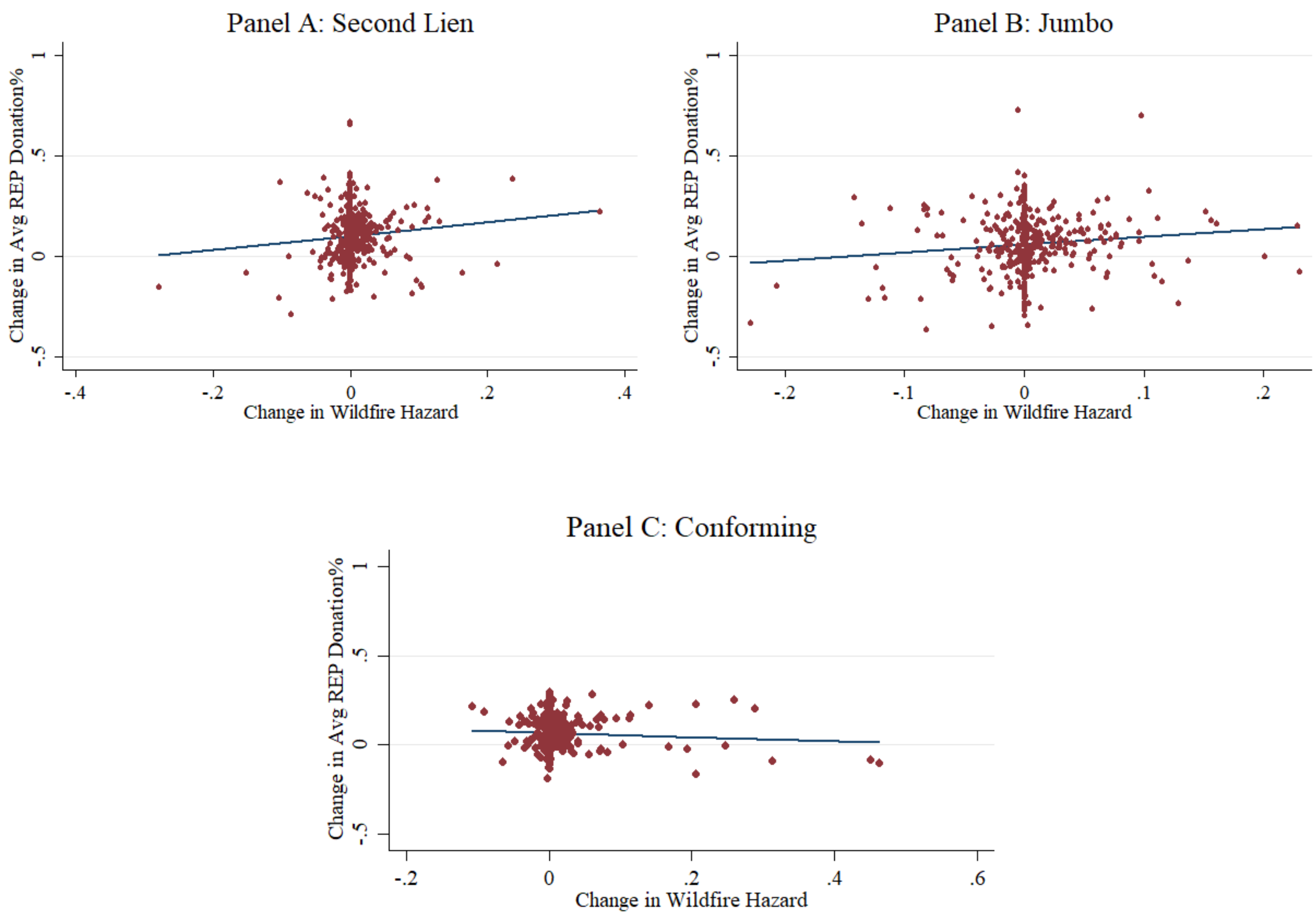


Table 1.1: Summary Statistics - Mortgage Application Sample

This table presents summary statistics of the mortgage approval sample. All samples are at the mortgage application level. Panel A represents the sample on second-lien mortgage applications, Panel B represents the sample on jumbo mortgage applications, and Panel C represents the sample on conforming mortgage applications.

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Second-Lien	N	mean	p25	p50	p75	sd
Approval	2,464,455	0.425	0.000	0.000	1.000	0.494
REP Donation%	2,464,455	0.702	0.596	0.659	0.830	0.139
Log(WFH)	2,464,455	3.894	2.213	4.130	5.407	2.238
High Risk	2,464,455	0.095	0.000	0.003	0.142	0.154
VHigh Risk	2,464,455	0.029	0.000	0.000	0.002	0.084
Income Level	2,464,455	2.221	2.000	2.000	3.000	0.648
Male	2,464,455	0.573	0.000	1.000	1.000	0.495
White	2,464,455	0.703	0.000	1.000	1.000	0.457
Log(Loan Amount)	2,464,455	10.062	9.210	10.127	10.915	1.208
Log(#Tot Lender Applications)	2,464,455	11.249	10.467	11.169	12.891	1.703

	(1)	(2)	(3)	(4)	(5)	(6)
Panel B: Jumbo	N	mean	p25	p50	p75	sd
Approval	1,786,382	0.811	1.000	1.000	1.000	0.392
REP Donation%	1,786,382	0.632	0.586	0.624	0.659	0.110
Log(WFH)	1,786,382	5.025	3.156	5.164	7.087	2.386
High Risk	1,786,382	0.171	0.000	0.084	0.314	0.183
VHigh Risk	1,786,382	0.072	0.000	0.000	0.134	0.102
Income Level	1,786,382	2.970	3.000	3.000	3.000	0.198
Male	1,786,382	0.751	1.000	1.000	1.000	0.432
White	1,786,382	0.688	0.000	1.000	1.000	0.463
Log(Loan Amount)	1,786,382	13.617	13.318	13.534	13.820	0.430
Log(#Tot Lender Applications)	1,786,382	11.775	10.927	12.162	12.896	1.529

	(1)	(2)	(3)	(4)	(5)	(6)
Panel C: Conforming	N	mean	p25	p50	p75	sd
Approval	19,288,215	0.755	1.000	1.000	1.000	0.430
REP Donation%	19,288,215	0.648	0.585	0.636	0.705	0.129
Log(WFH)	19,288,215	4.120	2.295	4.296	5.767	2.323
High Risk	19,288,215	0.107	0.000	0.009	0.171	0.158
VHigh Risk	19,288,215	0.037	0.000	0.000	0.021	0.083
Income Level	19,288,215	2.301	2.000	2.000	3.000	0.606
Male	19,288,215	0.614	0.000	1.000	1.000	0.487
White	19,288,215	0.731	0.000	1.000	1.000	0.443
Log(Loan Amount)	19,288,215	11.978	11.562	12.014	12.476	0.685
Log(#Tot Lender Applications)	19,288,215	11.922	11.053	12.292	12.879	1.416

Table 1.2: Firm-Level Climate Risk Exposure and *REP Donation%*

The regressions are estimated at the lender-quarter level. *Climate Change Exposure* measures the relative frequency of managers mentioning climate change on conference calls. *Climate Change Risk* measures the relative frequency of managers mentioning climate change together with words like risk. *Climate Change Sentiment* captures the sentiments when managers mention climate change. Standard errors double clustered by quarter and lender are reported in parentheses below the coefficients. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

	(1) Climate Change Risk	(2) Climate Change Exposure	(3) Climate Change Sentiment
REP Donation%	-0.007**	-0.083**	0.021*
	(0.003)	(0.034)	(0.012)
Log(#Tot Lender Applications)	-0.000	-0.001**	-0.000
	(0.000)	(0.000)	(0.000)
Observations	1,549	1,549	1,549
R-squared	0.128	0.471	0.182
Quarter FE	Yes	Yes	Yes
Lender FE	Yes	Yes	Yes

Table 1.3: Mortgage Approval Rates

This table presents the estimations results of Equation 2. All samples are at the mortgage application level. The dependent variable, Approval, is an indicator variable set to one if the mortgage application is approved. Panel A includes *Second-Lien* mortgage applications. Panel B includes *Jumbo* mortgage applications. Panel C includes *Conforming* mortgage applications. Fixed effects specifications are indicated at the bottom of each panel. Standard errors double clustered by both property state and lender are reported in parentheses below the coefficients. For the full specification including control variables, see appendix Table 1.A.4. *** p < 0.01, ** p < 0.05, * p < 0.1

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Second-Lien	Approval	Approval	Approval	Approval	Approval	Approval	Approval
REP Donation%	0.331	-0.161	0.249	0.305	-0.293	0.211	0.298
	(0.268)	(0.243)	(0.248)	(0.261)	(0.196)	(0.260)	(0.277)
REP Donation% × Log(WFH)		0.171***			0.232***		
		(0.049)			(0.045)		
REP Donation% × High Risk			1.741***			2.634***	
			(0.384)			(0.401)	
REP Donation% × VHigh Risk				1.933***			3.283***
				(0.175)			(0.335)
Observations	2,464,455	2,464,455	2,464,455	2,464,455	2,464,455	2,464,455	2,464,455
R-squared	0.191	0.192	0.192	0.192	0.894	0.894	0.894
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Lender-County FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Similar Application FE	No	No	No	No	Yes	Yes	Yes

Panel B: Jumbo	(1) Approval	(2) Approval	(3) Approval	(4) Approval	(5) Approval	(6) Approval	(7) Approval
REP Donation%	0.067	-0.024	0.044	0.042	0.034	0.108	0.045
	(0.126)	(0.135)	(0.148)	(0.130)	(0.166)	(0.160)	(0.135)
REP Donation% × Log(WFH)		0.018***			0.022***		
		(0.001)			(0.006)		
REP Donation% × High Risk			0.136***			0.241**	
			(0.043)			(0.106)	
REP Donation% × VHigh Risk				0.333***			1.100***
				(0.021)			(0.152)
Observations	1,786,382	1,786,382	1,786,382	1,786,382	1,786,382	1,786,382	1,786,382
R-squared	0.118	0.118	0.118	0.118	0.918	0.918	0.918
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Lender-County FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Similar Application FE	No	No	No	No	Yes	Yes	Yes

Panel C: Conforming	(1) Approval	(2) Approval	(3) Approval	(4) Approval	(5) Approval	(6) Approval	(7) Approval
REP Donation%	-0.132	-0.102	-0.131	-0.129	-0.103	-0.084	-0.070
	(0.151)	(0.133)	(0.150)	(0.151)	(0.107)	(0.107)	(0.107)
REP Donation% × Log(WFH)		-0.008			0.011		
		(0.006)			(0.012)		
REP Donation% × High Risk			-0.012			0.228**	
			(0.086)			(0.099)	
REP Donation% × VHigh Risk				-0.093			0.178
				(0.291)			(0.240)
Observations	19,288,215	19,288,215	19,288,215	19,288,215	19,288,215	19,288,215	19,288,215
R-squared	0.091	0.091	0.091	0.091	0.925	0.925	0.925
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Lender-County FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Similar Application FE	No	No	No	No	Yes	Yes	Yes

Table 1.4: Mortgage Approval Rates in "Blue" and "Red" Areas

This table presents estimation results by splitting the sample into "blue" and "red" counties. All estimations are at the mortgage application level. The dependent variable, Approval, is an indicator variable set to one if the mortgage application is approved. Columns 1 to 3 represent mortgage applications that are second-lien and non-jumbo. Columns 4 to 6 represent mortgage applications that are jumbo and first-lien. Panel A includes "red" counties that voted Republican in the 2012 presidential election. Panel B includes "blue" counties that voted Democratic in the 2012 presidential election. Fixed effects specifications are indicated at the bottom of each panel. Standard errors double clustered by both property state and lender are reported in parentheses below the coefficients. *** p < 0.01, ** p < 0.05, * p < 0.1

	Second-Lien			Jumbo		
Panel A: Blue Counties	(1)	(2)	(3)	(4)	(5)	(6)
	Approval	Approval	Approval	Approval	Approval	Approval
REP Donation%	-0.072	0.293	0.329	-0.034	0.043	0.037
	(0.271)	(0.261)	(0.269)	(0.145)	(0.159)	(0.137)
REP Donation% × Log(WFH)	0.154**			0.020***		
	(0.057)			(0.001)		
REP Donation% × High Risk		1.375***			0.111**	
		(0.427)			(0.048)	
REP Donation% × VHigh Risk			1.237***			0.322***
			(0.165)			(0.030)
Log(Loan Amount)	0.007	0.007	0.007	-0.041***	-0.041***	-0.041***
	(0.011)	(0.010)	(0.011)	(0.006)	(0.006)	(0.006)
Income Level	0.172***	0.172***	0.172***	0.400***	0.400***	0.400***
	(0.008)	(0.008)	(0.008)	(0.018)	(0.018)	(0.020)
Male	0.004	0.004	0.004	0.001	0.001	0.001
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
White	0.100***	0.100***	0.100***	0.009**	0.009**	0.009**
	(0.007)	(0.007)	(0.007)	(0.004)	(0.004)	(0.004)
Log(#Tot Lender Applications)	0.007	0.007	0.008	-0.026	-0.026	-0.026
	(0.006)	(0.006)	(0.007)	(0.017)	(0.017)	(0.017)
Observations	1,367,279	1,367,279	1,367,279	1,371,703	1,371,703	1,371,703
R-squared	0.188	0.188	0.187	0.100	0.100	0.100
County-Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Lender-County FE	Yes	Yes	Yes	Yes	Yes	Yes

	Second-Lien			Jumbo		
Panel B: Red Counties	(1) Approval	(2) Approval	(3) Approval	(4) Approval	(5) Approval	(6) Approval
REP Donation%	-0.354	0.157	0.240	0.025	0.043	0.057
	(0.239)	(0.237)	(0.250)	(0.102)	(0.118)	(0.112)
REP Donation% × Log(WFH)	0.209***			0.011**		
	(0.051)			(0.004)		
REP Donation% × High Risk		2.379***			0.254***	
		(0.408)			(0.042)	
REP Donation% × VHigh Risk			5.040***			0.461***
			(0.608)			(0.044)
Log(Loan Amount)	0.010	0.010	0.010	-0.062***	-0.062***	-0.062***
	(0.009)	(0.009)	(0.009)	(0.006)	(0.006)	(0.006)
Income Level	0.158***	0.158***	0.158***	0.381***	0.381***	0.381***
	(0.009)	(0.009)	(0.009)	(0.019)	(0.020)	(0.018)
Male	0.016***	0.016***	0.016***	0.004	0.004	0.004
	(0.003)	(0.003)	(0.003)	(0.004)	(0.004)	(0.004)
White	0.092***	0.092***	0.092***	0.015***	0.015***	0.015***
	(0.008)	(0.008)	(0.008)	(0.005)	(0.005)	(0.005)
Log(#Tot Lender Applications)	0.008	0.009	0.009	-0.025*	-0.025*	-0.025*
	(0.006)	(0.006)	(0.006)	(0.015)	(0.015)	(0.015)
Observations	1,097,176	1,097,176	1,097,176	414,679	414,679	414,679
R-squared	0.196	0.195	0.195	0.166	0.166	0.166
County-Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Lender-County FE	Yes	Yes	Yes	Yes	Yes	Yes

Table 1.5: General Risk Tolerance

This table tests whether Republican-leaning and Democratic-leaning lenders have significant difference in risk tolerance. The sample is at the mortgage application level. Loan to Income represents the ratio between loan amounts and applicant income. The other variables are the same as the previous tables. Fixed effects specifications are indicated at the bottom of each panel. Standard errors double clustered by both property state and lender are reported in parentheses below the coefficients. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

	Second-Lien		Jumbo	
	(1) Approval	(2) Approval	(3) Approval	(4) Approval
REP Donation%	0.313	0.357	0.086	0.067
	(0.251)	(0.262)	(0.123)	(0.119)
Loan to Income	-0.051**	-0.048*	-0.068***	-0.060***
	(0.024)	(0.027)	(0.006)	(0.007)
REP Donation% × Loan to Income	-0.051	-0.035	0.000	0.007
	(0.035)	(0.038)	(0.010)	(0.010)
Log(Loan Amount)		0.042***		-0.026***
		(0.012)		(0.005)
Income Level		0.120***		0.207***
		(0.009)		(0.017)
Male	0.031***	0.009***	-0.002	-0.002
	(0.003)	(0.003)	(0.004)	(0.004)
White	0.109***	0.096***	0.009*	0.009**
	(0.008)	(0.006)	(0.004)	(0.004)
Log(#Tot Lender Applications)	0.010	0.009	-0.025	-0.025
	(0.009)	(0.007)	(0.017)	(0.016)
Observations	2,464,455	2,464,455	1,786,382	1,786,382
R-squared	0.168	0.198	0.141	0.150
County-Year FE	Yes	Yes	Yes	Yes
Lender-County FE	Yes	Yes	Yes	Yes

Table 1.6: Securitization of Mortgage Loans After Origination

This table presents the estimation results of how lenders securitize their mortgages after origination. The sample is at the mortgage level and only includes originated mortgages that are *conforming* loans (the most liquid mortgages). The dependent variable, Hold, is an indicator variable that equals one if lenders do not sell the mortgage in the secondary market. Fixed effects specifications are indicated at the bottom of each panel. Standard errors double clustered by both property state and lender are reported in parentheses below the coefficients. *** p < 0.01, ** p < 0.05, * p < 0.1

	(1) Hold	(2) Hold	(3) Hold	(4) Hold
REP Donation%	0.072	-0.010	0.045	0.054
	(0.146)	(0.124)	(0.140)	(0.143)
REP Donation% × Log(WFH)		0.021**		
		(0.009)		
REP Donation% × High Risk			0.281**	
			(0.108)	
REP Donation% × VHigh Risk				0.532**
				(0.239)
Log(Loan Amount)	-0.053**	-0.053**	-0.053**	-0.053**
	(0.026)	(0.026)	(0.026)	(0.026)
Income Level	0.029***	0.029***	0.029***	0.029***
	(0.008)	(0.009)	(0.008)	(0.008)
Male	-0.003	-0.003	-0.003	-0.003
	(0.002)	(0.002)	(0.002)	(0.002)
White	-0.009**	-0.009**	-0.009**	-0.009**
	(0.004)	(0.004)	(0.004)	(0.004)
Log(#Tot Lender Applications)	-0.073**	-0.073*	-0.073*	-0.073*
	(0.036)	(0.036)	(0.036)	(0.036)
Observations	14,559,710	14,559,710	14,559,710	14,559,710
R-squared	0.231	0.231	0.231	0.231
County-Year FE	Yes	Yes	Yes	Yes
Lender-County FE	Yes	Yes	Yes	Yes

Table 1.7: Reasons for Mortgage Applications Been Denied

This table presents the estimation results for the reasons for a mortgage denial. The sample is at the mortgage application level and only includes denied mortgage applications. The dependent variable, Collateral, is an indicator variable that equals one if lenders list collateral as the reason for rejecting the mortgage applications. Columns 1 to 4 include the denied mortgages that are second-lien and non-jumbo. Columns 5 to 8 include the denied mortgages that are jumbo and first lien. Fixed effects specifications are indicated at the bottom of each panel. Standard errors double clustered by both property state and lender are reported in parentheses below the coefficients. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

	Second-Lien				Jumbo			
	(1) Collateral	(2) Collateral	(3) Collateral	(4) Collateral	(5) Collateral	(6) Collateral	(7) Collateral	(8) Collateral
REP Donation%	0.177	0.548*	0.254	0.210	0.130	0.177	0.154	0.127
	(0.145)	(0.268)	(0.154)	(0.146)	(0.126)	(0.127)	(0.141)	(0.135)
REP Donation% × Log(WFH)		-0.122*				-0.009***		
		(0.069)				(0.002)		
REP Donation% × High Risk			-1.196**				-0.138***	
			(0.458)				(0.037)	
REP Donation% × VHigh Risk				-1.576***				0.037
				(0.186)				(0.113)
Log(Loan Amount)	0.030**	0.030**	0.030**	0.030**	0.025**	0.025**	0.025**	0.025**
	(0.013)	(0.013)	(0.013)	(0.013)	(0.010)	(0.010)	(0.010)	(0.010)
Income Level	0.055***	0.055***	0.055***	0.055***	0.121***	0.121***	0.121***	0.121***
	(0.013)	(0.013)	(0.013)	(0.013)	(0.014)	(0.014)	(0.014)	(0.014)
Male	0.010**	0.010**	0.010**	0.010**	-0.004*	-0.004*	-0.004*	-0.004*
	(0.004)	(0.004)	(0.004)	(0.004)	(0.002)	(0.002)	(0.002)	(0.002)
White	0.012**	0.012**	0.012**	0.012**	0.021***	0.021***	0.021***	0.021***
	(0.005)	(0.005)	(0.005)	(0.005)	(0.007)	(0.008)	(0.008)	(0.008)
Log(#Tot Lender Applications)	-0.007	-0.005	-0.006	-0.006	-0.024*	-0.024*	-0.024*	-0.024*
	(0.006)	(0.005)	(0.006)	(0.006)	(0.013)	(0.013)	(0.013)	(0.013)
Observations	1,325,066	1,325,066	1,325,066	1,325,066	338,368	338,368	338,368	338,368
R-squared	0.220	0.221	0.221	0.221	0.167	0.167	0.167	0.167
County-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Lender-County FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Table 1.8: Staggered Difference-in-Differences Tests

The table presents the staggered Difference-in-Differences tests around wildfires incidents. The sample is at the mortgage-application level. *Wildfire Happened* is an indicator variable of whether a county has been exposed to wildfires that consume over 1% of the county's area. The first 2 columns represent counties with fewer than or equal to 3 historical wildfire incidents, and the last 2 columns represent the counties with more than 3 historical wildfires. Fixed effects specifications are indicated at the bottom of each panel. Standard errors double clustered by both property county and lender are reported in parentheses below the coefficients. *** p < 0.01, ** p < 0.05, * p < 0.1

	Counties (<=3 Fires)		Counties (>3 Fires)	
	(1) Approval (2nd-Lien)	(2) Approval (Jumbo)	(3) Approval (2nd-Lien)	(4) Approval (Jumbo)
REP Donation%	0.655**	0.234*	1.022***	0.180
	(0.289)	(0.123)	(0.214)	(0.185)
REP Donation% × Wildfire Happened	0.504***	-0.007	0.241	-0.160
	(0.027)	(0.047)	(0.145)	(0.110)
Log(Loan Amount)	-0.009	-0.046***	-0.021***	-0.052***
	(0.007)	(0.012)	(0.007)	(0.007)
Income Level	0.156***	0.373***	0.187***	0.407***
	(0.007)	(0.023)	(0.015)	(0.026)
Male	0.007**	-0.002	0.009*	0.004
	(0.003)	(0.011)	(0.005)	(0.004)
White	0.089***	0.021***	0.081***	0.009*
	(0.010)	(0.005)	(0.012)	(0.005)
Log(#Tot Lender Applications)	-0.002	-0.001	-0.037***	-0.030*
	(0.004)	(0.014)	(0.011)	(0.018)
Observations	38,999	24,529	148,885	364,594
R-squared	0.144	0.143	0.134	0.088
County-Year FE	Yes	Yes	Yes	Yes
Lender-County FE	Yes	Yes	Yes	Yes

Table 1.9: Number of Received Mortgage Applications

This table presents the estimation results for the number of mortgage applications received by lenders. The sample is at the lender-county-year level. The dependent variable, *Log(#App.)*, is the log of the number of mortgage applications received by lenders in the county and year. Columns 1 to 4 include the number of mortgage applications that are jumbo and first lien. Columns 5 to 8 include the number of mortgages applications that are non-jumbo and second lien. Fixed effects specifications are indicated at the bottom of each panel. Standard errors double clustered by both property state and lender are reported in parentheses below the coefficients. For robustness, I conduct the same tests with Poisson estimation in the appendix Table 1.A.8. *** p < 0.01, ** p < 0.05, * p < 0.1

	Second-Lien				Jumbo			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Log(#App.)	Log(#App.)	Log(#App.)	Log(#App.)	Log(#App.)	Log(#App.)	Log(#App.)	Log(#App.)
REP Donation%	0.074	-1.548	-0.154	0.035	0.058	-0.475	-0.033	0.008
	(0.676)	(1.028)	(0.685)	(0.672)	(0.334)	(0.349)	(0.336)	(0.338)
REP Donation% × Log(WFH)		0.587***				0.137***		
		(0.209)				(0.046)		
REP Donation% × High Risk			5.618**				0.932*	
			(2.125)				(0.468)	
REP Donation% × VHigh Risk				6.081**				1.571**
				(2.356)				(0.634)
Avg Loan Amount	0.054	0.056	0.055	0.054	0.054**	0.054**	0.054**	0.054**
	(0.062)	(0.062)	(0.062)	(0.062)	(0.022)	(0.022)	(0.022)	(0.022)
Avg Income Level	-0.102***	-0.101***	-0.101***	-0.101***	0.057**	0.056**	0.056**	0.056**
	(0.028)	(0.027)	(0.027)	(0.028)	(0.023)	(0.023)	(0.023)	(0.023)
Avg Male	-0.007	-0.007	-0.007	-0.008	-0.005	-0.004	-0.004	-0.004
	(0.022)	(0.022)	(0.021)	(0.022)	(0.015)	(0.015)	(0.015)	(0.015)
Avg White	-0.018	-0.015	-0.016	-0.017	-0.021*	-0.020	-0.021*	-0.021*
	(0.043)	(0.042)	(0.043)	(0.043)	(0.012)	(0.012)	(0.012)	(0.012)
Log(#Tot Lender Applications)	0.121	0.119	0.121	0.121	0.223***	0.222***	0.222***	0.222***
	(0.112)	(0.111)	(0.111)	(0.112)	(0.068)	(0.068)	(0.068)	(0.068)
Observations	92,340	92,340	92,340	92,340	96,325	96,325	96,325	96,325
R-squared	0.903	0.904	0.903	0.903	0.906	0.906	0.906	0.906
County-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Lender-County FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Table 1.10: Total Amount of Approved Mortgage Loans

This table presents the estimation results on the county-level amount originated by lenders. The sample is at the lender-county-year level. The dependent variable, *Log(Tot Amt)*, is the log of the total mortgage amounts originated by lenders at county level. Columns 1 to 4 represent the second-lien and non-jumbo. Columns 5 to 8 represent the jumbo and first-lien. Fixed effects specifications are indicated at the bottom of each panel. Standard errors double clustered by both property state and lender are reported in parentheses below the coefficients. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

	Second-Lien				Jumbo			
	(1) Log(Tot Amt)	(2) Log(Tot Amt)	(3) Log(Tot Amt)	(4) Log(Tot Amt)	(5) Log(Tot Amt)	(6) Log(Tot Amt)	(7) Log(Tot Amt)	(8) Log(Tot Amt)
REP Donation%	0.590	-1.628	0.275	0.533	0.163	-0.442	0.043	0.112
	(0.910)	(1.297)	(0.897)	(0.914)	(0.354)	(0.359)	(0.349)	(0.354)
REP Donation% × Log(WFH)		0.812***				0.156***		
		(0.252)				(0.042)		
REP Donation% × High Risk			8.359***				1.241***	
			(1.911)				(0.387)	
REP Donation% × VHigh Risk				8.769***				1.615***
				(1.568)				(0.454)
Avg Loan Amount	0.852***	0.871***	0.862***	0.855***	0.749***	0.748***	0.748***	0.749***
	(0.079)	(0.077)	(0.078)	(0.080)	(0.083)	(0.082)	(0.082)	(0.082)
Avg Income Level	0.078	0.082	0.077	0.082	0.138	0.139	0.139	0.138
	(0.230)	(0.217)	(0.223)	(0.228)	(0.086)	(0.087)	(0.086)	(0.086)
Avg Male	-0.027	-0.029	-0.026	-0.027	0.033	0.034	0.035	0.034
	(0.061)	(0.060)	(0.061)	(0.067)	(0.035)	(0.035)	(0.035)	(0.036)
Avg White	-0.241*	-0.222*	-0.236*	-0.240*	-0.057	-0.057	-0.057	-0.057
	(0.122)	(0.113)	(0.119)	(0.122)	(0.035)	(0.035)	(0.035)	(0.035)
Log(#Tot Lender Applications)	0.048	0.046	0.047	0.048	0.175**	0.174**	0.174**	0.175**
	(0.049)	(0.047)	(0.048)	(0.048)	(0.077)	(0.077)	(0.077)	(0.077)
Observations	63,787	63,787	63,787	63,787	82,939	82,939	82,939	82,939
R-squared	0.906	0.907	0.907	0.906	0.906	0.906	0.906	0.906
First Lien	No	No	No	No	Yes	Yes	Yes	Yes
Jumbo	No	No	No	No	Yes	Yes	Yes	Yes
County-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Lender-County FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Table 1.11: Mortgage Interest Rate

This table presents the estimation results on the interest rates charged by lenders. The sample is at the mortgage level and includes conforming loans in the single-family loan performance data from Fannie Mae and Freddie Mac. The dependent variable, Interest Rate, is the mortgage interest rate charged by lenders. Zip represents the 3-digit zip code. Fixed effects specifications are indicated at the bottom of each panel. Standard errors double clustered by both property state and lender are reported in parentheses below the coefficients. *** p < 0.01, ** p < 0.05, * p < 0.1

	(1) Interest Rate	(2) Interest Rate	(3) Interest Rate	(4) Interest Rate
REP Donation%	0.005	0.003	0.013	0.016
	(0.198)	(0.210)	(0.202)	(0.201)
REP Donation% × Log(WFH)		0.001		
		(0.015)		
REP Donation% × High Risk			-0.063	
			(0.135)	
REP Donation% × VHigh Risk				-0.237
				(0.217)
Credit Score	-0.001***	-0.001***	-0.001***	-0.001***
	(0.000)	(0.000)	(0.000)	(0.000)
Debt to Income	0.000**	0.000**	0.000**	0.000**
	(0.000)	(0.000)	(0.000)	(0.000)
Loan to Value	0.007***	0.007***	0.007***	0.007***
	(0.000)	(0.000)	(0.000)	(0.000)
Unpaid Balance	-0.000***	-0.000***	-0.000***	-0.000***
	(0.000)	(0.000)	(0.000)	(0.000)
Observations	12,376,170	12,376,170	12,376,170	12,376,170
R-squared	0.367	0.367	0.367	0.367
ZipCode-Year FE	Yes	Yes	Yes	Yes
Lender-ZipCode FE	Yes	Yes	Yes	Yes

Table 1.12: Loan Performance After Wildfires

This table presents the estimation results on mortgage delinquency after wildfire incidents. The sample is at the loan by year level and includes conforming loans in the single-family loan performance data from Fannie Mae and Freddie Mac. The dependent variable, Delinquent, is an indicator variable set to one if the mortgage is delinquent in the year. Zip represents the 3-digit zip code. Fixed effects specifications are indicated at the bottom of each panel. Standard errors double clustered by both property state and lender are reported in parentheses below the coefficients. *** p < 0.01, ** p < 0.05, * p < 0.1

	(1) Delinquent	(2) Delinquent	(3) Delinquent	(4) Delinquent
Wildfire Impacted Area%	0.010***			
	(0.001)			
REP Donation% × Wildfire Impacted Area%		0.039*		0.018
		(0.020)		(0.023)
REP Donation%			-0.024	-0.024
			(0.017)	(0.017)
Credit Score			-0.000***	-0.000***
			(0.000)	(0.000)
Debt to Income			0.000***	0.000***
			(0.000)	(0.000)
Loan to Value			0.000***	0.000***
			(0.000)	(0.000)
Observations	48,677,259	48,677,259	48,677,259	48,677,259
R-squared	0.457	0.460	0.020	0.020
Loan FE	Yes	Yes	No	No
ZipCode-Year FE	No	Yes	Yes	Yes
Lender-ZipCode FE	Yes	No	Yes	Yes

Table 1.13: Alternative Types of Climate Risk

All samples are at the mortgage application level. The dependent variable, Approval, is an indicator variable set to one if the mortgage application is approved. Columns 1 to 3 represent mortgage applications that are second-lien and non-jumbo. Columns 4 to 6 represent mortgage applications that are jumbo and first-lien. SLR 5 Feet represents the percentage of land that will be submerged into the sea if the sea level rises by 5 feet. High Risk (Flooding) represents the percentage of land that is classified as high risk by the FEMA Flood map. Fixed effects specifications are indicated at the bottom of each panel. Standard errors double clustered by both property state and lender are reported in parentheses below the coefficients. *** p < 0.01, ** p < 0.05, * p < 0.1

	Second-Lien				Jumbo			
	(1) Approval	(2) Approval	(3) Approval	(4) Approval	(5) Approval	(6) Approval	(7) Approval	(8) Approval
REP Donation%	0.286	0.220	0.249	0.235	0.069	0.066	0.044	0.037
	(0.260)	(0.282)	(0.248)	(0.278)	(0.126)	(0.122)	(0.148)	(0.147)
REP Donation% × SLR 5 Feet	7.767**			3.434	-0.256			-0.803
	(3.191)			(2.864)	(0.476)			(1.074)
REP Donation% × High Risk (Flooding)		0.932***		-0.011		0.008		0.110
		(0.239)		(0.323)		(0.052)		(0.076)
REP Donation% × High Risk (Wildfire)			1.741***	1.633***			0.136***	0.149
			(0.384)	(0.343)			(0.043)	(0.104)
Log(Loan Amount)	0.008	0.008	0.008	0.008	-0.045***	-0.045***	-0.045***	-0.045***
	(0.010)	(0.010)	(0.010)	(0.010)	(0.005)	(0.005)	(0.005)	(0.005)
Income Level	0.166***	0.166***	0.166***	0.166***	0.394***	0.394***	0.394***	0.394***
	(0.008)	(0.009)	(0.008)	(0.008)	(0.020)	(0.018)	(0.018)	(0.019)
Male	0.009***	0.009**	0.009***	0.009***	0.002	0.002	0.002	0.002
	(0.003)	(0.003)	(0.003)	(0.003)	(0.005)	(0.004)	(0.004)	(0.004)
White	0.097***	0.097***	0.097***	0.097***	0.010**	0.010**	0.010**	0.010**
	(0.006)	(0.009)	(0.006)	(0.008)	(0.004)	(0.004)	(0.004)	(0.004)
Log(#Tot Lender Applications)	0.009	0.009	0.008	0.008	-0.026	-0.026	-0.026	-0.026
	(0.007)	(0.007)	(0.006)	(0.006)	(0.016)	(0.016)	(0.017)	(0.017)
Observations	2,464,455	2,464,455	2,464,455	2,464,455	1,786,382	1,786,382	1,786,382	1,786,382
R-squared	0.191	0.191	0.192	0.192	0.118	0.118	0.118	0.118
County-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Lender-County FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Table 1.14: Real Effects

This table examines whether the concentration of climate risk in Republican-leaning lenders has any real effects. The samples are at the county-year level. The first column represents the dependent variable of growth in real estate price. The second column represent local employment growth. The third column represents growth of local GDP. Republican-Leaning Bank Presence is calculated as the average REP Donation% weighted by the number of bank branches within the county. High Risk is the same wildfire risk defined earlier: fraction of land that is classified as high wildfire risk.

	(1) Real Estate Price Growth	(2) Employments Growth	(3) GDP Growth
Republican-Leaning Bank Presence	-0.008*	0.007	-0.019
	(0.005)	(0.008)	(0.015)
High Risk	-0.016	-0.024	0.002
	(0.024)	(0.016)	(0.089)
Republican-Leaning Bank Presence × High Risk	-0.005	-0.037	0.103
	(0.030)	(0.051)	(0.084)
Observations	15,893	12,446	12,446
R-squared	0.568	0.388	0.218
Year FE	Yes	Yes	Yes
County FE	Yes	Yes	Yes

Appendix Figures and Tables

Figure 1.A.1: Distribution of REP Donation%

This figure plots the histogram of the *REP Donation%* variable for the mortgage application sample.

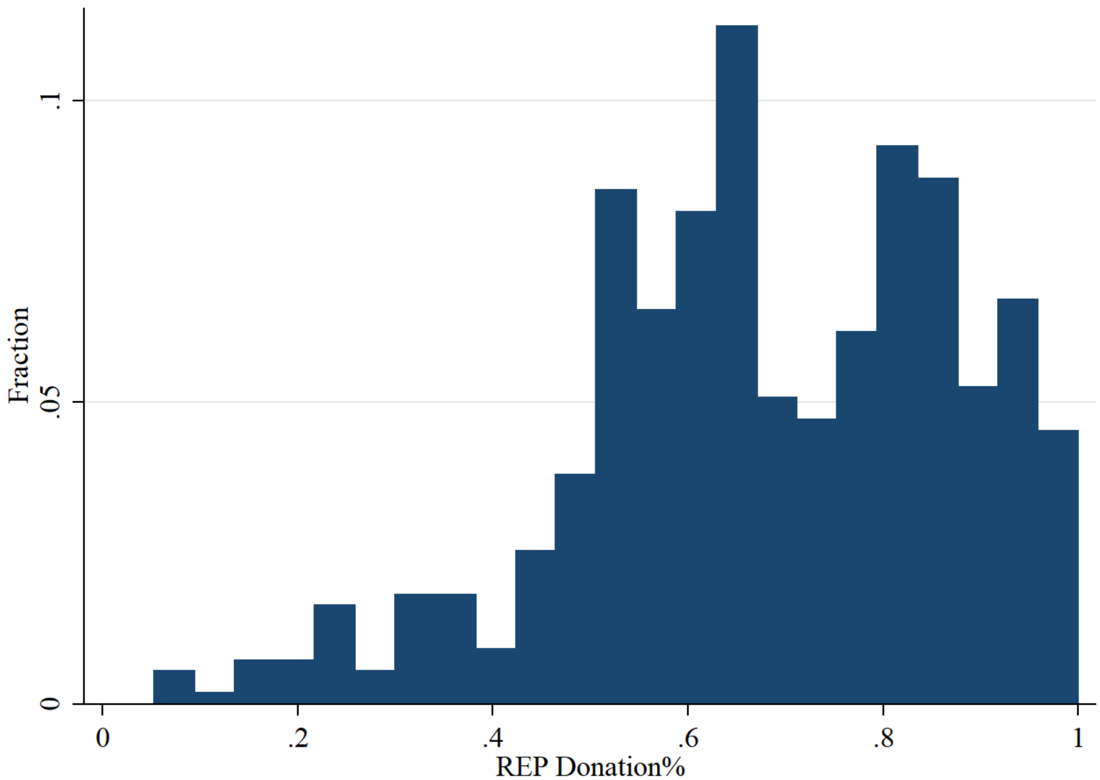


Table 1.A.1: Variable Descriptions

(1) Variable Name	(2) Description	(3) Data Source
Log(WFH)	A county-year level variable. Calculated as the Log of the average of the continuous version of wildfire hazard risk. The continuous version of wildfire hazard risk takes a value from 0 to 100,000	WHP Map
High Risk	A county-year level variable. Calculated as the percentage of land that is classified as High or Very High wildfire risk.	WHP Map
VHigh Risk	A county-year level variable. Calculated as the percentage of land that is classified as Very High wildfire risk.	WHP Map
High Risk (Flooding)	A county-year level variable. Calculated as the percentage of land that is classified as High Flooding risk (code A or V).	FEMA Flood Map
SLR 5 Feet	A county-year level variable. Calculated as the percentage of land that will be submerged into the sea if the sea level rises by 5 feet.	NOAA Sea Level Rise
REP Donation%	A lender-year variable from 0 to 1, measuring the percentage of political donations made by lenders' PACs over the last two election cycles.	FEC Campaign Contribution
Climate Change Risk	A lender-quarter level variable that measures lenders' relative frequency of mentioning climate change together with risk on conference calls.	Sautner et al. (2020)
Climate Change Exposure	A lender-quarter level variable that measures lenders' relative frequency of mentioning climate exposure on conference calls.	Sautner et al. (2020)
Climate Change Sentiment	A lender-quarter level variable that measures lenders' sentiment when mentioning climate change on conference calls.	Sautner et al. (2020)
Approval	An indicator variable at the mortgage-application level indicating whether the mortgage application has been approved.	HMDA
Income Level	A category variable at the mortgage-application level indicating the income level of the mortgage applicants.	HMDA
Male	An indicator variable at the mortgage-application level indicating whether the mortgage applicant is male.	HMDA
White	An indicator variable at the mortgage-application level indicating whether the mortgage applicant is white.	HMDA
Log(Loan Amount)	A mortgage-application level variable. The log of the loan amounts associated with mortgage applications.	HMDA
Log(#Tot Lender Applications)	A lender-year level variable. The log of the number of total mortgage applications received by the lender in the previous year.	HMDA

(1) Variable Name	(2) Description	(3) Data Source
Hold	An indicator variable at the mortgage-loan level indicating whether the mortgage loan is sold in the secondary market. Only for approved applications.	HMDA
Collateral	An indicator variable at the mortgage-loan level indicating whether the mortgage application is denied because of collateral related reasons. Only for denied applications.	HMDA
Log(#app)	A lender-county-year level variable. The log of the number of total mortgage applications received by the lender in the county and year.	HMDA
Avg Loan Amount	A lender-county-year level variable. The average of Log(Loan Amount) at the lender-county-year level.	HMDA
Avg Income Level	A lender-county-year level variable. The average of Income Level at the lender-county-year level.	HMDA
Avg Male	A lender-county-year level variable. The average of Male at the lender-county-year level.	HMDA
Avg White	A lender-county-year level variable. The average of White at the lender-county-year level.	HMDA
Interest Rate	A loan level variable. The interest rate of the mortgage loans.	Fannie Mae and Freddie Mac
Delinquent	A loan-year level indicator variable, indicating whether the loan is going delinquent.	Fannie Mae and Freddie Mac
Credit Score	A loan level variable. The credit score of the borrower at loan origination.	Fannie Mae and Freddie Mac
Debt to Income	A loan level variable. The debt-to-income ratio of the borrower at loan origination.	Fannie Mae and Freddie Mac
Loan to Value	A loan level variable. The loan to value of the borrower at loan origination.	Fannie Mae and Freddie Mac
Unpaid Balance	A loan-year level variable, on the unpaid balance. ?	Fannie Mae and Freddie Mac
Republican-Leaning Bank Presence	A county-year level variable. The average REP Donation% weighted by the number of bank branches within the county.	FDIC Summary of Deposits
%Vote for Republican President	A county-year level variable. The fraction of vote for Republican presidents in the most recent presidential election.	MIT Election Lab
Real Estate Price Growth	A county-year level variable. Growth in real estate price.	Zillow
Employments Growth	A county-year level variable. Growth in local employment.	BEA
GDP Growth	A county-year level variable. Growth in local GDP.	BEA

Table 1.A.2: Variable Correlations

This table presents pairwise correlations among variables. For detailed descriptions of the variables, see appendix Table A.1.

	(1) REP Donation%	(2) %Vote for Republican President	(3) ROE	(4) Log(Assets)	(5) Capital Ratio	(6) Log(#Tot Lender Applications)
REP Donation%	1					
%Vote for Republican President	0.28	1				
ROE	0.11	0.27	1			
Log(Assets)	-0.23	-0.12	0.05	1		
Capital Ratio	-0.03	-0.31	-0.46	-0.03	1	
Log(#Tot Lender Applications)	-0.08	0.06	0.03	0.68	0.13	1

Table 1.A.3: Anecdote Examples on Mortgage Lenders' View toward Climate Risk

This table presents anecdotal evidence on whether mortgage lenders take climate risk into account. The examples are collected from SEC fillings.

(1) Lender	(2) Date of Filing	(3) Filing Type	(4) Quote
JPMorgan Chase	2/23/2021	10-K	... Climate-related physical risks include both acute weather events and chronic shifts in the climate. Potential physical risks from climate change may include altered distribution and intensity of rainfall, prolonged droughts or flooding, increased frequency of wildfires, rising sea levels, or a rising heat index....
Citigroup Inc	11/4/2020	10-Q	...Citigroup also has incorporated environmental factors like climate risk assessment and reporting criteria for certain obligors, as necessary. Factors evaluated include consideration of climate risk to an obligor's business and physical assets and, when relevant, consideration of cost-effective options to reduce greenhouse gas emissions...
Truist Financial Corp.	2/24/2021	10-K	...Deterioration in economic conditions, housing conditions or real estate values, including as a result of climate change or natural disasters, in the markets in which the Company operates could result in materially higher credit losses. The Company is also subject to physical risks, which could manifest in the form of asset quality deterioration and could be exacerbated by specific portfolio concentrations ...
PNC Financial Services	3/1/2019	10-K	. . . Climate change may be increasing the frequency or severity of adverse weather conditions, making the impact from these types of natural disasters on us or our customers worse. . .
Bank of America Corp.	2/24/2021	10-K	... the impact of climate change, such as rising average global temperatures and rising sea levels, and the increasing frequency and severity of extreme weather events and natural disasters such as droughts, floods, wildfires and hurricanes could negatively impact collateral, the valuations of home prices or commercial real estate or our customers' ability and/or willingness to pay outstanding loans... This could also cause insurability risk and/or increased insurance costs to customers...

Table 1.A.4: Mortgage Approval Decisions

This table presents full estimations results of Table 1.3. All samples are at the mortgage application level. The dependent variable, Approval, is an indicator variable set to one if the mortgage application is approved. Panel A includes *second-lien* mortgage applications. Panel B includes *jumbo* mortgage applications. Panel C includes *conforming* mortgage applications. Fixed effects specifications are indicated at the bottom of each panel. Standard errors double clustered by both property state and lender are reported in parentheses below the coefficients. *** p < 0.01, ** p < 0.05, * p < 0.1

Panel A: Second-Lien	(1) Approval	(2) Approval	(3) Approval	(4) Approval	(5) Approval	(6) Approval	(7) Approval
REP Donation%	0.331	-0.161	0.249	0.305	-0.293	0.211	0.298
	(0.268)	(0.243)	(0.248)	(0.261)	(0.196)	(0.260)	(0.277)
REP Donation% × Log(WFH)		0.171***			0.232***		
		(0.049)			(0.045)		
REP Donation% × High Risk			1.741***			2.634***	
			(0.384)			(0.401)	
REP Donation% × VHigh Risk				1.933***			3.283***
				(0.175)			(0.335)
Log(Loan Amount)	0.008	0.008	0.008	0.008			
	(0.010)	(0.010)	(0.010)	(0.010)			
Income Level	0.166***	0.166***	0.166***	0.166***			
	(0.008)	(0.008)	(0.008)	(0.008)			
Male	0.009***	0.009***	0.009***	0.009***			
	(0.003)	(0.003)	(0.003)	(0.003)			
White	0.097***	0.097***	0.097***	0.097***			
	(0.006)	(0.006)	(0.006)	(0.006)			
Log(#Tot Lender Applications)	0.009	0.007	0.008	0.008	0.016**	0.016**	0.017**
	(0.007)	(0.006)	(0.006)	(0.006)	(0.007)	(0.008)	(0.008)
Observations	2,464,455	2,464,455	2,464,455	2,464,455	2,464,455	2,464,455	2,464,455
R-squared	0.191	0.192	0.192	0.192	0.894	0.894	0.894
Similar Application FE	No	No	No	No	Yes	Yes	Yes
County-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Bank-County FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Panel B: Jumbo	(1) Approval	(2) Approval	(3) Approval	(4) Approval	(5) Approval	(6) Approval	(7) Approval
REP Donation%	0.067	-0.024	0.044	0.042	0.034	0.108	0.045
	(0.126)	(0.135)	(0.148)	(0.130)	(0.166)	(0.160)	(0.135)
REP Donation% × Log(WFH)		0.018***			0.022***		
		(0.001)			(0.006)		
REP Donation% × High Risk			0.136***			0.241**	
			(0.043)			(0.106)	
REP Donation% × VHigh Risk				0.333***			1.100***
				(0.021)			(0.152)
Log(Loan Amount)	-0.045***	-0.045***	-0.045***	-0.045***			
	(0.005)	(0.005)	(0.005)	(0.005)			
Income Level	0.394***	0.394***	0.394***	0.394***			
	(0.018)	(0.018)	(0.018)	(0.020)			
Male	0.002	0.002	0.002	0.002			
	(0.004)	(0.004)	(0.004)	(0.004)			
White	0.010**	0.010**	0.010**	0.010**			
	(0.004)	(0.004)	(0.004)	(0.004)			
Log(#Tot Lender Applications)	-0.026	-0.026	-0.026	-0.026	-0.034	-0.034	-0.033
	(0.016)	(0.016)	(0.017)	(0.016)	(0.021)	(0.021)	(0.021)
Observations	1,786,382	1,786,382	1,786,382	1,786,382	1,786,382	1,786,382	1,786,382
R-squared	0.118	0.118	0.118	0.118	0.918	0.918	0.918
County-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Bank-County FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Similar Application FE	No	No	No	No	Yes	Yes	Yes

Panel B: Conforming	(1) Approval	(2) Approval	(3) Approval	(4) Approval	(5) Approval	(6) Approval	(7) Approval
REP Donation%	-0.132	-0.102	-0.131	-0.129	-0.103	-0.084	-0.070
	(0.151)	(0.133)	(0.150)	(0.151)	(0.107)	(0.107)	(0.107)
REP Donation% × Log(WFH)		-0.008			0.011		
		(0.006)			(0.012)		
REP Donation% × High Risk			-0.012			0.228**	
			(0.086)			(0.099)	
REP Donation% × VHigh Risk				-0.093			0.178
				(0.291)			(0.240)
Log(Loan Amount)	0.042***	0.042***	0.042***	0.042***			
	(0.008)	(0.008)	(0.008)	(0.008)			
Income Level	0.077***	0.077***	0.077***	0.077***			
	(0.008)	(0.008)	(0.008)	(0.008)			
Male	-0.003	-0.003	-0.003	-0.003			
	(0.002)	(0.003)	(0.003)	(0.002)			
White	0.060***	0.060***	0.060***	0.060***			
	(0.008)	(0.008)	(0.008)	(0.008)			
Log(#Tot Lender Applications)	-0.033	-0.033	-0.033	-0.033	-0.027	-0.027	-0.027
	(0.026)	(0.026)	(0.026)	(0.026)	(0.021)	(0.021)	(0.021)
Observations	19,288,215	19,288,215	19,288,215	19,288,215	19,288,215	19,288,215	19,288,215
R-squared	0.091	0.091	0.091	0.091	0.925	0.925	0.925
Jumbo Loan	No	No	No	No	No	No	No
First Lien	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Similar Application FE	No	No	No	No	Yes	Yes	Yes
County-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Bank-County FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Table 1.A.5: Mortgage Approval Rate at Lender-County-Year level

This table presents the estimation results for lenders' mortgage approval decisions. The sample is at the lender-county-year level. The dependent variable, Approval Rate, is the lender-county-year level mortgage approval rate. Columns 1 to 3 represent mortgage applications that are second-lien and non-jumbo, and columns 4 to 6 include mortgage applications that are jumbo and first lien. Fixed effects specifications are indicated at the bottom of each panel. Standard errors double clustered by both property state and lender are reported in parentheses below the coefficients. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

	Second-Lien				Jumbo			
	(1) Approval Rate	(2) Approval Rate	(3) Approval Rate	(4) Approval Rate	(5) Approval Rate	(6) Approval Rate	(7) Approval Rate	(8) Approval Rate
REP Donation%	0.210	-0.150	0.156	0.196	0.087	-0.004	0.074	0.086
	(0.216)	(0.170)	(0.199)	(0.212)	(0.129)	(0.131)	(0.131)	(0.129)
REP Donation% × Log(WFH)		0.131***				0.023**		
		(0.038)				(0.009)		
REP Donation% × High Risk			1.342***				0.118**	
			(0.418)				(0.053)	
REP Donation% × VHigh Risk				1.872***				0.014
				(0.485)				(0.098)
Avg Loan Amount	0.011	0.012	0.011	0.011	-0.071***	-0.072***	-0.071***	-0.071***
	(0.012)	(0.012)	(0.012)	(0.012)	(0.010)	(0.010)	(0.011)	(0.010)
Avg Income Level	0.157***	0.158***	0.157***	0.157***	0.385***	0.385***	0.385***	0.385***
	(0.016)	(0.015)	(0.016)	(0.016)	(0.020)	(0.020)	(0.022)	(0.021)
Avg Male	0.026**	0.027**	0.026**	0.026**	0.001	0.001	0.001	0.001
	(0.010)	(0.010)	(0.010)	(0.010)	(0.009)	(0.009)	(0.009)	(0.009)
Avg White	0.081***	0.083***	0.082***	0.081***	0.005	0.005	0.005	0.005
	(0.016)	(0.016)	(0.016)	(0.016)	(0.013)	(0.013)	(0.013)	(0.013)
Log(#Tot Lender Applications)	0.004	0.004	0.004	0.004	-0.037**	-0.037**	-0.037**	-0.037**
	(0.006)	(0.006)	(0.006)	(0.006)	(0.015)	(0.015)	(0.015)	(0.015)
Observations	56,609	56,609	56,609	56,609	45,243	45,243	45,243	45,243
R-squared	0.798	0.799	0.799	0.798	0.682	0.682	0.682	0.682
County-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Lender-County FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Table 1.A.6: Specification with Different Years to Calculate *REP Donation%*

This table presents the estimation from Table 3 but with alternative specifications of REP Donation%, based on 3 years and 5 years.. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

	Second-Lien		Jumbo	
	(1)	(2)	(3)	(4)
	Approval	Approval	Approval	Approval
REP Donation% (3 Years)	0.260		0.061	
	(0.220)		(0.107)	
REP Donation% (3 Years) × High Risk	1.350***		0.063	
	(0.353)		(0.039)	
REP Donation% (5 Years)		0.241		0.116
		(0.233)		(0.179)
REP Donation% (5 Years) × High Risk		1.860***		0.270***
		(0.418)		(0.072)
Log(Loan Amount)	0.009	0.008	-0.045***	-0.045***
	(0.010)	(0.010)	(0.005)	(0.005)
Income Level	0.166***	0.166***	0.395***	0.394***
	(0.008)	(0.008)	(0.019)	(0.019)
Male	0.009***	0.009***	0.002	0.002
	(0.003)	(0.003)	(0.004)	(0.004)
White	0.097***	0.097***	0.010**	0.010**
	(0.006)	(0.006)	(0.004)	(0.004)
Log(#Tot Lender Applications)	0.006	0.007	-0.025	-0.024
	(0.006)	(0.006)	(0.016)	(0.016)
Observations	2,464,455	2,464,455	1,784,881	1,786,382
R-squared	0.192	0.192	0.117	0.118
County-Year FE	Yes	Yes	Yes	Yes
Lender-County FE	Yes	Yes	Yes	Yes

Table 1.A.7: Excluding the Largest 10 Mortgage Lenders

This table presents the estimation of Table 3 but when the largest 10 mortgage lenders are excluded. *** p < 0.01, ** p < 0.05, * p < 0.1

	Second-Lien				Jumbo			
	(1) Approval	(2) Approval	(3) Approval	(4) Approval	(5) Approval	(6) Approval	(7) Approval	(8) Approval
REP Donation%	-0.108	-0.370	-0.127	-0.112	-0.124*	-0.122	-0.127	-0.113
	(0.177)	(0.244)	(0.178)	(0.177)	(0.068)	(0.105)	(0.084)	(0.076)
REP Donation% × Log(WFH)		0.123*				-0.000		
		(0.061)				(0.009)		
REP Donation% × High Risk			1.292***				0.020	
			(0.418)				(0.080)	
REP Donation% × VHigh Risk				1.702***				-0.162
				(0.256)				(0.100)
Log(Loan Amount)	0.009	0.009	0.009	0.009	-0.058***	-0.058***	-0.058***	-0.058***
	(0.010)	(0.010)	(0.010)	(0.010)	(0.006)	(0.006)	(0.006)	(0.006)
Income Level	0.159***	0.159***	0.159***	0.159***	0.398***	0.398***	0.398***	0.398***
	(0.012)	(0.012)	(0.012)	(0.012)	(0.015)	(0.015)	(0.016)	(0.015)
Male	0.005	0.005	0.005	0.005	-0.000	-0.000	-0.000	-0.000
	(0.004)	(0.004)	(0.006)	(0.004)	(0.003)	(0.003)	(0.003)	(0.003)
White	0.097***	0.097***	0.097***	0.097***	0.010***	0.010***	0.010***	0.010***
	(0.011)	(0.011)	(0.011)	(0.011)	(0.002)	(0.003)	(0.003)	(0.003)
Log(#Tot Lender Applications)	-0.004	-0.003	-0.003	-0.003	-0.036***	-0.036***	-0.036***	-0.036***
	(0.006)	(0.006)	(0.006)	(0.006)	(0.012)	(0.012)	(0.012)	(0.012)
Observations	866,699	866,699	866,699	866,699	497,977	497,977	497,977	497,977
R-squared	0.239	0.239	0.239	0.239	0.187	0.187	0.187	0.187
County-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Lender-County FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Table 1.A.8: Poisson Estimation on the Number of Mortgage Applications

This table presents the same estimation as Table 1.9 but using a Poisson estimation, Cohn et al. (2021). *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

	Second-Lien				Jumbo			
	(1) #App.	(2) #App.	(3) #App.	(4) #App.	(5) #App.	(6) #App.	(7) #App.	(8) #App.
REP Donation%	-0.240	-2.174	-0.421	-0.248	0.169	-0.971	-0.259	-0.068
	(0.997)	(1.437)	(1.017)	(0.998)	(0.622)	(0.698)	(0.668)	(0.633)
REP Donation% × Log(WFH)		0.684**				0.230***		
		(0.290)				(0.020)		
REP Donation% × High Risk			3.903				2.486***	
			(2.415)				(0.241)	
REP Donation% × VHigh Risk				0.634				3.232***
				(1.896)				(0.643)
Avg Loan Amount	-0.095	-0.086	-0.093	-0.095	-0.337**	-0.337**	-0.339**	-0.338**
	(0.141)	(0.144)	(0.142)	(0.141)	(0.171)	(0.171)	(0.171)	(0.171)
Avg Income Level	-0.265*	-0.255	-0.265*	-0.265*	0.405***	0.405**	0.403**	0.403**
	(0.160)	(0.156)	(0.159)	(0.160)	(0.157)	(0.164)	(0.161)	(0.161)
Avg Male	-0.034	-0.036	-0.035	-0.034	0.101*	0.102*	0.106**	0.103*
	(0.098)	(0.098)	(0.097)	(0.114)	(0.053)	(0.053)	(0.054)	(0.053)
Avg White	-0.087	-0.071	-0.086	-0.087	-0.118*	-0.113	-0.114	-0.115
	(0.100)	(0.098)	(0.099)	(0.110)	(0.070)	(0.074)	(0.084)	(0.083)
Log(#Tot Lender App.)	0.098	0.091	0.096	0.098	0.127	0.132	0.130	0.130
	(0.103)	(0.097)	(0.101)	(0.103)	(0.109)	(0.109)	(0.109)	(0.109)
Observations	92,340	92,340	92,340	92,340	96,325	96,325	96,325	96,325
County-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Lender-County FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Table 1.A.9: With Bank-Year Call Report Controls

The table presents the estimation from Table 1.3 with bank-year level controls, including Log(Total Assets), ROE, and Capital Ratio. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

	Second-Lien				Jumbo			
	(1) Approval	(2) Approval	(3) Approval	(4) Approval	(5) Approval	(6) Approval	(7) Approval	(8) Approval
REP Donation%	0.517*	0.031	0.426	0.485*	0.042	-0.050	0.007	0.017
	(0.275)	(0.229)	(0.250)	(0.266)	(0.074)	(0.116)	(0.096)	(0.084)
REP Donation% × Log(WFH)		0.149***				0.019		
		(0.048)				(0.011)		
REP Donation% × High Risk			1.565***				0.195*	
			(0.333)				(0.109)	
REP Donation% × VHigh Risk				1.778***				0.332*
				(0.165)				(0.183)
Log(Loan Amount)	0.009	0.009	0.009	0.009	-0.041***	-0.041***	-0.041***	-0.041***
	(0.010)	(0.010)	(0.010)	(0.010)	(0.004)	(0.004)	(0.005)	(0.005)
Income Level	0.167***	0.167***	0.167***	0.167***	0.404***	0.404***	0.404***	0.404***
	(0.008)	(0.008)	(0.008)	(0.008)	(0.021)	(0.021)	(0.021)	(0.023)
Male	0.009***	0.009***	0.009***	0.009***	0.001	0.001	0.001	0.001
	(0.003)	(0.003)	(0.003)	(0.003)	(0.004)	(0.004)	(0.004)	(0.004)
White	0.097***	0.097***	0.097***	0.097***	0.009**	0.009**	0.009*	0.009**
	(0.006)	(0.006)	(0.006)	(0.006)	(0.004)	(0.004)	(0.004)	(0.004)
ROE	0.231	0.191	0.210	0.209	0.056	0.059	0.058	0.058
	(0.367)	(0.342)	(0.344)	(0.357)	(0.135)	(0.134)	(0.135)	(0.135)
Capital Ratio	1.866**	1.238	1.675*	1.739**	1.201**	1.171**	1.181**	1.182**
	(0.830)	(0.808)	(0.834)	(0.840)	(0.461)	(0.467)	(0.463)	(0.465)
Log(Total Assets)	0.112	0.070	0.101	0.111	-0.013	-0.014	-0.013	-0.013
	(0.074)	(0.073)	(0.072)	(0.073)	(0.017)	(0.017)	(0.017)	(0.017)
Observations	2,423,487	2,423,487	2,423,487	2,423,487	1,636,762	1,636,762	1,636,762	1,636,762
R-squared	0.192	0.192	0.192	0.192	0.106	0.106	0.106	0.106
County-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Lender-County FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Table 1.A.10: Census Tract FEs Instead of County as FEs

The table presents the estimation from Table 1.3 but with *Census Tract-Year* fixed effects and *Lender-Census Tract* fixed effects instead of *County-Year* fixed effects and *Lender-County* fixed effects. *** p < 0.01, ** p < 0.05, * p < 0.1

	Second-Lien				Jumbo			
	(1) Approval	(2) Approval	(3) Approval	(4) Approval	(5) Approval	(6) Approval	(7) Approval	(8) Approval
REP Donation%	0.341	-0.208	0.246	0.311	0.078	-0.030	0.055	0.047
	(0.257)	(0.229)	(0.238)	(0.249)	(0.122)	(0.137)	(0.152)	(0.132)
REP Donation% × Log(WFH)		0.185***				0.021***		
		(0.051)				(0.002)		
REP Donation% × High Risk			1.817***				0.121*	
			(0.368)				(0.070)	
REP Donation% × VHigh Risk				2.024***				0.382***
				(0.192)				(0.057)
Log(Loan Amount)	0.002	0.002	0.002	0.002	-0.050***	-0.050***	-0.050***	-0.050***
	(0.008)	(0.008)	(0.008)	(0.008)	(0.006)	(0.006)	(0.006)	(0.006)
Income Level	0.156***	0.156***	0.156***	0.156***	0.400***	0.400***	0.400***	0.400***
	(0.006)	(0.006)	(0.006)	(0.007)	(0.022)	(0.022)	(0.022)	(0.027)
Male	0.010***	0.010***	0.010***	0.010***	0.003	0.003	0.003	0.003
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.004)	(0.004)
White	0.086***	0.086***	0.086***	0.086***	0.008**	0.008**	0.008**	0.008**
	(0.006)	(0.006)	(0.006)	(0.006)	(0.003)	(0.003)	(0.003)	(0.004)
Log(#Tot Lender Applications)	0.012*	0.009*	0.010*	0.011*	-0.024	-0.023	-0.024	-0.024
	(0.006)	(0.005)	(0.006)	(0.006)	(0.016)	(0.016)	(0.016)	(0.016)
Observations	2,464,455	2,464,455	2,464,455	2,464,455	1,786,382	1,786,382	1,786,382	1,786,382
R-squared	0.434	0.435	0.434	0.434	0.320	0.320	0.320	0.320
Census Tract-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Lender-Census Tract FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Chapter Two

Downsides of Corporate Political Spending: Evidence from Mass Shootings

2.1 Introduction

An extensive literature documents that corporate political connections are valuable to firms in various aspects (see, e.g., Cooper et al. 2010, Akey 2015, and Brown and Huang 2020). However, engaging in politics could also have costs for firms because not all stakeholders agree with corporate leaders' political views. For example, hosting fundraisers for particular political candidates could build valuable political connections for a firm, but at the same time, it could displease some stakeholders who dislike the candidates. Moreover, not all political connections are motivated by firm interests. There could be agency issues, with corporate managers pushing towards particular political ideologies. Few papers, however, study whether corporate political spending can negatively influence firms themselves, which this paper aims to study formally. In this paper, I explore the downsides of corporate political spending and find that when mass shootings take place, companies that primarily support pro-gun-rights politicians experience negative stock market reactions and worse operating performance. Overall, my study highlights the negative impacts on companies' bottom line resulting from conflicts in political views between companies and their stakeholders.

There is abundant anecdotal evidence showing that firms were boycotted or threatened with backlash for making corporate or executive donations to politicians. For example, in 2010, more than 240,000 people signed a petition to boycott Target and Best Buy for indirectly supporting congressman Tom Emmer, who is well known for opposing abortion and same-sex marriage. After the 2016 presidential election, some Trump supporters called to boycott PepsiCo because its CEO talked critically about the election results in an interview. In 2018, consumers threatened In-N-Out with a boycott for donating $25,000 to the California Republican Party, and Land O'Lakes and Purina were similarly threatened for making contributions to a nationalism candidate Steve King with both companies eventually withdrawing their donations. Lately, in 2019, consumers threatened Home Depot with backlash after its co-founder Bernie Marcus pledged to support President Trump for re-election in 2020. After the 2021 Capitol Hill Riot, several companies face political backlash for making political donations to Republican lawmakers who opposed the certification of the 2020 U.S. Presidential election. After the Riot, a list of companies halted their political donations to distance themselves from these Republican lawmakers.[1] A 2018 report from the Center for Political Accountability provides many similar anecdotes highlighting the unintended consequences of corporate political contributions.[2] All of this evidence suggests that stakeholders pay attention to how companies support political candidates and sometimes react negatively to corporate political contributions.

Motivated by these anecdotes, I investigate the negative impacts of corporate political spending on firm outcomes. Studying this question is challenging because companies endogenously make political donations to politicians based on firm interests and stakeholders' political preferences. Thus, naive regressions of corporate performance on corporate political contributions suffer from severe endogeneity bias. This paper

[1]See "Target Discovers Downside to Political Contributions", *The Wall Street Journal*, August 7, 2010; "Mad About Corporate Political Donations, Customers Boycott Target, Best Buy", *National Public Radio*, August 4, 2010; "People Are Calling For An In-N-Out Boycott Because Burger Chain Donated To GOP", *The Huffington Post*, August 30, 2010; "Land O'Lakes faces calls for boycott over donation to U.S. Rep Steve King", *MinnPost*, October, 30, 2018; "Land O'Lakes withdraws support for GOP Rep. Steve King after boycott calls", *The Washington Post*, October 30, 2018; "Purina cuts ties to GOP's Steve King after boycott threats", *CBS News*, October 30, 2018; "Home Depot Responds To Calls For Boycott Over Co-Founder's Support For Trump", *National Public Radio*, July 10, 2019; "The Equinox and SoulCycle boycotts, explained", *Vox*, August 8, 2019; "Hudson Yards and the CFDA Face Backlash Over Involvement With Stephen Ross", *Elle*, August 12, 2019; "Corporate Political Contributions", *Harvard Law School Forum on Corporate Governance*, February 3, 2021; "Capitol Riot: See the Full List of Companies Halting PAC Donations", *The Wall Street Journal*, January 12, 2021.

[2]See "Collision Course: The Risks Companies Face When Their Political Spending and Their Core Values Conflict, and How to Address Them", *Center for Political Accountability,*, June 19, 2018. For similar articles, see "Boycotts. Backlash. Breitbart: U.S. companies confront a volatile political climate", *The Washington Post*, December 2, 2016.

exploits mass shootings, which are plausibly unrelated to firm business fundamentals, as exogenous shocks to public perception on the gun control issue. Mass shootings take place repeatedly in the United States, see Figure 1. Moreover, these tragic events often polarize public opinions towards gun policy (see, e.g., Demszky et al. 2019, Barney and Schaffner 2019, and Yousaf 2018), which in turn can spill over to the firms that primarily support pro-gun-rights politicians. With the deepening political divide after mass shootings, companies' existing political spending can lead to partisan responses from their stakeholders. For example, after the 2018 school shooting at the Parkland High School, several protests took place at Publix grocery stores because of Publix's political donations to pro-gun-rights political candidates, and Publix subsequently apologized and suspended its political contributions.[3]

I conduct multiple tests to study whether mass shootings impact companies differently depending on how companies support politicians that are more supportive of gun rights. I rely on campaign finance data from the Federal Election Commission (FEC) to measure how companies donate to pro-gun-rights politicians. Specifically, I define pro-gun-rights politicians as politicians who receive campaign donations from the *National Rifle Association (NRA)*. Then, I define *Pro-Gun-Rights* firms as those that primarily donate to pro-gun-rights politicians based on both corporate political action committees (PACs) and corporate executives donations. The focus of this paper is how corporate political donations can backfire when mass shootings take place. Thus, I exclude all gun manufacturer companies from the analysis because mass shootings can have direct impacts on these companies in various ways (Levine and McKnight 2017).

As shown in Figure 2, the first key finding of this paper is that *Pro-Gun-Rights* firms experience significantly negative abnormal returns on mass shooting days. In mass shootings with more than 10 people get killed, relative to *Non-Pro-Gun-Rights* firms, (-1, +1) cumulative abnormal returns (CARs) based on the Fama-French three-factor model are 0.91% lower for *Pro-Gun-Rights* firms.[4] Importantly, I find that the stock price reactions are substantially weaker when incidents are less deadly, suggesting that the negative stock price reactions to *Pro-Gun-Rights* firms are stronger when mass shootings lead to greater political turmoil.

[3]See "Shoppers Boycott Publix Over Store's Donations to NRA-Backed Candidate", *The Huffington Post*, May 22, 2018.
[4]See Column 1, Table 3.

Next, I find that after mass shootings, *Pro-Gun-Rights* firms experience worse operating performance measured by sales and other variables. This finding is consistent across two different sets of specifications. The first set of specifications exploits the Google search volume index on shootings from companies' headquarter states to capture public awareness on mass shootings and then interacts the Google search volume index with the *Pro-Gun-Rights* variable. The second set of tests directly interacts the exact timing of mass shootings with the *Pro-Gun-Rights* variable. In both specifications, comparing with *Non-Pro-Gun-Rights* firms, *Pro-Gun-Rights* firms perform substantially worse, with lower sales and lower asset turnover, etc. The findings hold after controlling for firm fixed effects, industry-time fixed effects, and state-time fixed effects. The dynamic regression suggests that declines in operating performance are temporary. Lastly, I estimate a placebo regression by interacting the *Pro-Gun-Rights* variable with the Google search volume index on earthquakes and find no statistically significant effects.

Although I control for a battery of fixed effects, one may wonder whether the findings are driven by industry differences across firms; companies in different industries have varied political interests and can be impacted differently by mass shootings. To address this concern and to provide more granular evidence, I exploit the Summary of Deposits data from Federal Deposit Insurance Corporation (FDIC). The granularity of the deposit data allows me to include county-time fixed effects to absorb shocks common to a county and bank branch fixed effects to absorb branch-level unobserved heterogeneity. The data also allow me to test the importance of the physical proximity between mass shooting location and depositor location. I find similar evidence that *Pro-Gun-Rights* banks experience about 5.7% higher deposit outflow after mass shootings.[5] Moreover, the results suggest that physical proximity matters little and that deposit outflow is a bank-wide effect when deadliest incidents happen.

Admittedly, gun control is a partisan issue; Republican politicians are more supportive of gun rights than Democratic politicians, and thus *Pro-Gun-Rights* firms mostly donate to Republican politicians. Does the finding suggest that Republican-leaning companies also suffer when mass shootings occur? To answer this question, I include both *Pro-Gun-Rights* variable and *Pro-Republican* variable in the regressions.

[5]See Column 3, Table 12.

Although *Pro-Gun-Rights* and *Pro-Republican* are highly correlated, there are sufficient variations to disentangle two effects. Based on the FEC data, 27% of candidates who received NRA donations are not Republicans, and only 18% of Republican candidates ever receive contributions from the NRA. The results suggest that the effects come mostly from companies donating to pro-gun-rights politicians.

This paper attempts to distinguish two potential mechanisms through which mass shootings can have negative impacts on *Pro-Gun-Rights* firms. As discussed above, the first channel is about conflicts in political views between companies and their stakeholders. After mass shootings and subsequent media coverage, company stakeholders pay more attention to the gun control issue and can be displeased if the company mostly makes political donations to pro-gun-rights politicians. Consequently, stakeholders' disapproval of company political donations leads to lower sales and worse operating performance.

The second channel is related to the value of corporate political connections. If gun-rights politicians from the mass shooting states are less likely to win elections after mass shootings, political connections with these politicians can become less valuable for *Pro-Gun-Rights* firms. Testing whether gun-rights politicians lose office after mass shootings is beyond the scope of this paper. Results from two tests, however, suggest that the first mechanism is more plausible. First, if political connections to pro-gun-rights politicians from mass shooting states become less valuable, the negative stock price reactions should be stronger for firms that donated to pro-gun-rights politicians from mass shooting states. I find that when the deadliest mass shootings (more than 10 people get killed) take place, there is no statistically significant difference in stock market reactions of *Pro-Gun-Rights* firms headquartered in other states regardless of whether *Pro-Gun-Rights* firms have connections to pro-gun-rights politicians from mass shooting states. Second, the results from the deposit market strongly support the first channel because depositors represent the one of the main stakeholders of banks.

To further rule out the second channel, I test how firm donations change after mass shootings. I find that after incidents, firms substantially reduce political donations from corporate political action committees (PACs) to pro-gun-rights politicians, while personal contributions from corporate executives to pro-gun-rights politicians decrease only marginally. This finding is also similar to what happened after the 2021 Capitol Hill Riot. After the riot, many companies pulled back political donations to Republican politicians who opposed the certification of the 2020 U.S. Presidential election. Suspending political donations can help companies to protect themselves from the political chaos.

A large number of papers study the benefits of corporate political donations in the context of the United States.[6] Fewer papers, however, have studied the costs or risks of political spending. Di Giuli and Kostovetsky (2014) show that Democratic-leaning managers invest more in corporate social responsibility with little benefits to companies. Fisman and Wang (2015) find that in China, politically connected firms have a higher worker death rate. Bertrand et al. (2018) show that French firms support incumbent politicians by hiring more employees but receive little benefits after the incumbent politicians are re-elected. Using campaign donations from both CEOs and employees, Ren (2020) shows that conflicts in political ideologies between CEOs and employees are negatively associated with firms' future operating performance. The closest paper to mine is Painter (2020). Based on foot traffic data, Painter (2020) find that consumers respond strongly to the Walmart restrictive gun policy after the El Paso shooting. My paper complements this body of research by providing large sample empirical evidence on negative shocks to politically active companies after unexpected controversial events.

This paper also contributes to a burgeoning literature on how political beliefs impact individuals' behavior. Mian et al. (2018) and Meeuwis et al. (2018) study how political belief impact householders' decision in consumption and portfolio allocation. Barrios and Hochberg (2020) study the relationship between partisan belief and social distance behavior during the Covid-19 pandemic. Cookson et al. (2020) study how partisan beliefs impact investors' trading behavior. Kempf and Tsoutsoura (2018) study how partisan beliefs affect credit rating analysts' rating decisions. Duchin et al. (2019) study the role of political attitude in mergers and acquisitions. My paper adds to this literature by studying how an unexpected surge of divergence in political beliefs between corporations and stakeholders impacts companies' bottom line.

[6] For studies on stock market reactions when firms establish connections, see, e.g., Cooper et al. 2010, Akey 2015, Acemoglu et al. 2016, and Brown and Huang 2020; for studies on access to government resource, see, e.g., Khwaja and Mian 2005, Faccio et al. 2006, Claessens et al. 2008, Duchin and Sosyura 2012, Houston et al. 2014, Tahoun 2014, Goldman et al. 2013, Amore and Bennedsen 2013, Brogaard et al. 2015, and Aobdia et al. 2018; for evidence relative to law legislation and law enforcement, see, e.g., Mian et al. 2010, Ovtchinnikov and Pantaleoni 2012, Cohen et al. 2013, Yu and Yu 2011, Fulmer et al. 2012, Bourveau et al. 2016, Correia 2014, and Mehta et al. 2019.

The remaining sections of the paper are organized as follows. Section 2 describes the institutional background and data. Section 3 presents the results of stock market reactions around mass shootings. Section 4 presents tests on firms' operating performance. Section 5 presents results based on FDIC deposit data. Section 6 provides robustness test results and discussions. Section 7 concludes.

2.2 Institutional Background and Data

2.2.1. Brief Overview on Federal Campaign Finance Law

Since this paper relies on corporate political contributions from both corporate PACs and corporate executives to identify firms which primarily support gun-rights politicians, a few features of federal campaign contributions are worth describing.[7] Under Federal Election Campaign Act, 2 U.S.C. § 441b, political candidates are prohibited from accepting contributions from the treasury funds of corporations. In order to make campaign contributions, companies must form political action committees (PACs), which can solicit voluntary contributions by shareholders, employees, and family members of those two groups. Corporations often create internal oversight committees to manage PAC activities, and these internal oversight committees are usually chaired by senior corporate executives.[8] In addition, corporate PACs are subject to limits on the amount of contributions to each candidate per election, and the limits depend on whether corporate PACs are qualified as multicandidate PACs. In the 2019 - 2020 election cycle, the limit on qualified multicandidate PACs is $5,000 for each candidate per election, while the limit on non-qualified PACs is $2,800 for each candidate per election.

Similarly, federal law maintains strict regulations on individual contributions to federal candidates. For the 2019 – 2020 federal election, an individual can directly contribute up to $2,800 to each candidate per election. Moreover, federal law strictly prohibits contributions in the name of others. Under Federal law 52 U.S.C. §§ 30122 and 30109, reimbursing someone for a contribution or otherwise contributing in the

[7] See the Federal Election Commission website for more details.

[8] See "The 2018 CPA-Zicklin Index of Corporate Political Disclosure and Accountability", *Center for Political Accountability*, October 2, 2018.

name of another person can result in substantial civil penalties and jail time.

Under current regulations, corporations are not required to disclose political expenditures of their PACs or individual contributions from employees in their financial reports or on their websites. Political candidates, however, are required to itemize corporate PAC and individual contributions in excess of $200 and report amounts, dates, related companies, and occupations if the contributions were made by individuals. The FEC makes both PAC and individual contribution data publicly available on its website. Based on this information, organizations such as the Center for Responsive Politics and the Progressive Shopper independently track campaign contributions from corporate PACs and corporate employees.

2.2.2. Data

The sample consists of data from various sources. The data on mass shootings are from two non-profit projects: the Stanford "Mass Shootings in America" and the Mother Jones "A Guide to Mass Shootings in America".[9] Contribution data are from the Federal Election Commission (FEC), and standard data on firm stock returns and firm fundamentals are from CRSP and Compustat. Branch-level deposits are from the FDIC Summary of Deposits data. The following subsections describe the data in detail.

Data on Mass Shootings

This paper uses 20 years of mass shootings data starting from 1999, the year in which the Columbine High School mass shooting took place. The Stanford project stopped updating its dataset in 2017, while the Mother Jones project continues to the present. To attain a comprehensive coverage of mass shootings, I combine records from both data sources. In total, there are 242 mass shootings from 1999 to 2018. In about half of the 242 shootings, three or fewer people were killed.[10] These shootings are likely to be homicides and are less likely to reach a nation-wide impact. Thus, I restrict to incidents with more than 10 fatalities in most of specifications. Table 1 lists all 15 incidents with more than 10 fatalities from 1999 to

[9]Both data has been used in the literature, e.g., Newman and Hartman 2019, Balasubramaniam 2018, and Barney and Schaffner 2019.

[10]If the shooter died during the mass shooting, this individual is included in the number of fatalities.

2018.

Figure 1 graphically describes locations of 61 mass shootings across the United States. The 61 incidents include mass shootings in which at least 6 people were killed. Sizes of circle markers are positively associated with the numbers of fatalities of correspondent incidents. Based on this figure, there are two patterns of interest. First, except for the middle western region of the United States, mass shootings took place widely across the entire country. Second, mass shootings were disproportionally more likely to take place in metropolitan than in rural areas. This figure is informative because it suggests that the findings are not restricted to specific geographical regions of the United States.

Pro-Gun-Rights Politicians

I define pro-gun-rights politicians as federal politicians who receive campaign supports from the National Rifle Association (NRA) during their elections. The NRA supports candidates in several ways, including direct contributions, independent advocating, and public endorsement. In each election cycle, the NRA makes decisions on supporting a candidate depending on whether the candidate strongly supports gun rights. A gun-rights score is assigned to each candidate by the NRA. Candidates with high gun-rights score are much more likely to receive support from the NRA than candidates with low scores. See the following statement from the NRA-PVF website on how it ranks each candidate.

> *The NRA-PVF ranks political candidates - irrespective of party affiliation - based on voting records, public statements and their responses to an NRA-PVF questionnaire.*

Because NRA grades and endorsements are often unavailable, I rely on NRA contributions to identify pro-gun-rights candidates. A candidate in an election is identified as a pro-gun-rights candidate if the candidate receives supportive money from any of the three NRA related PACs during the campaign.[11] The first PAC (FEC ID: C00053553) is a qualified PAC which can contribute to candidates directly. The

[11] Beside supporting candidates, NRA can also spend money to oppose the election of some candidates. To avoid capturing candidates opposed by NRA, I restrict to all 3 types of supportive campaign transactions including 24E (Independent expenditure advocating election of candidate), 24F (Communication cost for candidate), and 24K (Contribution made to nonaffiliated committee).

second one (FEC ID: C70000716) is a communication cost PAC designed to advocate the election of specific candidates within *NRA* members. Both PACs have been active since the 1980 election cycle, which is the earliest cycle with contribution data available. The third PAC (FEC ID: C90013301), which was established after 2010, is a super PAC that makes independent expenditures to marketing services companies, such as Prolist and Broadnet Teleservices, to support or oppose elections of federal candidates.

There are two main concerns with the definition of pro-gun-rights politicians as candidates who receive campaign supports from the NRA. The primary concern is about party affiliations of pro-gun-rights politicians. Republican politicians are more supportive for gun rights than Democratic politicians. Are pro-gun-rights politicians perfectly aligned with Republican politicians? The answer is no. Appendix Figure 1 plots changes in the percentages of non-republican candidates funded by the NRA overtime. Before the 2012 election cycle, around 20% of NRA supported candidates are non-Republicans, and the percentage drops to less than 5% in the 2018 election cycle. The drop corresponds to President Obama's strong position in calling for stricter gun control policies during his tenure. Moreover, only 27% Republican politicians received campaign donations from the NRA from 2000 to 2018 election cycle. Later, I utilize the non-perfect correlation between Republican politicians and NRA funded politicians to specifically test whether Republican-leaning companies experience similar negative shocks around mass shootings.

The second concern is whether NRA funded candidates are indeed supportive of gun rights and whether the public knows political stances on gun control issues of these candidates. If the public is not aware of candidates' stances on gun control policy, corporate contributions to these candidates hardly represent anything for the public. To address this concern, I exploit the NRA endorsement data comes from *Everytown.org*, a nonprofit gun-control organization. The data covers 4 cycles of NRA endorsed candidates from 2008 to 2016. After matching with FEC data, I find that NRA endorsed candidates and NRA funded candidates are about 90% overlapped, suggesting that NRA funded candidates are candidates who strongly support gun rights. Also, after mass shootings, politicians tend to talk about gun control issues, and media usually cover their statements. Thus, the public are unlikely to be uninformed about the political stance of NRA funded candidates on gun control issues.

Corporate PAC Contributions

Following the literature, the first measurement of whether companies primarily support pro-gun-rights politicians is based on corporate PAC contributions to federal candidates. Corporate PAC contribution are widely used in the literature to identify corporate political connections (see, e.g., Stratmann 1992, Cooper et al. 2010, and Akey 2015).

To obtain PAC contributions, I match all PACs of corporation organization type (FEC ORG_TP: C) with firms from Compustat from 1980 to 2018. The matching procedure is by matching PACs' organization names with company names from Compustat. After the matching, I manually verify that each match is correct. The final matched sample includes 2,365 unique firms. The number of matched firms is comparable to that in Cooper et al. (2010), which have 1,930 matched firms from 1984 to 2005.

Corporate Executives Individual Contributions

Although corporate PAC contributions are widely used in the literature, firms can support politicians through many other avenues (see, e.g. Bebchuk and Jackson 2010, Bertrand et al. 2018, and Babenko et al. 2020). One major avenue is corporate executives' individual political donations. The amount of donations made by corporate executives is sizable. In the 2018 election cycle alone, CEOs from S&P500 companies made over $24 million political donations.[12] Only counting the corporate PAC contributions would miss an important piece of how firms support politicians. Thus, I construct another measurement based on corporate executives' individual contributions to federal political candidates.

To identify political contributions made by corporate executives, I rely on executives' employment records from Execucomp to match with individual contributions data from the FEC. Execucomp provides information on executive employment and executive compensations from S&P 1500 firms starting from 1992. I apply the matching method from Babenko et al. (2020). The match method consists of steps including both exact and fuzzy matches. The fuzzy match step produces a similarity score measuring the distance between two matched strings. I keep all fuzzy matches with score above 0.8. The 0.8 cut point is

[12] See "How America's top CEOs are spending their own money on the midterm elections", *MarketWatch*, October 22, 2018.

based on a balance between accuracy and number of matches. The final matched executive contributions sample between 1996 to 2018 includes 2,311 unique firms, 8,795 unique executives, and 65,837 political contributions.

An immediate question is whether the match process accurately captures executive contributions. Thanks to an occupation variable provided by the FEC starting from 2004, it is possible to verify these matches through self-reported occupations from the individual contributions data. Appendix Table A.6 reports most frequent self-reported occupations of the final matched executive contributions from 2004 to 2018. Based on the table, the vast majority of self-reported occupations are indeed top executives, which, to a large extent, addresses the accuracy concern of the matching procedure.

Pro-Gun-Rights Firms

I use the percentage of corporate political contributions donated to pro-gun-rights politicians to identify firms that are closely related to pro-gun-rights politicians.[13] A firm-year pair is identified as *Pro-Gun-Rights* if the firm has a high percentage of corporate PAC or executives' individual contributions to gun-rights politicians in the last three years. This measurement is also similar to the one used by a gun control group, Guns Down America.[14]

I calculate two variables *Pro-Gun-Rights (PAC)* and *Pro-Gun-Rights (Executives)* based on corporate PAC contributions and corporate executive contributions, respectively.[15] First, for each firm-year pair, I separately calculate the total direct contributions made by its corporate PAC or executives to all federal candidates in the last three years. For example, the total PAC (executives) contributions from a firm in 2018 equal the sum of its corporate PAC (executives) contributions to all federal candidates made in 2015, 2016, and 2017. To avoid extreme value driven by a small amount of contributions, I drop firm-year pairs if there are less than $2000 total PAC (executives) contributions to all candidates in the last three years. Second,

[13] Since both corporate PACs and executives rarely make direct contributions to NRA PACs, I do not rely on direct contributions from corporations to NRA to identify *Pro–Gun-Rights* firms.

[14] See "Gun Control Group's Report Card on U.S. Banks' Firearms Ties Has Several Fs," *New York Times*, April 4, 2019.

[15] I restrict PAC contributions to transaction code 24K (Contribution made to non-affiliated committee), which represent over 99% of all corporate PAC contributions. Similarly, corporate executives' contributions are restricted to transaction code 15 (direct contributions to candidates), which represents over 97% of all corporate executive contributions.

applying the same rule, I separately calculate the total direct contributions to pro-gun-rights politicians made by corporate PACs or executives. After the first and second steps, for each firm-year pair, there are four variables available, including *Total PAC (Executive) contributions to all candidates* and *Total PAC (Executive) contributions to NRA funded candidates*. The third step is to calculate two ratios based on the following equations.

$$Gun\ Right\ Ratio\ (PAC) = \frac{Total\ PAC\ Contributions\ to\ NRA\ Funded\ Candidates}{Total\ PAC\ Contributions\ to\ All\ Candidates}$$
$$Gun\ Right\ Ratio\ (Exe) = \frac{Total\ Executive\ Contributions\ to\ NRA\ Funded\ Candidates}{Total\ Executive\ Contributions\ To\ All\ Candidates}$$

Finally, based on these two ratios, a firm-year observation is identified as *Pro-Gun-Rights (PAC)* if Gun Right Ratio (PAC) is above 70 percent. Similarly, a firm-year combination is identified as *Pro-Gun-Rights (Executives)* if Gun Right Ratio (Executive) is above 70 percent. To have a complete view of how companies support pro-gun-rights politicians, I combine two treatment variables. If a firm-year is classified as either *Pro-Gun-Rights (PAC)* or *Pro-Gun-Rights (Executive)*, the firm-year is label as *Pro-Gun-Rights* in the combined treatment variable. The findings are not sensitive to the 70 percent cutoff. In section 6, I report robustness tests by replacing the *Pro-Gun-Rights* dummy with Gun Right Ratio. In the most of specifications, I use *Pro-Gun-Rights* as the independent variable. In section 6, I report robustness tests based on *Pro-Gun-Rights (PAC)* and *Pro-Gun-Rights (Executives)* separately, and the results are qualitatively similar.

Figure 3 plots the distribution of the *Pro-Gun-Rights* firms by industry and geographical region. In Panel A, the horizontal axis represents the 11 two-digit industries under the Global Industry Classification Standard (GICS) classification obtained from Compustat, and the vertical axis reports the fraction of observations with *Pro-Gun-Rights* variable equals one within each industry. Similarly, the horizontal axis in Panel B represents the 4 geographical regions following the U.S. Census Bureau-designated regions, and the vertical axis reports the fraction of observations with the *Pro-Gun-Rights* dummy equals one within each geographical region. Based on this figure, the *Pro-Gun-Rights* variable is particularly common among companies from the energy industry and Midwest and South area. This pattern is consistent with Cohen et al. (2019), who study the political preference of corporate CEOs.

2.3 Stock Price Reaction

To study the downsides of corporate political spending, I first examine equity market reactions of *Pro-Gun-Rights* firms around mass shootings. Cumulative abnormal returns (CARs) are calculated from CRSP based on the Fama-French three-factor model. I measure CARs using a one-year estimation window (253 day, with least 70 days in the estimation window) that ends 50 days before the event window.[16] To have an accurate measurement of CARs, I exclude events which occur on non-trading days from my analysis. Gun related firms under three-digit SIC code "348" (Ordnance And Accessories, Except Vehicles And Guided Missiles) are excluded from all specifications.

Among all firms headquartered in the mass shooting states, I find that on average *Pro-Gun-Rights* firms experience negative and significant stock price reactions around mass shootings. I do not observe the decline in stock prices of *Non-Pro-Gun-Rights* firms which also headquarter in the mass shooting states but are less supportive for pro-gun-rights politicians. Importantly, the negative stock price reaction for *Pro-Gun-Rights* firms is stronger when incidents are more deadly. The findings of stock price reactions are consistent across various types of specifications, including figures, t-tests, and regressions controlling for fixed effects.

Figure 2 presents the main finding on stock price reactions. The figure plots changes in CARs of *Pro-Gun-Rights* and *Non-Pro-Gun-Rights* firms that both headquarter in the mass shooting states around deadliest incidents (mass shootings with more than 10 fatalities). There is a clear divergence in CARs of *Pro-Gun-Rights* (red connected line) and *Non-Pro-Gun-Rights* (blue solid line) firms when mass shootings take place. After mass shootings, *Pro-Gun-Rights* firms experience a 2 percentage point drop in stock price. On the contrary, *Non-Pro-Gun-Rights* firms experience a slight upward trends in price reactions, suggesting that investors invest more in these firms after mass shootings.

[16]The findings is robust to other specifications of estimation window, such as 100 days, etc.

Table 3 provides simple t-tests comparing CARs of *Pro-Gun-Rights* firms with the CARs of *Non-Pro-Gun-Rights* firms around mass shootings. Panels A to C report CARs around mass shootings with increasing numbers of fatalities. Notably, the effects are stronger when mass shootings are more deadly. Panel C shows the strongest difference. Intuitively, it suggests that when mass shootings reach a national wide impact, the divergence in political view between company leaders and stakeholder become stronger, causing more negative impacts on companies. In Panel D, I study whether the physical proximity between company headquarter and mass shooting locations matters for stock price reactions. Based on deadliest mass shootings (#fatalities > 10), I examine stock market reactions of firms headquartered in non-mass shooting states. Though statistically significant, the economic magnitude of differences in CARs between *Pro-Gun-Rights* and *Non-Pro-Gun-Rights* firms is much smaller. The finding is also consistent with the equity home bias literature (see, e.g. Huberman 2001). When a mass shooting take place in companies' headquarter states, a large proportion of companies shareholders are impacted by the incident, leading to a stronger impact on companies' stock price.

In Panel E of Table 3, I test whether the finding in CARs is driven by company supported pro-gun-rights candidate losing office after mass shooting taking place. The preferred channel of this paper is that the difference in political beliefs between *Pro-Gun-Rights* companies and their stakeholders increase after mass shootings, leading to negative impacts on companies' stock prices and bottom line. Losing political connection is the second channel. The literature shows that firms contributing to winning politicians experience positive stock market reactions. If pro-gun-rights politicians are less likely to get reelected after mass shootings, Yousaf (2018), firms connected to these politicians should have negative stock market reactions on mass shooting days. In Panels E, the sample includes CARs of *Pro-Gun-Rights* firms located in non-mass shooting states when deadliest shootings(#Fatalities>10) occur. Treatment observations are *Pro-Gun-Rights* firms which corporate PACs or executives contributed to pro-gun-rights politicians from mass shooting states within last three years, including both representatives and senators, and control observations are *Pro-Gun-Rights* firms which did not contribute to these political candidates. If the finding in CARs is driven by pro-gun-rights candidates losing office, the effect should be stronger in the treatment group. However, the comparison shows no statistically significant difference between treatment and control firms, suggesting that the effects in CARs are unlikely to be driven by firms losing corporate political connections.

Despite being intuitive and straightforward, the figures and t-tests do not address the unobserved fixed differences between mass shootings. Thus, I run CAR regressions with mass shooting event fixed effects to compare CARs of *Pro-Gun-Rights* and *Non-Pro-Gun-Rights* firms within each mass shooting. Due to a small number of clusters (15 incidents), the standard errors are not clustered.[17] Additionally, the regression framework enables me to test whether CARs responds more to *Pro-Gun-Rights* or *Pro-Republicans*.

The results of CARs regressions are reported in Table 4. Columns 1 to 4 present dependent variable of CARs with various windows. Regressions in panel A include only *Pro-Gun-Rights* dummy, and regressions in panel B include both *Pro-Gun-Rights* and *Pro-Republicans*. The results in panel A continue to show consistent evidence that *Pro-Gun-Rights* firms have lower CARs relative to *Non-Pro-Gun-Rights* firms. In panel B, the coefficients on *Pro-Gun-Rights* continue to be negative and significant, and the coefficients on *Pro-Republicans* are positive and sometimes significant. This comparison suggests that the negative stock price reactions are mostly due to companies donating to pro-gun-rights politicians.

2.4 Corporate Operating Performance

To understand the negative stock price reaction, I further examine whether the operating performance of *Pro-Gun-Rights* firms changes in responses to mass shootings. The first test exploits Google search volume on *Shooting* as a measurement of public awareness on mass shootings and then interacts the Google search volume with the *Pro-Gun-Rights* indicator variable. The second set of tests directly examine whether the operating performance of *Pro-Gun-Rights* firms changes after mass shootings take place. In both specifications, I find that *Pro-Gun-Rights* firms have worse operating performance following mass shootings. Again, gun related firms under three-digit SIC code “348” (Ordnance and Accessories, Except Vehicles and Guided Missiles) are excluded from all specifications.

[17]The results hold after clustering at mass shooting level.

2.4.1. Google Trends under Topic: Shooting

One natural choice of public awareness on mass shootings is the number of Google searches under the topic of shootings (collected through Google Trends available from January 1, 2004).[18] Google Trends data have been increasingly used the finance literature (see, e.g., Ben-Rephael et al. 2017 and Michaelides et al. 2019). For each state, I download the Google Trends data under the topic of Shooting at the monthly level and then aggregate to the quarter-state level. The data are scaled from 0 to 100, with 0 (100) represents the bottom (peak) of search interests during the sample period.

An important concern is whether the data from Google trends accurately captures the public attention on mass shootings. Figure 4 plots the monthly-level search volume data from two states: Florida (panel A) and Nevada (panel B). Visually, both graphs peak at several deadliest events, including Newtown, Orlando, Las Vegas, and Parkland, and the number is larger when an incident occurs within state. Table 5 presents results from regressing quarter-state level search volume data on two dummy variables: state-level mass shooting and national-level mass shooting. Both State Event and National Event are dummies variables. State Event equals one for a state if a mass shooting with more than 5 fatalities occurs at the state in the quarter, and National Event mass shooting equals one for all states if a mass shooting with more than 10 fatalities occurs at any state in the quarter. In all specifications, Google search data react strongly to both state and national level mass shootings. The coefficients are also sizable relative to the average search volume on shootings. Hence, there is no concern about the accuracy of Google trend data on shootings.

2.4.2. Firm Operating performance: Google Trends Evidence

Based on a panel sample at firm-quarter level, I first interact Google Trends data on Shooting with the *Pro-Gun-Rights* indicator variable to test whether *Pro-Gun-Rights* firms perform worse when the public pays more attention on gun violence issues. For each firm, I match its quarterly level financial information from Compustat with Google Trends on Shootings from its headquarter state. To conduct analysis, I

[18]Topic of Google trend captures different keywords which are similar to shootings. For example, trend under topic of London captures both keywords of "Capital of UK" and the Spanish word "Londres".

estimate the following regression model as the baseline specification.

$$\begin{aligned} y_{ijst} = \alpha_i + \delta_t + \beta_1 \times ProGunRights_{ijst} + \beta_2 \times GoogleTrend_{st} + \beta_3 \times GoogleTrend_{st} \\ \times ProGunRights_{ijst} + \theta' Control_{ijst} + \epsilon_{ijst} \end{aligned}$$

Where i indexes firms, s indexes company headquarter states, j indexes industries, and t indexes time. y_{ijst} represents the several outcome variables of interests, including company sales, asset turnover, ROA, etc. *Pro-Gun-Rights* is the variable defined in previous sections. $GoogleTrend_{st}$ is the Google search measurement of public awareness on mass shootings from state s and time t. To capture the lagged effects of mass shootings, $GoogleTrend_{st}$ is a moving average of Google Trends from the current and the last two quarters. $GoogleTrend_{st} \times ProGunRights_{ijst}$ is the interaction term of interest, and β_3 captures how operating performance of *Pro-Gun-Rights* firms changes when mass shootings occur. $Control_{ijst}$ are control variables, which include only firm size to avoid potential bad control issues. For the same reason, Firm size is lagged by one year. Panel A of Table 2 presents statistics on outcome and control variables. In the baseline regression, I include firm fixed effects α_i and time fixed effects δ_t. Outcome variables are scaled by 100. Thus coefficients are interpreted in percentage points. All continuous variables are winsorized at one percent. Standard errors are double clustered by both states and time in all specifications. In total, there are 1,827 companies in the sample.

Table 6 presents the panel regression results based on Google Trends data. Before looking at the interaction regressions, it's important to examine the average effect of *Pro-Gun-Rights* indicator variables on firm operating performance. Panel A presents regression results without interaction terms. On average, *Pro-Gun-Rights* firms do not have statistically significant difference in firm sales and other operating performance variables relative to *Non-Pro-Gun-Rights* firms. Thus, findings from interaction regressions should not be attributed to the average effects from *Pro-Gun-Rights* variable.

Panel B of table 6 present the main finding on corporate operating performance. Based on column 1 and column 2 of Panel B, when public awareness on shooting increases, *Pro-Gun-Rights* experience negative shocks to corporate sales. In terms of economic magnitude, one standard deviation increase in Google Trend (Shooting) leads to about 5% drop in firm sales and 1% drop in asset turnover for *Pro-Gun-Rights* firms. Interestingly, based on column 4, the coefficients from interaction terms in the ROA regressions are economically small and statistically indifferent from zero. To examine why company profitabilities are not influenced, I conduct further tests on company operating costs. Columns 3 and 5 of panel B present evidence from log-transformed total operating costs and number of employees. Since the number of employees is only available at the yearly level, regressions of $log(Employees)$ are at the firm-year level. The results in both operating costs and number of employees show the similar change as firm sales, suggesting that *Pro-Gun-Rights* firms cut operating expenses and lay off employees in respond to a drop in firm sales.

Panel C of table 6 reports regression results with firm fixed effects, state-time fixed effects, and industry-time fixed effects. Comparing with the results in panel B, the economic magnitudes of coefficients on the interaction terms drop but remain economically and statistically significant. The difference is because after controlling for high dimension fixed effects, I only explore the variation of *Pro-Gun-Rights* variable within industry and within state. Thus, the economic magnitudes are smaller. Notably, in panel B and panel C of table 6, coefficients on *Pro-Gun-Rights* dummies are positive and statistically significant. These coefficients, however, needs to be interpreted cautiously because the coefficients are estimated when Google Trend (Shooting) equals zero. However, in the data, the quarterly level Google Trend (Shooting) is never zero.

At last, Table 7 reports the results of placebo tests using Google search volume under the topic of earthquakes. Instead of measuring public awareness on gun violence, Google Trends under the topic of earthquake captures the public attention on earthquakes, which are plausibly irrelevant to corporate political connections to pro-gun-rights politicians. Interacting Google Trends (Earthquake) with Pro-Gun-Rights should not show significant results on the interaction terms. Indeed, all coefficients on interaction terms are neither economically large nor statistically significant.

2.4.3. Panel Regression with Exact Timing of Mass Shootings

Google Trends data has advantages of capturing real-time local public attention on gun violence. However, there are also several concerns related to Google Trends data. First, Google trends data is scaled between 0 and 100 in a non-straightforward and nonlinear way, making it difficult to interpret in linear regressions. Second, a practical concern is the replicability with Google trends data, since Google trends data is constantly updated each day. To address these concerns, I also conduct tests based on the exact timing of mass shootings.

Instead of interacting with Google Trends, I interact the *Pro-Gun-Rights* dummy with a dummy variable $MassShooting$. Specifically, $MassShooting$ equals to one if a mass shooting with more than 10 fatalities takes place at company headquarter state in the current or the last two quarters. $MassShooting$ is defined in this way to capture the lasting effects from mass shootings. Similar to estimated models in the previous section, the first set of models include both firm dummies and time dummies. The second set of specifications include firm dummies, industry-time dummies, and state-time dummies.

Table 8 reports the estimation results. Models with firm dummies and time dummies are reported in panel A, and models with firm dummies, industry-time dummies, and state-time dummies are reported in panel B. The estimated results generally confirm the findings using Google Trends data. In terms of economic magnitude, when deadliest mass shootings take place, *Pro-Gun-Rights* firms experience about a 4.7% drop in firm revenue relative to Non-Pro-Gun-Rights firms.[19] The economic magnitudes are also similar to Table 7 based on Google Trends data.

2.4.4. Other Specifications

Table 9 presents estimation results from panel regressions including both *Pro-Gun-Rights* variable and *Pro-Republican* variable. For brevity, I only report coefficients on the interaction terms. Panel A and Panel B include specifications with firm fixed effects and time fixed effects, and Panel C and Panel D include

[19]See, column 1, Panel A, Table 7.

high dimensional fixed effects, including firm fixed effects, industry-time fixed effects, and state-time fixed effects. Panel A and panel C include interaction regressions based on $GoogleTrend(Shooting)$, and Panel B and panel D include interaction regressions based on $MassShooting$. Consistent with Table 3, the results suggest that the negative operating performance are mostly driven by companies donating pro-gun-rights politicians.

Table 10 reports the dynamics of changes in corporate sales in response to mass shootings. I include dummy variables indicating different time relate to mass shooting events. For example, $D(t = -3Quarter)$ indicates the quarter which are 3 quarters before a mass shooting. For brevity, I only report the interaction terms between *Pro-Gun-Rights* dummy and time indicator variables. Based on the table, the effects show up on mass shooting quarters and decay afterwards. The effects last for several quarters and then diminish within one year after the incidents, suggesting that the decline in firm operating performance is temporary.

Table 11 reports triple interaction regressions results. Columns 1 and 2 report regression results based on whether a firm has a significant fraction of sales to the government. The data on firm sales to governments comes from the Compustat segment database. *Pct of Revenue from Gov* measures the fraction of company sales to the government. Columns 3 and 4 of Table 10 report regression results of triple interaction specifications based on consumer-related industries. A firm is flagged as a consumer-related firm if the firm is classified as Consumer Discretionary or Consumer Staples under the GICS industry classification. A consumer-related firm is denoted as *Consumer Related Industry* in the regression. For brevity, I only report coefficients on the $ProGunRights * MassShooting$ and the triple interaction terms. Although not statistically significant, the results show that the effects are stronger for consumer-related firms and weaker for government related firms operating in other industries. These findings are consistent with the first channel because companies from the consumer-related industries are more impacted when stakeholders backlash.

2.5 Deposit Market Evidence

One may wonder whether the findings are driven by industry differences across firms; companies in different industries have varied political interests and can be impacted differently by mass shootings. To address this concern, I conduct study focusing explicitly on the bank deposit market using the Summary of Deposit Data from FDIC. The deposit market has several appealing features. First, bank deposits are homogeneous products across all banks. Second, the granularity of the deposit data enables me to include high dimensional fixed effects, such as country-year fixed effects and bank branch fixed effects. Third, with detailed bank branch location, I can explicitly test whether the geographic proximity between branch location and incidents location matters. Specifically, I estimate the following model.

$$\begin{aligned} Log(Deposits)_{bict} &= \alpha_b + \delta_{ct} + \beta_1 \times ProGunRights_{it} + \beta_2 \times GoogleTrend_{st} \\ &+ \beta_3 \times ProGunRights_{it} \times GoogleTrend_{st} + \theta' Control_{it} + \epsilon_{bict} \end{aligned}$$

Where i indexes bank, b indexes branch, c indexes county, and t indexes year. The sample is at branch year level. In total, there are 97 banks in the sample. I use $Log(Deposits)$ to measure branch deposit outflow. $ProGunRights_{it}$ is at bank-year level, and $GoogleTrend_{st}$ is the google trend data on shootings from branch states. I include lagged bank total assets as the control variable. α_b represents branch level fixed effects, and δ_{ct} are county-year level fixed effects. The county-time fixed effects absorb shocks common to a county, and bank branch fixed effects absorb branch-level unobserved heterogeneity. Standard errors are double clustered at bank and branch state level. In total, there are 97 banks in the sample.

Table 12 presents regression results based on the deposit market data. Column 1 report regression results of $Log(BranchDeposits)$ on the $ProGunRights$ indicator variable without interaction terms. Similar to panel A of table 6, regressing $Log(BranchDeposits)$ on $ProGunRights$ only doesn't show any significant results. In column 2, I report the interaction regression based on $GoogleTrend(Shooting)$. Again, I find the negative and statistically significant coefficient on the interaction term. In terms of economic magnitude, one standard deviation increase in the $GoogleTrend(Shooting)$ leads to about

7% increase in deposit outflow. In column 3, the $MassShooting$ variable equals one if a severe incident (#Fatalities>10) occur in the current or the past two quarters at the states in which bank branches locate. The results show that when a deadliest incident happens, $ProGunRights$ banks experience a 5.7% increase in deposit outflow for the branches which locates in the mass shooting states. To examine whether the deposit outflow effects are national wide, I control for the bank-year fixed effects in column 4. After controlling for bank-year fixed effects, the coefficient become statistically indifferent from zero, suggesting that deposit outflow effects are national wide. At last, to confirm the finding in column 3 and column 4, I interact $ProGunRights$ with $MassShooting(AllStates)$ in column 5. $MassShooting(AllStates)$ is an indicator variable that equals one if a severe incident (#Fatalities>10) occur in the current or the past two quarters at any states in the United States. Indeed, the coefficient on the interaction term is similar but statistically more significant than the one in column 3.

2.6 Discussion

In this section, I conduct several robustness tests showing that the results are internally consistent and discuss several important questions. I first show that the main findings on stock market reactions and operating performance are robust to samples based on either corporate PAC or corporate executive contributions. Next, I show that the findings are not sensitive to the 70 percent threshold to identify the Pro-Gun-Rights dummy by replacing the *Pro-Gun-Rights* dummy with the Gun Rights Ratio. At last, I discuss several important questions, such as how companies adjust their political spending after incidents, whether corporate political spending are costly for firms in general, and the external validity concern.

In most specifications, I employ the *Pro-Gun-Rights* dummy based on both corporate PAC and executive contributions, which provides a larger sample size and also potentially enables me to test whether the effect is stronger to corporate PACs or executive personal contributions. To ensure that the results are not driven by issues related to sample selection, I report tests results based on separate samples in the appendix. Appendix table A.1 reports CARs tests based on corporate PAC and corporate executive personal contributions. Appendix tables A.2 and A.3 report operating performance tests based on *Pro-Gun-Rights(PAC)* and *Pro-Gun-Rights(Executive)*. All results are qualitatively the same as those in main text. The paper identifies the *Pro-Gun-Rights* dummy based on a 70 percent cutoff. To ensure robustness, appendix tables A.4 and A.5 report the same tests by replacing *Pro-Gun-Rights* dummy with the Gun Rights Ratio. Again, the results on CARs and operating performance are consistent after replacing *Pro-Gun-Rights* dummy with Gun Rights Ratio.

Two important questions remain. First, how companies respond when mass shootings take place? If companies experience negative impacts after mass shootings, we would naturally expect that firms take actions ex post. One simple and straightforward test is to look at changes in corporate political contributions when mass shootings take place. Figure 5 plots changes in both corporate PAC (Panel A) and executive (Panel B) political contributions to pro-gun-rights candidates around mass shootings. Consistent with main specifications of the paper, I include all 15 mass shootings in which more than 10 people were killed. The figure includes companies which headquarter in states in which mass shootings occur. Interestingly, when a mass shooting take place, there is a substantial decline, about 5%, in corporate PAC contributions to pro-gun-rights candidates, while the decline is marginal in corporate executive contributions to pro-gun-rights candidates. This finding is consistent with companies suspending donations to the Republican Politicians who opposed the certification of 2020 election results after the Capitol Hill Riot.

Furthermore, given the downsides of corporate political connections documented above, are corporate political connections costly for companies in general? This paper does not argue that corporate political connections are bad for companies in general. Instead, I attempt to partially answer the question by comparing findings in this paper with findings in Akey (2015), which documents the upsides of corporate political contributions based on stock market reactions. Based on close and special elections, Akey (2015) shows that post-election abnormal returns of firms contributing to winning candidates are 3% higher than those of firms contributing to losing candidates. In this paper, I show that the difference in one-week CARs of Pro-Gun-Rights and Non-Pro-Gun-Rights firms around deadliest mass shootings are about 0.91% (Panel A, Table 4). Admittedly, deadliest mass shootings are uncommon in the United States. However, when it happens, simply comparing stock price reactions suggests that the downside effects are sizable and also comparable to the upsides.

Finally, the external validity concern is whether the finding in this paper is restricted to the specific setting of mass shootings and corporate contributions to pro-gun-rights politicians. In this paper, I aim to study a general question on whether under extraordinary circumstances, when political issues become suddenly controversial, existing corporate political connections can have negative impacts on firm outcomes. Anecdotally, as illustrated in the introduction, corporate political contributions can have impacts on firm outcomes when many other controversial issues occur. In addition, according to a Wall Street Journal article, companies actively avoid to place ads near a long list of words related to politics on news publishers. Among the list of words, "Shooting" and "Gun" are in the top 5 words that companies insist to avoid.[20] The Collision Report from the Center of Political Accountability discusses some other interesting cases such as LGBT rights, climate changes, supporting Trump, etc. Different from other controversial issues, mass shootings take place repeatedly in the United States and have exact dates available. Furthermore, mass shootings are unexpected and also plausibly exogenous to firm business fundamentals. Thus, I argue that the setting of mass shootings represents an ideal setting to study the broader question and the findings in this paper can be generalized to many other circumstances.

2.7 Conclusion

This paper studies whether corporate political contributions can have negative impacts on firm outcomes when stakeholders disapprove of political donations. To identify this effect, I exploit a setting of mass shootings and corporate political contributions to pro-gun-rights political candidates. I first find that firms which primarily donate to pro-gun-rights politicians experience negative and significant stock price reactions on mass shooting days. Furthermore, the difference in CARs between *Pro-Gun-Rights* and *Non-Pro-Gun-Rights* firms is stronger when the mass shooting is more deadly. I find that the operating performance of *Pro-Gun-Rights* firms deteriorates following mass shootings. Lastly, using the Summary

[20]See "'Shooting,' 'Bomb,' 'Trump': Advertisers Blacklist News Stories Online", The Wall Street Journal, August 15, 2019.

of Deposit data from FDIC, I find that when mass shootings occur, *Pro-Gun-Rights* banks also experience higher deposit outflow than *Non-Pro-Gun-Rights*.

This paper attempts to distinguish two main channels. The first channel is that stakeholders disapprove of political spending by *Pro-Gun-Rights* firms after mass shootings. The second channel is that *Pro-Gun-Rights* firms lose political connections because pro-gun-rights politicians from mass shooting states are less likely to win office after mass shootings. To separate these two channels, I first show that there are no stock market reactions on mass shooting days for *Pro-Gun-Rights* firms which headquarter in non-mass shooting states and also connect to pro-gun-rights politicians from mass shooting states. I provide further evidence based on cross-sectional studies and deposit market.

Finally, I find that firms substantially reduce corporate political contributions to pro-gun-rights politicians after mass shootings take place. In general, I do not argue that corporate political spending are bad for companies. Instead, I argue that under extraordinary circumstances and political turmoil, such as mass shootings and the Capitol Hill Riot, corporate political spending can have significant downsides on some firms, especially with the increasingly polarized environment.

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Figure 2.1： Locations of Mass Shootings in the United States

This figure plots locations of mass shootings took placed in the United States from 1999 to 2018. Each dot represents a mass shooting event with more than 5 fatalities (at least 6 people were killed). In total, there are 61 mass shootings included. Among the 61 incidents, 15 of them have a number of fatalities greater than 10. Sizes of circle markers are positively associated with numbers of fatalities.

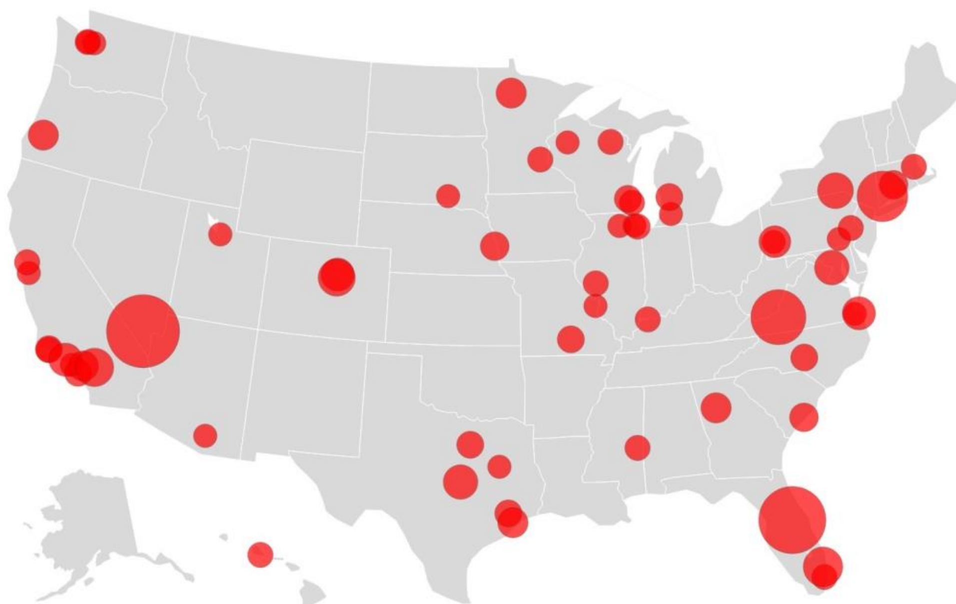


Figure 2.2: Market Reactions to Deadliest Mass Shootings

This figure plots Cumulative Abnormal Returns (CARs) around deadliest mass shootings, in which more than 10 people get killed. The horizontal axis represents the day relative to the event day (day 0), and the vertical axis represents CARs (in percentage point) calculated based on Fama-French three-factor model. The red-connected line represents CARs of *Pro-Gun-Rights* firms that primarily support pro-gun-rights politicians, while the blue-solid line represents CARs of *Non-Pro-Gun-Rights* firms that are less supportive for pro-gun-rights politicians. Both *Pro-Gun-Rights* and *Non-Pro-Gun-Rights* firms are headquartered in the states in which mass shootings took place.

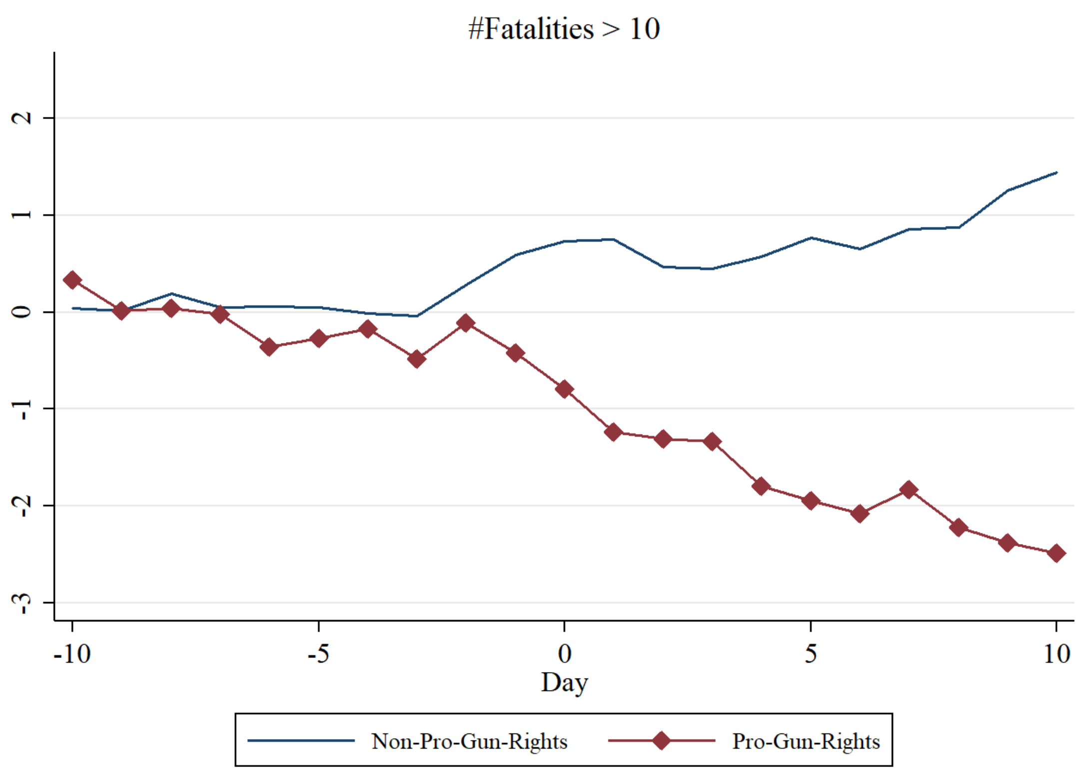
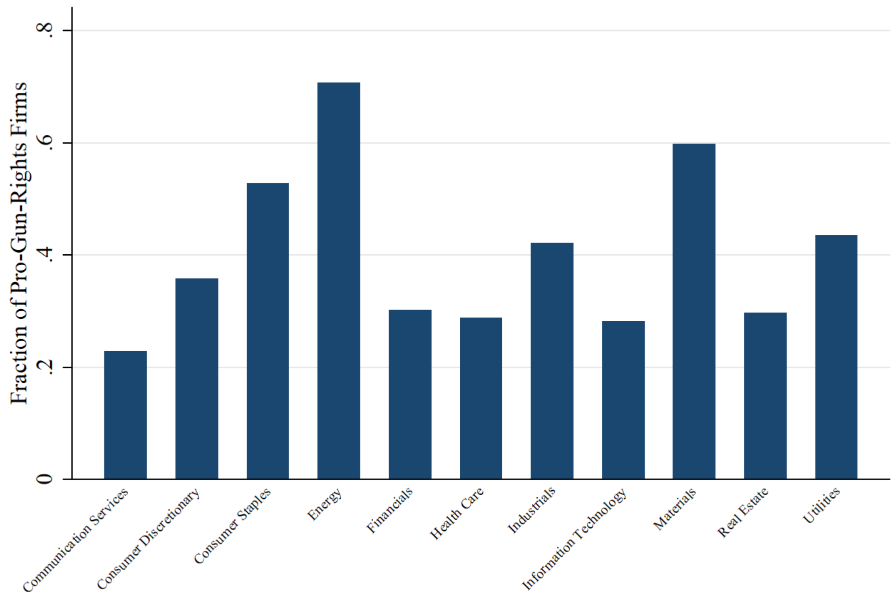


Figure 2.3: Distribution of Pro-Gun-Rights firms

These figures present the fractions of *Pro-Gun-Rights* firms by industries and geographical regions. Each bar represents the fraction of observations with *Pro-Gun-Rights* dummy equals one within an industry or a geographical region. Panel A presents the industry distribution. The industry classification follows the Global Industry Classification Standard (GICS) obtained from Compustat. Panel B presents the geographical region distribution. Following the U.S. Census Bureau-designated regions, I classify firms into 4 geographical regions based on headquarter states.

Panel A: Industries



Panel B: Geographical Regions

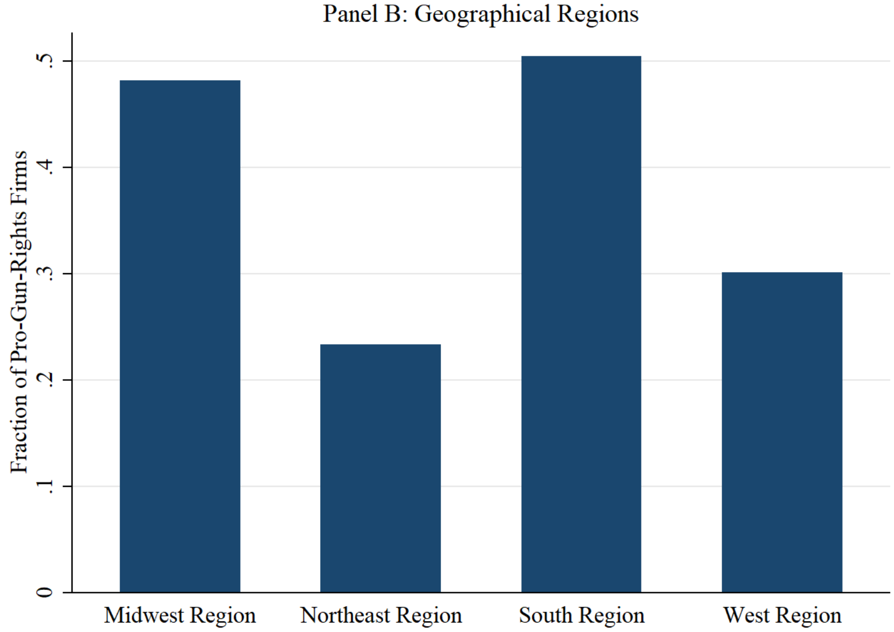


Figure 2.4: Google Trends (Shooting)

These figures plot the monthly Google Trends on Shooting from Florida (Panel A) and Nevada (Panel B). The sample period is from 2004 to 2018. The horizontal axis represents each calendar month, and the vertical axis represents search interests of shooting in that month. Google trend data are normalized from 0 to 100. 100 marks the maximum search interests for the selected time and location. I label the exact mass shootings corresponding to spikes of search interest.

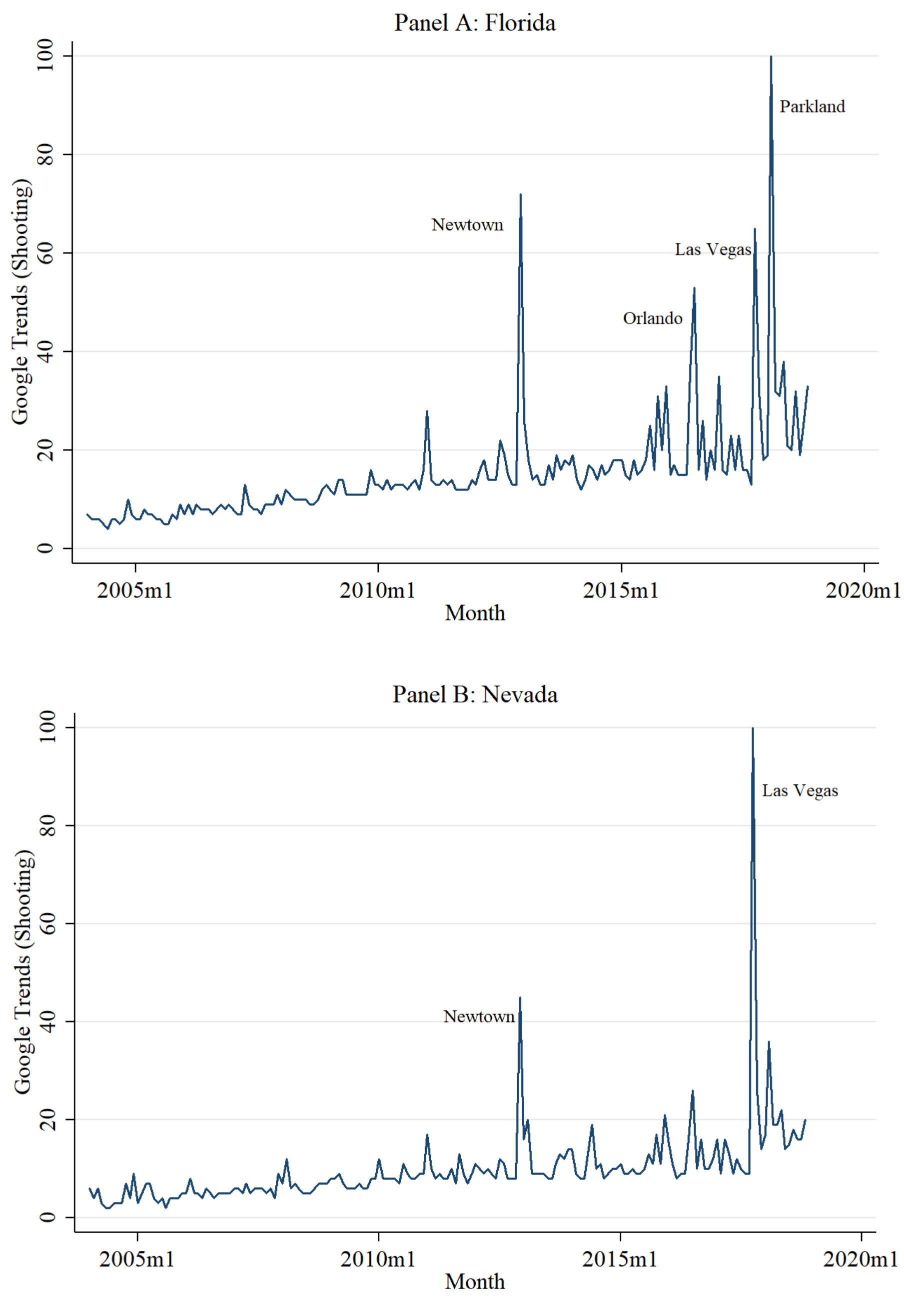


Figure 2.5: Corporate Contribution to Gun-Rights Politicians Around Mass Shooting

These figures plot the change in corporate political contributions to pro-gun-rights politicians around mass shootings, in which more than 10 people get killed. Political contributions are from companies that headquarter in the mass shooting states. Panel A plots the change in political donations from corporate PACs, and Panel B plots the change in political donations from corporate executives. The *y* axis represents the fraction (in percentage point) of total contributions that are donated to pro-gun-rights politicians, and the *x* axis represents the year relative to the mass shooting year. Year 0 on *x* axis represents the year in which mass shootings take place. The figures plot the means and corresponding 95% confidence intervals.

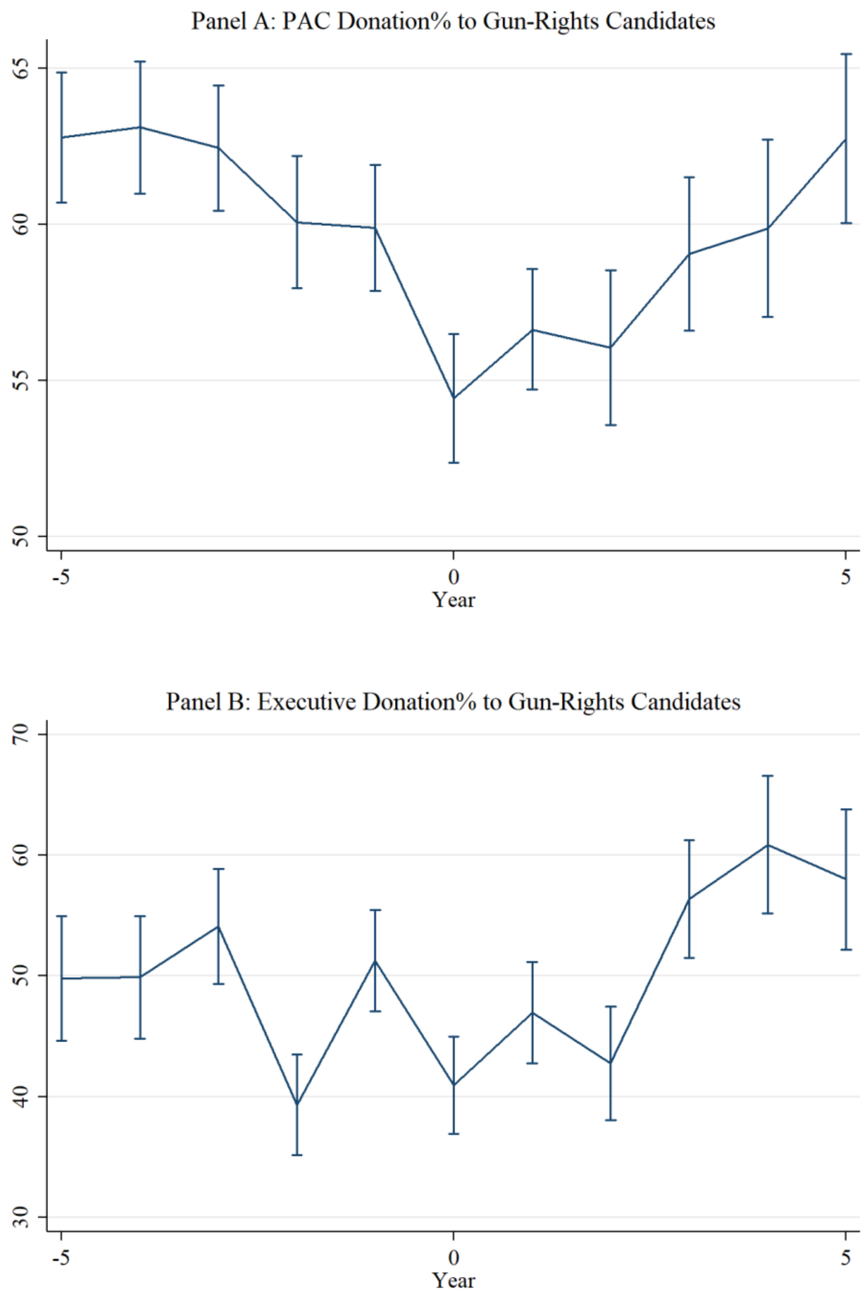


Table 2.1: Deadliest Mass Shootings

This table lists the deadliest mass shootings occurred in the United States from 1999 to 2018. Only incidents in which more than 10 people were killed are included. Columns 1 to 3 present dates, cities, and states in which mass shootings took place. Column 4 presents the number of people who were killed in the incident, and Column 5 presents the number of people who were injured.

Date	City	State	#Fatalities	#Victims
(1)	(2)	(3)	(4)	(5)
April 20, 1999	Littleton	CO	15	39
April 16, 2007	Blacksburg	VA	33	50
March 10, 2009	Geneva	AL	11	17
April 3, 2009	Binghamton	NY	14	18
November 5, 2009	Fort Hood	TX	13	45
July 20, 2012	Denver	CO	12	70
December 14, 2012	Newtown	CT	28	30
September 16, 2013	Washington	DC	13	16
December 2, 2015	San Bernardino	CA	16	37
June 12, 2016	Orlando	FL	49	102
October 1, 2017	Las Vegas	NV	58	604
November 5, 2017	Sutherland Springs	TX	26	46
February 14, 2018	Parkland	FL	17	34
October 27, 2018	Pittsburgh	PA	11	17
November 7, 2018	Thousand Oaks	CA	12	34

Table 2.2: Summary Statistics

This table reports summary statistics of the paper. Panel A reports summary statistics on the operating performance sample, and Panel B reports summary statistics on the bank deposit sample. In total, there are 1,827 companies in panel A, and 97 banks in panel B.

	N	Mean	25th Pctl.	Median	75th Pctl.	Std. Dev.
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Operating Performance Sample						
Pro-Gun-Rights	56,525	0.40	0.00	0.00	1.00	0.49
Gun Right Ratio (in p.p.)	56,525	58.99	41.94	62.42	82.10	30.46
Google Trends (Shooting)	56,525	18.72	12.00	16.67	22.00	9.42
Mass Shooting	56,525	0.03	0.00	0.00	0.00	0.17
Log(Sales) (in p.p.)	56,525	658.72	552.47	654.26	762.95	154.47
Sales/Assets (in p.p.)	56,525	22.49	9.28	17.78	30.33	18.04
Log(OP Cost) (in p.p.)	56,525	634.93	527.52	629.93	738.65	155.59
ROA (in p.p.)	56,525	3.1	1.72	2.89	4.3	2.48
Log(Total Assets)	56,525	8.38	7.23	8.32	9.50	1.66
Log(#Employees)	15,389	222.38	116.75	205.41	309.10	133.01
Total Contributions to NRA Funded Candidates (over last 3 years)	56,525	116,251.08	3,500.00	20,000.00	102,500.00	261,886.93
Total Contributions to All Candidates (over last 3 years)	56,525	191,097.18	8,000.00	37,250.00	170,300.00	429,559.63
Panel B: Bank Deposit Sample						
Pro-Gun-Rights	503,259	0.31	0.00	0.00	1.00	0.46
Google Trends (Shooting)	503,259	19.15	12.17	18.00	25.50	8.15
Mass Shooting	503,259	0.05	0.00	0.00	0.00	0.22
Mass Shooting (All States)	503,259	0.54	0.00	1.00	1.00	0.50
Log(Branch Deposits) (in p.p.)	503,259	1,038.57	1,007.26	1,071.87	1,131.39	212.11
Log(Bank Total Assets)	503,259	19.47	18.59	19.66	21.01	1.64

Table 2.3: T-Test of Cumulative Abnormal Returns

This table reports T-Test of Cumulative Abnormal Returns (CARs) around mass shootings. CARs are estimated using the Fama-French three-factor model. *Pro-Gun-Rights* and *Non-Pro-Gun-Rights* indicate whether a firm primarily donates to pro-gun-rights politicians in recent years. The sample period is from 1999 to 2018. Panel A compares CARs of *Pro-Gun-Rights* firms and *Non-Pro-Gun-Rights* firms around mass shootings in which less than five people were killed. Similarly, Panel B, Panel C, and Panel D compare CARs around incidents with higher number of fatalities. In Panel A to Panel C, CARs are from firms that headquarter in the same states in which mass shootings take place. In Panel D, CARs are from firms that headquarter in other states in which mass shootings do not occur. In Panel E, *Donated* represents CARs from *Pro-Gun-Rights* firms that headquarter in other states and also donate to pro-gun-rights politicians from the mass shooting states, and *Not Donated* represents CARs from *Pro-Gun-Rights* firms that headquarter in other states and *do not* donate to pro-gun-rights politicians from the mass shooting states. Column 1 and Column 3 present average CARs around mass shootings in each group, and Column 2 and Column 4 report the corresponding number of observations. Column 5 reports the difference in average CARs between *Pro-Gun-Rights* and *Non-Pro-Gun-Rights* firms, and Column 6 reports the corresponding *t*-statistics.

	Pro-Gun-Rights	*N*	Non-Pro-Gun-Rights	*N*	Difference	*t*-stat
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: CARs Around Mass Shootings (#Fatalities < 5) - All Firms in MS States						
Event Window (-1, +1)	-0.02	1678	-0.08	2506	0.06	0.58
Event Window (-1, +3)	0.01	1678	-0.04	2506	0.06	0.40
Event Window (-1, +5)	-0.02	1678	-0.01	2506	-0.01	-0.04
Event Window (-1, +7)	0.29	1678	0.15	2506	0.13	0.66
Panel B: CARs Around Mass Shootings (#Fatalities >5 & #Fatalities <=10) - All Firms in MS States						
Event Window (-1, +1)	-0.30	551	0.08	1055	-0.38	-1.91*
Event Window (-1, +3)	-0.46	551	-0.06	1055	-0.39	-1.56
Event Window (-1, +5)	-0.31	551	-0.03	1055	-0.28	-0.95
Event Window (-1, +7)	-0.05	551	0.15	1055	-0.21	-0.58
Panel C: CARs Around Mass Shootings (#Fatalities > 10) - All Firms in MS States						
Event Window (-1, +1)	-1.12	198	0.47	413	-1.59	-3.72***
Event Window (-1, +3)	-1.22	198	0.17	413	-1.39	-2.49**
Event Window (-1, +5)	-1.83	198	0.49	413	-2.32	-3.29***
Event Window (-1, +7)	-1.72	198	0.58	413	-2.29	-2.74***
Panel D: CARs Around Mass Shootings (#Fatalities > 10) - All Firms in Other States						
Event Window (-1, +1)	0.16	4508	0.33	7484	-0.17	-1.76*
Event Window (-1, +3)	-0.02	4508	0.29	7484	-0.31	-2.46**
Event Window (-1, +5)	0.26	4508	0.35	7484	-0.09	-0.60
Event Window (-1, +7)	0.61	4508	0.56	7484	0.05	0.24

**p<0.1, **p<0.05, ***p<0.01*

	Donated	*N*	Not Donated	*N*	Difference	*t*-stat
	(1)	(2)	(3)	(4)	(5)	(6)
Panel E: CARs Around Mass Shootings, (#Fatalities > 10) - Pro-Gun-Rights Firms in Other States						
Event Window (-1, +1)	0.11	1067	0.18	3441	-0.07	-0.38
Event Window (-1, +3)	-0.10	1067	0.01	3441	-0.11	-0.48
Event Window (-1, +5)	0.22	1067	0.27	3441	-0.05	-0.19
Event Window (-1, +7)	0.64	1067	0.61	3441	0.03	0.10

**p<0.1, **p<0.05, ***p<0.01*

Table 2.4: Regressions of Cumulative Abnormal Returns

This table uses OLS regressions to estimate whether Cumulative Abnormal Returns (CARs) of *Pro-Gun-Rights* firms are statistically different from CARs of *Non-Pro-Gun-Rights* firms around mass shootings. *Pro-Gun-Rights* is an indicator variable set to one if a firm primarily donates to pro-gun-rights politicians in recent years. *Pro-Republicans* is an indicator variable set to one if a firm primarily donates to Republican politicians in recent years. The sample period is from 1999 to 2018. All regressions control for event fixed effects (mass shooting fixed effects). CARs are estimated based on the Fama-French three-factor model. The dependent variables are CARs with various event windows. Panel A reports estimation results from regressions of CARs on *Pro-Gun-Rights*. Panels B reports estimation results from regressions of CARs on both *Pro-Gun-Rights* and *Pro-Republicans*. Due to the small number of events, standard errors reported in parentheses below the coefficients are not clustered.

Panel A: Mass Shootings (#Fatalities > 10)

	(-1, +1)	(-1, +3)	(-1, +5)	(-1, +7)
	(1)	(2)	(3)	(4)
Pro-Gun-Rights	-0.91**	-0.87	-1.63**	-1.65*
	(0.44)	(0.58)	(0.73)	(0.87)
Constant	0.25	-0.00	0.26	0.37
	(0.24)	(0.32)	(0.40)	(0.48)
Observations	611	611	611	611
R-squared	0.09	0.06	0.07	0.06
Event FE	Yes	Yes	Yes	Yes

**p<0.1, **p<0.05, ***p<0.01*

Panel B: Mass Shootings (#Fatalities > 10) – Add Pro-Republican

	(-1, +1)	(-1, +3)	(-1, +5)	(-1, +7)
	(1)	(2)	(3)	(4)
Pro-Gun-Rights	-1.07**	-1.19*	-2.19***	-2.20**
	(0.48)	(0.62)	(0.79)	(0.94)
Pro-Republicans	0.40	0.80	1.37*	1.34
	(0.45)	(0.59)	(0.76)	(0.89)
Constant	0.14	-0.23	-0.12	-0.01
	(0.27)	(0.36)	(0.46)	(0.54)
Observations	611	611	611	611
R-squared	0.09	0.07	0.07	0.07
Event FE	Yes	Yes	Yes	Yes

**p<0.1, **p<0.05, ***p<0.01*

Table 2.5: Google Trends (Shooting)

This table uses OLS regression to estimate whether the Google Trends (Shooting) variable responds to mass shooting incidents. The data are at the state-quarter level, and the sample period is from 2004 to 2018. *State Events* is an indicator variable that equals one for a state-quarter observation if a mass shooting with more than 5 fatalities take place in the state at the quarter, and *National Event* is an indicator variable that equals one for all observations at a quarter if a mass shooting with more than 10 fatalities occurs in the quarter at any states across the United States. Fixed effect specifications are reported at the bottom of the table. Standard errors double clustered by both state and time are reported in parentheses below the coefficients.

	(1) Google Trends (Shooting)	(2) Google Trends (Shooting)	(3) Google Trends (Shooting)
State Event (#Fatalities > 5)	2.29**	2.53**	2.50***
	(0.96)	(0.97)	(0.45)
National Event (#Fatalities > 10)	11.15***	11.15***	
	(3.77)	(3.77)	
Observations	3,060	3,060	3,060
R-squared	0.21	0.28	0.94
State FE	-	Yes	Yes
Time FE	-	-	Yes

**p<0.1, **p<0.05, ***p<0.01*

Table 2.6: Google Trends (Shooting) and Corporate Operating Performance

This table presents the estimation results from OLS regressions of corporate operating performance on *Pro-Gun-Rights* and Google Trends (Shooting). The data of Column 1 to Column 4 are at the firm-quarter level, and the data of Column 5 are at the firm-year level. The sample period is from 2004 to 2018. *Pro-Gun-Rights* is an indicator variable set to one if a firm primarily donates to pro-gun-rights politicians in recent years. *Google Trends (Shooting)* measures the search interests of topic "Shooting" from companies' headquarter states. In Column 1 to Column 4, *Google Trends (Shooting)* is a three-quarter moving average (including the current quarter) of the Google Trends data, and in Column 5, *Google Trends (Shooting)* is a one-year average of the Google Trends data. Dependent variables include various variables on corporate operating performance. Control variable includes one-year lagged *Log(Total Assets)$_{t-1}$*. Panel A presents regression results without interaction terms. Panel B and Panel C report regression results with interaction terms. Fixed effect specifications are reported at the bottom of each panel. Industry-related fixed effects are based on 2-digits SIC codes, and state-related fixed effects are based on firm headquarter states. All outcome and control variables are winsorized at 1 percent. Outcome variables are scaled by 100. Standard errors double clustered by both state and time are reported in parentheses below the coefficients.

Panel A: Firm FEs and Time FEs - No Interactions

	Log(Sales)	Sales/Assets	Log(OP Cost)	ROA	Log(#Emp)
	(1)	(2)	(3)	(4)	(5)
Pro-Gun-Rights	-0.29	0.08	0.73	-0.06*	-0.62
	(0.73)	(0.15)	(0.73)	(0.03)	(0.65)
Log(Total Assets) $_{t-1}$	60.72***	-5.74***	59.91***	-0.73***	42.78***
	(2.14)	(0.28)	(2.33)	(0.08)	(2.37)
Observations	56,525	56,525	56,525	56,525	15,389
Within R-squared	0.36	0.10	0.34	0.02	0.36
Firm FE	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes

**p<0.1, **p<0.05, ***p<0.01*

Panel B: Firm FEs and Time FEs - With Interactions

	Log(Sales)	Sales/Assets	Log(OP Cost)	ROA	Log(#Emp)
	(1)	(2)	(3)	(4)	(5)
Pro-Gun-Rights * Google Trends (Shooting)	-0.42***	-0.08***	-0.44***	-0.01	-0.40***
	(0.13)	(0.03)	(0.10)	(0.01)	(0.10)
Pro-Gun-Rights	7.49***	1.54***	8.88***	0.07	6.84***
	(2.24)	(0.47)	(1.76)	(0.13)	(1.92)
Google Trends (Shooting)	-0.09	-0.02	-0.10	-0.01	-0.12
	(0.11)	(0.02)	(0.11)	(0.00)	(0.29)
Log(Total Assets) $_{t-1}$	60.58***	-5.76***	59.76***	-0.74***	42.64***
	(2.08)	(0.27)	(2.28)	(0.08)	(2.32)
Observations	56,525	56,525	56,525	56,525	15,389
Within R-squared	0.36	0.11	0.35	0.02	0.36
Firm FE	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes

**p<0.1, **p<0.05, ***p<0.01*

Panel C: Firm FEs, Industry-Time FEs, and State-Time FEs - With Interactions

	Log(Sales)	Sales/Assets	Log(OP Cost)	ROA	Log(#Emp)
	(1)	(2)	(3)	(4)	(5)
Pro-Gun-Rights * Google Trends (Shooting)	-0.27***	-0.05**	-0.28***	-0.01	-0.13
	(0.09)	(0.02)	(0.09)	(0.00)	(0.08)
Pro-Gun-Rights	4.39***	0.86**	5.16***	0.08	1.68
	(1.54)	(0.40)	(1.47)	(0.08)	(1.53)
Log(Total Assets) $_{t-1}$	59.63***	-5.77***	58.45***	-0.71***	41.34***
	(2.21)	(0.30)	(2.55)	(0.07)	(2.10)
Observations	56,525	56,525	56,525	56,525	15,389
Within R-squared	0.36	0.11	0.33	0.02	0.35
Firm FE	Yes	Yes	Yes	Yes	Yes
State-Time FE	Yes	Yes	Yes	Yes	Yes
Industry-Time FE	Yes	Yes	Yes	Yes	Yes

**p<0.1, **p<0.05, ***p<0.01*

Table 2.7: Placebo Tests using Google Trend (Earthquake)

This table presents estimation results from OLS regressions of operating performance on the interaction between *Pro-Gun-Rights* and *Google Trends (Earthquake)*. The data of Column 1 to Column 4 are at the firm-quarter level, and the data of Column 5 are at the firm-year level. The sample period is from 2004 to 2018. *Pro-Gun-Rights* is an indicator variable set to one if a firm primarily donates to pro-gun-rights politicians in recent years. *Google Trends (Earthquake)* measures the search interests of topic "Earthquake" from companies' headquarter states. In Column 1 to Column 4, *Google Trends (Earthquake)* is a three-quarter moving average (including the current quarter) of the Google Trends data, and in Column 5, *Google Trends (Earthquake)* is a one-year average of the Google Trends data. Dependent variables include various variables on corporate operating performance. Control variable includes one-year lagged *Log(Total Assets)$_{t-1}$*. Fixed effect specifications are reported at the bottom of each panel. Industry-related fixed effects are based on 2-digits SIC codes, and state-related fixed effects are based on firm headquarter states. All outcome and control variables are winsorized at 1 percent. Outcome variables are scaled by 100. Standard errors double clustered by both state and time are reported in parentheses below the coefficients.

Panel A: Google Trend - Earthquake (Firm FE and Time FE)

	Log(Sales)	Sales/Assets	Log(OP Cost)	ROA	Log(#Emp)
	(1)	(2)	(3)	(4)	(5)
Pro-Gun-Rights * Google Trends (Earthquake)	0.05	0.00	0.06	-0.01	-0.01
	(0.09)	(0.02)	(0.09)	(0.00)	(0.06)
Pro-Gun-Rights	-0.88	0.04	0.00	0.00	-0.52
	(1.38)	(0.26)	(1.34)	(0.06)	(1.04)
Google Trends (Earthquake)	-0.03	0.00	0.00	-0.00*	-0.01
	(0.04)	(0.01)	(0.03)	(0.00)	(0.14)
Log(Total Assets)$_{t-1}$	60.72***	-5.74***	59.91***	-0.73***	42.78***
	(2.14)	(0.28)	(2.33)	(0.08)	(2.37)
Observations	56,525	56,525	56,525	56,525	15,389
Within R-squared	0.36	0.10	0.34	0.02	0.36
Firm FE	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes

**p<0.1, **p<0.05, ***p<0.01*

Panel B: Google Trend - Earthquake (Firm FE, Industry-Time FE, and State-Time FE)

	Log(Sales)	Sales/Assets	Log(OP Cost)	ROA	Log(#Emp)
	(1)	(2)	(3)	(4)	(5)
Pro-Gun-Rights * Google Trends (Earthquake)	0.03	0.00	0.03	-0.00	-0.04
	(0.07)	(0.01)	(0.07)	(0.00)	(0.04)
Pro-Gun-Rights	-0.99	-0.02	-0.33	-0.02	-0.20
	(1.35)	(0.29)	(1.20)	(0.07)	(0.94)
Log(Total Assets)$_{t-1}$	59.72***	-5.75***	58.54***	-0.71***	41.38***
	(2.23)	(0.29)	(2.56)	(0.07)	(2.10)
Observations	56,525	56,525	56,525	56,525	15,389
Within R-squared	0.36	0.11	0.33	0.02	0.34
Firm FE	Yes	Yes	Yes	Yes	Yes
State-Time FE	Yes	Yes	Yes	Yes	Yes
Industry-Time FE	Yes	Yes	Yes	Yes	Yes

**p<0.1, **p<0.05, ***p<0.01*

Table 2.8: Mass Shootings and Corporate Operating Performance

This table presents estimation results from OLS regressions of corporate operating performance on the interaction between *Pro-Gun-Rights* and *Mass Shooting*. The data of Column 1 to Column 4 are at the firm-quarter level, and the data of Column 5 are at the firm-year level. The sample period is from 2004 to 2018. *Pro-Gun-Rights* is an indicator variable set to one if a firm primarily donates to pro-gun-rights politicians in recent years. In Column 1 to Column 4, *Mass Shooting* is an indicator variable set to one if a severe incident (more than 10 people get killed) take place in the past three quarters in companies' headquarter states. In Column 5, *Mass Shooting* is an indicator variable set to one if a severe incident (more than 10 people get killed) in the year in companies' headquarter states. Control variable includes one-year lagged $Log(Total\ Assets)_{t-1}$. Fixed effect specifications are reported at the bottom of each panel. Industry-related fixed effects are based on 2-digits SIC codes, and state-related fixed effects are based on firm headquarter states. All outcome and control variables are winsorized at 1 percent. Outcome variables are scaled by 100. Standard errors double clustered by both state and time are reported in parentheses below the coefficients.

Panel A: Corporate Operating Performance (Firm FE and Time FE)

	Log(Sales)	Sales/Assets	Log(OP Cost)	ROA	Log(#Emp)
	(1)	(2)	(3)	(4)	(5)
Pro-Gun-Rights * Mass Shooting	-4.70***	-1.40***	-8.03***	-0.13	-2.15***
	(1.00)	(0.32)	(1.27)	(0.11)	(0.54)
Mass Shooting	4.08***	0.90***	4.40***	0.04	2.93***
	(1.19)	(0.27)	(1.61)	(0.04)	(0.59)
Pro-Gun-Rights	-0.14	0.12	0.96	-0.05*	-0.51
	(0.72)	(0.15)	(0.73)	(0.03)	(0.67)
Log(Total Assets) $_{t-1}$	60.70***	-5.74***	59.88***	-0.73***	42.74***
	(2.14)	(0.28)	(2.33)	(0.08)	(2.36)
Observations	56,525	56,525	56,525	56,525	15,389
Within R-squared	0.36	0.10	0.35	0.02	0.36
Firm FE	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes

**p<0.1, **p<0.05, ***p<0.01*

Panel B: Corporate Operating Performance (Firm FE, Industry-Time FE, and State-Time FE)

	Log(Sales)	Sales/Assets	Log(OP Cost)	ROA	Log(#Emp)
	(1)	(2)	(3)	(4)	(5)
Pro-Gun-Rights * Mass Shooting	-3.75***	-0.66***	-5.82***	-0.11	-1.17
	(1.05)	(0.14)	(1.16)	(0.09)	(0.91)
Pro-Gun-Rights	-0.55	0.01	0.18	-0.03	-0.60
	(0.83)	(0.20)	(0.76)	(0.04)	(0.70)
Log(Total Assets)$_{t-1}$	59.71***	-5.76***	58.52***	-0.71***	41.38***
	(2.22)	(0.29)	(2.56)	(0.07)	(2.11)
Observations	56,525	56,525	56,525	56,525	15,389
Within R-squared	0.36	0.11	0.33	0.02	0.34
Firm FE	Yes	Yes	Yes	Yes	Yes
State-Time FE	Yes	Yes	Yes	Yes	Yes
Industry-Time FE	Yes	Yes	Yes	Yes	Yes

**p<0.1, **p<0.05, ***p<0.01*

Table 2.9: Corporate Operating Performance – Pro-Gun-Rights and Pro-Republicans

This table presents estimation results from OLS regressions of corporate operating performance on *Pro-Gun-Rights*, *Pro-Republicans*, and corresponding interaction terms. The data of Column 1 to Column 4 are at the firm-quarter level, and the data of Column 5 are at the firm-year level. The sample period is from 2004 to 2018. *Pro-Republicans* is an indicator variable set to one if a firm primarily donates to Republican politicians in recent years. *Pro-Gun-Rights*, *Google Trends (Shooting)*, and *Mass Shooting* are the same as the variables in Table 5 and Table 7. Control variable includes one-year lagged *Log(Total Assets)*$_{t-1}$. Fixed effect specifications are reported at the bottom of each panel. Industry-related fixed effects are based on 2-digits SIC codes, and state-related fixed effects are based on firm headquarter states. All outcome and control variables are winsorized at 1 percent. Outcome variables are scaled by 100. Standard errors double clustered by both state and time are reported in parentheses below the coefficients. All regressions contain the same *Log(Total Assets)*$_{t-1}$ control variable as Table 6 – Table 7. For brevity, only coefficients on the interaction terms are reported.

Panel A: Google Trends (Shooting) (Firm FE and Time FE)

	Log(Sales)	Sales/Assets	Log(OP Cost)	ROA	Log(#Emp)
	(1)	(2)	(3)	(4)	(5)
Pro-Gun-Rights * Google Trends (Shooting)	-0.34***	-0.05**	-0.37***	-0.00	-0.36***
	(0.11)	(0.02)	(0.11)	(0.01)	(0.11)
Pro-Republican * Google Trends (Shooting)	-0.14	-0.05**	-0.14	-0.01	-0.08
	(0.10)	(0.02)	(0.09)	(0.01)	(0.13)
Observations	56,525	56,525	56,525	56,525	15,389
Within R-squared	0.36	0.11	0.35	0.02	0.36
Firm FE	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes

**p<0.1, **p<0.05, ***p<0.01*

Panel B:Mass Shooting (Firm FE and Time FE)

	Log(Sales)	Sales/Assets	Log(OP Cost)	ROA	Log(#Emp)
	(1)	(2)	(3)	(4)	(5)
Pro-Gun-Rights * Mass Shooting	-3.36**	-1.32***	-6.23***	-0.12	-1.11
	(1.27)	(0.34)	(1.78)	(0.13)	(1.14)
Pro-Republican * Mass Shooting	-2.96*	-0.19	-3.93*	-0.02	-2.20
	(1.76)	(0.29)	(2.21)	(0.08)	(1.65)
Observations	56,525	56,525	56,525	56,525	15,389
Within R-squared	0.36	0.11	0.35	0.02	0.36
Firm FE	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes

**p<0.1, **p<0.05, ***p<0.01*

Panel C: Google Trends (Shooting) (Firm FE, Industry-Time FE, and State-Time FE)

	Log(Sales)	Sales/Assets	Log(OP Cost)	ROA	Log(#Emp)
	(1)	(2)	(3)	(4)	(5)
Pro-Gun-Rights * Google Trends (Shooting)	-0.21**	-0.04*	-0.21**	-0.00	-0.10
	(0.09)	(0.02)	(0.09)	(0.00)	(0.09)
Pro-Republican * Google Trends (Shooting)	-0.14	-0.02	-0.15*	-0.01*	-0.06
	(0.08)	(0.02)	(0.09)	(0.00)	(0.14)
Observations	56,525	56,525	56,525	56,525	15,389
Within R-squared	0.36	0.11	0.33	0.02	0.35
Firm FE	Yes	Yes	Yes	Yes	Yes
State-Time FE	Yes	Yes	Yes	Yes	Yes
Industry-Time FE	Yes	Yes	Yes	Yes	Yes

**p<0.1, **p<0.05, ***p<0.01*

Panel D:Mass Shooting (Firm FE, Industry-Time FE, and State-Time FE)

	Log(Sales)	Sales/Assets	Log(OP Cost)	ROA	Log(#Emp)
	(1)	(2)	(3)	(4)	(5)
Pro-Gun-Rights * Mass Shooting	-2.75**	-0.87***	-4.83***	-0.12	-0.53
	(1.21)	(0.20)	(1.67)	(0.11)	(2.01)
Pro-Republican * Mass Shooting	-2.53*	0.49**	-2.50	0.04	-1.56
	(1.47)	(0.21)	(1.53)	(0.08)	(2.71)
Observations	56,525	56,525	56,525	56,525	15,389
Within R-squared	0.36	0.11	0.33	0.02	0.34
Firm FE	Yes	Yes	Yes	Yes	Yes
State-Time FE	Yes	Yes	Yes	Yes	Yes
Industry-Time FE	Yes	Yes	Yes	Yes	Yes

**p<0.1, **p<0.05, ***p<0.01*

Table 2.10: Dynamic Analysis around Mass Shooting

This table presents the dynamic analysis of firm operating performance around severe mass shootings (more than 10 people get killed). The data are at the firm-quarter level. The sample period is from 2004 to 2018. *Pro-Gun-Rights* is an indicator variable set to one if a firm primarily donates to pro-gun-rights politicians in recent years. *D(t = -4 Quarter)* to *D(t = 5 Quarter)* are indicator variables that indicate the time relate to the quarter in which mass shootings occur, and *D(t = 0 Quarter)* marks the quarter in which incidents take place. Fixed effect specifications are reported at the bottom of each panel. Industry-related fixed effects are based on 2-digits SIC codes, and state-related fixed effects are based on firm headquarter states. All outcome and control variables are winsorized at 1 percent. Outcome variables are scaled by 100. Standard errors double clustered by both state and time are reported in parentheses below the coefficients. All regressions contain the same *Log(Total Assets)* control variable as Table 6 – Table 8. For brevity, I only report coefficients on the interaction terms.

	(1) Log(Sales)	(2) Log(Sales)
Pro-Gun-Rights * D(t = -4 Quarter)	-5.38	-3.50
	(3.63)	(2.70)
Pro-Gun-Rights * D(t = -3 Quarter)	-2.93	-2.73
	(3.29)	(2.98)
Pro-Gun-Rights * D(t = -2 Quarter)	-4.24	-3.11
	(4.31)	(3.49)
Pro-Gun-Rights * D(t = -1 Quarter)	-3.63	-2.00
	(3.80)	(3.18)
Pro-Gun-Rights * D(t = 0 Quarter)	-4.35*	-3.18
	(2.36)	(2.25)
Pro-Gun-Rights * D(t = 1 Quarter)	-6.90***	-6.46***
	(1.39)	(1.11)
Pro-Gun-Rights * D(t = 2 Quarter)	-5.51***	-3.89**
	(1.83)	(1.86)
Pro-Gun-Rights * D(t = 3 Quarter)	-2.69	-3.93***
	(2.04)	(1.34)
Pro-Gun-Rights * D(t = 4 Quarter)	-3.91	-2.70
	(2.72)	(2.14)
Pro-Gun-Rights * D(t = 5 Quarter)	-3.28*	-2.07
	(1.90)	(2.46)
Observations	56,525	56,525
Within R-squared	0.36	0.35
Firm FE	Yes	Yes
Time FE	Yes	-
State-Time FE	-	Yes
Industry-Time FE	-	Yes

**p<0.1, **p<0.05, ***p<0.01*

Table 2.11: Triple Interaction Analysis

This table presents triple interaction regressions on firm operating performance. The data are at the firm-quarter level. The sample period is from 2004 to 2018. *Pro-Gun-Rights* is an indicator variable set to one if a firm primarily donates to pro-gun-rights politicians in recent years. *Mass Shooting* is an indicator variable set to one if a severe incident (more than 10 people get killed) take place in the past three quarters in companies' headquarter states. *Pct of Revenue from Gov.* measures the percentage of firms' revenues coming from the government based on Compustat Segment data. *Consumer Related Industry* is an indicator variable that equals one if the firm is from consumer staples or consumer discretionary based on GICS industry classification. Fixed effect specifications are reported at the bottom of each panel. Industry-related fixed effects are based on 2-digits SIC codes, and state-related fixed effects are based on firm headquarter states. All outcome and control variables are winsorized at 1 percent. Outcome variables are scaled by 100. Standard errors double clustered by both state and time are reported in parentheses below the coefficients. All regressions contain the *Log(Total Assets)* control variable. For brevity, I only report coefficients on the interaction terms.

	(1)	(2)	(3)	(4)
	Log(Sales)	Log(Sales)	Log(Sales)	Log(Sales)
Pro-Gun-Rights * Mass Shooting	-4.78***	-3.55***	-3.28	-2.91*
	(1.09)	(1.29)	(2.08)	(1.68)
Pct of Revenue from Gov. * Pro-Gun-Rights * Mass Shooting	5.60	1.32		
	(9.47)	(11.06)		
Consumer Related Industry * Pro-Gun-Rights * Mass Shooting			-7.42	-4.63
			(6.84)	(4.80)
Observations	56,525	56,525	56,525	56,525
Within R-squared	0.36	0.36	0.36	0.36
Firm FE	Yes	Yes	Yes	Yes
Time FE	Yes	-	Yes	-
State-Time FE	-	Yes	-	Yes
Industry-Time FE	-	Yes	-	Yes

**p<0.1, **p<0.05, ***p<0.01*

Table 2.12: Deposit Market

This table presents estimation results based on the summary of deposits data from FDIC. The data are at the bank branch-year level. The sample period is from 2004 to 2018. *Pro-Gun-Rights* is an indicator variable that equals one if a bank primarily donates to pro-gun-rights politicians in recent years. *Google Trends (Shooting)* is one-year average of the search interests of topic "Shooting" from branches' states. *Mass Shooting* is an indicator variable set to one if a severe incident (more than 10 people get killed) in the year in the branch states. *Mass Shooting (All States)* is an indicator variable that equals one for all observations in a year if a severe incident happens in any state. Control variable includes one-year lagged bank assets *Log(Bank Assets)*$_{t-1}$. Fixed effect specifications are reported at the bottom of each panel. All outcome and control variables are winsorized at 1 percent. Outcome variables are scaled by 100. Standard errors double clustered by both bank and branch state are reported in parentheses below the coefficients.

	(1) Log(Branch Deposits)	(2) Log(Branch Deposits)	(3) Log(Branch Deposits)	(4) Log(Branch Deposits)	(5) Log(Branch Deposits)
Pro-Gun Rights	1.53	13.07***	1.72		4.55**
	(1.79)	(2.85)	(1.84)		(1.85)
Pro-Gun Rights * Google Trends (Shooting)		-0.61***			
		(0.15)			
Pro-Gun Rights * Mass Shooting			-5.67*	1.86	
			(2.98)	(2.63)	
Pro-Gun Rights * Mass Shooting (All States)					-5.34***
					(2.00)
Log(Bank Assets)$_{t-1}$	14.55***	14.15***	14.52***		14.44***
	(4.06)	(3.79)	(4.02)		(3.94)
Observations	503,259	503,259	503,259	503,259	503,259
R-squared	0.96	0.96	0.96	0.96	0.96
Bank-Year FE	-	-	-	Yes	-
County-Year FE	Yes	Yes	Yes	Yes	Yes
Branch FE	Yes	Yes	Yes	Yes	Yes

**p<0.1, **p<0.05, ***p<0.01*

Appendix Figures and Tables

Figure 2.A.1: Percentage of Non-Republican Candidates Supported by the NRA

This figure plots the fraction of NRA funded candidates who are affiliated with Non-Republican party (mainly Democratic party). The sample period is from 2000 election cycle to 2018 election cycle. The horizontal axis represents each election cycle, and the vertical axis represents the fraction of non-Republican candidates (in percentage point) who are also funded by the NRA.

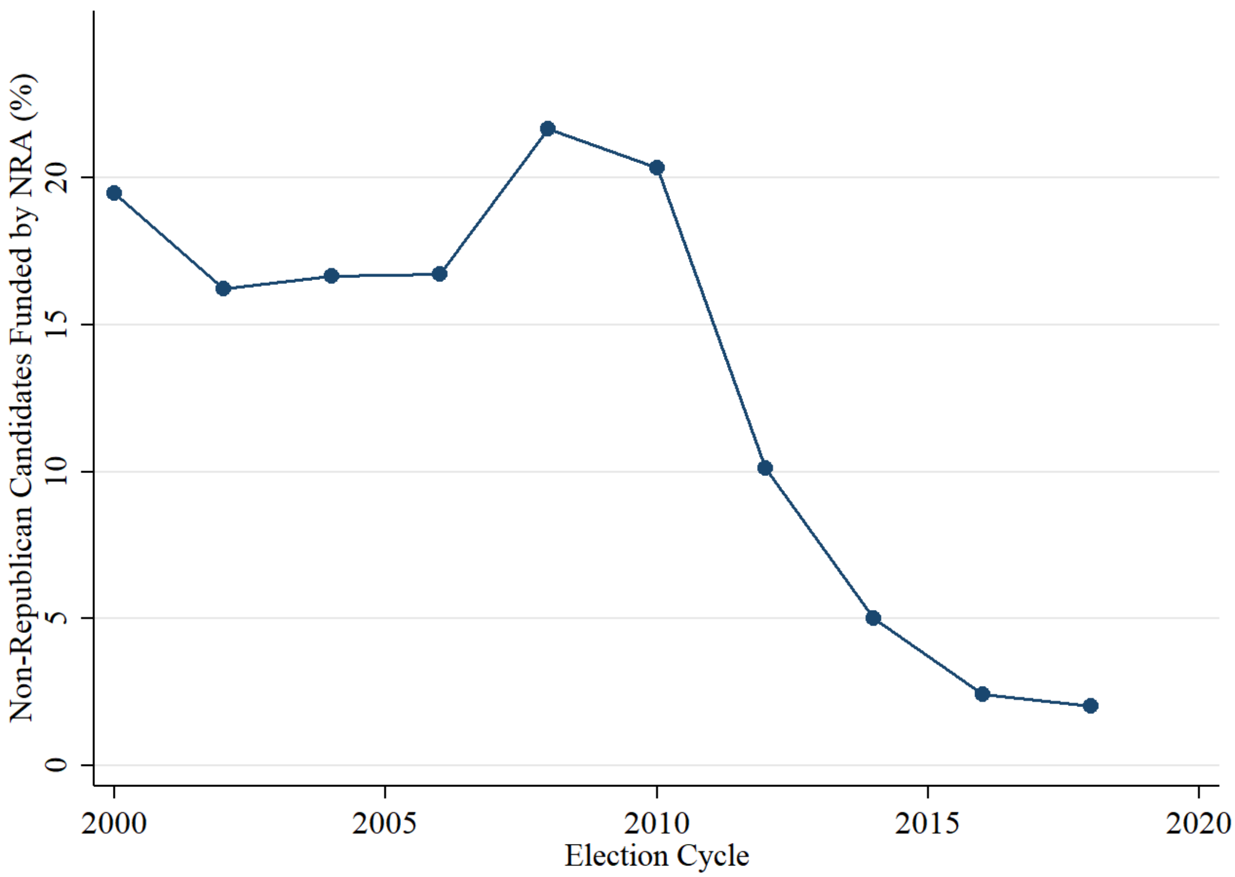


Table 2.A.1: T-Test of CARs Based on Pro-Gun-Rights (PAC) and Pro-Gun-Righs (Executive)

This table presents t-test results of Cumulative Abnormal Returns (CARs) calculated using Fama-French three-factor model around mass shooting days. Panel A presents tests based on *Pro-Gun-Rights (PAC)*, and Panel B presents tests based on *Pro-Gun-Rights (Executive)*. Columns 1 and 3 present average CARs of treatment and control firms. Columns 2 and 4 reports the number of observations in each group. Column 5 reports the difference in CARs between treatment and control firms, and column 6 reports the *t*-stats of the test.

	Pro-Gun-Rights (PAC)	*N*	Non-Pro-Gun-Rights (PAC)	*N*	Difference	*t*-stat
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: CARs Around Mass Shootings (#Fatalities > 10) - Based on Pro-Gun-Righs (PAC)						
Event Window (-1, +1)	-1.47	83	0.35	268	-1.82	-2.86***
Event Window (-1, +3)	-1.63	83	0.23	268	-1.86	-2.16**
Event Window (-1, +5)	-2.36	83	0.38	268	-2.74	-2.45**
Event Window (-1, +7)	-2.01	83	0.70	268	-2.71	-2.01**

**p<0.1, **p<0.05, ***p<0.01*

	Pro-Gun-Rights (EXE)	*N*	Non-Pro-Gun-Rights (EXE)	*N*	Difference	*t*-stat
	(1)	(2)	(3)	(4)	(5)	(6)
Panel B: CARs Around Mass Shootings (#Fatalities > 10) - Based on Pro-Gun-Righs (Executive)						
Event Window (-1, +1)	-1.07	135	0.08	314	-1.14	-2.45**
Event Window (-1, +3)	-1.21	135	-0.44	314	-0.77	-1.32
Event Window (-1, +5)	-1.84	135	-0.07	314	-1.78	-2.42**
Event Window (-1, +7)	-1.73	135	-0.11	314	-1.62	-1.95*

**p<0.1, **p<0.05, ***p<0.01*

Table 2.A.2: Corporate Operating performance on Pro-Gun-Rights (PAC)

This table presents estimation results of changes in operating performance with *Pro-Gun-Rights (PAC)* as explanatory variable. Panel A presents results of interaction regressions using Google Trends (Shooting), and panel B presents regression results using exact timing of mass shootings. Columns 1-4 are at the firm-quarter level, and columns 5 are at the firm-year level. Fixed effects are reported at the bottom of each panel. Industry-related fixed effects are based on 2 digits SIC codes, and state-related fixed effects are based on firm headquarter states. All outcome and control variables are winsorized at 1 percent. Outcome variables are scaled by 100. Standard errors double clustered by both state and time are reported in parenthesis below the coefficients.

Panel A: Google Trend (Shootings)

	Log(Sales)	Sales/Assets	Log(OP Cost)	ROA	Log(#Emp)
	(1)	(2)	(3)	(4)	(5)
Pro-Gun-Rights (PAC) * G_Trend(Shooting)	-0.64***	-0.11***	-0.68***	-0.01	-0.64***
	(0.19)	(0.04)	(0.16)	(0.01)	(0.17)
Pro-Gun-Rights (PAC)	8.77***	1.57**	10.57***	0.07	10.06***
	(2.98)	(0.63)	(2.60)	(0.12)	(3.11)
G_Trend(Shooting)	-0.10	-0.02	-0.10	-0.00	0.08
	(0.14)	(0.02)	(0.15)	(0.01)	(0.31)
Log(Total Assets)	60.23***	-6.11***	59.06***	-0.75***	44.51***
	(2.34)	(0.37)	(2.42)	(0.08)	(2.53)
Observations	35,802	35,802	35,802	35,802	9,825
Within R-squared	0.36	0.13	0.35	0.03	0.37
Firm FE	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes

*p<0.1, **p<0.05, ***p<0.01

Panel B: Mass Shooting

	Log(Sales)	Sales/Assets	Log(OP Cost)	ROA	Log(#Emp)
	(1)	(2)	(3)	(4)	(5)
Pro-Gun-Rights (PAC) * Mass Shooting	-8.15***	-1.77***	-11.98***	-0.21	-4.08**
	(2.19)	(0.57)	(2.63)	(0.16)	(1.67)
Mass Shooting	5.10***	0.80**	4.83**	0.08	3.75***
	(1.83)	(0.36)	(1.91)	(0.06)	(1.03)
Pro-Gun-Rights (PAC)	-2.41**	-0.41	-1.28	-0.17***	-1.17
	(1.11)	(0.26)	(1.26)	(0.05)	(1.00)
Log(Total Assets)	60.38***	-6.09***	59.21***	-0.74***	44.61***
	(2.43)	(0.37)	(2.50)	(0.08)	(2.57)
Observations	35,802	35,802	35,802	35,802	9,825
Within R-squared	0.36	0.12	0.34	0.03	0.37
Firm FE	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes

*p<0.1, **p<0.05, ***p<0.01

Table 2.A.3: Operating Performance on Pro-Gun-Rights (Executive)

This table presents estimation results of changes in operating performance with *Pro-Gun-Rights (Executive)* as explanatory variable. Panel A presents results of interaction regressions using Google Trends (Shooting), and panel B presents regression results using exact timing of mass shootings. Columns 1-4 are at the firm-quarter level, and columns 5 are at the firm-year level. Fixed effects are reported at the bottom of each panel. Industry-related fixed effects are based on 2 digits SIC codes, and state-related fixed effects are based on firm headquarter states. All outcome and control variables are winsorized at 1 percent. Outcome variables are scaled by 100. Standard errors double clustered by both state and time are reported in parenthesis below the coefficients.

Panel A: Google Trend (Shootings)

	Log(Sales)	Sales/Assets	Log(OP Cost)	ROA	Log(#Emp)
	(1)	(2)	(3)	(4)	(5)
Pro-Gun-Rights (EXE) * G_Trend(Shooting)	-0.26***	-0.05**	-0.24**	-0.00	-0.23**
	(0.09)	(0.03)	(0.10)	(0.01)	(0.10)
Pro-Gun-Rights (EXE)	4.21***	1.14**	4.35**	0.04	3.21
	(1.52)	(0.46)	(2.14)	(0.14)	(1.97)
G_Trend(Shooting)	-0.16	-0.03	-0.19*	-0.01*	-0.32
	(0.12)	(0.02)	(0.10)	(0.01)	(0.30)
Log(Total Assets)	60.42***	-5.51***	60.56***	-0.79***	43.91***
	(2.17)	(0.34)	(2.62)	(0.10)	(3.15)
Observations	38,775	38,775	38,775	38,775	10,513
Within R-squared	0.34	0.09	0.32	0.02	0.36
Firm FE	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes

*p<0.1, **p<0.05, ***p<0.01

Panel B: Mass Shooting

	Log(Sales)	Sales/Assets	Log(OP Cost)	ROA	Log(#Emp)
	(1)	(2)	(3)	(4)	(5)
Pro-Gun-Rights (EXE) * Mass Shooting	-0.36	-1.09**	-3.04	-0.10	-1.67
	(2.29)	(0.49)	(2.18)	(0.12)	(1.30)
Mass Shooting	1.91	0.81**	1.55	0.05	2.89***
	(1.46)	(0.32)	(2.01)	(0.07)	(0.85)
Pro-Gun-Rights (EXE)	-0.69	0.15	-0.09	-0.01	-1.13*
	(0.78)	(0.17)	(0.98)	(0.04)	(0.63)
Log(Total Assets)	60.50***	-5.50***	60.64***	-0.79***	43.94***
	(2.20)	(0.34)	(2.64)	(0.10)	(3.16)
Observations	38,775	38,775	38,775	38,775	10,513
Within R-squared	0.33	0.09	0.32	0.02	0.35
Firm FE	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes

*p<0.1, **p<0.05, ***p<0.01

Table 2.A.4: CARs Regression with Gun Rights Ratio

This table presents the Cumulative Abnormal Returns (CARs) regressions with *Gun Rights Ratio* as explanatory variable after controlling for event fixed effects (mass shooting fixed effects). The dependent variables are (-1, +1) CARs calculated based on Fama-French three-factor model.

	(-1, +1)	(-1, +3)	(-1, +5)	(-1, +7)
	(1)	(2)	(3)	(4)
Pro-Gun-Rights	-1.83***	-1.32	-2.14*	-1.41
	(0.67)	(0.88)	(1.12)	(1.33)
Constant	0.94**	0.43	0.89	0.60
	(0.41)	(0.54)	(0.69)	(0.82)
Observations	611	611	611	611
R-squared	0.09	0.06	0.06	0.06
Event FE	Yes	Yes	Yes	Yes

**p<0.1, **p<0.05, ***p<0.01*

Table 2.A.5: Operating performance on Gun Rights Ratio

This table presents estimation results of changes in operating performance with *Gun Rights Ratio* as explanatory variable. Panel A presents results of interaction regressions using Google Trends (Shooting), and panel B presents regression results using exact timing of mass shootings. Columns 1-4 are at the firm-quarter level, and columns 5 are at the firm-year level. Fixed effects are reported at the bottom of each panel. Industry-related fixed effects are based on 2 digits SIC codes, and state-related fixed effects are based on firm headquarter states. All outcome and control variables are winsorized at 1 percent. Outcome variables are scaled by 100. Standard errors double clustered by both state and time are reported in parenthesis below the coefficients.

Panel A: Google Trend (Shootings)

	Log(Sales)	Sales/Assets	Log(OP Cost)	ROA	Log(#Emp)
	(1)	(2)	(3)	(4)	(5)
Pro-Gun-Rights * Google Trends (Shooting)	-0.73***	-0.13***	-0.80***	-0.01	-0.65***
	(0.18)	(0.04)	(0.13)	(0.01)	(0.14)
Pro-Gun-Rights	12.78***	2.49***	15.49***	0.11	10.07***
	(3.17)	(0.79)	(2.36)	(0.23)	(2.29)
Google Trends (Shooting)	0.04	-0.00	0.04	-0.00	0.12
	(0.11)	(0.02)	(0.12)	(0.00)	(0.31)
Log(Total Assets)	60.54***	-5.77***	59.70***	-0.73***	42.60***
	(2.09)	(0.27)	(2.29)	(0.08)	(2.32)
Observations	56,525	56,525	56,525	56,525	15,389
Within R-squared	0.36	0.11	0.35	0.02	0.36
Firm FE	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes

*p<0.1, **p<0.05, ***p<0.01

Panel B: Mass Shooting

	Log(Sales)	Sales/Assets	Log(OP Cost)	ROA	Log(#Emp)
	(1)	(2)	(3)	(4)	(5)
Pro-Gun-Rights * Mass Shooting	-4.36**	-1.90***	-10.78***	0.03	-3.44***
	(2.02)	(0.69)	(1.69)	(0.11)	(0.30)
Mass Shooting	4.91***	1.47***	7.62***	-0.02	4.10***
	(1.50)	(0.23)	(1.76)	(0.05)	(0.57)
Pro-Gun-Rights	-0.53	0.16	1.12	-0.07	-1.87
	(1.18)	(0.30)	(1.28)	(0.07)	(1.22)
Log(Total Assets)	60.72***	-5.74***	59.90***	-0.73***	42.76***
	(2.14)	(0.28)	(2.33)	(0.08)	(2.36)
Observations	56,525	56,525	56,525	56,525	15,389
Within R-squared	0.36	0.10	0.35	0.02	0.36
Firm FE	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes

*p<0.1, **p<0.05, ***p<0.01

Table 2.A.6: Self-Reported Occupation

This table reports the list of self-reported occupations in the matched executive individual contribution sample. Occupations are reported in FEC individual contributions data available from the 2004 election cycle. The table covers all matched executive individual contributions from 2004 to 2018.

Self-Reported Occupation	Frequency	Percentage	Cum Percentage
	(1)	(2)	(3)
CEO	7397	16.09	16.09
EXECUTIVE	6280	13.66	29.75
PRESIDENT	2973	6.47	36.22
CHAIRMAN	2377	5.17	41.39
CHAIRMAN & CEO	1523	3.31	44.71
CFO	1466	3.19	47.89
PRESIDENT & CEO	1351	2.94	50.83
ATTORNEY	1047	2.28	53.11
EXECUTIVE VICE PRESIDENT	868	1.89	55
BANKER	647	1.41	56.41
VICE PRESIDENT	570	1.24	57.65
CHIEF EXECUTIVE OFFICER	558	1.21	58.86
CHAIRMAN AND CEO	551	1.2	60.06
PRESIDENT/CEO	504	1.1	61.16
CHAIRMAN/CEO	501	1.09	62.25
COO	482	1.05	63.29
PRESIDENT AND CEO	478	1.04	64.33
SENIOR VICE PRESIDENT	448	0.97	65.31
GENERAL COUNSEL	404	0.88	66.19
EXECUTIVE CHAIRMAN	364	0.79	66.98
C.E.O.	327	0.71	67.69
CHIEF FINANCIAL OFFICER	320	0.7	68.39
CHAIRMAN OF THE BOARD	310	0.67	69.06
VICE CHAIRMAN	305	0.66	69.72
EXECUTIVE VP	287	0.62	70.35
MANAGEMENT	271	0.59	70.94
MANAGER	260	0.57	71.5
PRESIDENT & COO	245	0.53	72.04
BUSINESS EXECUTIVE	222	0.48	72.52
REAL ESTATE	212	0.46	72.98
EVP	185	0.4	73.38
FINANCE	170	0.37	73.75
SENIOR VP	163	0.35	74.11
VP	159	0.35	74.45
CHIEF OPERATING OFFICER	158	0.34	74.8
CHAIRMAN, PRESIDENT & CEO	138	0.3	75.1