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Three Essays in Financial Economics

Private Deposit Insurance, Sustainable Finance and Climate Risk

Three Essays in Financial Economics: Private Deposit Insurance, Sustainable Finance and Climate Risk

By

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Abstract

This dissertation consists of three essays in the field of financial economics. Chapter 1 examines the role of private deposit insurance as a complement to federal deposit insurance for deposit flows, bank lending, and moral hazard during a financial crisis. This chapter shows that banks whose deposits are federally and privately insured obtain more deposits, expand lending, and remain prudent in the mortgage origination process during the subprime crisis, in contrast to banks whose deposits are only federally insured. Deposit inflows are stronger prior to the increase of the federal deposit insurance limit and introduction of the Transaction Account Guarantee Program. The results highlight a role for private sector solutions in the safety net.

Chapter 2 investigates whether mass shootings affect the deposit growth of banks that have publicly known lending relationships with gun manufacturers. This chapter finds that branches operated by such banks experience lower deposit growth rates in the years of mass shootings. This effect is greater for branches located near the mass shootings, in counties with fewer gun stores, more gun violence, more votes for the Democratic Party in the 2016 presidential election, and in counties with higher educational attainment. However, event study evidence shows that the mass shootings do not affect the banks' market value.

Can public policies addressing climate risks disrupt the housing market? Chapter 3 studies the impact of a UK flood reinsurance scheme in the property market. Leveraging a unique data set on the population of all property transactions in England, this chapter documents that this policy increases the value and transaction volume of flood-prone properties. The effect on property value is especially strong in urban areas and areas with wealthier households. The findings highlight the transition risk and wealth redistribution caused by the reinsurance scheme.

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Author's declaration

I declare that the work in this dissertation was carried out in accordance with the requirements of the University's Regulations and Code of Practice for Research Degree Programmes and that it has not been submitted for any other academic award. Except where indicated by specific reference in the text, the work is the candidate's own work. Work done in collaboration with, or with the assistance of, others, is indicated as such. Any views expressed in the dissertation are those of the author.

I confirm that Chapter 1 and 2 were jointly co-authored with Piotr Danisewicz and Klaus Schaeck, and Chapter 3 was jointly co-authored with Nicola Garbarino and Benjamin Guin.

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CHAPTER 1

PRIVATE DEPOSIT INSURANCE, DEPOSIT FLOWS, BANK LENDING, AND MORAL HAZARD

1.1 Introduction

This paper investigates the role of private deposit insurance as a complement to federal deposit insurance for deposit flows, bank lending, and moral hazard during a financial crisis. To this end, we exploit a novel empirical setting, the presence of a private deposit insurance fund, the Depositors Insurance Fund (MA-DIF) which insures deposits above the Federal Deposit Insurance Corporation (FDIC) coverage limit in state-chartered savings banks headquartered in Massachusetts since 1934. While an additional layer of protection in the form of private deposit insurance may incentivize customers of member banks of the MA-DIF not to withdraw their funds and motivate customers of non-members to reallocate funds into member banks during crises, its effect on liquidity provision, i.e., the transformation of deposits into loans, and moral hazard is not clear.[1] Moreover, the coexistence of banks in Massachusetts whose deposits are only insured by the FDIC may also increase deposit volatility, disrupt bank-borrower relationships, and ultimately affect the real economy.[2]

[1]Gatev and Strahan (2006) argue that banks provide liquidity during crises only when they are awash with funds, and Pennacchi (2006) states that government deposit insurance enables banks to be considered a safe haven during flights to quality. Ivashina and Scharfstein (2010) find that funding constraints during a crisis reduce lending but these contractions are less pronounced in banks with better access to deposit financing, reiterating the view by Kashyap et al. (2002) that lending is inextricably linked with deposit taking. However, increases in bank liquidity during crises do not necessarily increase lending because Acharya and Merrouche (2013) document that banks precautionary hoard liquidity during crises. Furthermore, Acharya and Mora (2015) find that the mechanism that allows banks to create liquidity by transforming deposits into loans collapsed prior to 3rd October 2008, when the federal government increased the FDIC deposit insurance coverage limit from 100,000 to 250,000 USD.

[2]Deposit volatility may increase the cost of supplying long-term funding with corresponding negative real effects. It interferes with banks' ability to engage in maturity transformation by converting short-term liquid deposits into

Despite the importance of understanding the intended and unintended effects of design features of deposit insurance schemes like ownership and management by private parties or the government, evidence on this subject is scarce for three reasons. First, prior work typically relies on cross-country data. This poses econometric challenges because limited variation of design features within countries over time hampers the inclusion of country-fixed effects (e.g., Demirgüç-Kunt and Detragiache (2002); Hovakimian et al. (2003); Demirgüç-Kunt and Huizinga (2004)). Second, data for individual countries that simultaneously operate multiple deposit insurance schemes managed by private parties and the government to conduct within-country estimations to rule out country-specific effects are difficult to obtain.[3] Third, empirically establishing the role of different deposit insurance systems during a crisis requires not only granular data on deposit flows and lending, but also a shock, i.e., a crisis, that is not driven by those banks whose behavior is the subject of study.

Our setting in Massachusetts is econometrically appealing to tackle these challenges. We compare deposit flows, bank lending, and mortgage origination for the period 2004-2015 in banks that are members of the MA-DIF with banks located in close proximity, i.e., within the state of Massachusetts (and the surrounding states), whose deposits are only insured by the FDIC using annual branch-level data from the Summary of Deposits (SoD), quarterly bank-level data from Call Reports, and annual mortgage-level data from the Home Mortgage Disclosure Act (HMDA). The banks in our sample are relatively small, with average total assets of 1,386 million USD. They are neither too-big-to-fail, nor are they drivers of the crisis.

To motivate our analysis, Figure 1.1 illustrates deposit flows in Massachusetts prior to, during, and after the financial crisis.[4] The graph highlights that MA-DIF member banks (represented by the dashed line) experience deposit inflows during the crisis (shaded area), while other banks (represented by the solid line) experience deposit outflows until 2009. In terms of the volume of deposits, total deposits of MA-DIF member banks increase by 4.4 billion USD over the crisis period, while total deposits of other banks decrease by 30 billion USD during the crisis.[5]

long-term illiquid loans (Levine and Zervos, 1998). Chodorow-Reich (2014) finds that funding shocks are transmitted to firms and households through credit supply, and Carvalho et al. (2015) show that bank distress is associated with equity valuation losses and investment cuts in borrower firms that cannot be offset by replacing bank funding with funding from bond markets. Similarly, Choudhary et al. (2017) show that greater deposit volatility increases the cost to access outside liquidity to replace volatile deposits which banks pass onto their creditors. Iyer et al. (2019) find that reallocating deposits from accounts above the insurance limit to accounts below the limit reduces banks' loan growth.

[3] Adema et al. (2019) report that only nine countries (Canada, Germany, Italy, Japan, Korea, Mexico, Brazil, the U.S., and Portugal) operate multiple deposit insurance schemes in 2019. Prior work by Beck (2002) provides a detailed description of different deposit insurance schemes in Germany. Demirgüç-Kunt et al. (2014) present a database that describes design features of deposit insurance schemes around the world.

[4] Note that total deposits of non-member banks headquartered in Massachusetts is much larger than the number of MA-DIF member banks. Figure 1.1 therefore uses two scales, one for MA-DIF members on the left-hand side, and one for non-members on the right-hand side to provide clearer insights into the evolution of deposits over time.

[5] A potential explanation for the net outflow of deposits is that these funds may also have been spent, invested

Our empirical results reinforce the visual evidence. We use branch-level data to present novel evidence that deposits of MA-DIF member banks increase relative to non-members during the crisis, irrespective of whether the non-members are located in Massachusetts or in the surrounding states. This effect is greater prior to the increase in the FDIC deposit insurance coverage limit and prior to the introduction of the Transactions Account Guarantee (TAG) Programme. Deposits in branches of MA-DIF member banks increase by 7.7% during the crisis when we constrain the sample to branches of banks headquartered in Massachusetts whose branches are located exclusively in this state to prevent the influence of time-varying state-specific effects which we cannot control for because of the structure of the data.[6] Further tests highlight that the volume of FDIC-uninsured deposits increases significantly in member banks of the MA-DIF but there is no such effect for FDIC-insured deposits. Likewise, we document a significant increase in the number of accounts above the FDIC coverage level for member banks during the crisis.

The documented changes in deposits are not homogeneous across banks. Additional cross-sectional tests show that members of the MA-DIF experience more deposit inflows relative to control group banks with more uninsured deposits. They also tend to attract more deposits in comparison to less profitable non-member banks.

Plausible alternative hypotheses cannot explain the increase in deposits for member banks. We rule out pricing effects. There is also no evidence that member banks are safer relative to the control group and therefore receive more deposits. Signalling effects for bank soundness arising from participation in the Troubled Asset Relief Programme (TARP), publicly known enforcement actions, different levels of opacity, and the use of brokered deposits also do not affect our conclusions.

Additional tests use bank-level data to examine the effect of MA-DIF membership on different types of deposits. The findings are more nuanced. Interest-bearing deposits increase significantly during the crisis in MA-DIF member banks but noninterest-bearing deposits, our proxy for those types of funds which were fully guaranteed by the government during the crisis, are unaffected.[7]

Together, our findings illustrate that depositors differentiate between MA-DIF member banks and non-member banks. Moreover, coexisting multiple deposit insurance schemes with different coverage levels can increase deposit volatility.

into government bonds, or been converted into cash.

[6]See Table 1.3, Panel B, Column 4. The effect is calculated as $exp(0.074) - 1 = 7.7\%$

[7]The TAG Programme guaranteed noninterest-bearing transactions accounts, low-interest Negotiable Order of Withdrawal (NOW) accounts, and Interest on Lawyers Trust Accounts (IOLTAs). Call Reports do not report the value of these items separately.

We also find that member banks of the MA-DIF lend significantly more than non-members during the crisis. The increase is driven by residential mortgage lending. Our initial lending tests are performed on the bank-level. To confirm that the differences in mortgage lending between MA-DIF members and non-members do not merely reflect demand conditions, we turn to HMDA loan application-level data. This data allows distinguishing between accepted and rejected loan applications. We can also better control for local economic conditions and mortgage applicants' demographic characteristics. The results suggest that MA-DIF member banks are more likely to accept mortgage applications during the crisis.

Our findings reject the view that the additional layer of protection provided by the MA-DIF increases moral hazard. The Tier 1 capital ratio, the charge off ratio, Z-Scores, and the ratio of nonperforming mortgages to total mortgages remain unaffected or show some limited evidence for improved soundness during the crisis. Using the loan-to-income ratio as a proxy for borrower risk, we find that MA-DIF member banks originate significantly less risky mortgages than the control group.

Although membership in the MA-DIF is compulsory for state-chartered savings banks in Massachusetts, banks' ability to choose and change their charter, relocate their headquarter, or merge with another institution gives rise to potential selection bias that may interfere with our inferences. Our tests therefore only include banks that are consistently members of the MA-DIF between 2004 and 2015. This mitigates concerns that banks acquire membership to benefit from the additional layer of protection and ensures that membership during the crisis is not conditional on deposits and lending behavior. Potential differences between MA-DIF member banks and the banks in the control group may also influence our inferences. To alleviate such concerns, we show that banks in the control group constitute a valid counterfactual for the MA-DIF member banks. Further tests based on a matching strategy also reinforce our inferences.

Our findings are important because deposits account for more than three quarters of all banks' funding, and over 62% of deposits in the U.S. banking system were uninsured at the onset of the crisis (Acharya and Mora, 2015). Since uninsured deposits are often impaired in a bank default, they are prone to runs (Egan et al., 2017).[8] It is therefore crucial to understand the effects of a private deposit insurance scheme during a crisis that protects such deposits.

Moreover, investigating coexisting private and government-run deposit insurance schemes to comprehend their effects for the behavior of banks, depositors, and deposit volatility has

[8] Diamond and Dybvig (1983); Goldstein and Pauzner (2005) highlight that run-prone uninsured depositors are the main source of bank fragility. Iyer and Puri (2012) show that uninsured depositors are most likely to run, and Iyer et al. (2016) show that the composition of depositors plays a role for which depositors run. Importantly, funding vulnerabilities are further aggravated by banks' use of short term wholesale debt.

attracted attention after the crisis. At present, debates focus on the establishment of a common deposit protection system to complete the banking union in the European Union. Our study not only highlights depositors' ability to differentiate between design features of deposit insurance schemes, but also illustrates the benefits as well as drawbacks of multiple deposit schemes. While member banks experience fewer deposit outflows, originate more loans, and act more prudently during crises, our results also suggest a dark side in the form of greater deposit volatility. This may result in sudden and unexpected deposit outflows in banks whose insurance schemes offer less protection.

Unlike previous work on deposit insurance, which is typically concerned with government deposit insurance, we focus on the effects of private deposit insurance as a complement to government deposit insurance. Although prior studies discuss the benefits of private deposit insurance that come in the form of reduced agency problems between owners, managers, and banks, fewer adverse selection problems, and incentives for banks to engage in peer monitoring to impose discipline on each other and avoid free-riding to mitigate moral hazard (Calomiris, 1989, 1990; English, 1993; Beck, 2002), empirical support is scant. We present evidence that private deposit insurance makes banks less vulnerable to short-term funding problems. This allows them to continue lending without increasing moral hazard in a crisis.

This paper is also related to the literature on the pros and cons of deposit insurance, and how design features affect the behavior of banks and depositors.[9] Cull et al. (2005) find that generous government-funded deposit insurance adversely affects financial development and growth when the rule of law and banking supervision are weak. Chernykh and Cole (2011) show that introducing deposit insurance increases intermediation, but also increases risk-taking. De Graeve and Karas (2014) show that deposit insurance mitigates deposit outflows of insured banks in the Russian deposit market during the turbulent period in 2004. Calomiris and Chen (2020) find that generous deposit insurance increases lending and leverage.

Moral hazard is the focus of many other studies. Keeley (1990) highlights that fixed-rate deposit insurance is a source of moral hazard, Grossman (1992) shows that insured thrifts are more prone to originate risky loans, and Demirgüç-Kunt and Detragiache (2002) find that deposit insurance increases the likelihood of crises. They show that this effect is greater, the more generous the coverage is and when the scheme is run by the government rather than by private parties. Demirgüç-Kunt and Huizinga (2004) demonstrate that deposit insurance lowers market discipline on bank risk-taking, and Pennacchi (2006) shows that moral hazard incentives remain even if deposit insurance premiums are fairly priced.[10] Anginer et al. (2014) report that the

[9] Demirgüç-Kunt et al. (2008) describe the determinants of deposit insurance adoption and design.

[10] Buser et al. (1981); Marcus and Shaked (1984); Pennacchi (1987a,b); Laeven (2002) focus on the pricing of deposit insurance, and Cooperstein et al. (1995) calculate the aggregate cost of deposit insurance.

effects of deposit insurance on bank risk-taking vary prior to and after the crisis. While moral hazard effects dominate in tranquil times, deposit insurance has stabilizing effects during a crisis. Calomiris and Jaremski (2019) also show that deposit insurance undermines market discipline, incentivizes bank risk-taking, and makes banks compete aggressively in deposit markets.

In contrast, Karels and McClatchey (1999) find no evidence that adopting deposit insurance increases risk-taking. Similarly, Gropp and Vesala (2004) show that explicit deposit insurance limits bank risk. We show that a private deposit insurance scheme – in combination with federal deposit insurance – does not increase bank risk-taking.

A growing literature focuses on depositor behavior and deposit flows, and how funding structure affects bank risk-taking. Boyle et al. (2015) use conjoint analysis to show the introduction of deposit insurance at the beginning of a crisis may not help prevent bank runs. Brown et al. (2017) use a laboratory setting to show that panic-based deposit withdrawals can trigger depositors to withdraw also at economically related banks, thus leading to contagion. Brown et al. (2020) find that the deposit withdrawal increase with the severity of bank distress, but bank-client relationship mitigates withdrawal risk of distressed banks. Bonfim and Santos (2020) document that depositors move savings into banks with access to more credible insurance schemes during the crisis, and Lambert et al. (2017) show that the increase in FDIC insurance coverage limit during the recent financial crisis resulted in heterogeneous inflows of insured deposits across banks, and it also affected these banks' propensity to engage in risky investments. Chen et al. (2020) establish a link between opacity and deposit flows. They show run-prone uninsured deposits display greater flow-performance sensitivity when banks are more transparent. Iyer et al. (2019) find that changes in insurance coverage in Denmark during the crisis distort deposit competition. They show that removing a blanket guarantee makes individuals with deposits above the coverage limit split their deposits across multiple accounts and reallocate funds to banks considered to be too-big-to-fail. We extend this literature by illustrating the effects of private deposit insurance on deposit flows.

1.2 Institutional background

The MA-DIF was established by the Massachusetts legislature in 1934 in response to the Great Depression as an industry-sponsored private insurance company to insure deposits in savings banks chartered in Massachusetts.[11] Membership of the FDIC and the MA-DIF was mutually exclusive until 1956, since then, the MA-DIF insures deposits above the FDIC insurance coverage

[11] In the U.S., number of insurance funds failed in the past, including state-sponsored and private deposit insurance funds. Several private insurance funds also ceased to exist because the federal government imposed a 10% federal tax on state-chartered bank notes in 1900 (Calomiris, 1990)

limit.[12]

We next describe the characteristics of the MA-DIF in 5 key aspects: (i) insurance coverage; (ii) membership; (iii) funding; (iv) management; and (v) public awareness.

(i) Insurance coverage

The MA-DIF offers full deposit insurance for its members' deposits and accrued interest without limit. All deposits above the FDIC insurance coverage limit, which rose from 100,000 USD per depositor at the beginning of our sample period to 250,000 USD in Q3:2008 (see Figure 1.2), in MA-DIF member banks are insured. The MA-DIF protects all types of deposit accounts, including savings accounts, checking, and NOW accounts, certificates of deposit (CDs), money market deposit accounts, and retirement deposit accounts. Whether or not MA-DIF insurance applies depends only on the membership of banks in the scheme, but not on the location of branches or residence of depositors.[13]

(ii) Membership

Membership in the MA-DIF is compulsory for all savings banks chartered in Massachusetts, as discussed above. However, the number of members varies over time due to mergers and acquisitions, changes in charters, and failures. During our sample period 2004-2015, 51 banks are consistently members of the MA-DIF.[14]

(iii) Funding

The MA-DIF is exclusively funded by its members, without any support from either the federal or the state government. Its sources of funds include accumulated annual assessments on its members and interest income from its investments. The board of directors determines the assessments rates based on excess deposits and risk classifications of each member bank. The MA-DIF's assessment schedule is modelled after the risk-assessment matrix developed by the FDIC in the late 1990s.[15] The current assessment rate for a well-capitalized member bank is 2

[12]In Massachusetts, there are two other private deposit insurance funds, the Share Insurance Fund, and the Massachusetts Credit Union Share Insurance Corporation. The former was exclusively available to co-operative banks and merged after our sample period with the MA-DIF in March 2020. The latter is only available to credit unions. Both funds are not relevant to our study because our sample only includes commercial and savings banks.

[13]Foreign deposits are not insured by the MA-DIF. Foreign deposits play a limited role for members of the MA-DIF. Most of the member banks record a value of zero for foreign deposits in Call Reports.

[14]The number of members during the period 2004-2015 is available from the annual report. No MA-DIF bank failed during the recent financial crisis.

[15]The classification of a bank for assessment purposes is based on the composite CAMELS rating from its most recent regulatory examination, and its capital classification.

basis points of excess deposits. The assessment rate must be approved by the Commissioner of Banks of the Commonwealth of Massachusetts.

Massachusetts law and the MA-DIF's investment policy restrict the investments to U.S. Treasury and federal agency obligations and obligations fully guaranteed by the U.S. government.[16]

(iv) Management

Unlike the FDIC which is a federal government agency managed by a board of directors with directors appointed by the president and confirmed by the senate, the MA-DIF is privately managed by its member banks without any government involvement. The board of directors primarily consist of presidents and chief executive officers of member banks. The MA-DIF is examined annually by the Massachusetts Division of Banks and audited by an independent auditor.

The MA-DIF quarterly reviews its members' financial reports. Additionally, the MA-DIF consults on a regular basis with both the FDIC and the Massachusetts Division of Banks, sharing information about the financial condition of its members. The MA-DIF has the authority to conduct a special examination of a member bank, with the approval of the Massachusetts Commissioner of Banks. However, this examination authority has rarely been requested in the past and if so, it was only requested in extreme cases. The MA-DIF's role in overseeing its members is largely focused on monitoring members rather than having broad regulatory powers. Unlike the FDIC, the MA-DIF has no role in the resolution process of member banks, and the MA-DIF has no authority to impose enforcement actions against its members.

(v) Public awareness

Member banks display the MA-DIF logo on websites, doors, and teller stations, depositors can access the details of the scheme on the website, brochures, and via customer service representatives.

During the crisis, increasing media attention focuses on the unlimited deposit insurance coverage provided by the MA-DIF. For example, an article "Massachusetts sets standard on deposits" published by the Wall Street Journal on 5th August 2008 reports on the unlimited insurance coverage of member banks.[17] The article also highlights that member banks report many inquiries from new and existing clients prior to publication of the article.

[16] Total assets of the MA-DIF amount to 355 million USD in 2008.

[17] The article of the Wall Street Journal is available on *https://www.wsj.com/articles/SB121789647048112087*.

"[...] turmoil in the banking industry has been a boon for state-chartered banks and some credit unions in Massachusetts. In recent weeks, they've been inundated with inquiries from new and existing clients."

This article provides evidence for the public awareness of the MA-DIF during the crisis. It also illustrates that unlimited coverage in member banks attracts depositors during the crisis. The annual report of the MA-DIF also reports increased enquiries from depositors during the crisis in 2008.[18]

"[...] the Depositors Insurance Fund received numerous telephone calls, emails, and letters from depositors as well as local and national media inquiring about Depositors Insurance Fund insurance, and I know that many of our members received increased inquiries as well."

Google Trends provides additional evidence that suggests an increasing interest in the MA-DIF during the crisis.[19] Figure 1.3 shows the monthly Google Trends index in 2004-2015, indicating the search volume for the term "Depositors Insurance Fund" in Massachusetts. Prior to the crisis, the index constantly stays at zero, indicating that the public paid little attention to the MA-DIF. However, the search volume index increases at the onset of the crisis, reaching its peak in the month of the bankruptcy of Lehman Brothers, September 2008. This illustration supports the view that depositors' interest in the MA-DIF increases amid greater concern about financial system soundness during the crisis.

Our appendix contains additional information about the MA-DIF. We present a detailed comparison between design features of the FDIC and the MA-DIF in Panel A of Table A1.1. In Panel B, we summarize common characteristics of different insurance mechanisms since the Antebellum period in the U.S. based on White (1981); Calomiris (1989, 1990); English (1993).

1.3 Hypothesis Development

We first develop predictions for deposit flows of MA-DIF member banks, followed by hypotheses for lending. Finally, we present hypotheses for the role of private deposit insurance for moral hazard.

[18] The 2008 annual report is available on *https://www.difxs.com/reports/AnnualReports/DIFAnnualReport2008.pdf*.

[19] Google Trends is a website by Google that records the popularity of search queries in Google Search across various regions and languages.

1.3.1 Deposit flows during the financial crisis

Unlike other financial intermediaries, banks have access to government support in the form of deposit insurance. The starting point for our hypotheses is the theory by Diamond and Dybvig (1983). They posit that government deposit insurance reduces the probability of bank runs and provides liquidity insurance to borrowers. Similar arguments are made by Pennacchi (2006). He argues that banks lacked the ability to attract deposits before the establishment of the FDIC. Iyer and Puri (2012); Demirgüç-Kunt et al. (2015) state that deposit insurance deters deposit outflows during crises.

Design features of deposit insurance schemes may affect deposit volatility. First, more generous insurance coverage is likely to limit outflows and attract higher inflows of deposits. Acharya and Mora (2015) show that banks do not experience additional deposit inflows in the initial stage of the financial crisis until the government increased the deposit insurance coverage limit from 100,000 USD to 250,000 USD per depositor and introduced the Transaction Account Guarantee Programme (TAGP). Second, ownership and management may also play a role for the credibility of the insurance scheme, and consequently affect deposit flows. Diamond and Dybvig (1983) argue that the government should provide deposit insurance to guarantee that the return will be paid to all depositors. In contrast to private insurers that are constrained by their reserves, governments can impose taxes to honour a deposit guarantee. On the other hand, a privately run deposit insurance scheme may also signal depositors that banks have greater incentives for peer monitoring and therefore take less risk than banks whose deposits are protected by the government (Beck, 2002).

Moreover, deposit flows are likely to respond more strongly to deposit insurance design features in crisis periods. Depositors' incentives to monitor banks and acquire signals about the protection of their claims increase during crises, reflecting more significant concerns about banks' liquidity, solvency, and doubts about the credibility and coverage offered by the deposit insurance scheme (Bonfim and Santos, 2020). In other words, financial crises act like a "wake-up call" that strengthens market discipline (Martinez Peria and Schmukler, 2001; Karas et al., 2013; Bennett et al., 2015). It is therefore plausible to expect depositors to take actions to protect themselves.[20]

Against this background, we argue that the coexistence of banks in Massachusetts that offer different levels of protection for depositors has potential to affect deposit flows. Higher levels of deposit insurance coverage will mitigate potential deposit outflows, and, more importantly, trigger deposit inflows to member banks during the crisis because they offer unlimited insurance

[20] Market discipline by depositors can be expressed in quantities and prices. Prior work suggests that both channels of market discipline exist (Billett et al., 1998; Park and Peristiani, 1998; Jagtiani and Lemieux, 2001; Goldberg and Hudgins, 2002; Maechler and McDill, 2006)

coverage.

Hypothesis 1a: Following the onset of the financial crisis, deposit inflows increase for banks that are members of the MA-DIF relative to banks whose deposits are only protected by the FDIC.

Alternatively, given that the MA-DIF is exclusively privately run and managed, depositors may not find it credible. Consequently, they may not put their deposits into member banks. With private deposit insurance, MA-DIF banks may rely more on federally uninsured deposits during the pre-crisis period. In the crisis, depositors become more risk-averse, they may therefore split their deposits that are privately insured into separate banks to obtain full protection via federal deposit insurance. If it is the case, deposits of MA-DIF banks may even decrease during the crisis.

Hypothesis 1b: Following the onset of the financial crisis, deposit inflows does not increase for banks that are members of the MA-DIF relative to banks whose deposits are only protected by the FDIC.

1.3.2 Lending during the financial crisis

Financial intermediation theory and prior work on bank lending during crises provide the foundation for our next set of hypotheses. Bryant (1980); Diamond and Dybvig (1983) predict banks create liquidity by financing relatively illiquid assets with relatively liquid liabilities, highlighting a key feature of a bank: the combination of deposit-taking and lending (Kashyap et al., 2002).

Provided that there are synergies between deposit-taking and lending, and there is cross-sectional variation in the availability of deposits across banks, it is plausible to expect that deposit flows correlate with lending. Ivashina and Scharfstein (2010) support this view. They show that lending contracts less in banks with better access to deposits in a crisis. If access to private deposit insurance isolates banks from liquidity constraints they may otherwise face, we hypothesise that MA-DIF members are likely to increase lending during the crisis. As privately insured banks expect fewer withdrawals during crises, they have greater incentives to transform liquid deposits into illiquid loans.

Hypothesis 2a: Following the onset of the financial crisis, lending by MA-DIF member banks increases, relative to banks whose deposits are only protected by the FDIC.

On the other hand, lending of member banks may remain unaffected or decrease during the crisis even if member banks enjoy more stable deposit funding. Banks minimize risk by preserving and hoarding liquidity during crises (Bernanke, 1983; Acharya and Skeie, 2011; Acharya and Merrouche, 2013). The additional deposit inflows therefore might increase member banks' holdings of more liquid assets, rather than increase lending. Further, the design features of the MA-DIF (see Section 1.2), are likely to incentivize member banks to be more prudent in originating loans in a crisis. Therefore, synergy effects between deposits and lending may be undermined by members' low risk appetite.

Hypothesis 2b: Following the onset of the financial crisis, lending by MA-DIF member banks does not increase, relative to banks whose deposits are only protected by the FDIC.

1.3.3 Private deposit insurance and moral hazard

Deposit Insurance is considered to increase moral hazard (Keeley, 1990). It undermines depositors' monitoring incentives and increases bank risk-taking. While this concern is supported by theory (e.g. Chan et al. (1992); Boot et al. (1993); Cooper and ROSS1 (2002)), the evidence is mixed. Empirical work suggests that design features of insurance schemes and the institutional environment affect the role of deposit insurance for moral hazard. We therefore discuss the role of design features of the MA-DIF below.

Compared with banks whose deposits are only federally insured, the additional coverage of MA- DIF member banks could undermine the monitoring incentives of depositors. From this perspective, member banks of the MA-DIF could therefore be expected to accumulate more risk in the pre-crisis period which will unfold during the crisis. Furthermore, MA-DIF member banks could use the additional deposit inflows to originate risky loans during the crisis.

Hypothesis 3a: Following the onset of the financial crisis, bank risk increases for banks that are members of the MA-DIF relative to banks whose deposits are only protected by the FDIC.

However, this view ignores one of the important incentive-compatible features of private deposit insurance: the completely private nature of the MA-DIF. Different from government-backed deposit insurance scheme, any costs stemming from paying out deposits of failed member banks will be entirely incurred by the member banks. This creates a strong incentive for peer-monitoring among members (e.g., Calomiris (1990); Beck (2002)).[21] In addition, the small number of MA-DIF members creates a club atmosphere which encourages mutual monitoring (Beck, 2002)). Therefore, we argue that membership in the MA-DIF does not reduce monitoring intensity by depositors, but instead, reallocates monitoring incentives from depositors to other member banks. Moreover, monitoring intensity may increase. While depositors tend to be small and unsophisticated, banks possess superior monitoring technologies (King, 2008). In other words, membership in the MA-DIF shifts monitoring incentives from less to more capable monitors. This reinforces market discipline (Danisewicz et al., 2018a, 2021). It is therefore possible, that

[21] Other incentive-compatible features of MA-DIF are detailed in our appendix , including risk-based premiums and high cost of exit.

MA-DIF banks are even more prudent than non-member banks, suggesting less risk-taking and more prudent loan origination during the financial crisis.

Hypothesis 3b:Following the onset of the financial crisis, bank risk remains constant nor decreases for banks that are members of the MA-DIF relative to banks whose deposits are only protected by the FDIC.

1.4 Data and Methodology

1.4.1 Data

We obtain annual data for 2004-2015 for branches of commercial and savings banks in the U.S. from the SoD, available from the FDIC.[22] We complement the SoD with quarterly data for commercial and savings banks from the Call Reports during Q1:2004-Q4:2015, available from the Federal Reserve Bank of Chicago. We choose this time span because information on MA-DIF membership is available from 2004 annually. Our sample period includes the crisis period.

To minimize geographic heterogeneity, our sample includes branches in Massachusetts and the five surrounding states: Connecticut, New Hampshire, New York, Rhode Island, and Vermont (Figure 1.4). We exclude banks if they have: (i) zero deposits; (ii) zero lending; (iii) balance sheet items with negative values; or (iv) missing data for the control variables.

Following Gatev et al. (2009), we use the most recent merger file from the Federal Reserve Bank of Chicago to identify mergers and acquisitions and drop observations during the year of the M&As. We only include branches of MA-DIF member banks and non-member banks that operate at least one year prior to and following the onset of the crisis. Applying these sample screens results in 69,108 observations for 7,006 branches operated by 365 banks in all 6 states.

To eliminate state-specific effects, most tests on the branch-level are based on branches in Massachusetts, resulting in a cleaner sample of 13,189 observations for 1,361 branches operated by 51 MA-DIF member banks and 52 non-member banks. On the bank-level, we focus on banks headquartered in Massachusetts, resulting in a sample of 3,449 observations for 51 MA-DIF member banks and 32 non-member banks, which account for around 2% of total assets for all U.S. commercial and savings banks. We refine the lending results from the Call Reports using annual mortgage-application-level data collected by the Federal Reserve under the HMDA, which records the year of the mortgage application, lender identity, borrower characteristics, loan amount, and the approval result. To be consistent with the bank-level analysis, we focus on the 83

[22] Deposits in the Summary of Deposits are measured as of 30$^{\text{th}}$ June each year. This helps our empirical setup to coincide with the start of the subprime crisis.

Massachusetts-headquartered banks in the bank-level sample, resulting in a sample of 371,898 mortgage applications.

Panel A of Table 1.1 presents descriptive statistics for the banks that enter our analyses. We summarize annual branch-level deposits for branches in Massachusetts and all other variables for banks headquartered in Massachusetts. We also report summary statistics of our quarterly bank-level variables.

To examine whether banks in Massachusetts are representative of the population of U.S. banks, Panel B of Table 1.1 compares key variables of MA-DIF member banks with other savings banks. This test suggests that MA-DIF member banks are similar to savings banks outside Massachusetts before the crisis, except that MA-DIF member banks have lower average deposit and loan rates.

1.4.2 Methodology

To examine whether MA-DIF member banks obtain additional deposits during the crisis, we estimate the following model on the branch-level:

$$Deposit_{v,i,t} = \beta_0 + \beta_1 Membership_{v,i} \times Crisis_t + \gamma X_{i,t} + \delta_v + \delta_t + \varepsilon_{v,i,t} \tag{1.1}$$

where $Deposit_{v,i,t}$ is the logarithm of deposits for branch v operated by bank i at time t, capturing deposits of each branch; $Membership_{v,i}$ indicates whether a branch is operated by MA-DIF member banks, it equals one if a bank is a member of the MA-DIF (0 otherwise). Since we require banks to be members of the MA-DIF throughout the sample period 2004-2015, $Membership_{v,i}$ is a time-invariant variable. $Crisis_t$ takes on the value of 1 if the observation is in the crisis period (0 otherwise). $Membership_{v,i} \times Crisis_t$ equals 1 for the observations of branches operated by MA-DIF member banks during the crisis period (0 otherwise). We define the crisis period as Q3:2007-Q2:2010.[23] β_1 is our coefficient of interest and informs Hypothesis 1.

$X_{i,t}$ is a vector of time-varying control variables, including a set of bank-level variables, the logarithm of total assets, the ratio of total deposit interest expenses to total deposits, the charge off ratio, the Tier 1 capital ratio, the deposits-to-liabilities ratio, the loans-to-assets ratio, and the mortgages-to-assets ratio. To address concerns that characteristics of depositors differ systematically across branches of member and non-member banks. $X_{i,t}$ also includes a set of county-level

[23] We define the crisis period as Q3:2007-Q2:2010 because data from the SoD are only available as of 30$^{\text{th}}$ June on an annual basis.

variables which measure financial literacy, social capital (to approximate trust), and population characteristics.[24] δ_v is a branch-fixed effect which captures branch-specific factors, and δ_t is a year-fixed effect. This battery of dummy variables allows us to rule out all unobservable and time-varying forces that might drive changes in deposit flows and coincide with the crisis period. We cluster heteroskedasticity-adjusted standard errors on the branch-level to account for serial correlation within each panel. To ensure that our findings are not due to the choice of fixed effects and method of adjusting standard errors, Table A1.2 in the appendix shows the estimation results with a pooled specification (column 1), switching branch-fixed effects to bank-fixed effects (column 2), and including county x year fixed effects (column 3), clustering standard errors on the bank level (column 4) and bootstrapping standard errors based on 600 bootstrap simulations (column 5). In all columns, the findings are similar to our baseline estimates.

On the bank-level, we estimate:

$$Y_{i,t} = \beta_0 + \beta_1 Membership_i \times Crisis_t + \gamma X_{i,t} + \delta_i + \delta_t + \varepsilon_{i,t} \tag{1.2}$$

where $Y_{i,t}$ is a dependent variable for bank i at time t, capturing either bank-level deposits, lending, or risk-taking. The coefficient β_1 informs our three hypotheses. The bank-level data is on a quarterly basis. δ_i controls for bank-fixed effects, and δ_t controls for quarter-fixed effects. We cluster heteroskedasticity-adjusted standard errors on the bank-level in equation 1.2.[25]

In the model for deposit flows, we control for the logarithm of total assets to measure bank size. To account for pricing effects, we use the ratio of total deposit interest expenses to total deposits as the measure of average interest rates on deposits. Furthermore, we use the charge off ratio to measure bank risk, and the Tier 1 capital ratio to measure capitalization. We also use the ratio of deposits-to-liabilities to measure the reliance on deposits. The loans-to-assets ratio and the mortgages-to-assets ratio are used to capture the difference in bank activities.[26] We obtain all control variables from the Call Reports. In our analysis on the branch-level, we collapse quarterly data into annual data. We use the same control variables for testing Hypothesis 2, except for the interest expense ratio, where we replace it with the ratio of total interest income to total loans. The control variables for testing Hypothesis 3 are total assets, the deposits-to-assets

[24] In the absence of specific measures of financial literacy on the county level, we approximate financial literacy by the proportion of individuals with a high school degree or above. We measure social capital using the index developed by Rupasingha et al. (2006). The proportions of individuals with age between 20 and 25 or above 65, of females, and of minorities are used to capture further population differences.

[25] Iyer et al. (2016) show heterogeneous depositor responses to solvency risk, and Iyer and Puri (2012) show that depositors' social networks mitigate bank runs. One of the limitations of our study is the lack of depositor-level data to control for the role of depositors' characteristics.

[26] All bank-level regressions do not include county-level control variables because the locations of bank headquarters could hardly reflect the demographic characteristics of banks' depositors.

ratio, and the loans-to-assets ratio. All control variables are lagged by three years to mitigate concerns about endogeneity.[27]

1.4.3 Selection into membership and sample choice

Our variable of interest, $Membership_i \times Crisis_t$ is plausibly exogenous for two reasons. First, to alleviate selection problems, we only include banks in our tests that are consistently members during the period 2004-2015.[28] This procedure mitigates concerns that banks select into MA-DIF membership by converting the charter to become Massachusetts-chartered savings banks. In our sample, banks acquire membership before the crisis. We exclude banks that join the MA-DIF during the sample period. Therefore, the membership of banks during the crisis is not conditional on deposits and lending in our analysis.[29]

Second, a driving force behind the crisis was a credit boom which fuelled a housing bubble. Potentially, the lending behavior of MA-DIF member banks could contribute to the build-up of the crisis. However, Acharya and Richardson (2009) suggest that the crisis is primarily driven by a shift of banks' business models towards securitization adopted by large, complex financial institutions. MA-DIF member banks are local savings banks, and none of them has assets over 50 billion USD during the sample period. These banks, at best, played a very limited role in triggering the financial crisis. Therefore, $Crisis_t$ is plausibly exogenous to their deposits and lending. Likewise, the "too-big-to-fail" explanation could hardly be invoked to explain deposit inflows to member banks.

1.4.4 Do non-member banks constitute a valid counterfactual?

The validity of our estimation requires non-member banks to constitute a valid counterfactual for the MA-DIF banks. If this is the case, our dependent variables of the member banks would have evolved in a similar fashion to non-member banks during the pre-crisis period.

This section shows that non-members are a valid counterfactual. Most of our tests are based on branches in Massachusetts and banks headquartered in Massachusetts. Panel A and B in Table 1.2 examines differences in the annual growth rate of branch-level deposits and in the

[27] Since the crisis period covers three years, a shorter lag may cause correlation between the variable of interest and the control variables, i.e., we would suffer from a "bad control" problem (see Angrist and Pischke (2008)).

[28] Considering the time-invariant MA-DIF membership status imposed on our data, using a Heckman (1979) model is inappropriate. Given the structure of our data, the Heckman (1979) model would yield a time-invariant inverse Mills (1926) ratio which would be perfectly collinear with the branch- or bank-fixed effects in our tests.

[29] In the appendix, Table A1.3, we show that our result is robust to the inclusion of Massachusetts savings banks that become MA-DIF members during the sample period. The results in the sample of column 1-3 also suggest that non-member banks switching to member banks receive additional deposits, even in non-crisis period.

quarterly growth rate of other dependent variables between the MA-DIF member banks and non-member banks during the pre-crisis period. The null of the equality of means cannot be rejected in any but 2 out of 44 cells, suggesting non-members plausibly constitute a valid counterfactual.

To highlight that our results are not driven by the non-parallel trend of balance sheet compositions prior to the crisis, we also compare the growth rate of deposits-to-liabilities ratio, mortgages-to-assets ratio, loans-to-assets ratio and Tier 1 capital ratio between MA-DIF member banks and non-member banks. The results in panel C suggest that the portfolio compositions of MA-DIF member banks and non-member banks evolve in similar fashion before the crisis.

1.5 Results: Private deposit insurance and deposit flows

We now examine the effect of the MA-DIF on deposit flows during the crisis. Further tests disentangle the impact of membership on deposit flows from alternative explanations.

1.5.1 Effect of the MA-DIF on deposits on the branch-level

Table 1.3 presents the results for deposit flows on the branch-level. Column 1 and Column 2 in Panel A show the results for the full sample, including all branches operating in Connecticut, Massachusetts, New Hampshire, New York, Rhode Island, and Vermont. The estimates for our coefficient of interest, β_1, are significant and positive. Column 1 only includes the interaction term between the dummy identifying MA-DIF member banks and the dummy for the crisis without any control variables. There is a significant increase in deposits of 1.8% (t-statistic of 2.01) for member banks during the crisis, supporting Hypothesis 1a. Column 2 includes control variables and confirms the significant deposit increase for member banks.

We expect the differential evolution of deposit flows to be more pronounced when we only consider branches of member and non-member banks located in Massachusetts. Depositors incur lower cost to transfer deposits within Massachusetts in terms of transportation, monitoring, and information cost.

Column 3 shows the results for the sample including all branches in Massachusetts, regardless of their headquarters' location. Deposits of members increase by 7.7% (t-statistic of 6.58). Compared with the results in Column 1 and 2, the magnitude of the increase in deposits is greater.

Column 4 only includes Massachusetts branches of banks headquartered in Massachusetts, while the control group in Column 5 only includes Massachusetts branches operated by non-member banks headquartered outside Massachusetts. Since depositors incur lower information cost and monitoring cost for banks headquartered in their state of residence, we expect that the coefficient of interest to be lower when we only include Massachusetts branches operated by Massachusetts banks in the sample. In other words, we expect depositors to transfer deposits from non-member banks to member banks, primarily from non-Massachusetts banks to member banks. Column 4 indicates an increase in deposits for member banks. Size and significance of the estimated is lower compared with Column 5, which shows that deposits of members increase by 8.2% (*t*-statistic of 6.85).

To alleviate concerns about systematic differences in the treatment and control group, we show an additional test using a matched sample, following Lemmon and Roberts (2010). We use nearest neighbor matching with replacement based on the locations (counties) of branches, pre-crisis averages of the growth rate of total deposits, average interest rates on deposits, total assets, the charge off ratio, and the Tier 1 capital ratio.[30] We then replicate the estimation with the matched sample in Column 6. The coefficient and the *t*-statistic using the matching strategy remain similar to the ones in the unmatched sample in Column 2. Our findings do not seem to be driven by differences between MA-DIF member banks and non-member banks in terms of branch location, bank size, soundness, credit risk, and deposit interest rates.[31]

Panel B adopts a narrower definition of the crisis period, classifying the crisis to occur from Q3:2007 to Q2:2008. We expect deposit growth of member banks to be higher prior to the increase of the FDIC deposit insurance coverage limit from 100,000 USD to 250,000 USD per depositor on 3$^{\text{rd}}$ October 2008 and introduction of the Transaction Account Guarantee Program on 14$^{\text{th}}$ October 2008. We expect the estimates for our coefficient of interest, to be larger in Panel B. If it is the case, it not only highlights the robustness of our result in terms of the definition of the crisis period, but rather, it reinforces the idea that unlimited insurance coverage of member banks explains the deposit inflows.

The results in all columns of Panel B support this view. The estimates for increase in magnitude across the samples. The significance level of the coefficient in Panel B of Column 4 (*t*-statistic of 3.76) is also greater than in Panel A of Column 4. In sum, there is strong evidence that MA-DIF membership is associated with deposit inflows during the crisis. Our results are robust to the definition of the crisis period. Deposit inflows are more significant prior to the expansion of the

[30] The matching procedure starts with regressing a binary variable indicating MA-DIF membership on the pre-crisis averages of those matching variables. Next, we compute propensity scores using predicted probabilities from this estimation and perform a 1:4 nearest neighbor matching with replacement. Changing the number of matches to any number between 1 and 4 has no effect on the results.

[31] To further alleviate the concern that our results are driven by geographical factors, we replicate our baseline results with county x year fixed effects which captures time-varying geographical effects. The results are shown in Column 3 of Table A1.2 in the appendix . The estimation results are similar to our baseline results.

government guarantees.[32]

We next trace out the dynamic effect of MA-DIF membership on deposits throughout the sample period by including a series of year dummy variables in the baseline regression:

$$\begin{aligned} Y_{i,t} = \beta_0 + \beta_1 Membership_i \times 2004_t + \beta_2 Membership_i \times 2005_t + ... \\ \beta_{10} Membership_i \times 2013_t + \beta_{11} Membership_i \times 2014_t + \gamma X_{i,t} + \delta_i + \delta_t + \varepsilon_{i,t} \end{aligned} \tag{1.3}$$

where the respective year dummy variable ($2004_t - 2014_t$) =1 if the observation is in the respective year, 0 otherwise. Definitions of other variables follow equation 1.1. Figure 1.5 plots the estimated coefficients and the 95% confidence intervals of our coefficients of interest, ($\beta_1 - \beta_{11}$), which are adjusted for branch-level clustering.

Figure 1.5 illustrates that MA-DIF member banks do not receive additional deposits before the crisis, providing additional evidence in supporting the parallel trend assumption. Starting from year 2007, the effect of MA-DIF membership on deposits become positive and statistically significant at 5% level. The positive effect gradually diminishes until year 2010 and the effect become statistically insignificant in year 2011-2012. Since year 2013, the figure shows that MA-DIF membership has negative effect on deposits. The pattern shown in Figure 1.5 is consistent with our expectation that depositors value the extra protection of MA-DIF banks the most at the onset of the crisis and gradually diminish when the FDIC deposit insurance coverage being increased and the crisis gradually settles down. After the crisis, other features of banks, for example the quality of customer service, plausibly outweigh the additional protection of MA-DIF banks, explaining the reversal of deposit growth after the crisis.

1.5.2 Ruling out alternative explanations

MA-DIF member banks experience deposit growth during the crisis. To establish a causal effect arising from the additional layer of protection, we need to identify whether the deposit growth is driven by deposits uninsured by the FDIC and rule out alternative explanations.

One potential alternative mechanism driving our results may be a pricing channel. Member banks may offer higher interest rates than other banks. Another plausible explanation may be that members are considered safer than other banks.

[32] The results based on the volume of branch-level deposits are further reinforced when using branch-level market shares, and also when we use branch-level deposits scaled by total assets, see appendix Table A1.4.

This subsection addresses these concerns. Considering that transportation cost and information asymmetries rise in distance (Degryse and Ongena, 2005), the costs of transferring deposits across branches in the same state are lower than across state borders. Therefore, we argue that the transfer of deposits is more likely to occur between MA-DIF member banks and non-members in Massachusetts. All tests in this subsection focus on branches in Massachusetts. First, we explore which types of deposits are most affected by the additional layer of protection. It is plausible to expect that uninsured deposits are reallocated towards MA-DIF members during a crisis. Acharya and Mora (2015) report that uninsured deposits account for 62% of total deposits in the U.S. banking system at the onset of the crisis. In our sample with all banks headquartered in Massachusetts, uninsured deposits account for 42% of deposit liabilities in Q2:2007. Assuming depositors transfer funds to MA-DIF member banks to obtain unlimited insurance coverage, non-member banks with higher levels of uninsured deposits should experience more significant deposit outflows, and vice versa for the non-member banks with lower uninsured deposits.

To test this idea, we include in Column 1 of Table 1.4 Massachusetts branches operated by non-member banks where the volume of uninsured deposits is below or equal to the median of non-member banks, and Column 2 includes branches in Massachusetts of non-member banks where the volume of uninsured deposits is above the median as the control group.

The results support our predictions. The coefficient for β_1 in Column 2 is significantly larger, compared with Column 1. Our tests following Paternoster et al. (1998) reject the equality of the coefficient of interest between the two samples.These findings suggest that depositors transfer deposits to member banks because of their unlimited insurance coverage.

The next test in Table 1.4 examine if the findings are simply driven by the profitability of banks. The test also provides some insights into whether depositors differentiate between high and low-profit banks during the crisis to examine whether this affects the deposit flows. Column 3 and 4 split the control group at the median of return on equity. The key coefficient remains significant in both columns. However, its magnitude is nearly twice as large for banks whose return on equity is below the median in comparison to the group of banks with greater profitability. This result is consistent with the view that depositors monitor banks and are more cautious during the crisis when their deposits are held in less profitable banks.

The next test splits the control group according to the pre-crisis value of the average interest expense ratio to address concerns regarding the pricing channel in Column 5 and 6 of Table 1.4. There is no support for the view that deposit inflows into member banks are purely driven by higher deposit interest rates. There is still an increase in deposits of member banks, even compared with non-member banks paying higher deposit interest rates.[33]

Our next set of tests splits the control group by the pre-crisis average values of the charge off ratio and the Z-score to examine the role of bank soundness.[34] Although it is plausible to expect that soundness plays a role for the magnitude of deposit flows, the positive effect of MA-DIF membership on deposit inflows should be robust to splitting the control group by measures of soundness.

The results in Column 7 to 10 of Table 1.4 show no evidence that deposit inflows into members are exclusively due to concerns about bank soundness. Even when we compare the treatment group with non-member banks with lower charge off ratios and higher Z-scores, deposits of member banks, relative to the control group, significantly increase during the crisis.

Signalling effects arising from participation in the Capital Purchase Programme (CPP) of the TARP may also interfere with the causal effect from MA-DIF membership. During the crisis, the U.S. treasury infused capital into numerous banks through the CPP, aiming to restore stability and facilitate credit availability. Berger et al. (2020) find that the CPP reduce its recipients' demand for deposits, Berger and Roman (2015) point out that recipients of the CPP are considered to be safer in comparison to non-recipients of the CPP, and Bayazitova and Shivdasani (2012) find that recipients of the CPP experience excess stock returns on the announcement date of the capital infusions. It is therefore also possible that participation in the TARP affects deposit flows between the treatment and control group.

Our tests compare member banks with CPP recipients in Column 1 and with non-CPP recipients in Column 2 of Table 1.5. A further benefit of these tests is to show that deposits of member banks increase regardless of whether the banks received CPP funds or not. Column 1 shows that deposits of member banks increase by 3.5% (t-statistics 2.44), compared with CPP recipients, and Column 2 indicates deposits of members increase by 4.9% (t-statistics 4.11), compared with non-recipients. The tests for differences of the coefficients are insignificant at 10% level.

The next test addresses the concern that deposit inflows are mechanically driven by banks

[33] In the appendix , Table A1.5, we investigate whether average deposit interest rates are affected by membership in the MA-DIF during the crisis. This is not the case. Figure A1.2 shows additional visual evidence that average deposit interest rates of MA-DIF member banks and other banks move in tandem during the crisis period and the magnitude of the difference during the crisis is economically negligible (around 2-5 basis points). Therefore, the additional deposit inflows of MA-DIF member banks cannot be explained by the relative increases of deposit interest rates of MA-DIF member banks during the crisis.

[34] The Z-score measures the distance to default, defined as the sum of return on assets (ROA) and the equity-to-asset ratio divided by the standard deviation of ROA (over a three-year rolling time window). Since the Z-score is not normally distributed, we use a log transformation of the Z-score, which is defined as Z-score (ln). The Z-score (ln) is negatively related to the probability of default.

in the control group being subject to supervisory enforcement actions.[35] Such actions identify banks that concern supervisors and require immediate remedial action to avoid further sanctions. Enforcement actions may also restrict sanctioned banks' deposit-taking activities. Since these actions are public and convey negative signals about soundness (Jordan et al., 2000; Delis et al., 2017; Danisewicz et al., 2018b), we expect the estimate for β_1 be higher (lower) when the control group only contains banks that are subject to enforcement actions (not subject to enforcement actions).

Among the 52 non-member banks in our sample, 19 are subject to enforcement actions. Two of the MA-DIF-insured banks are subject to enforcement actions during the crisis. Table 1.5 shows the results of splitting the control groups by enforcement actions. The control group in Column 3 includes branches operated by non-members of the MA-DIF that are subject to enforcement actions in Massachusetts, while the control group in Column 4 includes branches in Massachusetts operated by non-members that are not subject to enforcement actions.

Column 3 shows that deposits in branches operated by MA-DIF-insured banks increase by 9.2% (t-statistic 7.09), compared with non-member banks that are subject to enforcement actions. The estimated β_1 decreases in Column 4, which only includes non-member banks that are not subject to enforcement actions as a control group. Importantly, however, the effect is still statistically and economically significant. Deposits of branches operated by MA-DIF-insured banks increase by 4% (t-statistic 2.29), shown in Column 4.[36]

The next test in this subsection splits the data into banks with high and low levels of opacity because Chen et al. (2020) argue that depositors prefer opaque banks. They measure bank opacity based on depositors' ability to acquire information about the future performance of bank assets.[37] We follow Chen et al. (2020) in constructing the opacity measure. We classify banks whose opacity measure is equal to or below the median as opaque banks, and those whose opacity measure is above the median as transparent banks. While the key coefficient in Column 5 and 6 remains significant across the two subsamples, the results are consistent with Chen et al. (2020), suggesting that the increase of deposits of MA-DIF banks during the crisis is stronger comparing with less opaque banks. However, the difference in coefficient is marginally insignificant at 10% level.

[35] Enforcement actions are formal written agreements, cease and desist orders, prompt corrective action directive, deposit insurance threats, actions against personnel and individuals, formal memorandum of understanding, hearing notices, and sanctions due to HMDA violations.

[36] Our findings persist in unreported tests that differentiate further between severe and less severe enforcement actions. These regressions are available upon request.

[37] The measure of opacity follows Chen et al. (2020). It reflects whether uncertainty about future credit losses can be resolved based on depositors' information.

1.5.3 Effect of MA-DIF on deposit flows on the bank-level

We now check the robustness of our results on the bank-level. A benefit of aggregating the data to the bank-level is that it allows identifying the types of deposits that are flowing to MA-DIF member banks.

These tests focus on banks headquartered in Massachusetts for the reasons outlined in Sections 5.1 and 5.2. Additionally, limiting our sample to banks headquartered and operating only in Massachusetts avoids the influence of unobservable time-varying state effects.

Table 1.6 documents an increase in deposits of MA-DIF member banks on the bank-level. Column 1 shows a significant increase in deposits of 7.9% (t-statistic of 2.01) for member banks during the crisis. This result is robust to the inclusion of control variables, shown in Column 2.[38]

After the introduction of the TAGP in Q3:2008, all noninterest-bearing transaction deposits, low-interest NOW accounts, and IOLTAs are fully insured for participating banks until Q4:2010. After expiration of the TAGP, the Dodd-Frank Act (DFA) provided all insured depository institutions unlimited deposit insurance coverage on noninterest-bearing transaction accounts and IOLTAs, but not on low-interest NOW accounts during Q1:2011 and Q4:2012.[39]

Martin et al. (2018) show that these temporary deposit insurance measures reduce the outflow of deposits. Since noninterest-bearing deposits are already insured by the FDIC since Q3:2008, deposit inflows to MA-DIF member banks during the crisis should mainly consist of interest-bearing deposits. Our next set of tests confirms this is the case.

Call Reports do not record NOW accounts and IOLTAs independently, and they also do not separately report noninterest-bearing transaction deposits until Q1:2014. We therefore rely on noninterest-bearing deposits as a proxy of deposits insured by the TAGP.

The results in Column 3 and 4 of Table 1.6 are consistent with our expectations. Column 3 shows that the deposit inflows are attributable to interest-bearing deposits, the type of deposits that have not been fully insured during the crisis. The magnitudes and t-statistic of the coefficient of interest in column 3 are similar to the results in Column 2. Column 4 shows that the increase in non-interest-bearing deposits of MA-DIF member banks is statistically insignificant at 10% level, suggesting that noninterest-bearing deposits play a limited role in the increase of member banks' deposits during the crisis.

[38] We obtain virtually identical results when we redefine the crisis period for the tests on the bank-level to Q3:2007-Q4:2009 to reflect on the quarterly frequency of the data.

[39] Details of the TAGP and DFA guarantees are provided at *https://www.fdic.gov/regulations/resources/tlgp/*.

Using bank-level data also allows addressing a further concern. The potential use of brokered deposits by MA-DIF member banks' may interfere with our results. Members could exploit the unlimited insurance coverage to tap into the brokered deposit market to boost deposit funding. However, this explanation is unlikely to apply in our setting.[40] Our tests in Columns 5 and 6 of Table 1.6 show that MA-DIF membership even decreases the volume of brokered deposits, and has no effect on the proportion of brokered deposits to total deposits during the crisis. There is no evidence that suggests the increase in deposits of MA-DIF member banks during the crisis is driven by inflows of brokered deposits.[41]

Examining FDIC-uninsured deposits during the crisis is complicated by a delay in the updating Call Reports data. While the FDIC increased its coverage limit from 100,000 to 250,000 USD on 3$^{\text{rd}}$ October 2008, the relevant components in the Call Reports were not updated to reflect the increase until Q3:2009, potentially introducing measurement errors.

To mitigate this concern, we refine the sample to Q1:2004-Q2:2008 to examine the volume and the number of accounts of FDIC-uninsured and FDIC-insured deposits before the increase of the FDIC coverage limit. The results are shown in Table A1.6 of our appendix . The results in Column 1 indicate an increase in the volume of FDIC-uninsured deposits of MA-DIF member banks during the crisis, while Column 2 shows no evidence of an increase in FDIC-insured deposits of MA-DIF member banks during the crisis. The findings are similar for the number of accounts, shown in Column 3 and Column 4.[42] There is only an increase in the number of FDIC-uninsured deposit accounts of MA-DIF member banks during the crisis, but not in the number of FDIC-insured deposit accounts. The results further support the view that the increase in deposits of MA-DIF member banks during the crisis is driven the addition layer of protection by the MA-DIF.

1.5.4 Falsification tests

This section presents falsification tests using branch-level data. For the first test, we use equation 1.1 but exclude MA-DIF member banks. We investigate if randomly assigning non-member banks

[40] Some observations for MA-DIF member banks and non-member banks have zero brokered deposits. We transform the variable into (1+ brokered deposits) before taking the logarithm to avoid losing these observations.

[41] Reciprocal deposits may also interfere with our findings. Such deposits are received by a bank which places an equal amount of deposits at another banks with the aim to reallocate deposits that exceed the FDIC coverage limit. Prior to June 2009, brokered deposits reported in Call Reports include these reciprocal deposits. Therefore, our results in Columns 5 and 6 of Table 1.6 mitigate concerns that the increase in total deposits of MA-DIF banks during the crisis is driven by reciprocal deposits.

[42] The number of deposit accounts that are below the FDIC coverage limit is only reported once annually in the June Call Report, leading to the drop in the number of observations in Column 4.

a placebo membership in the MA-DIF by setting the variable "Membership" equal to 1 for placebo members (and equal to 0 for non-member banks) triggers significant effects for deposit flows.

We run Monte Carlo simulations, i.e., we estimate the regression and save the t-statistic on the coefficient of interest and repeat it 1,000 times to compute rejection rates of the null hypothesis =0 at the 1%, 5%, and 10% level. We also report the mean coefficient and the average standard error for the estimated β_1 Because we know that the placebo membership should play no role, the null of zero effect on deposit flows is true, and we should only reject the null by making Type I errors.

The rejection rates in Panel A of Table 1.7 are low, and the average value of the coefficient on the interaction terms is 0. In short, the effect on deposit flows only arises in banks that are members of the MA-DIF while no such effect is observable in banks with a placebo membership.

Since all MA-DIF banks are state-chartered savings banks, an alternative explanation for our findings could be that depositors prefer state-charted savings banks during the crisis, irrespective of being privately insured or not.[43] The final falsification test therefore examines whether state-chartered savings banks in the five neighbouring states of Massachusetts experience the same effect as MA-DIF banks during the crisis. Panel B shows regressions that replicate our tests from Panel A in Table 1.3 with five separate samples of branches in Connecticut (CT), New Hampshire (NH), New York (NY), Rhode Island (RI), and Vermont (VT) respectively, but branches of MA-DIF banks operated in these states are excluded. The key coefficient is the interaction term of the dummy variable indicating whether a bank is a state-chartered bank and the dummy variable for the crisis period, Q3:2007-Q2:2010.

This test soundly rejects the alternative explanation. The key coefficients in Columns 1 to 5 remain insignificant, suggesting that state-charted savings banks in other five states do not experience the same effect as MA-DIF banks do during the crisis. Consistent with these findings, the pattern in Figure 1.1 above cannot be observed in Figure A1.3 of our appendix which shows that total deposits of state-chartered savings banks in the other five states surrounding Massachusetts during the crisis do not increase relatively to their peers headquartered in the same states.

[43] This preference could potentially be explained by the fundamental difference between commercial banks and savings bank in terms of asset size, asset distributions, and regulatory authorities.

1.6 Results: Effect of the MA-DIF on bank lending and moral hazard

Our tests so far examine deposit inflows. A natural question that arises is how do these inflows affect bank lending and moral hazard?

1.6.1 Private deposit insurance and total loans and loan categories

Column 1 in Table 1.8 presents the results of regressions with the logarithm of total loans as the dependent variable. Our estimates show that MA-DIF member banks originate 7.0% (t-statistic 2.36) more loans during the crisis compared to non-MA-DIF banks. This pattern is consistent with observations by Ivashina and Scharfstein (2010) who find that banks funded by more deposits reduce their lending less during the recent crisis than institutions relying less on deposit funding.[44]

Columns 2-5 of Table 1.8 investigates loan categories. We classify loans in terms of four major categories: residential mortgages, construction loans, commercial and industrial loans, and individual loans. These categories account for 86% of total lending of our sample banks. Column 2 shows that MA-DIF member banks increase residential mortgage lending by 5.9% (t-statistic 2.18) during the crisis. However, there is no evidence that MA-DIF members increase other types of loans.

1.6.2 Private deposit insurance and mortgage origination

We further investigate the role of MA-DIF membership for mortgage origination using loan-application-level data collected under the HMDA.[45] Loan-application-level data of HMDA record the year of the loan application, lender identity, borrower characteristics, loan amount, and the approval result.[46] Using this data allows controlling for demand for these loans and applicants' credit risk. We also control for demographic characteristics of loan applicants, including sex, race, and ethnicity. We further control for economic conditions of property location through year-varying county-fixed effects. We combine the HMDA data with bank-level data to control for size, the Tier 1 capital ratio, the charge off ratio, the deposits-to-liabilities ratio, the loans-to-assets

[44] This finding also reflects predictions by Hakenes and Schliephake (2019) who posit that banks with a higher deposit base enjoy more stable funding and are less vulnerable to runs which increases long-term investments.

[45] The HMDA, enacted by Congress in 1975, aims to (i) determine whether financial institutions are serving the housing needs of their communities; (ii) assist public officials in distributing public-sector investments; and (iii) identify possible discriminatory lending patterns.

[46] HMDA only reports the rate spread for loans with spreads above designated thresholds. Therefore, rate spreads are reported for some, but not all mortgages. This feature in the data hinders further analyses regarding the effect of MA-DIF membership on the mortgage spread rate.

ratio, and the mortgages-to-assets ratio.

We report different specifications for this test. Each specification is estimated for a sample of all mortgages, and a sample that excludes mortgages for refinancing because these mortgages are more likely to be securitized (Gilje et al., 2016). The decline in the origination rate of mortgages for non-members of the MA-DIF may be due to their greater exposure to the market for securitized mortgages, and ultimately reflect the collapsing of the securitization activities during the crisis. Excluding refinancing mortgages helps alleviate concerns that an alternative explanation may be at play.

The results in Panel A of Column 1 in Table 1.9, estimated with a linear probability model, show that mortgage applications to MA-DIF member banks are more likely to be approved during the crisis. Our findings are also robust to the exclusion of refinancing mortgages, shown in Panel A of Column 2 and alleviate concerns about the alternative explanation. The coefficient of interest is significantly larger after we exclude refinancing mortgages. Panel A of Column 3 shows that MA-DIF membership does not affect refinancing mortgages.[47]

To further eliminate the role of securitization in our analyses, Panel B of Table 1.9 uses bank-level data to show that MA-DIF member banks neither increase nor decrease the volume of securitized residential mortgages. The proportion of securitized residential mortgages to total residential mortgages during the crisis period also remains unaffected by MA-DIF membership.[48]

1.6.3 Private deposit insurance and moral hazard

Prior work (e.g., Keeley (1990); Demirgüç-Kunt and Detragiache (2002) argues that deposit insurance increases moral hazard. The results in Section 1.6.2 document that MA-DIF institutions do not securitize more mortgages. This suggests that these banks are unlikely to originate more risky loans and therefore membership in MA-DIF does not increase moral hazard. In this subsection we consider several additional measures to further investigate the effect of private

[47] An alternative explanation for the increase in mortgage origination among MA-DIF members could be that the Federal Reserve purchased agency mortgage-backed securities (MBS) from those banks to conduct quantitative easing. Chakraborty et al. (2020) show that banks benefiting from MBS purchases increase mortgage origination, compared with other banks. However, this explanation is unlikely to apply in our setting because MA-DIF banks hold considerably smaller volumes of MBS than non-member banks. Non-member banks should therefore benefit relatively more from the MBS purchase program than MA-DIF banks. The average amount of MBS during the onset of the crisis (Q3:2007) and the announcement of the MBS Purchase Program (Q4:2008) in MA-DIF banks is 80 million USD, while the average amount of MBS in the non-member banks is 164 million USD.

[48] Some observations for MA-DIF member banks and non-member banks have zero securitized residential mortgage. We transform the variable into (1+ securitized residential mortgage) before taking the logarithm to avoid losing these observations.

deposit insurance on bank risk taking during a crisis.

First, we look at changes in three measures of bank soundness: the Tier 1 capital ratio, the Charge off ratio, and the Z-Score. Column 1 to 3 of Table 1.10 illustrate that membership in the MA-DIF does not increase banks risk-taking. All regressions include the lags of total assets, the deposits-to-assets ratio, the loans-to-assets ratio, and bank and year-fixed effects.

Second, we replace our dependent variable with the ratio of nonperforming mortgages measured at *t+4*, *t+8*, and *t+12* quarters to total mortgages at t_0. These tests, shown in Column 4 to 6 of Table 1.10, do not suggest any increase in risk-taking.

Third, we examine changes in the loan-to-income ratio, our final proxy for borrower risk (e.g., Dagher and Sun (2016)). We present results obtained for all mortgages and also separately for retained and sold mortgages. All tests control for bank and loan characteristics and include bank-fixed effects and an interaction of county-fixed effects with year-fixed effects.

Column 1 of Table 1.11 highlights that MA-DIF member banks originate loans with significantly lower loan-to-income ratios than non-members. The key coefficient suggests a 5.9% reduction in the loan-to-income ratio between (t-statistic -6.93). MA-DIF institutions are more conservative when we focus on retained mortgages in Column 2. The loan-to-income ratio of retained mortgages in member banks declines by 14.5% (t-statistic -10.01) relative to the control group. Column 3 shows that MA-DIF member banks reduce the loan-to-income ratio of sold mortgages by 1.3% (t-statistic -1.43), compared to non-member institutions.

1.7 Conclusion

We use the recent financial crisis to study the role of private deposit insurance for deposit flows, bank lending, and moral hazard. We do so by exploiting a hitherto undocumented setting, the existence of a private deposit insurance scheme that protects deposits above the FDIC insurance coverage limit in state-chartered savings banks in Massachusetts. The unique characteristics of our setup allow us to exploit within-state variation (and variation across neighbouring states) over time to compare the evolution of deposit flows, lending, mortgage origination, and bank risk-taking between banks that are members of the private insurance scheme, and banks whose deposits are only protected by the FDIC.

Our results highlight that depositors perceive the private insurance scheme as a credible additional layer of protection for their wealth during the crisis. This allows member banks to enjoy more stable deposit funding. We also show that the privately insured banks increase lending during the financial crisis relative to non-member banks. This finding is driven by residential mortgages, suggesting that the availability of stable sources of funding allows banks to commit to lending. Unlike many previous papers that focus predominantly on government-backed deposit insurance, our final set of results documents that membership in a private deposit insurance fund does not increase moral hazard during the recent financial crisis. Most importantly, we show that loan underwriting standards, approximated by loan-to-income ratios, tighten during the crisis for banks whose deposits are privately insured.

To conclude, this research is timely and important for at least two reasons. First, this work illuminates the current debate in Europe, where policy initiatives are under way to establish the third pillar of the European Banking Union, the European Deposit Insurance Scheme. Our results suggest that depositors can exploit differences in deposit insurance coverage. This carries the risk that countries with lower deposit insurance coverage may experience deposit outflows during crises. Therefore, harmonizing deposit insurance schemes under a European Deposit Insurance Scheme has potential to mitigate potentially destabilizing deposit outflows. Second, our findings also suggest that banks that have better access to deposits are less vulnerable to short-term funding shocks which mitigates adverse effects on their lending activities. These results highlight the synergies between deposits and lending.

While our research illustrates possible benefits of private deposit insurance for depositors, banks, and borrowers, and the findings provide valuable insights into the design features of a deposit insurance scheme that assigns a key role to private parties, we emphasise that these findings do not suggest that private deposit insurance can replace government-sponsored deposit insurance. We temper our summary by highlighting that the credibility of a private deposit insurance scheme does not only depend on its characteristics, but also on the institutional environment of a country.

1.8 Tables and figures

Table 1.1: Summary statistics

Panel A: Summary statistics for banks in Massachusetts					
Dependent variable	N	Mean	Std. Dev.	p5	p95
Branch-level deposits	13,189	144,268	1,801,583	10,141	212,853
Bank-level deposits	3,449	894,831	3,066,610	91,949	2,343,364
Interest-bearing deposits	3,449	682,588	1,528,540	71,148	2,022,918
Non-interest-bearing deposits	3,449	212,243	2,079,951	7,166	427,704
Brokered deposits	3,449	12,463	62,611	0	60,521
Proportion of brokered deposits (%)	3,449	1.514	3.587	0	10.051
Total loans	3,449	667,109	1,652,194	74,926	1,943,551
Residential mortgages	3,449	336,703	643,018	30,017	993,067
Construction and land development loans	3,449	31,511	48,767	725	124,743
Commercial and industrial loans	3,449	66,163	327,723	1,053	233,254
Individual loans	3,449	39,376	380,990	378	87,170
Independent variables					
Total assets	3,449	1,386,913	7,898,941	114,507	3,027,300
Z-score (ln)	3,449	4.935	1.121	2.791	6.624
Tier 1 capital ratio (%)	3,449	9.864	2.701	6.500	14.522
Charge-off ratio (%)	3,449	0.035	0.086	0.000	0.159
Interest expense ratio (%) – deposits	3,449	0.499	0.242	0.157	0.924
Interest income ratio (%) – total loans	3,449	1.488	0.229	1.145	1.880
Deposits-to-liabilities ratio (%)	3,449	87.756	9.550	69.801	99.137
Loans-to-assets ratio (%)	3,449	66.932	12.312	43.309	83.351
Mortgages-to-assets ratio (%)	3,449	40.364	13.902	14.063	60.368

Panel B: Comparisons between MA-DIF member banks and other savings banks in the U.S.			
Dependent variables	Non-member	Member	Difference
Bank-level deposits	581,919	514,239	67,681
Interest-bearing deposits	540,425	475,211	65,214
Non-interest-bearing deposits	41,495	39,028	2,467
Brokered deposits	17,042	4,391	11,686
Proportion of brokered deposits (%)	2.568	1.559	1.009
Total loans	529,176	436,820	92,356
Residential mortgages	315,230	280,427	34,803
Construction loans	80,041	59,883	20,158
Commercial and industrial loans	46,125	25,252	20,873
Individual loans	39,705	12,468	27,236
Total assets	827,285	670,708	156,577
Independent variables			
Z-score (ln)	4.078	4.201	-0.123
Tier 1 capital ratio (%)	12.081	10.758	1.323
Charge off ratio (%)	0.030	0.012	0.018
Interest expense ratio (%) – deposits	0.896	0.809	0.087***
Interest income ratio (%) - total loans	1.699	1.560	0.138***
Deposits-to-liabilities ratio (%)	87.910	86.271	1.640
Loans-to-assets ratio (%)	66.624	66.784	-0.160
Mortgages-to-assets ratio (%)	42.949	44.919	-1.970

Notes: We present summary statistics of branch-level deposits using a sample covering branches operating in Massachusetts between 2004-2015 and summary statistics for bank-level variables using a sample covering banks headquartered in Massachusetts between 2004-2015 in Panel A. Panel B compares the mean values of different variables of MA-DIF member banks and other savings banks in the U.S. in Q2:2007. All numbers are expressed in thousand dollars, apart from the proportion of brokered deposits, the Z-score, the interest expense ratio, the interest income ratio, the charge off ratio, and the Tier 1 capital ratio. All variables are winsorized at the 1 % level and 99 % level. *** p<0.01, ** p<0.05, * p<0.1.

Table 1.2: Differences in growth rates of dependent variables between MA-DIF member banks and non-member banks

	1	2	3	4
Panel A: Differences in annual growth rates of branch-level deposits				
Time	2004	2005	2006	2007
Variable	Difference	Difference	Difference	Difference
Branch-level deposits (ln)	-0.000	0.001	0.001	0.002
	(-0.16)	(0.60)	(0.83)	(1.41)
Panel B: Differences in quarterly growth rates of bank-level dependent variables				
Time	Q3:06	Q4:06	Q1:07	Q2:07
Variable	Difference	Difference	Difference	Difference
Bank-level deposits (ln)	0.002*	0.001	0.001	0.002
	(1.74)	(0.77)	(1.48)	(1.40)
Interest-bearing deposits (ln)	0.002**	0.000	0.002	0.003
	(2.04)	(0.26)	(0.85)	(1.24)
Non-interest-bearing deposits (ln)	-0.000	0.004	0.003	-0.004
	(-0.16)	(1.03)	(0.89)	(-1.22)
Brokered deposits (ln)	0.076	0.009	-0.033	0.025
	(1.54)	(0.10)	(-0.74)	(0.38)
Proportion of brokered deposits (%)	0.061	0.068	0.134	0.089
	(0.46)	(0.33)	(0.20)	(0.75)
Total loans (ln)	0.001	0.002	0.003	0.002
	(0.62)	(1.63)	(1.11)	(1.40)
Residential mortgages (ln)	0.001	0.003	0.005	0.002
	(0.58)	(0.94)	(1.07)	(1.11)
Construction loans (ln)	0.006	0.012	-0.000	-0.001
	(0.42)	(1.47)	(-0.02)	(-0.07)
Commercial and industrial loans (ln)	0.002	0.002	-0.006	0.005
	(0.68)	(0.18)	(-1.48)	(0.85)
Individual loans (ln)	-0.007	0.002	0.003	-0.001
	(-0.66)	(0.16)	(0.19)	(-0.12)
Panel C: Differences in evolution of balance sheet compositions				
Deposits-to-liabilities ratio (%)	-0.008	-0.001	-0.002	-0.004
	(-1.05)	(-0.12)	(-0.25)	(-0.87)
Mortgages-to-assets ratio (%)	-0.002	-0.252	-0.123	-0.009
	(-0.15)	(-0.69)	(-0.87)	(-0.47)
Loans-to-assets ratio (%)	-0.002	-0.372	-0.071	-0.022
	(-0.23)	(-0.45)	(-0.78)	(-0.95)
Tier 1 capital ratio (%)	0.018	0.002	0.004	-0.009
	(1.31)	(0.24)	(0.38)	(-0.67)

Notes: In Panel A, we present the difference in annual growth rates of branch-level deposits between branches of MA-DIF member banks and non-member banks. In Panel B, we present the difference in the quarterly growth rate of various dependent variables between MA-DIF banks and non-MA-DIF banks headquartered in Massachusetts over different pre-crisis periods. In Panel C, we present the difference in the quarterly growth rate of various balance sheet compositions between MA-DIF banks and non-MA-DIF banks headquartered in Massachusetts over different pre-crisis periods. The associated *t*-statistics are presented in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 1.3: Effect of MA-DIF membership on branch-level deposits

Panel A: Effect of MA-DIF membership on branch-level deposits (Crisis definition Q3:2007-Q2:2010)						
	1	2	3	4	5	6
Dependent variable			Branch deposits (ln)			
Sample	Full sample	Full sample	All branches in MA	MA branches operated by MA banks	MA branches of Members & Non-MA banks	Matched sample
Membership*Crisis	0.018**	0.018**	0.074***	0.037**	0.079***	0.048***
	(2.01)	(2.04)	(6.58)	(2.41)	(6.85)	(4.75)
Total assets (ln)		0.029***	0.042***	0.138***	0.044***	0.024***
		(6.80)	(6.17)	(5.24)	(7.05)	(3.79)
Interest expense ratio (%)		0.073***	0.065***	0.143***	0.052***	0.020
		(10.05)	(3.93)	(5.49)	(2.95)	(1.30)
Charge off ratio (%)		0.005*	0.033***	-0.037**	0.073***	0.036***
		(1.70)	(3.67)	(-2.14)	(6.72)	(4.61)
Tier 1 capital ratio (%)		-0.022***	-0.023***	0.004	0.005	-0.038***
		(-9.06)	(-5.30)	(0.59)	(0.78)	(-8.60)
Deposits-to-liabilities ratio (%)		0.001**	0.006***	0.006***	0.003***	0.008***
		(2.36)	(8.02)	(3.77)	(3.94)	(11.07)
Loans-to-assets ratio (%)		-0.002***	0.002	0.005***	-0.000	0.001*
		(-5.20)	(1.53)	(2.96)	(-0.07)	(1.72)
Mortgages-to-assets ratio (%)		0.003***	-0.004***	-0.005***	-0.001	-0.002*
		(7.09)	(-3.09)	(-3.23)	(-0.70)	(-1.95)
Population of 20-25 & above 65 (%)		0.001	0.000	0.012	-0.005	-0.004
		(0.25)	(0.06)	(1.11)	(-0.89)	(-0.77)
High school or above (%)		0.005**	-0.005	-0.013	-0.009	-0.015***
		(2.31)	(-0.82)	(-1.50)	(-1.27)	(-3.15)
Female population (%)		0.020***	0.006	0.048	0.007	0.033*
		(2.99)	(0.27)	(1.45)	(0.26)	(1.65)
Minority population (%)		0.000	0.010**	-0.011*	0.009**	0.006
		(0.37)	(2.42)	(-1.75)	(2.18)	(1.46)
Social capital index		-0.070***	-0.014	-0.018	0.060	-0.116**
		(-4.58)	(-0.24)	(-0.13)	(0.97)	(-2.34)
Branch FE	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES
Adjusted R-squared	0.192	0.211	0.354	0.336	0.404	0.317
Observations	69,108	69,108	13,189	7,098	10,369	13,584
No. of branches	7,006	7,006	1,361	888	1,149	1,214
SE Cluster	Branch	Branch	Branch	Branch	Branch	Branch
Panel B: Effect of MA-DIF membership on branch-level deposits (Crisis definition Q3:2007-Q2:2008)						
Membership*Crisis	0.034***	0.061***	0.129***	0.083***	0.113***	0.074***
	(2.78)	(4.94)	(8.91)	(3.76)	(8.11)	(5.99)
Total assets (ln)		0.029***	0.044***	0.137***	0.048***	0.024***
		(6.78)	(6.40)	(5.20)	(7.61)	(3.87)
Interest expense ratio (%)		0.074***	0.068***	0.145***	0.057***	0.022
		(10.17)	(4.10)	(5.55)	(3.19)	(1.44)
Charge off ratio (%)		0.004	0.018**	-0.041**	0.053***	0.027***
		(1.43)	(2.03)	(-2.34)	(5.02)	(3.57)
Tier 1 capital ratio (%)		-0.022***	-0.022***	0.003	0.007	-0.038***
		(-9.06)	(-5.20)	(0.53)	(1.05)	(-8.53)

Continued on next page

Table 1.3: Effect of MA-DIF membership on branch-level deposits

Deposits-to-liabilities ratio (%)		0.001**	0.006***	0.006***	0.003***	0.008***
		(2.35)	(8.11)	(3.75)	(4.02)	(11.36)
Loans-to-assets ratio (%)		-0.002***	0.002	0.005***	-0.000	0.002*
		(-5.17)	(1.55)	(2.92)	(-0.02)	(1.88)
Mortgages-to-assets ratio (%)		0.003***	-0.004***	-0.005***	-0.000	-0.002*
		(7.07)	(-2.80)	(-3.22)	(-0.20)	(-1.81)
Population of 20-25 & above 65 (%)		0.001	0.001	0.012	-0.004	-0.004
		(0.25)	(0.19)	(1.15)	(-0.73)	(-0.69)
High school or above (%)		0.005**	-0.007	-0.015*	-0.010	-0.016***
		(2.22)	(-1.10)	(-1.71)	(-1.48)	(-3.26)
Female population (%)		0.019***	0.002	0.044	0.003	0.031
		(2.93)	(0.09)	(1.33)	(0.11)	(1.56)
Minority population (%)		0.000	0.009**	-0.011*	0.009**	0.006
		(0.32)	(2.32)	(-1.80)	(2.11)	(1.37)
Social capital index		-0.071***	-0.025	-0.033	0.053	-0.125**
		(-4.66)	(-0.41)	(-0.25)	(0.85)	(-2.52)
Branch FE	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES
Adjusted R-squared	0.192	0.212	0.355	0.336	0.404	0.317
Observations	69,108	69,108	13,189	7,098	10,369	13,584
No. of branches	7,006	7,006	1,361	888	1,149	1,214
SE Cluster	Branch	Branch	Branch	Branch	Branch	Branch

Notes: We present results obtained using equation 1.1. The dependent variable is the logarithm of branch deposits (in $000) and the main explanatory variable is an interaction term between the dummy indicating MA-DIF membership and the dummy variable denoting the crisis period. In Panel A, the crisis period covers Q3:2007-Q2:2010. In Panel B, the crisis period covers Q3:2007-Q2:2008. Column 1 uses the full sample, including branches of all banks from Massachusetts, New York, New Hampshire, Connecticut, Vermont, and Rhode Island. Column 2 presents the results with a set of 3 year-lagged bank-level control variables, including the logarithm of total bank assets (Total assets (ln)); the percentage of total interest expense over total deposits (Interest expense ratio (%)); the ratio of charged off loans over total loans (Charge-off ratio (%)); the Tier 1 capital ratio (Tier 1 capital ratio (%)) the ratio of total deposits over total liabilities (Deposits-to-liabilities ratio (%)); the ratio of total loans over total assets (Loans-to-assets ratio (%)); and the ratio of total mortgages over total assets (Mortgages-to-assets ratio(%)), and a set of county-level variables, including the proportion of the population with high school or above education (High school or above (%)), the proportion of the population with age between 20 and 25 or above 65 (Population of 20-25 & above 65 (%)), the social capital index (Social capital index), the proportion of females (Female population (%)) and the proportion of minorities (Minority population (%)). Column 3 shows the results obtained using a sample covering branches of all banks operating in Massachusetts. Column 4 includes the results obtained using a sample including only Massachusetts branches of banks headquartered in Massachusetts (members and non-members of the MA-DIF). Column 5 includes the results obtained using a sample where the control group includes branches of banks headquartered outside Massachusetts. Column 6 includes the results obtained using a matched sample. Robust *t*-statistics are presented in parentheses. Standard errors are clustered at the branch level. *** p<0.01, ** p<0.05, * p<0.1.

Table 1.4: Effect of MA-DIF membership on branch-level deposits: Alternative explanations

	1	2	3	4	5	6	7	8	9	10
Dependent variable	Branch deposits (ln)									
Control group split	Uninsured deposits		Return on equity		Interest expense ratio (%)		Charge off ratio (%)		Z-score (ln)	
Percentile	$\leq p50$	$>p50$	$\leq p50$	$>p50$	$\leq p50$	$>p50$	$\leq p50$	$>p50$	$\leq p50$	$>p50$
Membership*Crisis	0.027	0.083***	0.086***	0.045***	0.079***	0.041***	0.083***	0.072***	0.036**	0.069***
	(1.44)	(7.45)	(6.97)	(2.74)	(5.73)	(3.18)	(3.43)	(6.12)	(2.28)	(4.95)
Control variables	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Branch FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Adjusted R-squared	0.349	0.358	0.338	0.385	0.314	0.384	0.354	0.350	0.419	0.299
Observations	5,429	12,038	11,628	5,839	7,201	10,266	5,099	12,368	7,980	9,487
No. of branches	516	1,254	1,175	595	689	1,081	506	1,264	788	982
SE Cluster	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Test of difference in coefficient p-value (two-tailed)	0.010		0.045		0.044		0.683		0.117	

Notes: We present results obtained using equation 1.1. The dependent variable is the logarithm of branch deposits (in $000) and the main explanatory variable is an interaction term between the dummy indicating MA-DIF membership and the dummy variable denoting the crisis period. In The crisis period covers Q3:2007-Q2:2010. Column 1 presents the results with a control group set of branches operated by banks with a lower volume of uninsured deposits in the pre-crisis period, while Column 2 presents the results with a control group set of branches operated by banks with a higher volume of uninsured deposits in the pre-crisis period. Column 3 presents the results of a sample where the control group includes branches of banks with a lower return on equity (ROE), while Column 4 presents the results of a sample where the control group includes branches of banks with a higher return on equity (ROE). Column 5 presents the results with a control group of branches operated by banks with a lower interest expense ratio in the pre-crisis period, while Column 6 presents the results with a control group of branches operated by banks with a higher interest expense ratio in the pre-crisis period. Column 7 presents the results of a sample where the control group includes branches of banks with a lower charge off ratio, while Column 8 presents the results of a sample where the control group includes branches of banks with a higher charge off ratio. Column 9 presents the results with a control group of branches operated by banks with a lower Z-score in the pre-crisis period, while Column 10 presents the results with a control group of branches operated by banks with a higher Z-score in the pre-crisis period. Definitions of all control variables are shown in the notes of Table 1.3. The p-value for the test of difference in the coefficient of interest is shown at the bottom of each pair of columns. The null hypothesis of the equality test is that the difference between the pairs of the coefficient of interest equals zero. Robust t-statistics are presented in parentheses. Standard errors are clustered at the branch level. *** p<0.01, ** p<0.05, * p<0.1.

Table 1.5: Effect of MA-DIF membership on branch-level deposits: Alternative explanations (TARP, Enforcement actions, Opacity)

	1	2	3	4	5	6
Dependent variable	Branch deposits (ln)					
Control group	TARP banks	Non-TARP banks	EAs banks	Non-EAs banks	Less opaque banks	Opaque banks
Membership*Crisis	0.034**	0.048***	0.088***	0.039**	0.069***	0.036**
	(2.44)	(4.11)	(7.09)	(2.29)	(4.87)	(2.30)
Control variables	YES	YES	YES	YES	YES	YES
Branch FE	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES
Adjusted R-squared	0.442	0.285	0.343	0.38	0.299	0.418
Observations	7,941	9,526	11,817	5,650	10,424	7,043
No. of branches	808	962	1,222	548.00	1,099	671
SE Cluster	Branch	Branch	Branch	Branch	Branch	Branch
Test of difference in coefficient p-value (two-tailed)	0.441		0.020		0.118	

Notes: We present results obtained using equation 1.1. The dependent variable is the logarithm of branch deposits (in $000) and the main explanatory variable is an interaction term between the dummy indicating MA-DIF membership and the dummy variable denoting the crisis period. The crisis period covers Q3:2007-Q2:2010. Column 1 presents results with a control group of branches operated by banks that participate in the TARP, while Column 2 presents the results with a control group of branches operated by banks that are not participants of the TARP. Column 3 presents the results of a sample where the control group includes branches of banks that are subject to enforcement actions during the crisis period, while Column 4 presents the results of a sample where the control group includes branches of banks that are not subject to enforcement actions during the crisis period. Column 5 presents the results of a sample where the control group includes branches of banks that are less opaque, while Column 6 presents the results of a sample where the control group includes branches of banks that are more opaque. Definitions of all control variables are shown in the notes of Table 1.3. The p-value for the test of difference in the coefficient of interest is shown at the bottom of each pair of columns. The null hypothesis of the equality test is that the difference between the pairs of the coefficient of interest equals zero. Robust t-statistics are presented in parentheses. Standard errors are clustered at the branch level. *** p<0.01, ** p<0.05, * p<0.1.

Table 1.6: Effect of MA-DIF membership on bank-level deposits

	1	2	3	4	5	6
Dependent variable	Bank-level deposits (ln)	Bank-level deposits (ln)	Interest-bearing deposits (ln)	Noninterest-bearing deposits (ln)	Brokered deposits (ln)	Proportion of brokered deposits (%)
Membership*Crisis	0.076**	0.084***	0.087***	-0.015	-1.240**	-0.014
	(2.01)	(2.97)	(2.77)	(-0.20)	(-2.08)	(-1.46)
Control variables	NO	YES	YES	YES	YES	YES
Bank FE	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES
Adjusted R-squared	0.548	0.755	0.658	0.548	0.086	0.082
Observations	3,449	3,449	3,449	3,449	3,449	3,449
No. of banks	83	83	83	83	83	83
SE Cluster	Bank	Bank	Bank	Bank	Bank	Bank

Notes: We present results obtained using equation 1.2. The main explanatory variable is an interaction term between the dummy indicating MA-DIF membership and the dummy variable denoting the crisis period. The crisis period covers Q3:2007-Q2:2010. The dependent variable in Column 1 and Column 2 is the logarithm of bank-level deposits (in $000). Column 2 presents the result with a set of 3 year-lagged bank-level control variables. The dependent variable in Column 3 is the logarithm of interest-bearing deposits (in $000). The dependent variable in Column 4 is the logarithm of noninterest-bearing deposits (in $000). The dependent variable in Column 5 is the logarithm of brokered deposits (in $000). The dependent variable in Column 6 is the proportion of brokered deposits over total deposits. Definitions of all control variables are shown in the notes of Table 1.3. Robust *t*-statistics are presented in parentheses. Standard errors are clustered at the bank-level. *** p<0.01, ** p<0.05, * p<0.1.

Table 1.7: Placebo tests

Panel A : Monte Carlo simulations for the effect of MA-DIF membership on branch-level deposits during the crisis	
	1
Dependent variable	Branch-level deposits (ln)
Rejection rate at the 1% level (2-tailed test)	1.3%
Rejection rate at the 5% level (2-tailed test)	5.8%
Rejection rate at the 10% level (2-tailed test)	10.6%
Mean coefficient	0.009
(t-statistic)	0.06

Panel B : Deposits of savings banks in the five neighbouring states of Massachusetts during the crisis					
	1	2	3	4	5
Dependent variable	Branch-level deposits (ln)				
Sample	CT	NH	NY	RI	VT
Savings Bank* Crisis	0.039	0.058	0.005	0.020	-0.045
	(1.43)	(1.20)	(0.36)	(0.28)	(-0.85)
Control variables	YES	YES	YES	YES	YES
Bank FE	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES
Adjusted R-squared	0.330	0.276	0.232	0.257	0.327
Observations	3,647	1,595	17,434	1,734	1,117
No. of banks	439	226	3,168	178	107
SE Cluster	Branch	Branch	Branch	Branch	Branch

Notes: Panel A presents the placebo test results with a random selection of membership in the MA-DIF. We report Monte Carlo simulations based on 1,000 replications for the effect of MA-DIF membership on branch-level deposits. We estimate equation 1.1 using the full sample of branch-level data. We exclude MA-DIF member banks and randomly assign banks to placebo MA-DIF membership status and set the variable "Membership" equal to 1 for 'member' banks and equal to 0 for 'non-member' banks. We estimate the regression and save the t-statistic on the coefficient of interest and repeat this process 1,000 times. Panel A reports the rejection rates of the null hypothesis=0 at the 1%, 5%, and 10% levels, respectively. Panel A also reports the mean coefficient and the average t-statistic for β_1. In Panel B, we present results obtained using equation 1.1. The dependent variable is the logarithm of branch deposits (in $000), but the main explanatory variable is an interaction term between the dummy indicating state-chartered savings bank and the dummy variable denoting the crisis period. The crisis period in this table covers Q3:2007-Q2:2010. Robust t-statistics are presented in parentheses. Standard errors are clustered at the branch-level. *** p<0.01, ** p<0.05, * p<0.1.

Table 1.8: Effect of MA-DIF membership on bank lending

	1	2	3	4	5
Dependent variable	Total loans	Residential mortgages	Construction and land development loans	Commercial and industrial loans	Individual loans
Membership*Crisis	0.068*	0.058**	0.138	0.066	0.198
	(2.36)	(2.18)	(0.86)	(0.47)	(1.57)
Control variables	YES	YES	YES	YES	YES
Bank FE	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES
Adjusted R-squared	0.823	0.782	0.135	0.318	0.127
Observations	3,449	3,449	3,449	3,449	3,449
No. of banks	83	83	83	83	83
SE Cluster	Bank	Bank	Bank	Bank	Bank

Notes: We present results obtained using equation 1.2. The main explanatory variable is an interaction term between the dummy indicating MA-DIF membership and the dummy variable denoting the crisis period. The crisis period covers Q3:2007-Q2:2010. The dependent variable in Column 1 is the logarithm of total loans (in $000). The dependent variable in Column 2 is the logarithm of residential mortgages (in $000). The dependent variable in Column 3 is the logarithm of construction and land development loans (in $000). The dependent variable in Column 4 is the logarithm of commercial and industrial loans (in $000). The dependent variable in Column 5 is the logarithm of individual loans (in $000). Definitions of all other control variables are shown in the notes of Table 1.3. Robust t-statistics are presented in parentheses. Standard errors are clustered at the bank level. *** p<0.01, ** p<0.05, * p<0.1.

Table 1.9: Effect of MA-DIF membership on residential mortgage origination and securitization

Panel A: Effect of MA-DIF membership on mortgage securitization			
	1	2	3
Dependent variable	Acceptance of loan applications		
Sample	All mortgages	Home purchase mortgages	Refinancing mortgages
Membership*Crisis	0.011***	0.025***	-0.002
	(2.90)	(4.28)	(-0.47)
Control variables	YES	YES	YES
Bank FE	YES	YES	YES
County x Year FE	YES	YES	YES
Adjusted R-squared	0.054	0.048	0.065
Observations	371,898	184,174	187,724
No. of banks	83	83	83

Panel B: Effect of MA-DIF membership on mortgage securitization		
	1	2
Dependent variable	Securitized mortgages (ln)	Proportion of securitized mortgages (%)
Membership*Crisis	0.085	3.542
	(0.89)	(0.99)
Control variables	YES	YES
Bank FE	YES	YES
Year FE	YES	YES
Adjusted R-squared	0.013	0.009
Observations	3,449	3,449
No. of banks	83	83
SE Cluster	Bank	Bank

Notes: In Panel A, we present results obtained using the following linear probability model: $Accept_{a,i,t} = \beta_0 + \beta_1 Membership_i \times Crisis_t + \gamma X_{i,t} + \xi Z_a + \delta_i + \delta_{c,t} + \varepsilon_{a,i,t}$ where the dependent variable $Accept_(a,i,t)$, is is a dummy variable indicating whether a loan application a issued by bank i in year t and the main explanatory variable is an interaction term between the dummy variable, $Membership_i$, indicating MA-DIF membership, and the dummy variable, $Crisis_t$, denoting the crisis period. $X_{i,t}$ captures a vector of bank-level control variables, and Z_a captures a vector of loan-level control variables. δ_i is a bank fixed effect and $\delta_{c,t}$ is a county-year fixed effect. The crisis period covers the years 2007-2010. Column 1 presents the results with a sample of all mortgage applications. Column 2 presents the results with a sample of home purchase mortgage applications. Column 3 presents the results with a sample of refinancing mortgage applications. In Panel B, we present results obtained using equation 1.2. The main explanatory variable is an interaction term between the dummy indicating MA-DIF membership and the dummy variable denoting the crisis period. The dependent variable in Column 1 is the logarithm of securitized residential mortgages (in $000). The dependent variable in Column 2 is the proportion of securitized residential mortgages to total residential mortgages. Definitions of all control variables are shown in the notes of Table 1.3. Robust t-statistics are presented in parentheses. Standard errors are clustered at the bank-level. *** p<0.01, ** p<0.05, * p<0.1.

Table 1.10: Effect of MA-DIF membership on bank soundness

	1	2	3	4	5	6
Dependent variable	Tier 1 capital ratio (%)	Charge off ratio (%)	Z-score	Nonperforming t+4Qs	Nonperforming t+8Qs	Nonperforming t+12Qs
Membership*Crisis	0.036	-0.009	3.850	-0.001	0.001	-0.000
	(1.43)	(-0.88)	(0.36)	(-0.38)	(0.35)	(-0.02)
Control variables	YES	YES	YES	YES	YES	YES
Bank FE	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES
Adjusted R-squared	0.230	0.081	0.0303	0.355	0.322	0.275
Observations	3,449	3,449	3,449	3,388	3,302	3,217
No. of banks	83	83	83	82	79	79
SE Cluster	Bank	Bank	Bank	Bank	Bank	Bank

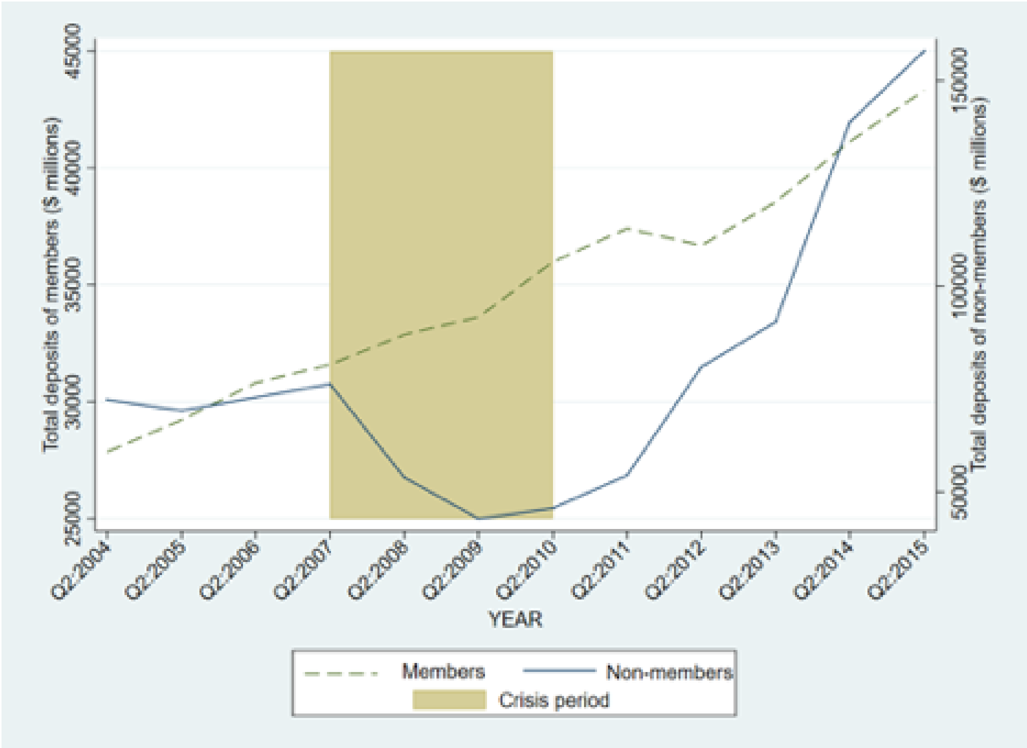
Notes: We present results obtained using equation 1.2. The main explanatory variable is an interaction term between the dummy indicating MA-DIF membership and the dummy variable denoting the crisis period. The crisis period covers Q3:2007-Q2:2010. The dependent variable in Column 1 is the Tier 1 capital ratio (%). The dependent variable in Column 2 is the charge off ratio (%). The dependent variable in Column 3 is the Z-score. The dependent variable in Column 4-6 is the nonperforming mortgages$_{t+4Qs}$ ratio, nonperforming mortgages$_{t+8Qs}$ ratio and nonperforming mortgages$_{t+12Qs}$ ratio, respectively. Definitions of all control variables are shown in the notes of Table 1.3. Robust *t*-statistics are presented in parentheses. Standard errors are clustered at the bank-level. *** p<0.01, ** p<0.05, * p<0.10.

Table 1.11: Effect of MA-DIF membership on loan-to-income ratio

	1	2	3
Dependent variable	Loan-to-income ratio (ln)		
Sample	All mortgages	Retained mortgages	Sold mortgages
Membership*Crisis	-0.057***	-0.135***	-0.013
	(-6.93)	(-10.01)	(-1.43)
Control variables	YES	YES	YES
Loan level characteristics	YES	YES	YES
Bank FE	YES	YES	YES
County x Year FE	YES	YES	YES
Adjusted R-squared	0.238	0.256	0.300
Observations	291,605	176,243	115,362
No. of banks	83	83	83

Notes: We present results using the following multiple linear regression model. $LIR_{a,i,t} = \beta_0 + \beta_1 Membership_i \times Crisis_t + \gamma X_{i,t} + \xi Z_a + \delta_i + \delta_{c,t} + \varepsilon_{a,i,t}$ where the dependent variable $LIR_{a,i,t}$, is the logarithm of loan-to-income ratio of approved mortgages a issued by bank i in year t and the main explanatory variable is an interaction term between the dummy variable, $Membership_i$, indicating MA-DIF membership, and the dummy variable, $Crisis_t$, denoting the crisis period. $X_{i,t}$ captures a vector of bank-level control variables, and Z_a captures a vector of loan-level control variables. δ_i is a bank fixed effect and $\delta_{c,t}$ is a county-year fixed effect. The crisis period covers the years 2007-2010. Column 1 presents the results with a sample of all approved mortgage applications. Column 2 presents the results with a sample of approved retained mortgage applications. Column 3 presents the results with a sample of approved sold mortgage applications. Definitions of all control variables are shown in the notes of Table 1.3. Robust t-statistics are presented in parentheses. Standard errors are clustered at the bank-level. *** p<0.01, ** p<0.05, * p<0.1.

Figure 1.1: Total deposits of member banks of the Depositors Insurance Fund and of non-member banks headquartered in Massachusetts (2004-2015)



Notes: This figure presents total deposits of MA-DIF member banks and non-member banks headquartered in Massachusetts. The dashed line represents the total deposits of the MA-DIF members, while the solid line represents the total deposits of non-member banks. The shaded area indicates the crisis period (Q3:2007-Q2:2010). The figure uses two scales, one for MA-DIF member banks on the left and one for non-members on the right to provide better insights into the evolution of deposits. Total deposits are scaled by 1,000,000.

Figure 1.2: Timeline of government responses during the crisis

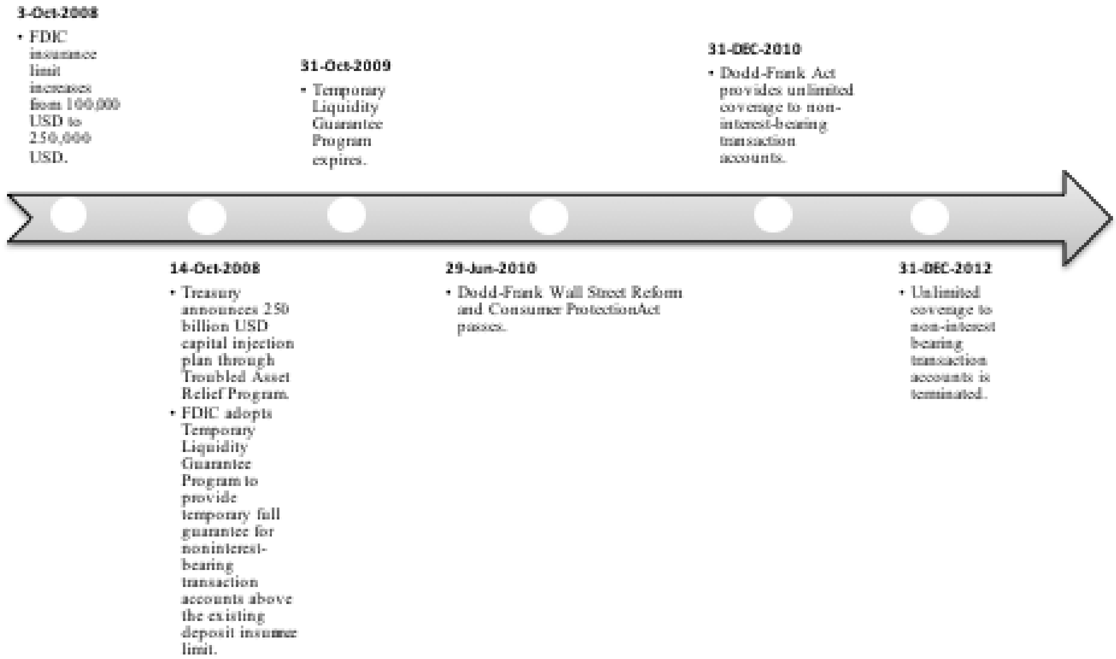
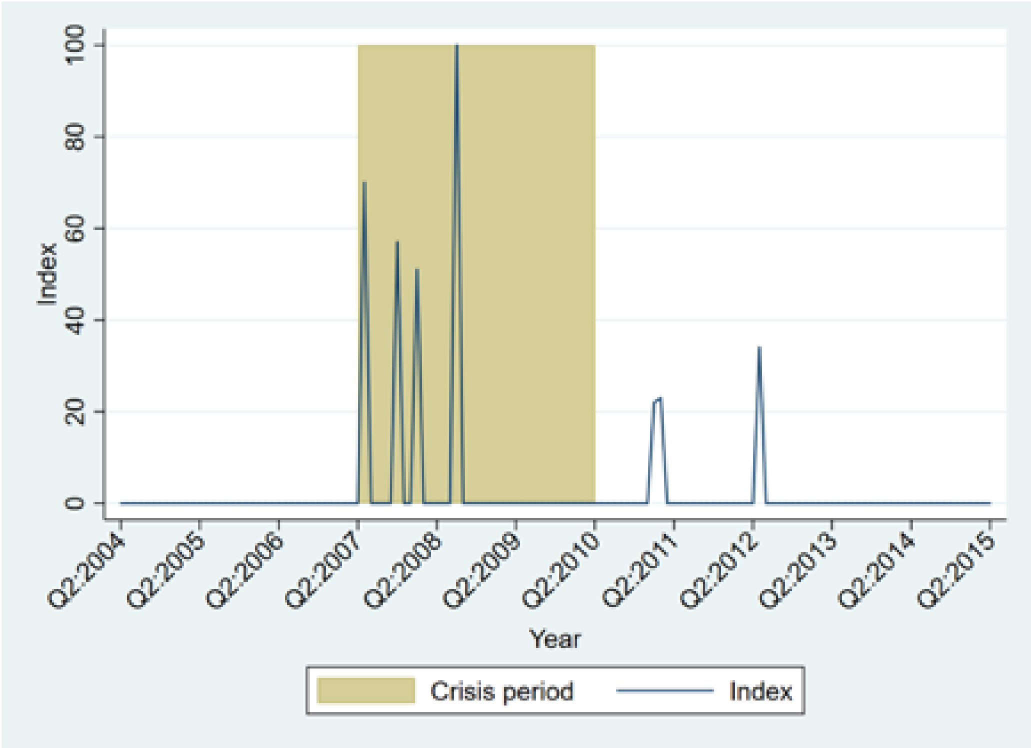
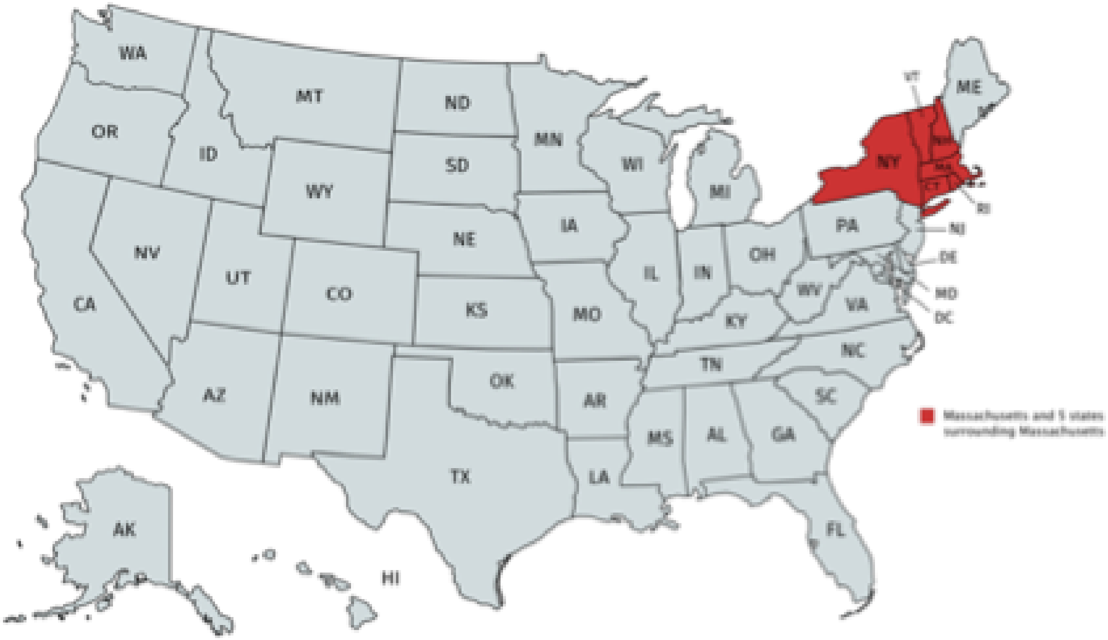


Figure 1.3: Google Trends search volume index of the keyword "Depositors Insurance Fund"



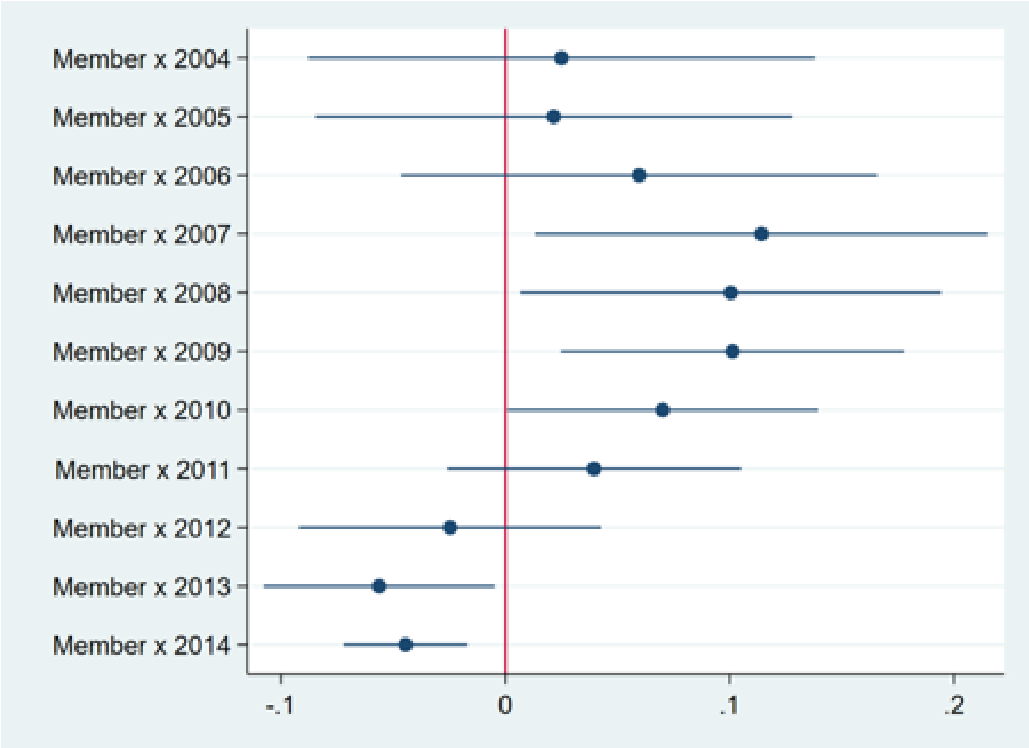
Notes: This figure presents the Google Trends search volume index for the keywords "Depositors Insurance Fund" in Massachusetts between 2004 and 2015. The shaded area indicates the crisis period (Q3:2007-Q2:2010).

Figure 1.4: U.S. map



Notes: This figure shows the coverage of our sample, which includes banks headquartered in Massachusetts (MA) and the five surrounding states: Connecticut (CT), New Hampshire (NH), New York (NY), Rhode Island (RI) and Vermont (VT).

Figure 1.5: Dynamic impact of MA-DIF membership on deposits



Notes: The figure plots the dynamic impact of MA-DIF membership on deposits throughout the sample period. The solid lines represent 95% confidence intervals, and the dots represent the estimated coefficients from equation 1.3.

1.9 Appendix

Further details on the MA-DIF and FDIC

Table A1.1 compares in Panel A design features of the MA-DIF with the FDIC. Panel B contrasts four characteristics of successful insurance mechanisms with those that failed in the U.S. to evaluate the credibility of the MA-DIF based on historical experience. Calomiris (1989) defines a successful bank insurance fund as one that completely protects the payment system without motivating risk-taking of banks, while a failure is defined as a situation where a bank insurance fund fails to protect the payment system or collapses due to design flaws.[49]

Panel A highlights two distinguishing features between the MA-DIF and the FDIC: the unlimited insurance coverage for deposits held in member banks of the MA-DIF and its private management. A detailed review of the design features of the MA-DIF suggests that many of its characteristics resemble those of successful bank insurance funds in the past.

(i) Power to regulate and discipline banks

For most of the successful insurance funds, their board of directors can investigate bank operations and discipline banks. The disciplinary actions include setting limits on asset-to-capital ratios, and even bank closure upon a two-thirds majority vote of the board (Calomiris, 1989).

While the management board of the MA-DIF is less powerful compared with successful bank insurance funds in the past, its management board can adjust the assessment rate according to members' risk categories and require members to take measures to mitigate risk. In contrast to the pre-FDIC period, all MA-DIF member banks are already monitored by the FDIC and the Massachusetts Division of Banks. The MA-DIF may therefore not need to have strong board power.

(ii) Cost of exit

The low cost of exit contributes to adverse selection problems that undermine the reliability of the bank insurance fund (English, 1993). The cost of exit is high when the board of the deposit insurance fund can restrict exit and exit undermines banks' competitive advantage. Membership in the MA-DIF is compulsory for all Massachusetts-chartered savings banks, MA-DIF member

[49] Our comparison considers both the experience of bank liability insurance funds in the Antebellum era and deposit insurance funds after the Antebellum era to gain a holistic overview. During the Antebellum era, some of the schemes insured all debt of the participating banks, i.e., circulating notes and deposits, while some of them only insured circulating notes (Golembe, 1956). To avoid any confusion with the labelling used in prior work, we follow Calomiris (1989) and use the phrase 'bank insurance' to cover both bank liability insurance in the Antebellum era and deposit insurance after the Antebellum era.

banks can only leave the fund by switching their charter.

(iii) Reserves to cover insured liabilities

A common characteristic of failed insurance funds are limited reserves. Due to the small amount of reserves, such insurance funds run out of reserves when one of the large member banks fails or when many member banks fail simultaneously (English, 1993).

The MA-DIF maintained sufficient reserves to survive the most challenging period in the history of the Massachusetts savings bank industry in the early 1990s. Back then, the MA-DIF was capable to pay out more than 50 million USD to protect over 6,500 depositors in 19 failed member banks.

Figure A1.1 compares the gross coverage ratio of the MA-DIF with the equivalent figure of the FDIC.[50] While fundamental differences between the FDIC and the MA-DIF render the comparison imperfect, the figure serves to illustrate whether the MA-DIF was financially vulnerable during the recent financial crisis by comparing the evolution of its gross coverage ratio to the FDIC.[51] We define the gross coverage ratio as total assets over insured deposits. The gross coverage ratio of the MA-DIF is higher than the ratio of the FDIC. The gross coverage ratio of the MA-DIF ranges from 3.5% to 6%, while the coverage ratio for the FDIC stays below 2%. The gross coverage ratio of both insurance funds rises during the crisis. The adjustment of the FDIC deposit insurance limit causes a sharp increase in the gross coverage ratio of the MA-DIF in 2008. In short, there is no evidence showing an abnormal decline of the gross coverage ratio of the MA-DIF during the crisis.

(iv) Risk adjusted premium

A flat rate insurance premium is known to give rise to moral hazard (Keeley, 1990). In the absence of effective regulations and enforcement actions, a flat rate insurance scheme subsidizes banks' risk-taking, thus undermining the credibility of insurance. Most of the failed insurance funds charge a flat rate assessment, and some of them set an upper limit on the assessment rate. In contrast, the MA-DIF charges its members based on their risk categories without limit to restrict excessive risk-taking of the member banks.

[50] Information on gross coverage ratios of the MA-DIF and the FDIC is available in the respective annual report.

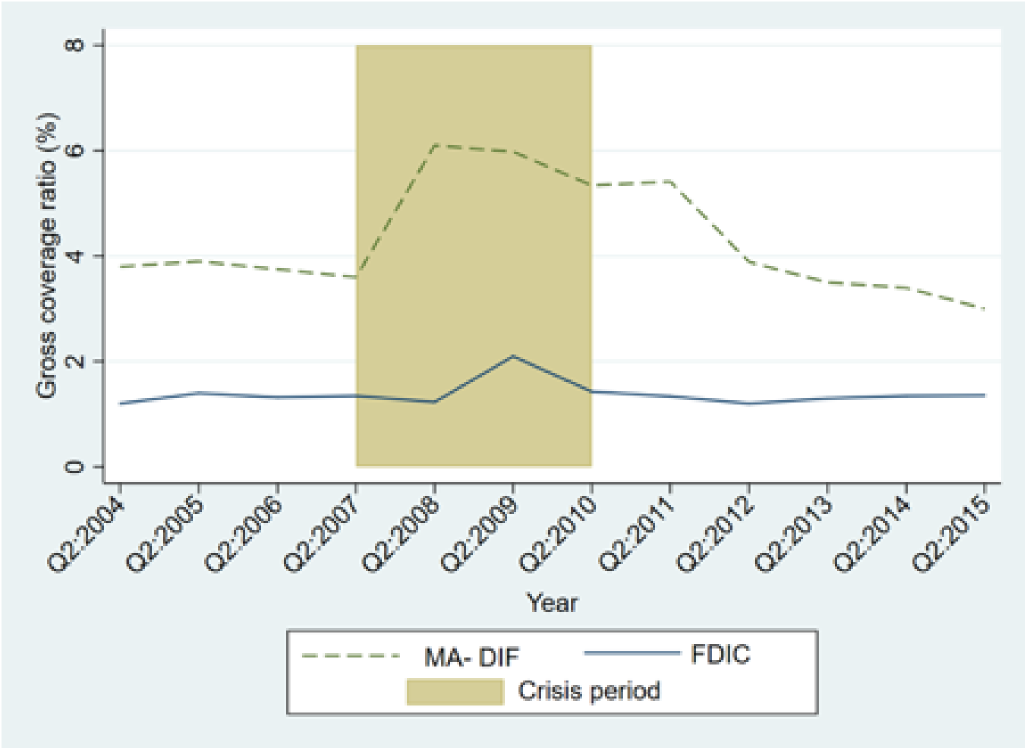
[51] A key distinguishing feature between both insurance schemes is that the FDIC is backed by the full faith and credit of the U.S. government, while the MA-DIF is neither backed by the federal nor the state government. The FDIC can rely on a line of credit from the U.S. Treasury when reserves disappear, but the MA-DIF does not have such backup.

(v) Management board consisting of member banks' managements

The board of directors in a successful bank insurance fund generally consist of the managers of its member banks (Calomiris, 1989). While this composition ensures that the board members of the bank insurance fund have skin in the game, it also lowers the monitoring cost of the board in the sense that the managers of member banks tend to know more about their peers and the local environment than outsiders appointed by regulatory authorities. Beck (2002) argues that there is a positive effect of member banks' management on peer monitoring in the context of the German private banks' deposit insurance fund. The management board of the MA-DIF primarily consists of presidents and chief executive officers of the MA-DIF-insured banks.

Our brief survey suggests that the MA-DIF is designed with an incentive compatible mechanism, and sufficient reserves against losses. These factors are likely to have played a major role for the survival of the MA-DIF during numerous crises since 1934.

Figure A1.1: Gross coverage ratio of the Depositors Insurance Fund and the Federal Deposit Insurance Corporation Fund



Notes: This figure compares the gross coverage ratio of the MA-DIF and the FDIC during 2004-2015. The shaded area indicates the crisis period (Q3:2007-Q2:2010).

Table A1.1: Comparison between the MA-DIF and other deposit insurance funds

Panel A: Comparison of the MA-DIF with the FDIC		
	MA-DIF	FDIC
Explicit	Yes	Yes
Coverage limit	Unlimited	250,000 USD
Coinsurance	No	No
Sources of funds	Banks	Banks
Management	Private	Public
Membership	Compulsory	Compulsory
Risk adjusted premium	Yes	Yes

Panel B: Comparison of successful and failed bank insurance funds and the MA-DIF			
	Successful bank insurance fund	Failed bank insurance fund	MA-DIF
Power to regulate and discipline banks	Yes	No	Yes
Reserve to cover insured deposits	Abundant	Limited	Abundant
Management primarily comprises member banks'management	Yes	No	Yes
Risk adjusted premium	Yes	No	Yes

Notes: Panel A compares characteristics of the MA-DIF and the FDIC. Panel B compares characteristics of the MA-DIF to successful and failed bank insurance funds in the U.S.

Table A1.2: Methodological Robustness Checks

Panel A: Effect of MA-DIF membership on branch-level deposits (Crisis definition Q3:2007-Q2:2010)					
	1	2	3	4	5
Dependent variable	Branch-level deposits (ln)				
Sample	All branches in MA				
Membership*Crisis	0.068***	0.068***	0.067***	0.073***	0.074***
	(6.15)	(3.32)	(5.69)	(2.83)	(6.17)
Crisis	-0.001				
	(-0.12)				
Membership	0.151*				
	(1.88)				
Control variables	YES	YES	YES	YES	YES
Bank FE	NO	YES	NO	NO	NO
Branch FE	NO	NO	YES	YES	YES
County x Year FE	NO	NO	YES	NO	NO
Year FE	NO	YES	NO	YES	YES
Adjusted R-squared	0.334	0.193	0.945	0.944	0.280
Observations	13,189	13,189	13,189	13,189	13,189
No. of branches	1,361	1,361	1,361	1,361	1,361
SE cluster	Branch	Branch	Branch	Bank	Bootstrap
Panel B: Effect of MA-DIF membership on branch-level deposits (Crisis definition Q3:2007-Q2:2008)					
Membership*Crisis	0.109***	0.105***	0.114***	0.130***	0.129***
	(7.84)	(4.71)	(7.56)	(4.90)	(8.68)
Crisis	-0.070***				
	(-8.27)				
Membership	0.147*				
	(1.85)				
Control variables	YES	YES	YES	YES	YES
Bank FE	NO	YES	NO	NO	NO
Branch FE	NO	NO	YES	YES	YES
County x Year FE	NO	NO	YES	NO	NO
Year FE	NO	YES	NO	YES	YES
Adjusted R-squared	0.334	0.193	0.945	0.944	0.280
Observations	13,189	13,189	13,189	13,189	13,189
No. of branches	1,361	1,361	1,361	1,361	1,361
SE cluster	Branch	Branch	Branch	Bank	Bootstrap

Notes: We present the results obtained using equation 1.1 with different fixed effects and methods in adjusting standard errors. The dependent variable is the logarithm of branch deposits (in $000). The main explanatory variable is an interaction term between the dummy indicating MA-DIF membership and the dummy variable denoting the crisis period. In Panel A, the crisis period covers Q3:2007-Q2:2010. In Panel B, the crisis period covers Q3:2007-Q2:2008. In Column 1-3, we present the results obtained using equation 1.1 with alternative fixed effects. Column 1 show the results without any fixed effects. Column 2 present the results with bank fixed effects, rather than branch fixed effects. Column 3 introduces County x Year fixed effects into the model. In Column 4-5, we present the results obtained using equation 1.1 with alternative methods of adjusting stand errors. Column 4 are clustered at the bank-level, while standard errors in Column 5 are bootstrapped based on 600 bootstrap simulations. Definitions of all control variables are shown in the notes of Table 1.3. Robust *t*-statistics are presented in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table A1.3: Effect of MA-DIF membership on branch-level deposits (including banks which switch membership)

Panel A: Effect of MA-DIF membership on branch-level deposits (Crisis definition Q3:2007-Q2:2010)					
	1	2	3	4	5
Dependent variable	Branch-level deposits (ln)				
Sample	Full sample	Full sample	All branches in MA	MA branches operated by MA banks	MA branches of Members and Non-MA banks
Membership*Crisis	0.024***	0.023***	0.073***	0.038**	0.078***
	(2.79)	(2.74)	(6.73)	(2.52)	(6.94)
Membership	0.070*	0.081**	0.089**	0.055	0.050
	(1.73)	(2.09)	(2.02)	(1.51)	(0.92)
Control variables	NO	YES	YES	YES	YES
Branch FE	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES
Adjusted R-squared	0.190	0.210	0.339	0.312	0.385
Observations	69,877	69,877	13,817	7,726	10,997
No. of branches	7,067	7,067	1,421	951	1,229
SE Cluster	Branch	Branch	Branch	Branch	Branch
Panel B: Effect of MA-DIF membership on branch-level deposits (Crisis definition Q3:2007-Q2:2008)					
Membership*Crisis	0.037***	0.063***	0.130***	0.091***	0.118***
	(3.23)	(5.40)	(9.39)	(4.32)	(8.86)
Membership	0.070*	0.078**	0.089**	0.052	0.054
	(1.75)	(2.00)	(2.06)	(1.45)	(1.01)
Control variables	NO	YES	YES	YES	YES
Branch FE	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES
Adjusted R-squared	0.190	0.210	0.340	0.313	0.385
Observations	69,877	69,877	13,817	7,726	10,997
No. of branches	7,067	7,067	1,421	951	1,229
SE Cluster	Branch	Branch	Branch	Branch	Branch

Notes: We present results obtained using equation 1.1. The dependent variable is the logarithm of branch deposits (in \$000) and the main explanatory variable is an interaction term between the dummy indicating MA-DIF membership and the dummy variable denoting the crisis period. In Panel A, the crisis period covers Q3:2007-Q2:2010. In Panel B, the crisis period covers Q3:2007-Q2:2008. Column 1 uses the full sample, including branches of all banks from Massachusetts, New York, New Hampshire, Connecticut, Vermont, and Rhode Island. Column 2 presents the results with a set of 3 year-lagged bank-level control variables. Column 3 shows the results obtained using a sample covering branches of all banks operating in Massachusetts. Column 4 includes the results obtained using a sample including only Massachusetts branches of banks headquartered in Massachusetts (members and non-members of the MA-DIF). Column 5 includes the results obtained using a sample where the control group includes branches of banks headquartered outside Massachusetts. Definitions of all control variables are shown in the notes of Table 1.3. Robust t-statistics are presented in parentheses. Standard errors are clustered at the branch-level. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table A1.4: Effect of MA-DIF membership on market shares and scaled branch-level deposits of MA-DIF member banks

Panel A: Effect of MA-DIF membership on market share and scaled branch-level deposits (Crisis definition Q3:2007-Q2:2010)		
	1	2
Dependent variable	Market share (ln)	Branch deposits scaled by total assets (ln)
Sample	All branches in MA	
Membership*Crisis	0.071***	0.074***
	(5.57)	(6.58)
Control variables	YES	YES
Branch FE	YES	YES
Year FE	YES	YES
Adjusted R-squared	0.444	0.846
Observations	13,189	13,189
No. of branches	1,361	1,361
SE Cluster	Branch	Branch
Panel B: Effect of MA-DIF membership on market share and scaled branch-level deposits (Q3:2007-Q2:2008)		
Membership*Crisis	0.129***	0.129***
	(8.46)	(8.91)
Control variables	YES	YES
Branch FE	YES	YES
Year FE	YES	YES
Adjusted R-squared	0.444	0.846
Observations	13,189	13,189
No. of branches	1,361	1,361
SE Cluster	Branch	Branch

Notes: We present the results obtained using equation 1.1. The sample cover branches of all banks operating in Massachusetts and the main explanatory variable is an interaction term between the dummy indicating MA-DIF membership and the dummy variable denoting the crisis period. In Panel A, the crisis period covers Q3:2007-Q2:2010. In Panel B, the crisis period covers Q3:2007-Q2:2008. The dependent variable in Column 1 is the logarithm of the market share, defined in terms of branch-level deposits to total deposits of all branches operating in Massachusetts. The dependent variable in Column 2 is the logarithm of branch deposits scaled by total assets, defined in terms of branch-level deposits to 3 year-lagged total assets. Definitions of all control variables are shown in the notes of Table 1.3. Robust *t*-statistics are presented in parentheses. Standard errors are clustered at the branch-level. *** p<0.01, ** p<0.05, * p<0.1.

Table A1.5: Effect of MA-DIF membership on average interest rate of deposits

	1
Dependent variable	Average deposit interest rate (%)
Membership*Crisis	0.019
	(0.98)
Control variables	YES
Bank FE	YES
Year FE	YES
Adjusted R-squared	0.932
Observations	3,449
No. of banks	83
SE Cluster	Bank

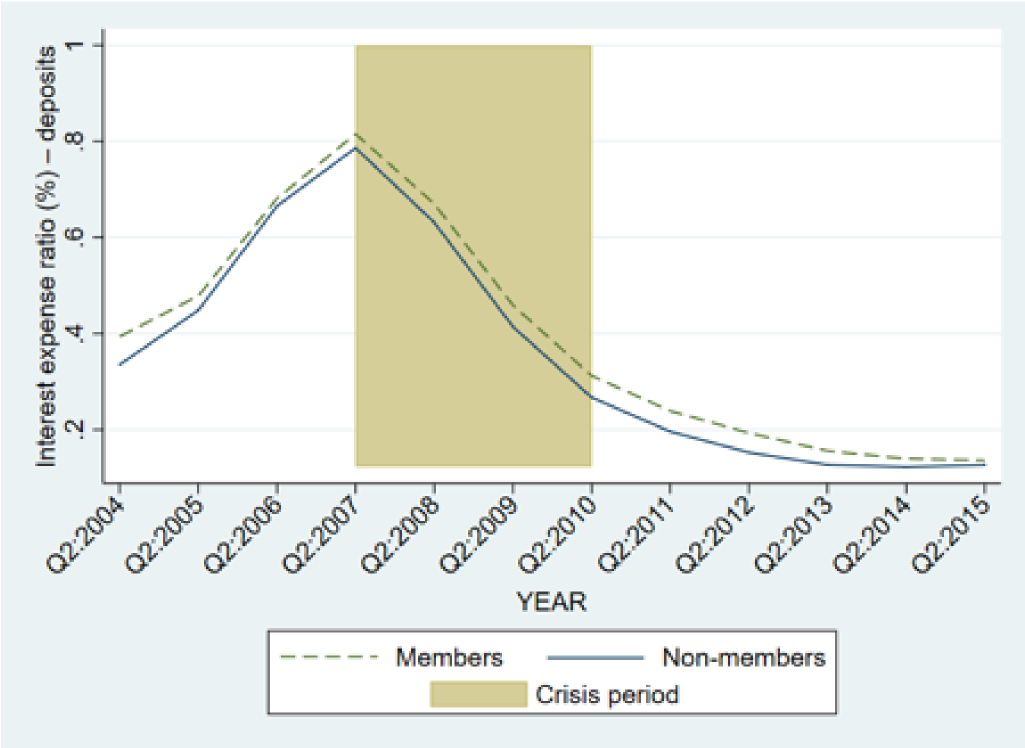
Notes: We present results obtained using equation 1.2. The main explanatory variable is an interaction term between the dummy indicating MA-DIF membership and the dummy variable denoting the crisis period. The crisis period covers Q3:2007-Q2:2010. The dependent variable in this table is the average interest rate on deposits. Definitions of all control variables are shown in the notes of Table 1.3. Robust *t*-statistics are presented in parentheses. Standard errors are clustered at the bank-level. *** p<0.01, ** p<0.05, * p<0.1.

Table A1.6: Effect of MA-DIF membership on FDIC-insured and FDIC-uninsured deposits

	1	2	3	4
Dependent variable	Total FDIC-uninsured deposits (ln)	Total FDIC-insured deposits (ln)	Number of FDIC-uninsured deposit accounts (ln)	Number of FDIC-insured deposit accounts (ln)
Membership*Crisis	0.077***	0.026	0.072***	-0.008
	(3.51)	(1.31)	(4.12)	(-0.47)
Control variables	YES	YES	YES	YES
Bank FE	YES	YES	YES	YES
Year FE	YES	YES	YES	YES
Adjusted R-squared	0.314	0.124	0.163	0.023
Observations	1,374	1,374	1,374	382
No. of banks	83	83	83	83
SE Cluster	Bank	Bank	Bank	Bank

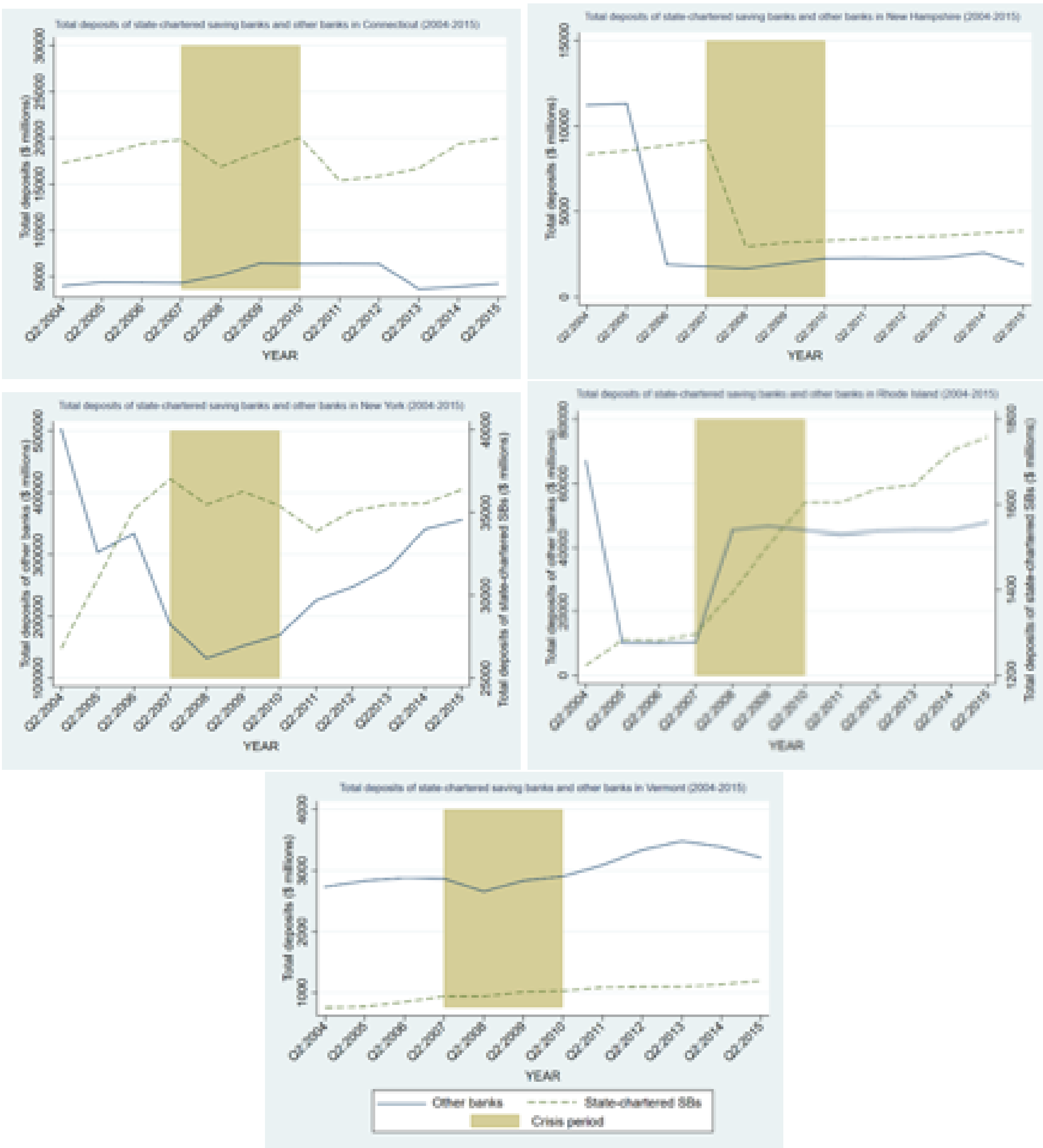
Notes: We present results obtained using equation 1.2. The main explanatory variable is an interaction term between the dummy indicating MA-DIF membership and the dummy variable denoting the crisis period. The sample period covers Q1:2004-Q2:2008 and the crisis period covers Q3:2007-Q2:2008. The dependent variable in Column 1 is the logarithm of total FDIC-uninsured deposits. The dependent variable in Column 2 is the logarithm of total FDIC-insured deposits. The dependent variable in Column 3 is the logarithm of number of FDIC-uninsured deposit accounts. The dependent variable in Column 4 is the logarithm of number of FDIC-insured deposit accounts. Definitions of all control variables are shown in the notes of Table 1.3. Robust *t*-statistics are presented in parentheses. Standard errors are clustered at the bank-level. *** p<0.01, ** p<0.05, * p<0.1.

Figure A1.2: Average interest rate of deposits of MA-DIF member banks and other banks headquartered in Massachusetts



Notes: This figure presents average interest rate of deposits of MA-DIF member banks and non-member banks headquartered in Massachusetts. The dashed line represents the average interest rate of deposits of the MA-DIF members, while the solid line represents the average interest rate of deposits of non-member banks. The shaded area indicates the crisis period (Q3:2007-Q2:2010).

Figure A1.3: Total deposits of state-chartered savings banks and other banks in the five states surrounding Massachusetts (2004-2015)



Notes: This figure shows total deposits of state-chartered savings banks and other banks headquartered in the respective five states surrounding Massachusetts in 2004-2015. The dashed line represents the total deposits of state-chartered savings banks, while the solid line represents the total deposits of all other banks. The shaded area indicates the crisis period (Q3:2007-Q2:2010). Total deposits are scaled by 1,000,000.

CHAPTER 2

Guns, Mass Shootings and Deposits: Do Depositors Discipline "Loaded" Banks?

2.1 Introduction

In this chapter, we link deposit flows to consumer boycotts that originate from two topics of broad and current interest in the U.S.: gun controls and mass shootings.[1] Our paper investigates whether mass shootings affect the deposit growth of banks that have publicly known lending relationships with gun manufacturers. In other words, we ask whether depositors discipline gun-related banks by withdrawing deposits after mass shootings. If they do, where do they transfer their deposits to? Is the effect of mass shootings sufficiently large to affect bank value? We examine these questions by exploring three prominent mass shootings in the U.S. between 2013 and 2018.

Answering these questions is important for two reasons. First, they address the growing recognition of corporate social responsibility (CSR) and environmental, social, and governance (ESG) risk in banking. With the rise of social awareness, public sentiment becomes ever less tolerant of inappropriate bank practices. However, there is still limited evidence for how this form of dissatisfaction channels into a potential threat to the financial condition of banks. Our paper fills this gap by documenting the effect of depositor boycotts on banks' deposit growth. This

[1] Boycott campaigns targeting gun-related corporations are becoming increasingly widespread. These campaigns gain force with the occurrence of high-profile mass shootings. A prominent example is the boycott campaign against the National Rifle Association (NRA), and its related companies after the Stoneman Douglas High School shooting in Parkland, Florida (14th February 2018). The campaign calls for companies to terminate their relationships to the NRA. It also resulted in several companies ending business relationships with the NRA, such as Delta Air Lines. This campaign is well-documented in news sources, e.g., in the Financial Times, the Washington Post, and the Wall Street Journal.

finding contributes to the current debate about the rule proposed by the Office of the Comptroller of the Currency (OCC), aiming to prohibit large banks from denying lending to controversial industries, such as gun and oil drilling industry.[2] Our results highlight the potential deposit loss of banks being forced to fund perceptually unsustainable and unethical industries. Second, the questions shed light on a fundamental economic question: the role of identity in economic decision making (Akerlof and Kranton, 2010). While the banking literature rarely considers identity as the main driving force of individual behavior, our work examines the role of identity for depositors. This paper therefore bridges the gap between the field of identity economics and banking.

Despite the seemingly simple research questions, we face three major obstacles to empirically examine the reaction of depositors to gun-related banks after mass shootings. First, depositors must be able to distinguish which banks provide funding for gun manufacturers. Second, the mass shooting must be sufficiently prominent to attract public attention. Third, other confounding events potentially drive the deposit growth of banks, especially for big banks, and such confounding events need to be disentangled from the causal effect arising from mass shootings.

We overcome the first challenge by exploiting media coverage of two letters written by Chicago Mayor Rahm Emanuel urging TD Bank and Bank of America to stop financing gun manufacturers. In these letters, Rahm Emanuel revealed that TD Bank had a $60-million revolving line of credit with Smith & Wesson, and Bank of America had a $25-million letter of credit relationship to gun maker Sturm, Ruger and Company Inc. The media widely reported on these two letters on 25th January 2013, exposing that TD Bank and Bank of America are funding gun manufacturers. In addition, the New York City Public Advocate, Letitia James, urged TD Bank to stop funding gun manufacturers in December 2015. Moreover, there was a protest in front of the TD Bank branch on West 68th Street, New York, on 8th December 2015 demanding the bank to stop doing business with gun makers. With such widespread media coverage, Bank of America and TD Bank are expected to be the targeted banks, if depositors indeed discipline banks financing gun manufacturers after mass shootings. Importantly, this setting does not have to assume depositors keep remembering the news in 2013. As long as depositors are motivated to search for which banks are funding gun manufacturers on the internet, they can identify these two banks through widespread media coverage.

We overcome the second obstacle by identifying three high-profile mass shootings which drew great attention. These three cases include the Orlando nightclub shooting, the shooting in Las Vegas, and the Stoneman Douglas High School shooting. We select these shootings based on

[2]The rule was proposed by the OCC on 20th November 2020. It was then quickly finalized on 14th January 2021, despite of the considerable opposition from the banking industry. On 28th January 2021, the OCC put the rule on hold for reviews by the new administration. For further details, please see *https://www.occ.gov/news-issuances/news-releases/2021/nr-occ-2021-14.html*.

the number of fatalities and the corresponding media attention. Our selection maximizes the likelihood that an average U.S. citizen is aware of the occurrence of shootings. In additional tests, we relax these selection criteria to check whether our baseline results are also applicable to a standard definition of mass shootings, i.e., shootings that result in three or more killings in a single incident.

We tackle the final challenge using branch-level data of deposits from the Summary of Deposits. Deposit growth of national banks, like Bank of America and TD Banks, is likely to be driven by confounding factors in the years of the mass shootings. Therefore, detailed geographical information of each branch in the Summary of Deposits allows us to examine heterogeneous responses of depositors located in different proximity to the incident to examine the effect of mass shootings. Our identification strategy builds on Newman and Hartman (2019); Cuculiza et al. (2021). Newman and Hartman (2019) find that public support for stricter gun control increases with the proximity to a mass shooting. Cuculiza et al. (2021) show that negative incidents, such as mass shootings, weaken the sentiment of equity analysts, resulting in more pessimistic forecast and the effect is stronger for analysts near the events. Following this line of the reasoning, we argue that observing a mass shooting near one's community provides greater incentives for depositors to discipline banks that are funding gun manufacturers. In other words, if the negative deposit growth of related banks is genuinely caused by mass shootings, we expect the branches operated by affected banks located in close proximity to the incidents to suffer greater negative deposit growth in the years of the shooting. The geographical information in the Summary of Deposits allows us to test this hypothesis.

We arrive at three main results. First, branches operated by banks financing gun manufacturers experience 2% lower deposit growth in the years of the mass shootings. To rule out that confounding factors contribute to the negative deposit growth, we investigate whether the reduction in deposit growth is greater for branches located near the incidents. The reduction of deposit growth is indeed greater for branches operated by banks located in the same state of the shootings; the same county of the shootings; and the fifth percentile ($p5$) distance within the incidents.[3] A gun-related branch located in the same state (county) (within $p5$ distance of the incident) recorded an additional 0.7% (1.9%) (3.8%) negative deposit growth in the years of the shootings. Our findings are consistent with the hypothesis that depositors discipline banks that are funding gun manufacturers after mass shootings, and the results are more pronounced among depositors near the shooting incidents.

Second, savings banks benefit from depositor boycotts. Our tests show that savings banks located near the mass shootings experience greater deposit growth in the years of incidents. This

[3] We calculate the distance of all branches in the U.S. to each shooting incident. A branch is within the $p5$ distance within a shooting incident if the calculated distance is at the fifth percentile of the entire sample.

is intuitive because depositors who discipline banks that fund gun manufacturers can either transfer deposits to other banks or leave the banking system. If most of the depositors remain in the banking system, this group of depositors are expected to transfer their deposits to local saving banks, instead of other national banks, like Bank of America and TD Bank because savings banks are less likely to have a relationship with gun manufacturers, and, to a lesser extent, are less likely to have a publicly known relationship with gun manufacturers.

Third, there is no evidence that the mass shootings trigger adverse effects for the market value of affected banks. Our event study results show that the share prices of affected banks do not record abnormal negative returns after the shootings. This finding highlights the heterogeneous responses between depositors and shareholders. The difference in composition of shareholders and depositors plausibly explains this disparity. Depositors of local branches mainly constitute individuals and businesses around the local areas and residing in the U.S., they are expected to be more emotionally affected by the mass shootings than shareholders around the globe.

Deposit-taking is one of the distinctive features of banks (Kashyap et al., 2002) and banks are largely financed by deposits (Hanson et al., 2015).[4] Intuitively, depositors should have played an important role in contributing to the stability of the banking system by penalizing banks for excessive risk.[5] However, the existence of depositor discipline is questionable in the real world with deposit insurance and implicit government guarantee. To answer this question, a body of literature examines depositor discipline in response to financial information, the findings show that uninsured depositors discipline distressed banks by either withdrawal or asking for a higher deposit interest rate (e.g., Martinez Peria and Schmukler (2001); Goldberg and Hudgins (2002); Maechler and McDill (2006); Bonfim and Santos (2020); Chen et al. (2020); Iyer et al. (2019)). Recent works also show that non-financial factors affect depositors' response to financial information, such as press rumours (Hasan et al., 2013), depositors' social networks, and bank-depositor relationships (Iyer and Puri, 2012; Iyer et al., 2016). However, all these studies are based on depositors' reactions to potential financial loss. Without any risk of financial loss, what are other factors driving depositors' behaviours? This paper fills this gap by showing depositor discipline driven by the conflict between banks' investments and depositors' identity.

Akerlof and Kranton (2010) argue that identity is one of the critical factors in economic decisions. Economic agents are inclined to make decisions in line with their identity in society.[6]

[4]Hanson et al. (2015) document that deposits have financed 80% of bank assets on average with an annual standard deviation of 8% from 1896 to 2012 in the U.S.

[5]Apart from depositors, market discipline is also exerted by shareholders (e.g., Bliss and Flannery (2002); Schaeck et al. (2012)) and other debt holders, such as subordinated debt holders (e.g., Flannery and Sorescu (1996); Goyal (2005).

[6]Identity includes different aspects that constitute a person's sense of self, such as gender, race, and core values

We link this argument to the banking literature by showing that depositors' behavior after mass shootings is driven by one's identity. Consistent with our argument, Homanen (2018) documents that banks which finance a controversial construction project, the Dakota Access Pipeline, experience a decrease in deposit growth. Similarly, Chen et al. (2020) show an increased likelihood of large deposit outflows after the announcement of the banks' inferior performance ratings for community development services. Our paper differs from these studies by considering two ongoing and deeply rooted issues in the U.S.-gun controls and mass shootings by examining the effects of mass shootings on banks.

Our research also adds to the literature on corporate social responsibility (CSR) and environmental, social and governance (ESG) in banking. A growing literature highlights the effect of CSR and ESG of corporations on bank loans. Goss and Roberts (2011) find that loan spreads are higher for firms with lower corporate social responsibility investments, and Hasan et al. (2014) show that tax avoidance increases firm loan rates. Kleimeier and Viehs (2018) document that firms disclosing larger carbon emissions face higher cost of credit than firms with low emissions. As delegated monitors, banks possess unique skill sets in assessing the ability of the firm to repay its debts, the findings thus highlight the value of CSR and ESG to borrower firms. Under the increased scrutiny of regulatory authorities, media and social activists, the findings could also be the results of banks internalizing the cost of funding "unethical" firms, yet we still know little about the cost of "unethical" investment faced by banks.

While a large body of literature study the relationship between CSR and financial performance of firms, for example, Servaes and Tamayo (2013) finds that while CSR increase firm value for firms with high customer awareness, the effect cannot be found in firms with low customer awareness; Flammer (2015) shows that approved proposals for CSR lead to positive abnormal stock returns; Albuquerque et al. (2019) develop an industry equilibrium model predicting that CSR decreases systematic risk and increases firm value, only a few studies discuss the impact of CSR and ESG on banks.[7] Wu and Shen (2013) shed some light into this issue and find that banks that perform better in CSR have superior financial performance and lower non-performing loans. Cornett et al. (2016) also find that financial performance of banks in the U.S. is positively related to CSR score. However, both do not identify the channel of how CSR affects banks' financial performance. Our work contributes to this debate by identifying the deposit channel of how CSR and ESG affect banks.

We also contribute to studies in related disciplines. There is a large literature on consumer

(Akerlof and Kranton, 2010). The concept of identity in this paper focuses on one's social identity for gun issues in the U.S..

[7]Gillan et al. (2021) provide a detailed review of the financial economics-based research on the effects of ESG and CSR on corporate finance.

boycotts in sociology and management. These studies document the process of boycotts, discuss the motivation of boycotts, and evaluate the effects of boycotts (e.g., Friedman (1985); Nathanson (1999); John and Klein (2003); Klein et al. (2004). Other studies employ event-study methodology to evaluate the effect of protests and social movements on stock prices of affected firms. The results are heterogeneous (e.g., Pruitt and Friedman (1986); Pruitt et al. (1988); Davidson III et al. (1995); Koku et al. (1997); Teoh et al. (1999).[8,9] Our paper integrates these different strands of literature into the banking literature by examining the effects of depositor boycotts on banks.

Our paper is timely in addressing the rising social movement against gun-related companies. Prior work on gun control focuses on the socio-economic factors in affecting the preference towards gun control (Wright, 1975; Lizotte and Bordua, 1980; Dixon and Lizotte, 1987); the effectiveness of gun control in reducing gun-related crime (Wolpert and Gimpel, 1998; Kleck and Patterson, 1993; Jacobs, 2002; Celinska, 2007; Lott, 2013); and political economics of gun control (Sears and Funk, 1991; Spitzer, 2015; Luca et al., 2020). However, the literature does not evaluate the effectiveness of boycotts against gun-related corporations, especially for the movement against banks. To the best of our knowledge, this paper is the first to document the effect of depositor boycotts driven by mass shootings and evaluate their impact on bank value.[10]

The remainder of the paper is structured as follows: Section 2.2 describes the institutional background. Section 2.3 develops hypotheses, and Section 2.4 details the data. In Section 2.5, we present our findings on deposits, and we discuss the event study findings in Section 2.6. Section 2.7 concludes.

2.2 Background

The U.S. has a unique history of gun-related issues. While carrying and keeping guns is usually illegal in most countries, the Second Amendment of the United States Constitution authorizes U.S. citizens to keep and bear arms. Although state authorities can enact state laws to restrict the accessibility of guns, guns remain relatively accessible. The U.S. also has the largest number of mass shootings globally. The public therefore often relates these two phenomena to conclude that gun control is the solution to avoid mass shootings. This viewpoint may not be valid

[8] Pruitt and Friedman (1986); Pruitt et al. (1988); Davidson III et al. (1995) find a negative effect of consumer boycotts on stock price of affected firms, while Koku et al. (1997) show a positive abnormal return of affected firms after the announcement of the boycott. Teoh et al. (1999) find that the effect is statistically insignificant.

[9] Apart from event studies, few papers study the effect of consumer boycotts driven by the Iraq war on French wine sale (Chavis and Leslie, 2009; Ashenfelter et al., 2007; Bentzen and Smith, 2007). Chavis and Leslie (2009); Bentzen and Smith (2007) show that the boycott led to a drop of French wine sales in the U.S., while Ashenfelter et al. (2007) finds no statistically significant effect of the boycott.

[10] Another related literature concerns the impact of mass shootings on gun policy (e.g. Muschert (2007); Fox and DeLateur (2014); Metzl and MacLeish (2015); Newman and Hartman (2019); Luca et al. (2020).

from an academic perspective, but mass shootings frequently boost public support for tightening gun control (Goss, 2010; McGinty et al., 2013; Newman and Hartman, 2019; Luca et al., 2020).

To illustrate the effect of mass shootings for the attention on the debate about gun controls, Figure 2.1 shows the Google trend index for the keywords “gun control” and “mass shootings” in the respective states of the three high-profile mass shootings detailed in Section 2.2.2 below. Similar patterns can be observed for all three cases. There is no particular interest in mass shootings and gun controls during the 90 days prior the respective shooting, but the indices for the keywords “gun control” and “mass shootings” surge during the 90 days after the shootings and reach their peak on the day of the shooting. Considering the random nature of mass shootings, the figure suggests that mass shootings fuel the debate on gun control.

As a traditional way of demanding tighter gun controls, the general public exerts pressure on their political representatives to enact laws. Fuelled by the development of internet and social media, people now increasingly participate in boycotting gun-related corporations after mass shootings. It is not only an act of showing dissatisfaction, but also signals efforts to isolate the gun industry. However, it is difficult for consumers to identify companies related to the gun industry. The situation becomes even more complicated when the subject of boycotts are banks because banks’ client data is normally confidential, and the general public face difficulty to identify which banks finance gun manufacturers.

2.2.1 Identification of banks financing gun manufacturers

In 2013, Chicago Mayor, Rahm Emanuel wrote two letters to urge TD Bank and Bank of America to stop financing gun manufacturers after a mass shooting caused twelve fatalities in Aurora, Colorado, on 20^{th} July 2012. His letters revealed that TD Bank had a 60-million USD revolving line of credit with gun manufacturer Smith & Wesson; and Bank of America had 25-million USD letter of credit to gun maker Sturm, Ruger and Company Inc. This event was well-documented in the news. Most of the news articles are available online, e.g., in the New York Times, the Washington Times, and on CNN. Being the only two banks whose relationships with gun manufacturers are highlighted in reliable sources and widely reported in the media, they are likely to be the target of boycotts, assuming depositors do discipline gun-related banks after mass shootings.

2.2.2 Identification of high-profile mass shootings

According to the Investigative Assistance for Violent Crimes Act of 2012, a mass killing is defined as three or more killings in a single incident.[11] In line with this definition, there were 220 mass

[11]Please see *https://www.congress.gov/112/plaws/publ265/PLAW-112publ265.pdf* for the original document.

shootings between 1st July 2013 and 30th June 2018.[12] Some of them may not be prominent enough to attract the awareness of depositors. Apart from the number of fatalities, the nature of the shootings also affects the response of depositors. For instance, gang shootings may attract less attention, compared with random shootings. To ensure the shootings are sufficiently prominent to draw depositors' attention, we select three mass shootings which generate a tremendous response in society, to test whether depositors withdraw their funds from banks financing gun manufacturers in response to these shootings.

There are 2 criteria for the selection. First, we rank the number of fatalities of each mass shooting and pick out the shooting cases that exceed the 95th percentile of the number of deaths in a mass shooting. Figure 2.2 shows that the 95th percentile of the number of deaths in a single case is 9, thus we select all cases above 9 fatalities to conduct the next step of our selection. The details of these shootings are described in Table A2.2 in the appendix. The main purpose for selecting the top three cases is to make sure that the shootings are prominent enough to motivate depositors to discipline related banks. Therefore, the last step of the selection process requires a media search in the Vanderbilt Television News Archive (VTNA).[13] We count the number of the name of the mass shooting appearing on evening news programs of the five major television networks—ABC, CBS, NBC, CNN and Fox News on the day of the shooting and the following 30 days, then pick the top-three as the key shooting cases in our study. To address concerns over the selection criteria, we present additional tests to show that our result is robust to the inclusion of other cases leading to three or more fatalities in Section 2.5.5. The findings remain consistent with our baseline results. The magnitude of the negative deposit growth increases with the number of deaths; the number of injuries; and the media attention of mass shootings.

These three high-profile mass shootings include the Orlando nightclub shooting, the Las Vegas shooting, and the Stoneman Douglas High School shooting. Figure 2.3 presents the timeline of these three shootings.

On 12th June 2016, a shooter killed 49 people and injured 53 others in a mass shooting inside a nightclub in Orlando, Florida, (the red dot in Figure 2.4 shows the location of the shooting). At that time, this was the deadliest mass shooting in the history of the U.S..

On 1st October 2017, a gunman opened fire at a music festival in Clark, Nevada (the red dot in Figure 2.5). The gunman killed 58 people and wounded 441.

[12]Information about mass shootings is extracted from the Gun Violence Archive, a non-profit research group with an accompanying website and social media delivery platforms which documents every incident of gun violence in the U.S.. The database reports the date, the geographical information, the number of fatalities, and the number of injuries of each mass shooting incident.

[13]This source has been employed in other studies (e.g. Eisensee and Strömberg (2007) and Luca et al. (2020)).

On 14th February 2018, a gunman shot at Marjory Stoneman Douglas High School in Parkland, Florida (the red dot in Figure 2.6) killing 17 students and staff members and injuring 17 others.

These three shootings generated a huge impact in motivating the debate about gun control. As expected, TD Bank and Bank of America were urged to cease the lending relationship with gun manufacturers after these shootings. For instance, New York City Public Advocate, Letitia James urged people to stop financing TD Bank after the Orlando nightclub shooting. After the Stoneman Douglas High School shooting, Bank of America was under a significant pressure of stop lending to gun manufacturers and eventually responded by ending the relationships with manufacturers producing military-style weapons.

2.3 Hypothesis Development

Hypothesis 1 focuses on the potential effect of depositor boycotts on the deposit growth of the banks funding gun manufacturers. Hypothesis 2 centres on the potential effect of mass shootings on the market value of the related banks.

2.3.1 Hypothesis 1

The starting point for Hypothesis 1 is Tajfel (1981), highlighting that social identity is formed by different aspects of an individual's self-understanding which results from membership in a social group, combined with the significance one attaches to that membership. Akerlof and Kranton (2010) incorporate social identities into economics by expanding the standard utility function to include identity utility. They posit that apart from economic incentives, one's identity also drives economic decisions. In line with this argument, Shiller (2017) argues that human behavior is directed by narratives within groups that are used to explain phenomena, regardless of any factual basis.

With polarized opinions on gun control, U.S. society forms a unique social identity, an identity in terms of whether individuals are in favour of tightening gun control or not. This form of identity is documented to have influence on government policies and election results (Lacombe, 1988; Lacombe et al., 2019). These two groups disagree on various topics related to firearms. Importantly, they have different interpretations for the role of guns in mass shootings. The anti-gun group blames easy access to guns to be the root cause of mass shootings.

Against this background, we expect anti-gun individuals to respond by severing any ties with gun-related corporations after mass shootings, and banks with lending relationships with gun manufacturers are one of them. It is plausible that the pro-gun group rewards gun-related corporations after mass shootings to counteract the response of the anti-gun group. However, the boycotting force from the anti-gun group is likely to be larger for two reasons. First, negative information is far more effective in convincing others to boycott than positive information (Kam and Deichert, 2020). In other words, the incentive for anti-gun depositors to discipline gun-related banks should be stronger than for pro-gun depositors to reward gun-related banks. Second, the overall population in favour of stricter gun control is larger and keeps increasing over time. The overall force from the anti-gun group should therefore be stronger. Taken together, we expect deposit growth of the gun-related banks to drop after mass shootings.

Hypothesis 1a: Mass shootings have negative effect on deposit growth of banks financing gun manufacturers.

Hypothesis 1b: Mass shootings do not have negative effect on deposit growth of banks financing gun manufacturers.

Alternatively, the relationship with gun manufacturers does not suggest any concern over the soundness of the gun-related banks after mass shootings. Thus, depositors have no financial incentive to withdraw deposits from the gun-related banks. Even if depositors have the incentive to boycott gun-related banks, the actual effect on deposit growth is subject to two constraints. First, switching and search costs may deter depositors from disciplining banks (Yorulmazer, 2014). Second, consumer boycotts are subject to coordination concerns, the free-riding issue causes uncertainty over the effectiveness of consumer boycotts (Sen et al., 2001; John and Klein, 2003). Therefore, deposit growth of gun-related banks may not decrease after mass shootings.

2.3.2 Hypothesis 2

Hypothesis 2a builds on the theory by Freeman (2010). He suggests that most of the corporate decisions involve aligning the interests of different stakeholders of the organizations.[14] Based on this rationale, failure to avoid and resolve conflicts between different stakeholders could therefore incur additional cost for firms. Fombrun and Shanley (1990) posit that different groups of stakeholders construct the reputation of a corporation based on their economic and non-economic criteria, and the reputation of a firm reflects its ability in fulfilling the requirements and expectations of different stakeholders. Since reputation is an intangible asset that firms use

[14] A stakeholder is broadly defined to be any party that is influenced by a corporation, such as employees, suppliers, customers, local communities, social activists, environmental organizations, and governments.

to create shareholder value (Deephouse, 2000; Sanders and Boivie, 2004), any damage to a firm's reputation could plausibly be reflected in its share price.

Hypothesis 2a: Mass shootings have negative effect on share prices of banks financing gun manufacturers.

Hypothesis 2b: Mass shootings do not have negative effect on share prices of banks financing gun manufacturers.

Mass shootings acts as a catalyst to deepen and expose the conflict between different stakeholders of the affected banks. The costs of the conflict arise from the following issues: the first-order effect of mass-shootings on the affected banks is the threat of boycotts by investor and consumer activism. While consumer boycotts impose financial costs on funding and revenue of banks, investor boycotts pose a direct downward force on the stock price of affected banks. The second-order effect originates from market expectations: conflicts between stakeholders of the affected banks send a negative signal to investors about a bank's future performance. This negative expectation could be formed out of different reasons, such as the expected change in government regulations after mass shootings, the effect of boycotts on banks' financial performance, and doubts about managers' abilities. The stock price of the affect banks therefore reflects these potential effects of mass shootings by showing negative abnormal movements in stock price.

However, there are several reasons to suspect whether mass shootings influence the share price of the affected banks. First, the composition of shareholders and depositors are different. A key assumption of the paper is that increased proximity to a mass shootings intensifies emotional responses (Newman and Hartman, 2019), we therefore expect shareholders around the globe are less driven by mass shootings emotionally, but more driven by the financial effects of mass shootings on banks. From this perspective, the participants of consumer and investor boycotts could be too limited to pose any threat to related banks' future income stream because the affected banks are national banks with a diversified consumer base. It is therefore plausible that the direct and indirect effects of the boycott would remain muted on the market value of the affected banks. Moreover, mass shootings do not provide new information about banks. According to a semi-strong form of the efficient market hypothesis, publicly available information is already priced in firms' stock price (Sharpe, 1970). In the context of our paper, depositor boycotts are based on publicly available information about the relationship between the banks and gun manufacturers, and mass shootings occur frequently in the U.S. Thus, the potential effect of mass-shootings on gun-related corporations may already be reflected in their stock price once their relationship with gun manufacturers is exposed.

2.4 Data

The Summary of Deposits (SoD), available from the Federal Deposit Insurance Corporation (FDIC), is the best publicly available dataset to implement our identification approach. The dataset is based on an annual survey of branch office deposits as of 30th June for all branches operated by FDIC-insured institutions. All FDIC-insured institutions with branch offices are required to complete the survey by 31st July. The dataset includes detailed geographical information of branches, including the state, county, latitude, and longitude of branches.

To avoid abnormal deposit growth, all branches with more than 1 billion USD and less than 100,000 USD deposits are excluded from the dataset. We also exclude branches which changed either banks or bank holding companies during the sample period. This process avoids abnormal deposit growth caused by mergers and acquisitions. Considering the importance of geographical information for our study, all branches with missing geographical identifier and contradictory geographical information are excluded. We further exclude branches in inhabited territories of the U.S. to avoid outliners for the distance variables and missing information of county-level data.[15] After the sample screening, we obtain a clean sample with observations for 406,180 observations operated by 77,700 branches of 5,734 banks between 2013 and 2018.

To control for bank characteristics, we obtain bank-level data from the Call Report. The Call Report is a quarterly dataset recording balance sheet and income statement items of each bank. We obtain the data of each year for each bank for the fourth quarter and match the Call Report data to the SoD by each individual bank's unique identifier. All control variables in all regressions in this paper are lagged by one year.

To capture geographical heterogeneity in the support of gun control, four county-level measures are employed, including the number of gun stores per capita extracted from the Bureau of Alcohol, Tobacco, Firearms and Explosives, gun-related deaths per capita obtained from CDC WONDER, the 2016 presidential election results extracted from the MIT election Data Science Lab, and the proportion of the population with a bachelor's degree or higher education obtained from the U. S. Census Bureau.

Summary statistics are shown in Table 2.1. Variable definitions are presented in Table A2.1 in the appendix.

[15] Inhabited territories of the U.S. include American Samoa, Guam, Northern Mariana Islands, Puerto Rico and Virgin Island.

2.5 Empirical Results: Deposit Growth

In this section, we examine Hypothesis 1. Our first exercise is a preliminary analysis to test whether branches operated by the affected banks experience lower deposit growth in the years of the three prominent mass shootings. Next, we utilize several geographical variables to determine whether the lower deposit growth of the affected banks is attributed to the mass shootings.

2.5.1 Baseline results

2.5.1.1 Effect on branches operated by related banks

Using branch-level data, we estimate the following equation to examine whether depositors discipline TD Bank and Bank of America for funding gun manufacturers after the mass shootings:

$$DepositGrowth_{i,b,t} = \beta_0 + \beta_1 AffectedBank_b \times ShootingYear_t + \gamma X_{b,t} + \delta_i + \delta_t + \varepsilon_{i,b,t} \tag{2.1}$$

where $DepositGrowth_{i,b,t}$ refers to deposit growth of branch i operated by bank b at year t. $AffectedBank_b = 1$ if branch i is operated by either TD Bank or Bank of America, 0 otherwise. $ShootingYear_t = 1$ if the observation is either in 2016 or 2018, 0 otherwise. The interaction term, $AffectedBank_b \times ShootingYear_t$, equals to 1 if the observation is a branch operated by either TD Bank or Bank of America in either year 2016 or 2018, 0 otherwise. β_1 is our coefficient of interest, a negative coefficient of β_1 support Hypothesis 1a.

$X_{b,t}$ is a vector of time-varying control variables which include the logarithm of total assets; the average interest rate on deposits; return on assets; the charge off ratio; and the equity capital ratio. δ_i is a branch-fixed effect which captures branch-specific factors. δ_t is a year-fixed effect. This battery of fixed effects allows us to rule out unobservable factors that might drive deposit growth. We cluster heteroskedasticity-adjusted standard errors on the branch level to account for serial correlation within each panel.

Column 1 of Table 2.2 shows the results of equation 2.1 excluding our control variables. Deposit growth of the affected banks' branches declines by 3.3% in the years of the mass shootings. In column 2, we include bank-level control variables, ensuring the bank-level characteristics do not drive our results. The estimated coefficient of interest in column 2 is similar to the one in column 1, suggesting that affected banks experience 2% (t-statistic -16.15) lower deposit growth in the years of the mass shootings. While branch-level deposits and average growth rates in the sample are 70,308,000 USD and 6.9% respectively, a 2% negative deposit growth rate implies a loss of 1,406,000 USD per branch. Considering Bank of America and TD Bank have 4,934 branches and 1,242 branches respectively in our sample, the loss of deposits accounts for 0.6% of total domestic deposits of Bank of America and 0.8% of total domestic deposits of TD Bank.[16]

2.5.1.2 Effect on branches operated by related banks in the shooting states

To rule out confounding factors, we exploit the uniqueness of branch-level deposit data to examine whether branches of TD Bank and Bank of America located near the shootings experience an even lower deposit growth rate in the years of the mass shootings with the following equation:

$$DepositGrowth_{i,b,t} = \beta_0 + \beta_1 AffectedBank_b \times ShootingYear_t + \beta_2 AffectedBank_b \times DistanceVariable_{i,b,t} + \beta_3 DistanceVariable_{i,b,t} + \gamma X_{b,t} + \delta_i + \delta_t + \varepsilon_{i,b,t} \tag{2.2}$$

where $DistanceVariable_{i,b,t}$ represents the respective measures of the proximity of branch i to the mass shootings in the following sections. In Section 2.5.1.2, $DistanceVariable_{i,b,t} = 1$ if branch i is in Florida in 2016 or 2018; or branch i is in Nevada in 2018, 0 otherwise. In Section 2.5.1.3, $DistanceVariable_{i,b,t} = 1$ if branch i is in Orange, Florida, in 2016; or branch i is in Clark, Nevada, in 2018; or branch i is in Broward, Florida, in 2018, 0 otherwise. In Section 2.5.1.4, $DistanceVariable_{i,b,t} = 1$ if branch i is located within a certain percentile of the distance to the mass shootings and the observation is in the year of the respective mass shooting, 0 otherwise. All other notations follow equation 2.1. β_2 is our coefficient of interest, a negative β_2 support Hypothesis 1a, suggesting that branches of TD Bank and Bank of America have lower deposit growth rate in the years of the three prominent mass shootings, especially in the areas near the mass shootings.

The results in column 3 support Hypothesis 1a, the branches operated by Bank of America and TD Bank record an additional 0.7% (t-statistics -1.82) drop in deposit growth in the mass shooting states in the years of the shootings. The result reinforces the argument that the lower deposit growth is attributed to the reaction of depositors towards the mass shootings.

2.5.1.3 Effect on branches operated by related banks in the counties where shootings took place

We now further extend our analysis to examine the deposit growth of the affected branches operated in the counties of the incidents.

[16] Across the sample period, the average total domestic deposits of Bank of America are about 1.2 trillion, and total domestic deposits of TD Bank are about 200 billion in the sample period.

The result in column 4 of Table 2.2 shows that the branches operated by Bank of America and TD Bank in the counties of the incidents record an additional 1.9% (t-statistics -2.17) negative deposit growth in the years of the three shootings. Compared with the result in column 3, which captures the interaction term of the affected branches and the affected states, the magnitude of the coefficient is higher. It is consistent with the conjecture that the response of depositors is stronger for depositors nearer the incidents.

2.5.1.4 Effect on branches operated by related banks within a certain distance to the shootings

Using state and county to measure the proximity to the mass shootings can be misleading because certain areas can be very close to the place of the shooting, but not necessarily in the same state or county of the shootings. To mitigate such measurement errors, we use the distance between each branch and the respective incidents as an alternative measure of proximity. After we calculate the distance between each branch and the respective incident, we separate branches into different groups, according to the percentile of the minimum distance to the incidents. We then estimate equation 2.2 and obtain the following results.

Column 5 of Table 2.2 suggests that the branches within the 5^{th} percentile distance of the mass shooting recorded 3.8% (t-statistics -11.17) lower of deposit growth in the years of the three shootings. Column 6 highlights that the effect of mass shootings for branches located within the 10^{th} percentile of the shootings reduces to 2.3% (t-statistics -8.55). The effect is similar for branches located within the 15^{th} percentile of the shootings, shown in column 7 of Table 2.2. Figure 2.7 illustrates the diminishing magnitude of the coefficient of interest from the 5^{th} percentile distance to the 10^{th} percentile. From the 10^{th} percentile onwards, the estimated coefficient of interest becomes stable.

The estimation results in this section suggest that the branches operated by TD Bank and Bank of America near the shooting incidents experience lower deposit growth in the respective year of mass shootings and the deposit growth of the affected branches decreases with the proximity to the incidents. This pattern could hardly be explained by any other confounding factors, the results in this section therefore support Hypothesis 1a.

2.5.2 Sub-sample analysis

Our baseline results include all branches in the U.S.. We now conduct a sub-sample analysis, presented in Table 2.3, to address concerns over the heterogeneities of different areas.

We separate the sample according to the location of branches. In column 1 of Table 2.3, the result in Panel A (B) (C) presents the results with the sample of all branches in Florida (Nevada) (Florida) in 2013-2018 to investigate whether the affected branches in the respective states record a lower deposit growth in the respective year of the mass shooting.

The results are consistent with our baseline result, affected branches record a lower deposit growth in the respective year of the three mass shootings in the affected state. All results are significant at conventional significance levels, except for the shooting in Nevada. This is plausibly caused by the small number of affected branches in Nevada. Also, the Las Vegas shooting is the one with the least media attention among the three high-profile shooting cases, despite the highest number of fatalities.

We next repeat the exercise by restricting the sample to branches located within the 5^{th} and the 1^{st} percentile distance to the respective shootings in column 2 and column 3 of Table 2.3. The results support the view that the related branches within the 1^{st} and the 5^{th} percentile to the shootings experience a lower deposit growth rate in the year of the shooting.

The results in the sub-sample analysis are consistent with our main analysis. Branches operated by the banks financing gun manufacturers experience lower deposit growth. The magnitude is also consistent with the baseline results, suggesting that the specification of our baseline tests does not bias the coefficients downward.

2.5.3 Savings Banks

So far, we document that the affected branches near the mass shootings experience lower deposit growth in the years of mass shootings. We now explore spillover effects.[17]

We argue that the lower deposit growth of affected banks reflects their relationships with gun manufacturers. Thus, the motivation for depositors to withdraw deposits from affected banks should be unrelated to bank soundness. Compared with national banks, local savings banks should be less likely to maintain relationships with gun manufacturers, and, to a lesser extent, it is less likely that any such relationship would be publicly revealed. We therefore expect that branches of savings banks near the incidents to have higher deposit growth in the respective year of the three mass shootings.

[17]Homanen (2018) finds that savings banks experience an additional positive deposit growth out of the depositor movement against banks that finance a controversial infrastructure.

To investigate whether savings banks are the beneficiaries of the depositor boycotts following the three mass shootings, we replicate the baseline results using equation 2.2, while replacing $AffectedBank_i$ with $SavingsBank_i$, a dummy variable that indicates whether a branch is operated by a savings bank.

The results in Table 2.4 suggest that savings banks near the incidents of mass shootings experience additional deposit growth in the years of the mass shootings. The results reinforce our view that depositors withdraw deposits from those affected banks due to concerns over their relationship with gun manufacturers, suggesting that savings banks are the beneficiaries of the depositor boycotts.

2.5.4 Factors amplifying depositor boycotts

We now study the factors reinforcing the response of depositors following the three high-profile mass shootings. The exercises in this section are based on the expectation that depositors who have an anti-gun identity are more likely to discipline banks financing gun manufacturers after mass shootings.

To this end, we collect four county-level variables that capture general preferences towards guns in different counties, including the number of gun stores per capita, gun-related deaths per capita, the election result of the 2016 presidential election, and the proportion of the population with a bachelor's degree or higher education. According to previous studies, counties with stronger negative beliefs towards guns tend to have fewer gun stores, to have more gun violence, to have more votes for the Democratic Party, and to have higher educational attainment. The effects on affected banks are therefore expected to be stronger in these counties. We estimate the following equation:

(2.3)

$$DepositGrowth_{i,b,t} = \beta_0 + \beta_1 AffectedBank_b \times ShootingYear_t + \beta_2 AffectedBank_b \times ShootingYear_t \times CountyCharacteristics_b + \gamma X_{b,t} + \delta_i + \delta_t + \varepsilon_{i,b,t}$$

where all variables follow the definitions in equation 2.1, the only variation is the component of the interaction terms with $CountyCharacteristics_b$. In this section, we consider the characteristics of the county to reflect the response of depositors toward the affected banks following the three prominent mass shootings, therefore, β_2 is the coefficient of interest.

We first consider whether the response is relatively mild in counties with greater accessibility and use of guns. We approximate this by the number of gun stores per capita in a county. Studies point out that gun owners are less supportive of gun control (Kleck, 1996; Wolpert and Gimpel, 1998; Celinska, 2007). Thus, we expect counties with more gun stores to have a muted response toward the banks financing gun manufacturers after the mass shootings. The result in column 1 of Table 2.5 is consistent with our expectation. A one percent increase of gun stores per capita leads to an additional 1.1% (*t*-statistic 4.92) deposit growth rate of related branches in the years of the mass shootings.

Our next analysis uses gun-related death per capita as another dimension of testing the heterogeneity of depositors' responses. Places with more gun-related violence may have a more negative attitude towards guns. Thus, we expect depositors in counties with more gun related violence to respond stronger (Wright, 1975; Primm et al., 2009). Column 2 of Table 2.5 supports our expectation. A one percent increase in gun-related deaths per capita additionally results in a 2% (*t*-statistic -2.54) negative deposit growth rate of related branches in the years of the shootings.

We further examine the effect of political preferences in different counties. Studies suggest that Republicans tend to have less adverse views towards guns, compared with democrats (Filindra and Kaplan, 2016; Joslyn et al., 2017). We therefore expect the deposit outflow to be stronger in counties where the Democratic Party dominates the 2016 presidential election. The result in column 3 of Table 2.5 is consistent with our argument. A one percent increase in votes for the Democratic Party contributes to an additional 1.8% (*t*-statistic -2.09) negative deposit growth rate of related branches in the years of the mass shootings.

The final test in this section builds on the findings that people in the U.S. with higher educational attainment are more likely to support gun control legislation and are less likely to own guns (Wright, 1981; Kleck, 1996).[18] We expect the deposit outflow to be stronger in the counties with higher proportions of the population with a bachelor's degree or higher education. The result in column 4 of Table 2.5 matches our expectation. A one percent increase in the proportions of the population with a bachelor's degree or higher education contributes to an additional 1.3% (*t*-statistic -1.70) negative deposit growth rate of related branches in the years of the mass shootings.

2.5.5 Less severe mass shootings

This section considers the effect of less severe shootings on affected branches' deposit growth and whether the effect of mass shootings on affected banks is positively linked to the number of fatalities, the number of injuries, and the corresponding media attention.

[18]This finding is supported by recent surveys, e.g., the survey conducted by the Pew Research Center *https://www.pewresearch.org/fact-tank/2019/10/16/share-of-americans-who-favor-stricter-gun-laws-has-increased-since-2017/ft*$_1 9-10-16_{gunlaws_sizable-gender-education-differences-support-stricter-gun-laws_2}$/.

We first examine whether our baseline results are still valid when we consider all shootings causing three or more fatalities within the sample period. The following exercise replicates the estimation of equation 2.2, while defining $Distance_{b,t} = 1$ when branches are located within the 5$^{\text{th}}$ percentile distance of a shooting with three or more fatalities in the respective year of the shooting, 0 otherwise.[19] Considering these less severe killings, we examine whether these shootings also generate negative effects on deposit growth and whether the baseline results are biased by our selection of the three prominent mass shootings. We expect these less severe mass shootings to lower the deposit growth for related branches in the shooting years, but the magnitude of depositors' reaction is expected to be smaller than the reaction in the three major shocks in our main analysis.

The first column of Table 2.6 shows that branches operated by affected banks experience a 0.5% (t-statistic -3.15) lower deposit growth in the respective shooting year when we include all mass shootings with three or more fatalities as shocks to the related banks. Compared with the main results which only consider the three prominent mass shootings, the magnitude of the coefficient of interest is lower but still statistically significant. The result is consistent with the conjecture that depositors still react to the less severe shootings, but the responses are weaker.

To further explore the links between the severity of mass shootings and deposit outflows, we conduct two exercises that consider the severity of shooting based on the number of fatalities and the number of injuries in mass shootings.

Column 2 uses the logarithm of fatalities as a measure of severity. Before taking the logarithm of the number of fatalities, we add 1 to all county-level numbers of fatalities on all observations to ensure that counties without any mass shooting enter our empirical tests. The result suggests that a one percent increase in the number of fatalities reduces deposit growth by around 0.4%. Column 3 uses the logarithm of the number of injuries as an alternative measure of the severity of mass shootings. A one percentage increase in the number of injuries reduces deposit growth of related branches by 0.7%.

To explore the potential role of the media in amplifying the boycotts, we test whether the effect of mass shootings being more widely reported in national news results in greater effects. Column 4 of Table 2.6 shows that the greater media attention contributes to a stronger response towards related banks financing gun manufacturers. A one percent increase in media coverage led to around 0.4% of negative deposit growth for the related branches located within the 5$^{\text{th}}$ percentile of the incidents.[20]

[19] The variable *Shooting year* is omitted in the estimation, because every year in the sample period has mass shootings causing 3 or more deaths.

[20] In this test, we only consider the shooting cases caused over 9 deaths because the media attention of those less

Overall, the results in this section suggest that our baseline findings are not only applicable to high-profile mass shootings, but are also valid for less severe cases. Importantly, the reaction of depositors depends on the severity of the mass shootings and the corresponding media attention.

2.5.6 Falsification tests

This section presents two sets of falsification tests. The first test replicates the estimation in column 4 of Table 2.2 using equation 2.2, but the sample excludes counties experiencing mass shootings which caused three or more deaths in the sample period. We then investigate if randomly assigning three counties pseudo mass shootings in a random year triggers a negative deposit growth of affected branches in the years of the pseudo mass shootings.[21]

We estimate the regression, and save the coefficient and *t*-statistic on the variable of interest (Affected bank x Pseudo affected county) and repeat it 1,000 times to compute rejection rates of the null hypothesis that the coefficient on the variable= 0 at the 10%, 5% and 1% level. We also report mean coefficient and the average *t*-statistic on the variable of interest. The rejection rates in Panel A of Table 2.7 are low, and the average value of the coefficient on the variable of interest is close to 0. In short, the effect on deposit growth of the branches operated by banks financing gun manufacturers only arises in counties where actually happen mass shootings in the year of the observation, while there is no such effect for the affected branches in a pseudo "shooting county".

Our second falsification test focuses on the timing of the mass shootings. Panel B in Table 2.7 shows regressions that replicate our tests in column 4 of Table 2.2 with the full sample. We lag the mass shooting year period by t-1 and t-2. None of the interaction terms of the affected branches dummy with these 'pseudo mass shootings' enters significantly, reiterating our claim that the effect can only be observed in the affected branches in the years of the three prominent mass shootings.

2.6 Event Study Results

Thus far, we show that mass shootings contribute to negative deposit growth of the affected banks. We now focus on Hypothesis 2 and investigate the effect of mass shootings on banks' market value.

severe cases is limited and homogeneous.

[21]The test ensures that the randomly selected counties have a presence of branches operated by the affected banks.

We use event-study methodology to study the effect of the three mass shootings on the market value of banks that have publicly known relationship with gun manufacturers. Our empirical strategy proceeds in two steps. First, we examine cumulative abnormal returns (CARs) over 1-day, 3-day and 5-day event windows to estimate aggregate and average market reactions to the three shootings. Second, we examine whether the CARs are statistically significant. The estimation process of CARs is detailed in the appendix.[22]

We adopt the MSCI North America, the S&P 500, and the TSX composite index as proxies for the market portfolio. The S&P 500 and the TSX composite index are national indices for the U.S. and Canada, respectively. We select these two indexes as benchmark indices because the related banks of our study are listed in the U.S. and in Canada. To address concerns over potential effects of mass shootings on the stock prices of other firms in a country, we also employ the MSCI North America as a further benchmark index.

Irrespective of the choice of our benchmark indices and event windows, the results in Table 2.8 show that the three mass shootings do not lead to negative abnormal returns for the affected banks, rejecting Hypothesis 2a.

2.7 Conclusion

We use three high-profile mass shootings as exogenous shocks to investigate whether depositors discipline banks that provide credit to gun manufacturers.

Our results suggest that depositors discipline the related banks after mass shootings, especially for the branches near the shootings. While banks financing gun manufacturers experience negative deposit growth in the years of mass shootings, savings banks benefit from the boycotts, plausibly because savings banks are less likely to be related to gun manufacturers, and also because any existing relationship is less likely to be exposed publicly. Apart from geographical factors, depositors in counties that are more likely to support stricter gun control display a stronger responses towards affected banks after mass shootings. We also show that the severity and media attention of mass shootings also affects the magnitude of depositor discipline. However, we find no evidence that the mass shootings result in any downward pressure for banks' share prices.

[22] An additional set of tests investigates whether there is an abnormal cumulative mean abnormal return (CMAR) for affected banks using the same set of event windows, estimation windows, and benchmark indices for CAR. The results do not suggest any abnormal returns for the affected banks after the mass shootings. The results are available upon request.

Our findings raise several additional questions and suggest some promising directions for future research. First, what is the proportion of depositors who discipline related banks after the mass shootings? Second, what are the demographic characteristics of depositors who discipline the banks? Third, is the funding shortfall large enough to affect bank behavior, in particular banks' lending activities?

2.8 Tables and figures

Table 2.1: Summary statistics

Variable	N	Mean	Std. Dev.	*p*5	*p*95
Panel A: branch-level deposits and bank-level variables					
Branch-level deposits (USD, in thousands)	406,180	70,308.36	83,228.50	6,557	211,203
Branch-level deposits (USD, in thousands)-related banks	34,081	114,832.50	96,284.65	26,786	286,122
Branch-level deposits (USD, in thousands)-other banks	372,099	66,230.34	80,710.21	6,050	199,443
Branch-level deposits growth rate	406,180	1.07	0.13	0.89	1.37
Branch-level deposits growth rate-related banks	34,081	1.09	0.11	0.95	1.37
Branch-level deposits growth rate-other banks	372,099	1.07	0.13	0.89	1.37
Total Assets (ln)	406,180	17.03	3.39	11.75	21.39
Charge off/Total Loans (%)	406,180	0.55	0.54	0.03	1.41
Equity/Assets (%)	406,180	11.32	2.21	8.46	15.56
Net Income/Total Assets (%)	406,180	0.96	0.47	0.31	1.64
Interest On Deposits/Total Deposits (%)	406,180	0.29	0.25	0.08	0.71
Panel B: Residing county's characteristics of branches					
Gun store per capita (%)	406,180	0.01	0.02	0	0.04
Gun related death per capita (%)	406,180	0.01	0.01	0	0.02
Proportion of vote to Democratic Party (%)	406,180	46.52	17.66	18.87	75.87
Educational level (%)	406,180	0.2	0.04	0.15	0.27
Panel C: Distance- the Orlando nightclub shooting (in year 2016)					
Distance to the incident (km)	66,529	1,731.61	1,031.71	316.96	3,884.49
Panel D: Distance-the Las Vegas shooting (in year 2018)					
Distance to the incident (km)	66,239	2,400.07	1,055.74	389.25	3,668.61
Panel E: Panel E: Distance- the Stoneman Douglas High School shooting (in year 2018)					
Distance to the incident (km)	66,239	1,965.99	1,062.51	407.73	4,098.34

Notes: This table shows summary statistics of dependent variable and various independent variables. All variables are winsorized at the 1% and 99% level.

Table 2.2: Effect of mass shootings on deposit growth of "loaded" banks

	1	2	3	4	5	6	7
Sample	All branches in the United States in year 2013-2018						
Dependent variable	Branch-level deposit growth rate						
Affected bank × Shooting year	-0.033**	-0.020***	-0.020***	-0.020***	-0.014***	-0.014***	-0.013***
	(-2.50)	(-16.15)	(-15.30)	(-15.93)	(-11.03)	(-10.35)	(-9.39)
Affected bank × Affected state			-0.007*				
			(-1.82)				
Affected state			0.017***				
			(8.82)				
Affected bank × Affected county				-0.019**			
				(-2.17)			
Affected county				0.017***			
				(3.16)			
Affected bank × $p5$ Distance					-0.038***		
					(-11.17)		
$p5$ Distance					0.005***		
					(3.06)		
Affected bank × $p10$ Distance						-0.023***	
						(-8.55)	
$p10$ Distance						0.001	
						(0.63)	
Affected bank × $p15$ Distance							-0.023***
							(-9.28)
$p15$ Distance							0.002**
							(2.27)
Total assets (ln)		0.010***	0.010***	0.010***	0.010***	0.010***	0.010***
		(4.42)	(4.54)	(4.44)	(4.41)	(4.36)	(4.34)
Charge off ratio (%)		-0.021***	-0.021***	-0.021***	-0.021***	-0.021***	-0.021***
		(-26.95)	(-26.57)	(-26.92)	(-26.83)	(-26.86)	(-26.75)
Equity ratio (%)		0.001***	0.001***	0.001***	0.001***	0.001***	0.001***
		(3.30)	(3.41)	(3.30)	(3.18)	(3.12)	(3.14)
ROA (%)		0.007***	0.007***	0.007***	0.007***	0.007***	0.007***
		(9.14)	(8.98)	(9.13)	(9.19)	(9.19)	(9.19)
Average interest rate (%)		-0.086***	-0.087***	-0.086***	-0.087***	-0.087***	-0.087***
		(-28.21)	(-28.51)	(-28.29)	(-28.54)	(-28.46)	(-28.57)
Branch FE	YES	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES	YES
Observations	406,180	406,180	406,180	406,180	406,180	406,180	406,180
No. of branches	77,700	77,700	77,700	77,700	77,700	77,700	77,700
Adjusted R-squared	0.12	0.20	0.20	0.20	0.20	0.20	0.20
SE Cluster	Branch	Branch	Branch	Branch	Branch	Branch	Branch

Notes: Column 1 and column 2 in Table 2.2 presents the results obtained using equation 2.1 where the dependent variable is the branch-level deposit growth rate and the main explanatory variable is an interaction term between the dummy variable for the related banks and the dummy variable for the shooting years. Column 3 to column 7 in Table 2.2 presents the results obtained using equation 2.2 where the dependent variable is the branch-level deposit growth rate and the main explanatory variable is an interaction term between the dummy variable for the related banks and the respective distance variable. Definitions of all variables in Table 2.2 are presented in the appendix. Robust t-statistics are presented in parentheses. Standard errors are clustered at the branch level. *** p<0.01, ** p<0.05, * p<0.1.

Table 2.3: Effect of mass shootings on deposit growth of "loaded" banks: Sub-sample analysis

Dependent variable	Branch-level deposit growth rate		
Panel A	1	2	3
Sample	All branches in Florida	$p5$ distance	$p1$ distance
Affected bank × Year 2016	-0.012***	-0.014***	-0.018*
	(-2.87)	(-3.15)	(-1.85)
Branch FE	YES	YES	YES
Year FE	YES	YES	YES
Bank-level Controls	YES	YES	YES
Observations	24,001	20,311	4,065
No. of branches	4,613	3,940	801
Adjusted R-squared	0.22	0.22	0.21
SE Cluster	Branch	Branch	Branch
Panel B			
Sample	All branches in Nevada	$p5$ distance	$p1$ distance
Affected bank × Year 2018	-0.017	-0.093***	-0.064***
	(-1.22)	(-19.61)	(-5.98)
Branch FE	YES	YES	YES
Year FE	YES	YES	YES
Bank-level Controls	YES	YES	YES
Observations	2,295	20,311	4,065
No. of branches	423	3,924	764
Adjusted R-squared	0.22	0.32	0.33
SE Cluster	Branch	Branch	Branch
Panel C			
Sample	All branches in Florida	$p5$ distance	$p1$ distance
Affected bank × Year 2018	-0.029***	-0.037***	-0.042***
	(-5.75)	(-6.83)	(-3.80)
Branch FE	YES	YES	YES
Year FE	YES	YES	YES
Bank-level Controls	YES	YES	YES
Observations	24,001	20,311	4,065
No. of branches	4,613	3,957	776
Adjusted R-squared	0.22	0.24	0.27
SE Cluster	Branch	Branch	Branch

Notes: Table 2.3 presents the results obtained using equation 2.1 where the dependent variable is the branch-level deposit growth rate and the main explanatory variable is an interaction term between the dummy variable for the related banks and the dummy variable for the shooting year of the respective shooting. Definitions of all variables in Table 2.3 are presented in the appendix . Robust t-statistics are presented in parentheses. Standard errors are clustered at the branch level. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table 2.4: Effect of mass shootings on deposit growth of savings banks

	1	2	3	4	5
Sample	All branches in the United States in year 2013-2018				
Dependent variable	Branch-level deposit growth rate				
Saving banks × Shooting year	0.006***	0.007***	0.006***	0.005***	0.005***
	(7.24)	(8.83)	(6.86)	(6.51)	(6.16)
Saving bank × Affected state	0.023***				
	(6.69)				
Affected state	0.004*				
	(1.83)				
Saving bank × Affected county		0.064***			
		(5.01)			
Affected county		-0.004			
		(-0.98)			
Saving bank × $p5$ Distance			0.023***		
			(9.10)		
$p5$ Distance			-0.024		
			(-1.19)		
Saving bank × $p10$ Distance				0.011***	
				(6.59)	
$p10$ Distance				0.003	
				(0.29)	
Saving bank × $p15$ Distance					0.007***
					(5.22)
$p15$ Distance					-0.005
					(-0.73)
Branch FE	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES
Bank-level controls	YES	YES	YES	YES	YES
Observations	406,180	406,180	406,180	406,180	406,180
No. of branches	77,700	77,700	77,700	77,700	77,700
Adjusted R-squared	0.20	0.20	0.20	0.20	0.20
SE Cluster	Branch	Branch	Branch	Branch	Branch

Notes: Table 2.4 presents the results obtained using equation 2.2 where the dependent variable is the branch-level deposit growth rate and the main explanatory variable is an interaction term between the dummy variable for savings banks and the respective distance variable. Definitions of all variables in Table 2.4 are presented in the appendix . Robust t-statistics are presented in parentheses. Standard errors are clustered at the branch level. *** p<0.01, ** p<0.05, * p<0.1.

Table 2.5: Effect of mass shootings on deposit growth of "loaded" banks: County characteristics

	1	2	3	4
Sample	All branches in the United States in year 2013-2018			
Dependent variable	Branch-level deposit growth rate			
Affected bank × Shooting year	-0.011***	-0.001	-0.010*	0.019
	(-4.50)	(-0.12)	(-1.95)	(0.84)
Affected bank × Shooting year × Gun store per capita (%)	0.001***			
	(4.92)			
Shooting year × Gun store per capita (%)	0.000***			
	(3.04)			
Affected bank × Shooting year × Gun-related death per capita (%)		-0.020**		
		(-2.54)		
Shooting year × Gun-related death per capita (%)		0.004***		
		(4.26)		
Affected bank × Shooting year × Votes to the Democratic Party (%)			-0.018**	
			(-2.09)	
Shooting year × Votes to the Democratic Party (%)			-0.001	
			(-0.24)	
Affected bank × Shooting year × Educational level (%)				-0.013*
				(-1.70)
Shooting year × Educational level (%)				-0.007***
				(-3.69)
Branch FE	YES	YES	YES	YES
Year FE	YES	YES	YES	YES
Bank-level controls	YES	YES	YES	YES
Observations	406,180	406,180	406,180	406,180
No. of branches	77,700	77,700	77,700	77,700
Adjusted R-squared	0.20	0.20	0.20	0.20
SE Cluster	Branch	Branch	Branch	Branch

Notes: Table 2.5 presents the results obtained using equation 2.3 where the dependent variable is the branch-level deposit growth rate and the main explanatory variable is a triple interaction term between the dummy variable for related banks; the dummy variable for the shooting year; and the respective county-level variable. Definitions of all variables is in Table 2.5 are presented in the appendix . Robust *t*-statistics are presented in parentheses. Standard errors are clustered at the branch level. *** p<0.01, ** p<0.05, * p<0.1.

Table 2.6: Effect of less severe mass shootings on deposit growth of "loaded" banks

	1	2	3	4
Sample	All branches in the United States in year 2013-2018			
Dependent variable	Branch-level deposit growth rate			
Affected bank × *p*5 distance	-0.005***			
	(-3.15)			
*p*5 distance	0.001**			
	(2.15)			
Affected bank × Number of fatalities (ln)		-0.004***		
		(-7.71)		
Number of fatalities (ln)		0.001***		
		(3.21)		
Affected bank × Number of injuries (ln)			-0.007***	
			(-13.06)	
Number of injuries(ln)			0.000	
			(-0.16)	
Affected bank × Number of media attention (ln)				-0.004***
				(-7.54)
Number of media attention (ln)				0.001***
				(4.80)
Branch FE	YES	YES	YES	YES
Year FE	YES	YES	YES	YES
Bank-level controls	YES	YES	YES	YES
Observations	406,180	406,180	406,180	406,180
No. of branches	77,700	77,700	77,700	77,700
Adjusted R-squared	0.20	0.20	0.20	0.20
SE Cluster	Branch	Branch	Branch	Branch

Notes: Table 2.6 presents the results obtained using equation 2.2 where the dependent variable is the branch-level deposit growth rate and the main explanatory variable is the interaction term between the dummy variable for related banks and another variable in the respective column. Definitions of all variables in Table 2.6 are presented in the appendix. Robust t-statistics are presented in parentheses. Standard errors are clustered at the branch level. *** p<0.01, ** p<0.05, * p<0.1.

Table 2.7: Placebo and falsification tests

Sample	All branches in the United States in year 2013-2018
Dependent variable	Branch-level deposit growth rate
Panel A	1
Rejection rate at the 10% lelvel (2-tailed test)	11.20
Rejection rate at the 5% lelvel (2-tailed test)	5.50
Rejection rate at the 1% lelvel (2-tailed test)	1.40
Mean coefficient	-0.001
Mean t-statistic	(-0.07)
Panel B	
Affected bank × Shooting year$_{t-1}$	0.004**
	(2.55)
Affected bank × Affected county$_{t-1}$	0.008
	(0.91)
Affected county$_{t-1}$	-0.017***
	(-3.43)
Affected bank × Shooting year$_{t-2}$	-0.010***
	(-7.47)
Affected bank × Affected county$_{t-2}$	0.006
	(0.72)
Affected county$_{t-2}$	-0.014***
	(-3.16)
Branch FE	YES
Year FE	YES
Bank-level controls	YES
Observations	406,180
No. of branches	77,700
Adjusted R-squared	0.20
SE Cluster	Branch

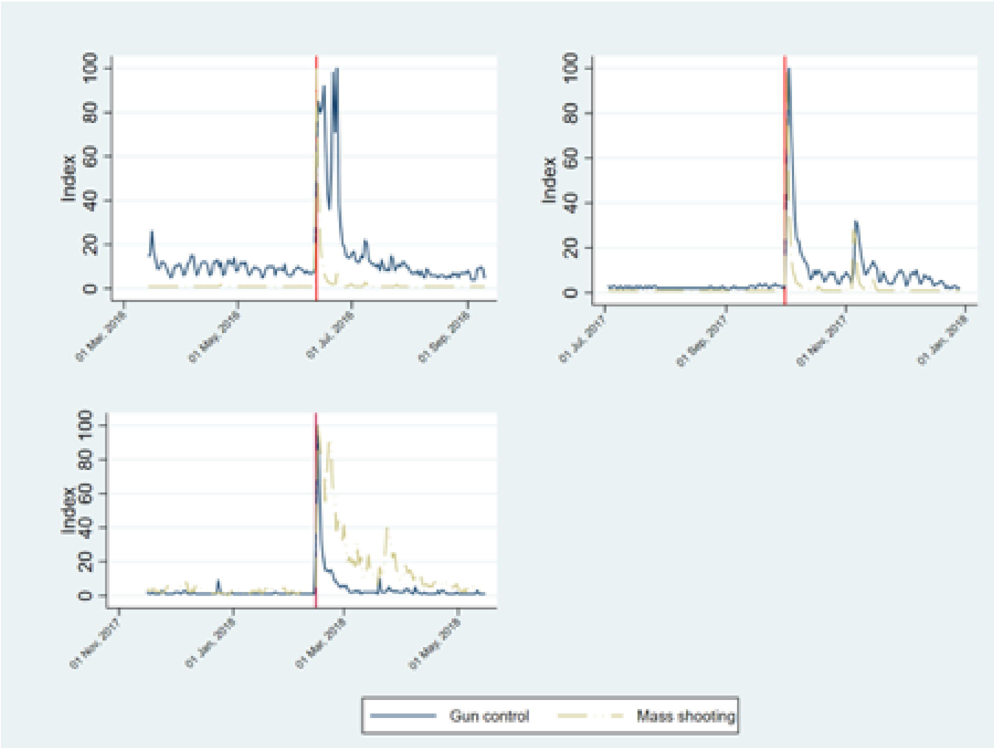
Notes: Panel A reports Monte Carlo simulations based on 1,000 replications for the effect of mass shootings on branch-level deposit growth rates of the affected banks. We replicate the estimation in column 4 of Table 2.2. We exclude all counties that experience mass shootings during the sample period and randomly assign 3 counties to a pseudo mass shooting in a randomly selected year. The variable "Affected county" equals 1 for the randomly assigned counties in the randomly assigned years and equals 0 for all other observations. We estimate the regression and save the t-statistic on the coefficient of interest and repeat this process 1,000 times and compute the rejection rates of the null hypothesis =0 at the 1% ,5%, and 10% levels, respectively. We also report the mean coefficient and the average t-statistic for β_2. In Panel B, we replicate the estimation in column 4 of Table 2.2 with a fake shooting year of the respective mass shooting. Shooting year$_{t-1}$ =1 if the observation is in year 2015 or year 2017, while Shooting year$_{t-2}$ =1 if the observation is in year 2014 or year 2016. Affected county$_{t-1}$.=1 if the observation is in year 2015 and the branches are in Orange, Florida; or the observation is in 2017 and the branches are in Broward, Florida or the observation is in 2017 and the branches are in Clark, Nevada, 0 otherwise. Affected county$_{t-2}$=1 if the observation is in year 2014 and the branches are in Orange, Florida; or the observation is in 2016 and the branches are in Broward, Florida or the observation is in 2016 and the branches are in Clark, Nevada, 0 otherwise.. Robust t-statistics are presented in parentheses. Standard errors are clustered at the branch level. *** p<0.01, ** p<0.05, * p<0.1.

Table 2.8: Event study results

	1	2	3
Dependent Variable	CAR(0,4)	CAR(0,2)	CAR(0)
Panel A: MSCI North America			
Total (all events)	-0.008	-0.024	0.024**
Average (all events)	-0.003	-0.008	0.008
BS p-value	0.485	0.245	0.051
Panel B: Market portfolio Proxy: S&P 500			
Total (all events)	-0.005	-0.021	0.027**
Average (all events)	-0.002	0.007	0.009
BS p-value	0.576	0.349	0.028
Panel C: Market portfolio Proxy: TSX composite index			
Total (all events)	-0.010	-0.025	0.014
Average (all events)	-0.003	-0.008	0.005
BS p-value	0.500	0.135	0.218
Panel D: Market portfolio Proxy: S&P 500 & TSX composite index			
Total (all events)	0.002	0.016	0.006
Average (all events)	0.001	0.005	0.002
BS p-value	0.952	0.079	0.639

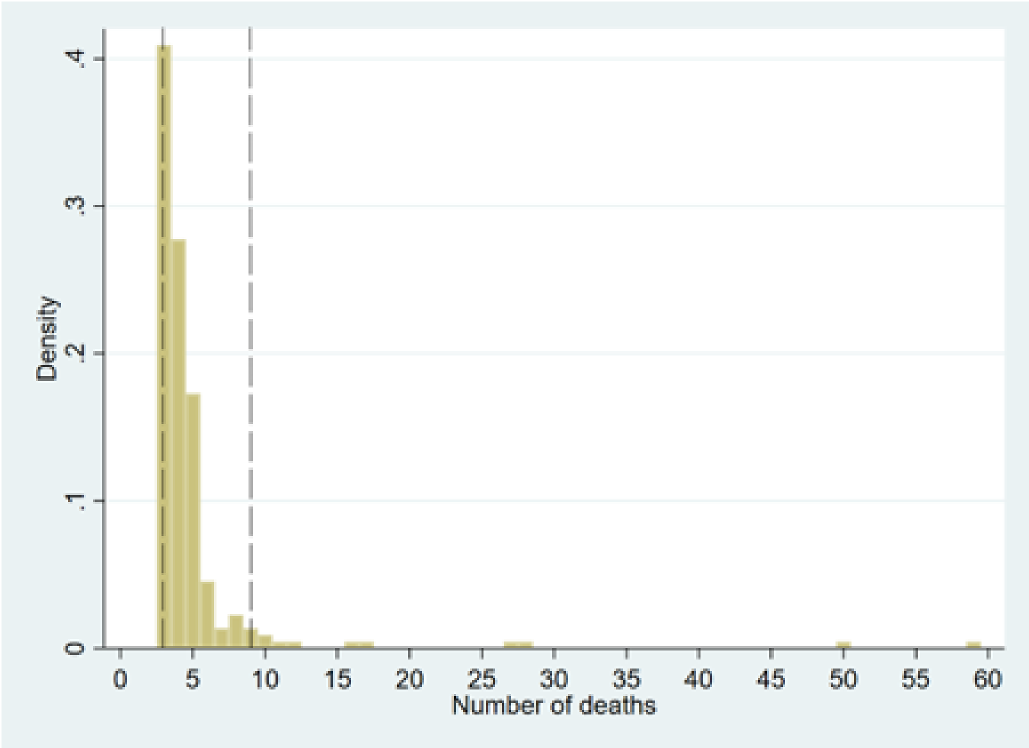
Notes: Table 2.8 presents the result of the CAR with different benchmark indexes and event windows, the estimation procedure is detailed in the appendix. BS p-value is the p-value for the average CAR calculated according to 800 bootstrap simulations. *** p<0.01, ** p<0.05, * p<0.1

Figure 2.1: Google Trends search volume index



Notes: Figure 2.1 shows the Google trend index for the keyword “gun control” and “mass shooting” in the respective states of the 3 high-profile mass shootings over time. The red dashed line highlights the date of the mass shooting. The blue solid line represents the trend index for the keyword “gun control”, while the khaki dashed line represents the trend index for the keyword “mass shooting”.

Figure 2.2: Density of the number of fatalities in a mass shooting



Notes: Figure 2.2 shows the distribution of the number of deaths in a mass shooting incident. The dashed line on the left denotes the 5^{th} percentile of the number of deaths in a mass shooting incident, while the dashed line on the right denotes the 95^{th} percentile of the number of deaths in a mass shooting incident.

Figure 2.3: Timeline

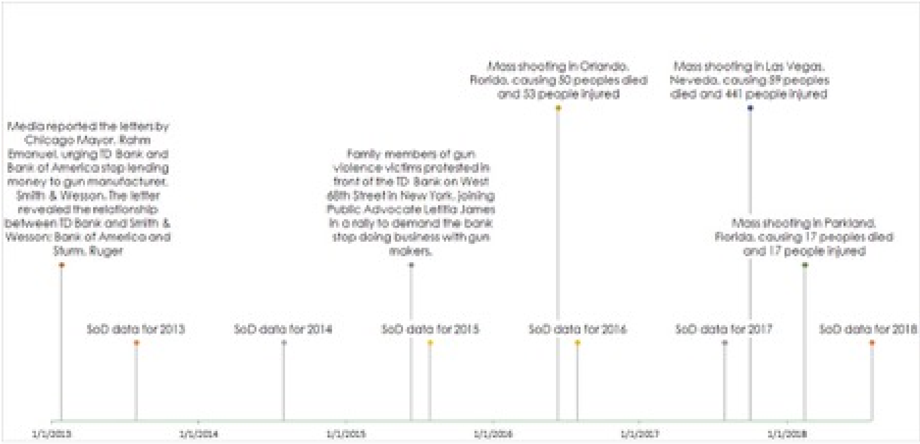
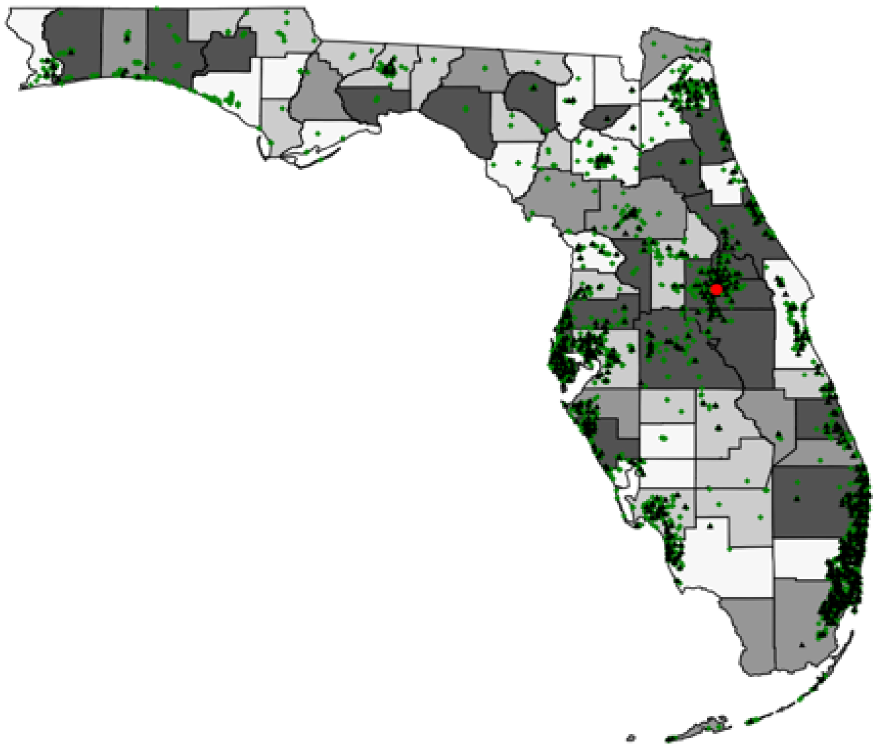
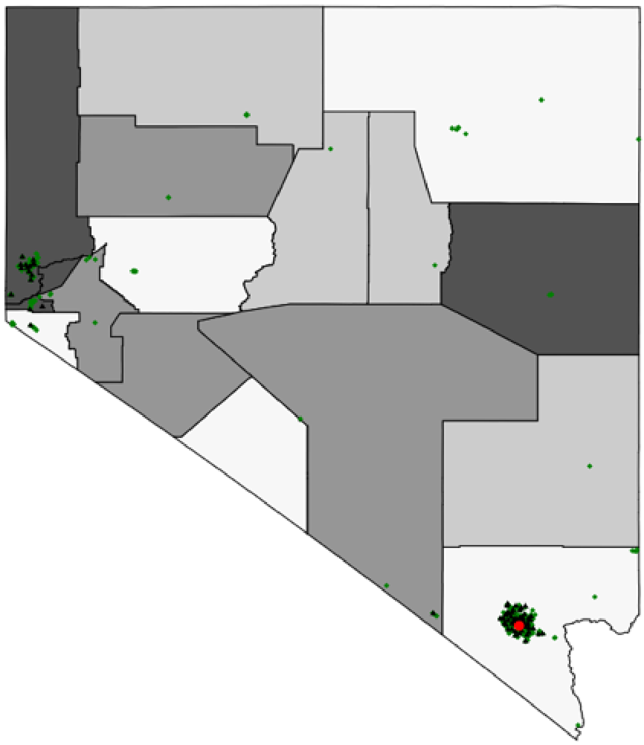


Figure 2.4: Location of the Orlando nightclub shooting



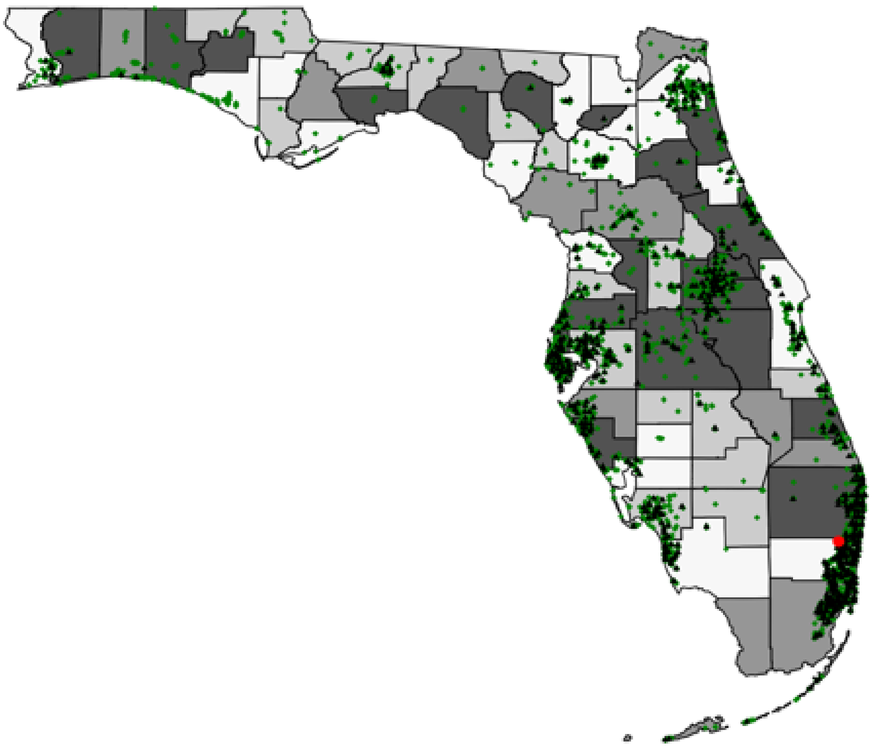
Notes: Figure 2.4 shows the location of the mass shooting and the location of branches in Orange, Florida in 2016. Black triangles indicate the location of affected banks in Florida in 2016, while green squares show the location of all other branches in Florida in 2016. The red dot indicates the location of the mass shooting.

Figure 2.5: Location of the Las Vegas shooting



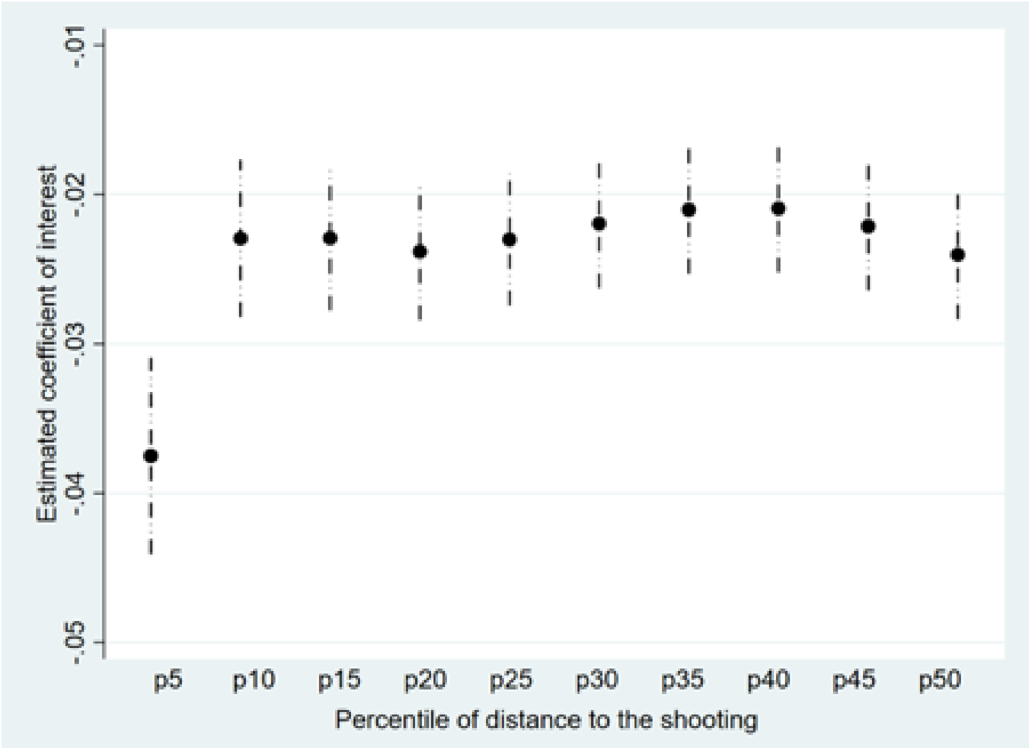
Notes: Figure 2.5 shows the location of the mass shooting and the location of branches in Clark, Nevada in 2018. Black triangles indicate the location of affected banks in Nevada in 2018, while green squares show the location of all other branches in Nevada in 2018. The red dot indicates the location of the mass shooting.

Figure 2.6: Location of the Stoneman Douglas High School shooting



Notes: Figure 2.6 shows the location of the mass shooting and the location of branches in Broward, Florida in 2018. Black triangles indicate the location of affected banks in Florida in 2018, while green squares show the location of all other branches in Florida in 2018. The red dot indicates the location of the mass shooting.

Figure 2.7: The diminishing coefficient of interest



Notes: Figure 2.7 displays the coefficient of interest in equation 2.2 where distance is measured by whether the branch is located within a certain percentile of the distance to the shootings in the years of mass shootings. The diamond indicates the estimated coefficient of interest, while the dashed line shows the 95% confidence interval.

2.9 Appendix

Table A2.1: Variable definitions

Variable	Definition	Source
Branch-level deposit growth rate	branch-level deposit in current year over branch level deposit in the last year	Summary of Deposits
Shooting year	Shooting year=1 if the observation is in 2016 and 2018, 0 otherwise.	Authors' calculation
Affected state	Affected state =1 if the observation is in year 2016 and the branches is in Florida; or the observation is in 2018 and the branches is in Florida or Nevada, 0 otherwise.	Authors' calculation
Affected county	Affected county =1 if the observation is in year 2016 and the branches is in Orange, Florida; or the observation is in 2018 and the branches is in Broward, Florida or the observation is in 2018 and the branches is in Clark, Nevada, 0 otherwise. In Table 2.6, Affected county=1 if the observation is in a county occurs mass shootings that cause 3 or more fatalities in the year of observation, 0 otherwise.	Authors' calculation
*p*5 Distance	*p*5 Distance=1 if the observation is within the 5^{th} percentile distance of the mass shootings in the year of the respective mass shooting, 0 otherwise.	Authors' calculation
*p*10 Distance	*p*10 Distance =1 if the observation is within the 10^{th} percentile distance of the mass shootings in the year of the respective mass shooting, 0 otherwise.	Authors' calculation
*p*15 Distance	*p*15 Distance =1 if the observation is within the 15^{th} percentile distance of the mass shootings in the year of the respective mass shooting, 0 otherwise.	Authors' calculation
Year 2016	Year 2016=1 if observation is in 2016	Authors' calculation
Year 2018	Year 2018=1 if observation is in 2018	Authors' calculation
Number of fatalities (ln)	The logarithm of (1+number of fatalities in mass shootings in the specified county in a specified year). If the observation is in a county without mass shootings in the year, Number of fatalities (ln) equals to the logarithm of 1.	Gun Violence Archive and authors' calculation
Number of injuries (ln)	The logarithm of (1+number of injuries in mass shootings in the specified county in a specified year). If the observation is in a county without mass shootings in the year, Number of injuries (ln) equals to the logarithm of 1.	Gun Violence Archive and authors' calculation
Number of media attention (ln)	The logarithm of the number of reports of respective mass shootings for news coverage on the national evening news programs of the 5 major television networks—ABC, CBS, NBC, CNN and Fox News on the shooting day and the following 30 days.	Vanderbilt Television News Archive (VTNA)
Gun store per capita (%)	Gun stores per capita in each county in 2016	Bureau of Alcohol, Tobacco, Firearms and Explosives
Gum-related violence (%)	Gun-related deaths per capita in each county in 2016	CDC Wonder

Continued on next page

Table A2.1 – continued from previous page

Variable	Definition	Source
Votes to the Democratic party (%)	The proportion of vote to the Democratic Party of each county in 2016 president election	MIT election Data Science Lab
Educational level (%)	The average percentage of the population with a bachelor's degree's degree or higher of each county during the sample period.	U.S. Census Bureau
Total assets (ln)	The logarithm of bank total assets.	Call Reports and authors' calculation
Charge off ratio (%)	The percentage term of total charge off over total loans	Call Reports and authors' calculation
Equity ratio (%)	The percentage term of total equity over total assets	Call Reports and authors' calculation
ROA (%)	The percentage term of net income over total assets	Call Reports and authors' calculation
Average interest rate (%)	The percentage term of total interest expense on deposits over total deposits	Call Reports and authors' calculation

Table A2.2: Mass shootings resulting in over 9 fatalities in 2013-2018

Shooting	Date	Deaths (injuries)	County	State	Latitude	Longitude	Media attention
Navy Yard shooting	16-Sep-13	11 (3)	District of Columbia	District of Columbia	38.87414	-76.99777	94
Umpqua Community College shooting	1-Oct-15	10 (9)	Douglas	Oregon	43.29104	-123.334699	74
San Bernardino attack	2-Dec-15	16 (19)	San Bernardino	California	34.0755	-117.277699	52
Orlando nightclub shooting	12-Jun-16	50 (53)	Orange	Florida	28.5196	-81.376799	171
Las Vegas shooting	1-Oct-17	59 (441)	Clark	Nevada	36.09317	-115.176332	103
Sutherland Springs church shooting	5-Nov-17	27 (20)	Wilson	Texas	29.27314	-98.056574	40
Stoneman Douglas High School shooting	14-Feb-18	17 (17)	Broward	Florida	26.30444	-80.269532	111
Santa Fe High School shooting	18-May-18	10 (13)	Galveston	Texas	29.39269	-95.142015	81

Estimation approach of the event study

For calculating CARs, we first estimate the abnormal return (AR) using the market model considering day-of-the-week effects (Kaplanski and Levy, 2010; Bruno et al., 2018):

$$AR_{i,t} = R_{i,t} - (\vartheta_i + \beta_i R_{m,t} + \sum_{d=2}^{5} \nu_d D_d) \tag{2.4}$$

where $AR_{i,,t}$ refers to abnormal return of bank i at time t; $R_{i,t}$ is the return of bank i at time t; $R_{m,t}$ is the return of the respective benchmark index i at time t; D_2 equals 1 for Tuesdays, D_3 equals 1 for Wednesdays, D_4 equals 1 for Thursdays, and D_5 equals 1 for Fridays, 0 otherwise.

We use an estimation window of 260 trading days (-260, -1) for the market model. Because the 3 high-profile mass shootings happen on different dates, we use different estimation windows for each event. The Prais–Winsten method is employed to adjust for first-order of the market model regression (Allen and Wilhelm, 1988).

We calculate the market-adjusted return (MAR) as the difference between the return of bank i ($R_{i,t}$) and the market return ($R_{m,t}$).

$$MAR_{i,t} = R_{i,t} - R_{m,t} \tag{2.5}$$

$$CAR_{i,t} = \sum_{t=t1}^{t2} AR_i, t \tag{2.6}$$

$$CMAR_{i,t} = \sum_{t=t1}^{t2} MAR_i, t \tag{2.7}$$

For our regressions we rely on 1-day, 3-day and 5-day event windows, where t_1 is the trading day before the event and t_2 is the trading day after the event.

CHAPTER 3

Climate Risks and House Prices: The Insurance Channel

3.1 Introduction

Real estate property is one of the most vulnerable physical assets exposed to extreme weather events.[1] At the same time, it is one of the major vehicles of household wealth accumulation (Bhatia, 1987; Benjamin et al., 2004; Bach et al., 2020) and one of the major types of collateral in the financial system (Chaney et al., 2012; Ramcharan, 2020). Therefore, it is important to understand the implications of climate-related risks on property values in a world with an increasing frequency and intensity of extreme weather events.[2] While there is no lack of literature examining the effect of extreme weather events on property values (e.g. Hallstrom and Smith (2005); Beltrán et al. (2018)), much less is known about the role of public policies against extreme weather events in property markets. To address this gap, we exploit a novel empirical setting, the introduction of a UK public reinsurance scheme which provides cross-subsidized reinsurance to flood prone properties. Our findings highlight hitherto unexplored effect of public reinsurance mechanisms against extreme weather events in affecting property prices and transaction volume.

The UK public reinsurance scheme, Flood Re was introduced in April 2016. Its key policy objective is ensuring the availability and affordability of flood insurance to homeowner in flood

[1]The UK Environment Agency estimates that one in every six properties, in total 5.2 millions properties, across England are at risk of flooding. The National Oceanic and Atmospheric Administration estimates that $106 billion worth of coastal property in the U.S. will be below sea level by 2050.

[2]Recent examples of catastrophic flooding include the the series of floods in western Germany in July 2021, causing over 200 deaths and over 4 billion euros insured losses; another example is the flood in Henan province of China in July 2021, leading to over 20 deaths.

prone areas (FloodRe, 2016).[3] In achieving this objective, Flood Re provides insurers an option to pass on the flood risk element of their policies to the re-insurer, Flood Re, at a highly-discounted price. As a result, the scheme reduces current and expected future insurance premiums for home-owners in flood risk areas. According to the 2020 Flood Re annual report (FloodRe, 2020), 80% of households with previous flood claims found quotes that are more than 50% cheaper after the implementation of Flood Re. In terms of pound sterling, Flood Re is estimated to reduce average annual insurance premium of flooded properties from around £650 to less than £325.[4] The report also finds that Flood Re increases availability of flood insurance among those households that were exposed to flooding.[5]

Beyond the introduction of Flood Re, the UK residential real estate market offers several characteristics which makes it an ideal laboratory to study the introduction of a public reinsurance scheme for flood risk. First, home ownership rates in the UK are high. About two-thirds of households own a property, a higher proportion than Germany or France where only about every second household owns a property.[6] Hence, properties play a crucial role in wealth accumulation in the UK. Second, take-up rates of home insurance, which entail the coverage of flood risk, are very high, reaching over 95% in England (Surminski, 2018).[7] While this is a much higher take-up rate than the U.S., where only 12% of households have flood insurance (Hu, 2020), other countries like Belgium, France, Switzerland have a take-up rate comparable to the UK (CEA, 2009). Such a high take-up rate allows us to estimate the effect of Flood Re on property prices without explicitly looking at the level of insurance coverage. Third, information on the risk of flooding is publicly available to all participants of the real estate market. The UK Environment Agency has been publishing highly granular flood maps since 2008. Hence, not only insurance companies and mortgage lenders but also home owners and prospective buyers have access to this public information.

Under this ideal setting, we examine three *ex-ante* uncertain questions. First, we study the effect of Flood Re, which reduces current and future insurance premiums of flood prone properties, on transaction prices. Second, we examine the distributional consequences of the introduction of the reinsurance scheme by estimating heterogeneous effects of Flood Re based on regional

[3] Another policy objective of the scheme is managing the transition to risk reflective pricing for flood insurance by the end of 2039.

[4] Information about average house insurance premiums of flooded properties is limited, the estimation is based on DEFRA (2013) which shows that the average household insurance premiums of flooded properties to be £650 before the introduction of Flood Re in 2010.

[5] The report finds that none of the household with prior flood claims received quotes from more than four insurers before the introduction of Flood Re, and 94% of them can receive quotes from five or more insurers after the introduction of Flood Re.

[6] The figure is similar to Spain and Netherlands. Information about home ownership rate of European countries is available at: *https://ec.europa.eu/eurostat/cache/digpub/housing/bloc-1a.html?lang=en*.

[7] In the UK, buildings insurance is required for getting a mortgage and the insurance coverage must at least covers the outstanding mortgage amount.

characteristics. Lastly, we study the effect of Flood Re on market liquidity by examining its effect on transaction volume of flood prone properties. We conjecture that the reduction in current and future insurance premiums increase value and transaction volume of flood-prone properties. However, the actual effect depends on the expectation of the reduction in future insurance premiums caused by Flood Re, and the discount rate in discounting future insurance premiums. It is also uncertain how these factors vary across different demographic groups.

The major empirical challenge in identifying the effect of flood risk and the policy implementation on property values and transaction volume lies in isolating it from other confounding factors driving property prices.[8] We overcome this empirical challenge by leveraging a comprehensive data set of the population of all property transactions in England. The detailed geographical information of each transacted property allows us to compare price changes of properties within a small local area but with heterogeneous exposure to flood risk. The data set also allows us controlling for the effect of other observable property characteristics (e.g. property type such as terraced, detached or semi-detached) on price. We use a repeat transaction approach comparing the same property transacted multiple times which allows us to further control for unobservable and time-invariant property characteristics. We are also able to differentiate the effect of price trends in local areas on property prices by comparing closely-located properties with different level of flood risk exposure sold in the same year of the current transaction and in the same year of the previous transaction.

We find that flood events reduce property values before the introduction of Flood Re. Yet, this negative effect is completely mitigated by the introduction of Flood Re. Results in our preferred specification suggest that a property experienced a flood longer than a day within four years before the property transaction experiences 1.6% reduction of property values before the introduction of Flood Re. However, there is no reduction in the values of flooded properties after the introduction of Flood Re. On average, the introduction of Flood Re increases the value of flooded properties by GBP 4,083.[9] Among the 5.2 million properties that are at risk of flooding in England (EnvironmentAgency, 2009), the subsidization of Flood Re increases the total value of flooded properties by GBP 212.3 million per year assuming there is only 1% of the at-risk properties are flooded annually.[10] The total effect of Flood Re on property values would double to GBP 424.6 million if flood risk probability further increases to 2%.[11] We also find heterogeneous effects of Flood Re in different areas across England, the effect of Flood Re is stronger in areas

[8] See Lustig and Van Nieuwerburgh (2005); Piazzesi et al. (2007) for other drivers of property prices.

[9] The average property price is GBP 226,840 and the calculation is based on the estimation results of our preferred specification shown in column 5 of Table 3.2: GBP 226,840 × 1.8% = GBP 4,083.

[10] The Environment Agency does not specify the average annual flood probability for those 5.2 millions at-risk properties. We therefore conservatively assume that all at-risk properties are on 100-year flood plain (i.e. 1% annual flood probability).

[11] i.e. 5.2m properties at risk × 1% risk × GBP 4,083 = GBP 212.3m.

with wealthier and older population, and urban areas, suggesting the distributional effect of Flood Re. Importantly, the results highlight a plausibly unintended effect of Flood Re for only benefiting wealthier households, in terms of the appreciation of property values. Lastly, we find that Flood Re increase the transaction volume of properties in at-risk areas, our results suggest that a flooded property has 3.6% reduction in the annual probability of transacting before Flood Re, Flood Re mitigates the negative effect and increase additional 3% in the annual transaction probability.[12]

To verify the relationship between property values and Flood Re, we conduct a set of placebo tests which employ the extension of an existing agreement between the government and insurance providers as a placebo treatment. We do not find any effect of the extension on property values. We also conduct simulations in testing the placebo effect of flood events and Flood Re on properties that are not actually flooded, the results of the simulations suggest that our findings are unlikely driven by factors other than flood events and Flood Re. Our findings are also robust to two *ex-ante* measurements. We find that properties that are at flood risk and located near to river or sea are sold at discount before the introduction of Flood Re. The results imply that our findings are not fully explained by the physical damages caused by the historical flood events, but also related to the expectation of future flood risk.

Our paper contributes to two strands of literature. First, our paper contributes to the growing body of literature examining the linkage between climate risk and government interventions. The increasing frequency and severity of extreme weather events motivate governments to enhance availability and affordability of extreme-weather insurance, it therefore poses a question over the implications of these interventions. For example, Zahran et al. (2009) show that government implementation of flood risk mitigation measures increases flood insurance uptake; Hu (2020) finds that a national reform that publicises flood risk information across U.S. counties on the take-up rate of flood insurance. Closest to our paper, Sen and Tenekedjieva (2021) study the effect of the heterogeneous regulatory frictions in flood insurance pricing across U.S. states. They find that insurers overcome pricing frictions by cross-subsidizing insurance across states. A missing puzzle of Sen and Tenekedjieva (2021) is whether the cross-subsidization is capitalized into property values. Our paper fills this gap by showing that the cross-subsidization induced by government intervention has implications beyond home insurance market. To the best of our knowledge, we present the first work that shows the effect of a public flood-reinsurance scheme on value and liquidity of properties at climate-related risk.

Our paper also relates to the broad literature examining distributional effect of public policy interventions (e.g. Beck et al. (2010); DeFusco and Mondragon (2020)). In particular, our paper

[12] The base transaction rate in the sample is 14.6%.

contributes to the strand of this literature related to public policy interventions addressing environmental risk (e.g. Grainger (2012); Bento et al. (2015); da Silva Freitas et al. (2016); Isen et al. (2017)). More related to our study, few papers show the distributional effect of the National Flood Insurance Program (NFIP) in the U.S.. With NFIP claim and premium data, Bin et al. (2012) find no evidence that the NFIP creates any distributional effect. With similar approach and more recent data, Bin et al. (2017) show that the net premium (premiums-payouts) of the NFIP is regressive, implying that the NFIP disproportionally benefits wealthier segments of population. While the two papers focus on the progressivity of the NFIP, they do not study the redistributional effect of the NFIP in terms its impacts on property values. Our paper fills this gap by documenting that the mitigating effect of a public flood reinsurance scheme on at-risk properties are much stronger among richer households. The results provide an unique insight in examining the objectives of public flood reinsurance schemes and other policy interventions in mitigating environmental risk.

The rest of the paper is structured as follows. Section 3.2 summarizes the policy background of Flood Re; section 3.3 present the conceptual framework and empirical strategy; section 3.4 details the data of our analysis; section 3.5 discusses the results; section 3.6 concludes.

3.2 Background on the policy

Since the 1960s, there had been a series of "Gentlemen's Agreement" between the UK government and the insurance industry to ensure the availability of flood insurance in flood-prone areas. These agreements were based on the mutual commitment between the insurers providing insurance in high risk areas and the government increasing investments in flood defenses. These agreements formed the foundation for flood insurance for the next 40 years, until the unprecedented series of floods between 1998 and 2000.[13]

Despite of these agreements, the losses from the series of floods caused insurers to be more prudent in underwriting flood insurance, leading to many flooded households finding it difficult to renew their policy in 2000 (Dlugolecki, 2000). On one side, fueled by the increasing media attention and the widespread criticism over government's responses to the series of floods, the government was pressurized to formalize an agreement with the insurance industry to ensure the availability of flood insurance. On the other side, the insurance industry took this opportunity to request for the right to refuse insuring the highest risk areas and adjust insurance premiums

[13]Sustained heavy rain in Midland from 9 April to 10 April 1998 led to severe flood. Approximately 4,200 properties were inundated and economic losses were estimated to be GBP 350 million (MetOffice, 2012). The autumn of 2000 was the wettest on record since 1766. over 10,000 properties were flooded across the country, and transportation services were severely disrupted, causing economic losses over GBP 1 billion (EnvironmentAgency, 2001).

according to the level of flood risk. Under this circumstance, the formal policy agreements "Statement of Principles on the Provision of Flood Insurance" (SoP) was agreed by the representative of all insurance companies in the UK, Association of British Insurers (ABI), and the government in 2002. Under the SoP, insurers were obligated to provide flood insurance, but properties in the highest flood risk categories (properties with annual flood probability above 1.3%) were excluded in the SoP, and properties built after 2009 were also excluded since the revision of the SoP in 2004. The government, in return, promised to invest in flood risk mitigation measures.[14] While the SoP dealt with availability of flood insurance, it remained silent on affordability. There was no restriction on the size of the insurance premiums. Therefore, any increase in premiums did not violate the SoP.

In the 1990's and early 2000's, as map technology and computing power were still underdeveloped, insurance firms found it difficult to measure flood risk. Flood risk therefore was largely not priced into insurance premiums until the introduction of 2004 EA flood risk map. With the increasing frequency of extreme flood events, concerns about affordability of flood insurance and its implications for mortgage affordability was growing since then. Coming close to the expiration of the SoP in 2013, the insurance industry and the government agreed on creating a reinsurance scheme, Flood Re, to replace the SoP. Flood Re has two major purposes. The first purpose is to promote both the availability and the affordability of flood insurance, the second is to provide a smooth transition to risk reflective pricing for flood insurance. After extending the SoP for another three years in 2013, Flood Re was approved by the parliament in 2014 and started operation in April 2016 to replace the SoP (Surminski and Eldridge, 2017). Flood Re is planned to phase out in 2039 when the flood insurance market fully transitions to risk-reflective pricing.

Flood Re lowers the cost of providing flood insurance in high risk areas by providing an option for insurers to reinsure policies at a subsidized price which only increases with the tax banding of the insured property. The subsidies are covered by the insurers through an annual levy which estimated to pass on all insurees for £10.50 per policy (Surminski, 2018). Flood Re is eligible for properties at all flood risk level, but properties built after 2009 are excluded to discourage home-building in flood risk areas.[15] Since insurers can now pass on their risk for subsidized price for properties at all flood risk level, Flood Re has increased the availability and reduced the cost of flood insurance in high risk areas (FloodRe, 2020).

Figure 3.1 (Crick et al., 2018) outlines the mechanism of Flood Re and the relationship between government and industry. In terms of the awareness of Flood Re, the survey data of 2018 Availability and Affordability of Insurance report suggests that 45% of the respondents in flooded

[14]See Butler and Pidgeon (2011) for discussions on flood risk mitigation measures adopted by the UK government.

[15]Despite of that, a large number of properties are still being built in flood prone areas, particularly in deprived neighbourhoods (Rözer and Surminski, 2021).

areas are aware of Flood Re, while only 29% of the respondents in control (i.e. "not flooded") areas are aware of Flood Re (see Figure 3.2). Under the design features of Flood Re, households cannot influence insurers' decision to pass on the flood risk component of the insurance contract. Hence, households' awareness of Flood Re does not influence the degree to which this reinsurance scheme affects house prices.

3.3 Conceptual framework and empirical strategy

In this section, we provide a conceptual framework which supports our understanding of the mechanism of the introduction of Flood Re on property values. Based on the framework, we develop the empirical strategy.

3.3.1 Conceptual framework

To start with, we consider a simple, one period hedonic pricing model (Rosen, 1974). The property price is a function of observable property characteristics z, e.g. whether it is a flat or house. It is reduced by the insurance premium which a home owner pays. This insurance premium is itself a function of flood risk the property is exposed to:[16]

$$Property\,price(z, Premium, Flood\,risk) = f(z) - Premium(Flood\,risk) \tag{3.1}$$

From equation 3.1, it can be seen that higher flood risk decreases property price via higher insurance premium. In mathematical terms, the derivative of property price with respect to flood risk is the negatively proportional to the derivative of insurance premium with respect to flood risk, i.e. $\frac{\partial Property\,price}{\partial Flood\,risk} = -\frac{\partial Premium}{\partial Flood\,risk}$.

In absence of a public reinsurance scheme such as Flood Re, insurance companies have a strong incentive to price flood risk into insurance premium, i.e. the derivative of premium with respect to flood risk is positive, $\frac{\partial Premium}{\partial Flood\,risk}$ >0. As property price is a function of insurance premium, the derivative of property price with respect to flood risk is negative, $\frac{\partial Property\,price}{\partial Flood\,risk}$ <0. Hence, we expect to observe higher flood risk to be associated with lower property price.

After the introduction of Flood Re, insurance companies can transfer the flood risk component of their policies to Flood Re. Therefore they have limited incentives to price flood risk into premiums. Thus, we expect the derivative of premium with respect to flood risk to be zero, $\frac{\partial Premium}{\partial Flood\,risk}$ =0.

[16] There is a number of other potential factors affecting insurance premium, such as property structure and claim record. For simplicity, we assume insurance premium is only affected by flood risk of a property.

As a result, property price is no longer sensitive to flood risk, $\frac{\partial Property\,price}{\partial Flood\,risk}$ =0.

In our empirical analyses, we examine these conjectures by testing the change in the derivative of property price with respect to flood risk after the introduction of Flood Re, detailed in section 3.3.2.

3.3.2 Empirical strategy

We estimate the following equation to identify the effect of flood risk on property prices, more importantly, the mitigating effect of Flood Re:

$$\begin{aligned}\Delta Price(ln)_{i,g,t} = Price(ln)_{i,g,t} - Price(ln)_{i,g,t-1} = \beta_0 + \beta_1 FloodRisk_{i,g,t} + \\ \beta_2 FloodRisk_{i,g,t} \times PostFloodRe_t + \gamma X_{i,g,t} + \delta_{g,t} + \delta_{g,t-1} + \varepsilon_{i,g,t}\end{aligned} \tag{3.2}$$

where $\Delta Price(ln)_{i,g,t}$ is the outcome variable, calculated as the difference between $Price(ln)_{i,g,t}$, the natural logarithm of the value of the property i in 3 digit post code g in year t in the current transaction and the natural logarithm of the value of the same property in the previous transaction, $Price(ln)_{i,g,t-1}$.[17]

$FloodRisk_{i,g,t}$ indicates flood risk of property i, its coefficient β_1 captures the derivative of property prices with respect to flood risk discussed in Section 3.3.1, $\frac{\partial Property\,price}{\partial Flood\,risk}$, before the introduction of Flood Re. There are three sets of flood risk indicator. The primary measurements are a dummy variable $Flooded_{i,g,t}$ which indicates whether the property experiences at least a flood event lasting for more than a day four years before the transaction and a dummy variable $Flash\,flooded_{i,g,t}$ which equals to one if the property only experiences flood event last for a day four years before the transaction. The second measurement is a dummy variable, $Risk(L+M+H)_{i,g,t}$, indicating if the flood risk category of the property is above "very low". The third measurement is a dummy variable, $Distance\,to\,water(<100m)_{i,g,t}$, indicating whether the property is within 100 meters of river or sea. $PostFloodRe_t$ is a dummy variable indicating whether the property transaction is after the implementation of Flood Re, its effect are captured by fixed effects.

The interaction term, $FloodRisk_{i,g,t} \times PostFloodRe_t$, is our variable of interest, the coefficient, β_2, captures the effect of Flood Re on at-risk property price. The derivative of property price

[17] An alternative strategy is comparing the change in transaction prices between flooded properties that are eligible and ineligible to Flood Re. However, eligibility of Flood Re depends on the built year of properties and built year reflects the change in building standard in terms flood resilience, particularly properties built after 2002 (see discussion in section 3.4.2). Therefore, we expect the hypothetical effect of flood event and Flood Re to be different between the eligible and ineligible properties, leading to the underestimation of the mitigating effect of Flood Re.

with respect to flood risk after Flood Re is therefore measured by the summations of β_1 and β_2 in equation 3.2. A negative β_1 and a positive β_2 with similar magnitude support the conjecture that Flood Re brings the derivative of property price to flood risk down to 0 (i.e. mitigate the negative effect of flood risk on property price). $X_{i,g,t}$ is a vector of control variables, reflecting property characteristics (property type, built year and form of tenure).

$\delta_{g,t}$ and $\delta_{g,t-1}$ are 3-digit postcode × year (current transaction) and 3-digit postcode × year (previous transaction) fixed effects respectively. $\delta_{g,t}$ and $\delta_{g,t-1}$ captures the confounding factors, such as the supply of new properties, affecting property values in the 3-digit postcode areas in the years of current and previous transaction[18]; $\varepsilon_{i,g,t}$ is the error term. Standard errors are clustered at the local authority level.

Equation 3.2 is plausibly insufficient to capture the effect of price trend in local property markets because the fixed effects, $\delta_{g,t}$ and $\delta_{g,t-1}$, in equation 3.2 do not precisely capture the price trend within the time interval between the two transactions of each property. To address this concern, we estimate equation 3.3 which control the interactions of $\delta_{g,t}$ and $\delta_{g,t-1}$. The interaction, $\delta_{g,t} \times \delta_{g,t-1}$, allows us to isolate the effect of flood risk and Flood Re on flood-prone properties from other confounding factors and price trend driving value of all properties within the same 3 digit post code area whose current and previous transactions are in the same respective years.

$$\begin{aligned} \Delta Price(ln)_{i,g,t} = Price(ln)_{i,g,t} - Price(ln)_{i,g,t-1} = \beta_0 + \beta_1 FloodRisk_{i,g,t} + \\ \beta_2 FloodRisk_{i,g,t} \times PostFloodRe_t + \gamma X_{i,g,t} + \delta_{g,t} \times \delta_{g,t-1} + \varepsilon_{i,g,t} \end{aligned} \tag{3.3}$$

3.4 Data and Sample

3.4.1 Data

To implement our empirical strategy, we employ three categories of data sets. The first category is the property transaction data, the second category is the measurements of property flood risk and the third category is the characteristics of local authority district.[19] We summarize these data sets below.

[18] Each 3-digit postcode contains on average around 6,000 properties (Garbarino and Guin, 2021).

[19] Local authority district is a level of administrative division of England. There are a total of 343 local authority districts in England, comprising five types of local authority: county councils, district councils, unitary authorities, metropolitan districts and London boroughs.

3.4.1.1 HM Land Registry Price Paid Data

We use Price Paid Data (PPD) from HM Land Registry, which covers the universe of property transactions in England and Wales since 1995. In particular, it provides information on the exact address of each property, the transaction date and the transaction price and the property characteristics.[20] The set of geographical information and property characteristics allow us to differentiate other confounding factors driving property values. This data set does not differentiate whether the transacted property is buy-to-let or buy-to-live, however the difference in buying purpose unlikely affects our results. Because homeowners are in charge for repairs and restoration of the flooded properties even they let the properties, therefore it hardly affects the incentive of homeowners to get their properties insured. This data set was used by several researches in studying the UK property market (e.g. Giglio et al. (2015); Bracke and Tenreyro (2021)).

3.4.1.2 Recorded Flood Outlines

Our primary measurement of flood risk is based on historical flood events, we employ the Recorded Flood Outlines produced by the Environment Agency to identify flood history of each property. The Recorded Flood Outlines records historic flooding from rivers, the sea, groundwater and surface water since 1946 as GIS layers.[21] To match them with the property transaction data set, we map these layers to 6-digit postcode units.[22] In the interest of this paper, the data records the exact dates of the start and end of each flood outline, thus allowing us to calculate the duration of each flood event and the time interval between each property transaction and the latest flood event experienced by the respective property. To highlight the differential effect of flood events on property values, we identify property as "flooded" if there is at least a flood event lasting for more than a day within the four years before the transaction and we identify property as "flash-flooded" if there are only flood events lasting for a day within the four years before the transaction. The locations of the "flooded" properties are depicted in Figure 3.3. It shows that most of the flooded properties are clustered in North West, Yorkshire and the Humber, South West and South East. Midlands and East of England are less exposed to flood events.

[20] The property characteristics include property type (Detached or Semi-detached or Terraced or Flat or Other); whether the property is new-built; and the forms of tenure (Freehold or Leasehold).

[21] Completeness of the data in early years was questionable, but it has improved over the years and flood events in recent years, including our sample period, were well-recorded.

[22] 6-digit postcode covers a small area which on average only have 15 properties and there are around 1.7 million postcodes in the UK.

3.4.1.3 Flood Map

Flood Map provided by the Environment Agency indicates the number of property in each flood risk categories per 6-digit postcode unit.[23] The map has been available online and updated annually since 2004.[24] For our analysis, we use the 2016 Flood Map and follow Garbarino and Guin (2021), using the midpoint of the flood risk probability for each bucket to calculate the average annual flood probability of each 6-digit post code. Different from historical flood events, the Flood Map offers an estimation of the *ex-ante* flood risk of properties. In the paper, we identify properties in 6 digit post codes with average annual flood probability over 0.1% as at-risk property and the locations of these properties are shown in Figure 3.4. It shows that at-risk properties are clustered in similar areas that have been exposed to flood events, i.e. most of the flooded properties are clustered in North West, Yorkshire and the Humber, South West and South East. It also shows that there are more at-risk properties than properties that are actually flooded, as shown by Figure 3.3.

3.4.1.4 Distance to water

As another alternative measurement of flood risk, we calculate the shortest distance between each 6-digit postcode to river or sea, whichever the distance is shorter. This measurement estimates flood risk based on the distance to water. Properties that are within 100 meters to water are defined as at-risk categories in the paper. Figure 3.5 shows the locations of these properties, suggesting that properties that are near to river and sea are scattered in different parts of England, apart from the areas connected to Wales and Scotland.[25]

3.4.1.5 Local authority characteristics

To examine the heterogeneous effects of Flood Re in different areas. We employ the English Indices of Deprivation to measure the deprivation level of local authorities; the population estimates by the Office of National Statistics to measure proportion of population with National Qualifications Framework (NQF) level 4 or above qualification (e.g. degree with honours and postgraduate certificate), average income and age of local authorities; 2001 Rural-urban classification produced by the Office for National Statistics to differentiate urban and rural areas; general election results recorded by the House of Common to measure the percentage of votes for the Green Party in local authorities in the 2019 United Kingdom general election; and EU

[23] There are four categories in 2016 flood map: very low (one-year ahead flood probability less than 0.1%), low (between 0.1% and 1%), medium (between 1% and 3.3%) and high (greater than 3.3%)

[24] Although the Flood Map is updated annually, the variations across year are rather limited, apart from the update in 2013-2014 (Garbarino and Guin, 2021).

[25] A caveat of this measurement is that it does not consider elevation. But we argue it would only marginally affect the classification of at-risk properties, because it is rather rare that elevation tremendously increases within 100 meters.

referendum results recorded by the Data.gov.uk.[26]

3.4.2 Sample construction

The initial sample starts with the universe of all property transactions in 1995-2020 in England. The first step of sample filtering addresses the concern over the change in the public planning of new buildings after the publication of the Planning Policy Guidance Note 25 (PPG25) (DTLR, 2001). The PPG25 required local planning authorities to employ a set of decision rules accounting for flood risk. It also required them to consult with the EA on approvals for permissions to build in areas at the risk of flooding. As a results, the EA rejection rate of development permission on flood risk ground increased from 10% in 2001 to 22% in 2002, and further increased to 33% in 2004 (Porter and Demeritt, 2012). Properties built after the publication of the PPG25 are therefore expected to be less prone and more resilient to flood risk. To alleviate this concern, our sample excludes properties built after 2002.

To examine the price change of the same property over time, we construct the subsample of properties that were transacted at least twice since 1995 and at least one transaction is in the sample period which covers the four years before and after Flood Re. We then convert the data into panel structure by identifying the series of transactions of the same property by using address information.[27] After taking first difference of the transaction price, it results in 1,754,067 observations of 1,563,062 properties. With this sample, we then match the three flood risk indicators with the 6-digit postcode units and match local authority-level variables with the local authority identifier.

Summary statistics of the sample are shown in Table 3.1. In Panel A, we present the summary statistics of property-level variables. Average property price in our sample is GBP 226,840 with a growth rate of 42.4% between transactions. The appreciation of properties is rather large because of the long time interval between transactions. The average transaction time interval of a property in the sample is around eight years and four months. For property characteristics, a small proportion of properties are newly built at the time of the previous transaction; majority of property types in the sample is detached, semi-detached or terraced, and around 15% of them are flats. Regarding the tenure type, a large majority of the properties are freehold and the remaining are leasehold. In Panel B, we shows the summary statistics of the four flood risk measurements. There are around 0.3% of observations experience at least one flood event last for more than a

[26]Because the seven local authority level variables are produced in different years and the classification of local authority changes over time, a very small number of observations in the property transaction data set fail to match with the measurements.

[27]Restricting the pre-Flood Re period to four years mitigates the concern that our findings are simply driven by the improvement of flood defence over time, which could potentially explain why the effect of flood event on property prices disappear in the later years of the sample period (post-Flood Re period).

day four years before property transaction and 0.1% of observations experience only flood event(s) last for a day four years before property transaction. 11% of properties are classified as at-risk properties in terms of the annual probability of being flooded and 7.5% of properties are located within 100 meters of river and sea. In Panel C, we summarize the seven local-authority level characteristics used for sample-split tests.

3.5 Results

3.5.1 Effect of flood events and Flood Re on property prices

This section starts with examining the average effect of flood and flash flood on property prices over the sample period. Without differentiating the period before and after the implementation of Flood Re, we expect the negative effect of flood events to be underestimated because Flood Re is expected to mitigate the negative effect of flood on property values. This exercise allows us to compare the estimation results after introducing the variable that differentiate the sample period after the implementation of Flood Re from the period before the implementation of Flood Re. Column 1 in Table 3.2 presents the estimation results of equation 3.2 without interacting $FloodRisk_{i,g,t}$ with $PostFloodRe_t$ and without any property level control variables. The results confirm some previous findings (Lamond and Proverbs, 2006; Kousky, 2010; Bernstein et al., 2019), suggesting that flooded property experience a 0.9% (t-statistics -2.21) decrease in property prices, while there is no effect of flash flood on property prices, reflecting the salience of flood events affects the impacts on property values.

We then introduce the variable $PostFloodRe_t$ into the estimations to differentiate the effect of flood after the introduction of Flood Re from before the introduction of Flood Re. The estimation results are shown in column 2-5 of Table 3.2. The interaction term, $Flooded_{i,g,t} \times PostFloodRe_t$, indicates whether Flood Re plays a role in mitigating the negative effect of flood events, a positive coefficient suggests that Flood Re mitigates the effect of flood events on property values. Apart from the interaction term, we also expect the introduction of the interaction term $Flooded_{i,g,t} \times PostFlood_t$ to increase the magnitude of the estimated coefficient of $Flooded_{i,g,t}$, comparing with the results in column 1.

Estimation results in all specifications consistently suggest that flood events lower property prices and Flood Re completely mitigates the negative pricing effect. Consistent with the findings in column 1, there is no evidence that flash floods affect property prices in either periods (before and after the implementation of Flood Re). Column 2-3 present the estimation results of equation 3.2. In column 2, the results suggest that flood event longer than a day reduces property values by 1.8% (t-statistics -3.14) before Flood Re and the negative effect reduces to only 0.3% after the introduction of Flood Re. Column 3 presents the results with property control variables. The inclusion of the control variables generates similar results, although the coefficients of $Flooded_{i,g,t}$ and $Flooded_{i,g,t}$ x $PostFlood_t$ are slightly reduced. Column 4-5 show the estimation results with our preferred specification in equation 3.3, introducing the interaction of $\delta_{g,t}$ and $\delta_{g,t-1}$ in the specification. Column 4 presents the results without control variables and column 5 shows the results with control variables. The results are similar to column 2-3. Column 4 shows that value of flooded properties drop by 2.1% (t-statistics -3.39) before Flood Re and the negative effect of flood on property prices reduce to only 0.2%. With control variables, column 5 shows that a flooded properties experience a 1.6% drop in value. The estimated coefficient of the variable, $Flooded_{i,g,t} \times PostFlood$, is 0.018 ($t$-statistics 2.68), suggesting that flood events do not reduce property values after the implementation of Flood Re.

3.5.2 Falsification tests

To examine whether property prices are indeed affected by Flood Re, we conduct two falsification tests. The first test relates to the introduction of Flood Re.[28] The second test then relates to flood events.

In the first test, we redefine the sample to property transactions in April 2010 to April 2016 and use the extension of the Statements of Principal (SoP) in July 2013 as a placebo treatment to flooded properties. We replace the variable $PostFloodRe_t$ in equation 3.2 and 3.3 with $PostSoP\,extension_t$, which equals to 1 if the transaction is after July 2013 (0 otherwise).

This specification estimates how the SoP extension affects flooded property prices. Because the SoP had already been in place before the extension, it should not affect flooded property prices. Different specifications in column 1-4 in Panel A of Table 3.3 shows that the interaction term is not different from zero, suggesting that value of flooded properties is unaffected by the placebo treatment.

In the second falsification test, we employ the genuine Flood Re introduction date but verify the effect of flood treatment. Specifically, we constrain the sample to properties that are not being flooded in the past four years of transactions. We then randomly assign properties to be "flooded" properties and replicate the estimation equation 3. We then run Monte Carlo simulations with 1,000 replications of equation 3.3 to check whether non-flooded properties are affected by Flood Re.

This exercise estimates how non-flooded properties affected by Flood Re. Because Flood Re should not affect properties that are not at flood risk, the null of zero effect is true. Thus, we

[28] It implicitly tests whether there are announcement effects.

should only reject the null by making Type 1 errors. Panel B of Table 3.3 shows that the rejection rates are in line with those that would occur through Type 1 errors. In most cases, the average value the coefficients of $Pseudoflood_{i,g,t}$ and $Pseudoflood_{i,g,t} \times PostFloodRe_t$ are close to 0, suggesting that non flooded properties are unaffected by Flood Re.

3.5.3 Heterogeneous effects of Flood Re

In this section, we examine the heterogeneous effects of flood and Flood Re on property prices. Specifically, we examine whether Flood Re has different effects in subsamples, e.g. across property values and across different regions.

3.5.3.1 Demographic characteristics

To start with, we provide evidence on the heterogeneous effects of Flood Re in terms of property values. To do so, we replicate the estimation in column 5 of Table 3.2 with samples of specific percentiles of the property prices in the first transaction.[29] Figure 3.6 shows the estimated coefficient and 95% confidence interval of the variable $Flooded_{i,g,t}$ x $PostFlood_t$ in each sub-sample. We find that Flood Re has a stronger effect on more expensive properties (properties whose value is higher than the 60^{th} percentile ($p60$) of property prices in the sample) and having limited effect on lower-value properties (properties whose value is lower than or equals to the $p60$ of property prices in the sample). Yet the figure does not inform the population characteristics of areas benefited more from Flood Re.

We then go on and provide a richer picture on the heterogeneous effects of Flood Re. To do so, we combine different local authority level indicators with property transaction data. Then we split the sample based on the median value of each indicator (apart from the urban/rural indicator) and replicate the estimation of equation 3.3.[30] The results in this section inform us whether the effects of flood and Flood Re are stronger in certain areas, and whether the difference is statistically significant. While the results in this section shed light on different channels leading to the heterogeneous effects, we do not seek to fully disentangle the different channels without any more granular data.

First, we examine the heterogeneous effects of Flood Re in terms of income level. The result is important to evaluate the policy objective of Flood Re. With the aim of promoting affordability of flood insurance, the targeted beneficiaries of Flood Re should be the lower income groups. However, social class often reflects the differences in financial sophistication and awareness of climate risk (Fielding and Burningham (2005); Fielding (2012)). The differences could eventually

[29] The specified percentiles used in the estimations are $p20$, $p40$, $p60$, $p80$ and $p100$.
[30] The correlation matrix of the indicators is shown in Table A3.3 in the appendix.

lead to the heterogeneous effects of Flood Re in different social classes. We employ average income level of local authority district to examine this conjecture. In Table 3.4, column 1 (2) shows the estimation results with the properties in the local authorities with higher (lower) average income. The results suggest that local authorities that have higher average income have a stronger negative effect of flood event on property prices. More importantly, the coefficients of $Flooded_{i,g,t} \times PostFlood_t$ across the two columns suggest that the households with higher income benefit more from Flood Re through the appreciation of property values. The Chow test F-statistics verify that the coefficients of the two groups are significantly different at 5% significance level.

To address the concern that income is an unreliable measure of deprivation and poverty (Ringen, 1987, 1988), we employ the English indices of deprivation to measure relative deprivation. Apart from income, the English indices of deprivation provides an all-rounded measurements of deprivation which takes into account of other six domains of deprivation, including employment, education, health, crime, barriers to housing and local services, and living environment.[31] Column 3 (4) presents the estimation results with the properties in the more (less) deprived local authorities. Consistent with the results in column 1-2, the results suggest that local authorities that are less deprived have a stronger negative effect of flood event on property prices and the less deprived households benefit more from Flood Re. The Chow test F-statistics also suggests that the coefficients of the two groups are significantly different at 5% significance level. Taken the results of the first and second set of sample split together, we show that Flood Re disproportionately benefit wealthier households, in terms of the appreciation of flood-prone properties' value.

The next set of sample split builds upon the finding that the awareness of Flood Re is positively related to age.[32] Because the older people are more aware of the introduction of Flood Re, we expect the effect of Flood Re is stronger in areas with a higher average age. Consistent with the finding in the survey data, column 5-6 in Table 3.4 suggest that effect of flood events are similar across older and younger group, but the effect of Flood Re is stronger in the areas with older households. Column 5 shows that flooded property in local authorities with older households sell at 2.1% discount and the introduction of Flood Re completely mitigate the negative effect. Column 6 shows that flooded property in local authorities with younger households sell at 1.7% discount and the introduction of Flood Re has no effect on the value of flooded properties. The Chow test F-statistics verifies that the coefficients of the two groups are significantly different at 5% significance level. The results imply that the difference in the awareness of Flood Re affects

[31] See Payne and Abel (2012) for more details of the background and computation method of the English indices of deprivation.

[32] We employ the survey data of the 2018 Availability and Affordability of Insurance report conducted by the DEFRA to examine the correlation between different demographic characteristics and the awareness of Flood Re. We find that older respondents in at risk areas are more likely to know Flood Re. The results are presented in Table A3.2 in the appendix.

its impact on property values.

Education level plausibly reflect households' financial sophistication and awareness of public policy change. If that is true, Flood Re could have a stronger impact in higher educated areas. In column 7 and 8, we find that areas with more educated population have a stronger effect of Flood Re on flooded properties value, however, the Chow test F-statistics suggest that the difference in coefficients is statistically insignificant at 10% level.

We then examine the heterogeneous effects of flood risk and Flood Re in urban and rural areas. Due to the subtle differences in property market structure, characteristics of properties, demographic composition and types of flooding in urban and rural areas, the effect of flood and Flood Re in urban areas could be different from rural areas. If this is the case, Flood Re could imply a wealth redistribution among urban and rural population. For example, Beltrán et al. (2019) show that the value of rural properties is less affected by flood events. In column 9-10 of Table 3.4, we find that both the effect of flood and Flood Re is stronger in urban areas. The Chow test F-statistics suggests that the coefficients of the two groups are significantly different at 1% significance level.

3.5.3.2 Revealed believes

Heterogeneous beliefs in climate risks affect property values. Baldauf et al. (2020) find that value of properties at climate risk in areas with more believers of future climate risk are more likely to sell at discount. We therefore expect that areas with greater concern of climate risks respond stronger to flood risk and Flood Re.

We employ the percentage of votes for the Green Party in the 2019 United Kingdom general election results to measure the difference in belief of climate change risk across local authorities. If awareness of climate risk is the driver of the heterogeneous effects, the effect of flood and Flood Re is expected to be stronger in local authorities with higher share of votes to the Green Party. Column 1-2 in Table 3.5 present the estimation results. Surprisingly, the Chow test suggests that there is no significant difference across the two groups. Apart from the Chow test, the coefficients of the two key variables, $Flooded_{i,g,t}$ and $Flooded_{i,g,t} \times PostFloodRe_t$, are similar across the two groups, despite of the lower statistical significance in the group with more votes for the Green Party. The results imply that the difference in concern in climate risk do not explain the heterogeneous effects of Flood Re across different local authorities.

A survey conducted by Savanta ComRes suggests that Brexit voters are almost twice as unlikely to believe in climate change risk.[33] We therefore use the vote results for Brexit as an alternative measurement of average level of climate risk concern on local authority level. The results in column 3-4 suggest that areas with higher vote percentage for Brexit show a stronger impact of Flood Re, yet the Chow test suggests that the differences in coefficients among the two sub-group are statistically insignificant at 10% significance level.

3.5.4 Alternative measurements of flood risk

In this section, we show that our findings are robust to alternative measurements of flood risk. In panel A of Table 3.6, we use the the flood risk categories in the flood map of the Environment Agency to measure *ex-ante* flood risk of properties. Properties that are in the flood risk categories above "very low" are classify as at-risk properties. The results are similar across different specifications in column 1-4. In the preferred specification in column 4, we find that property at flood risk decrease 0.4% (*t*-statistics -3.11) in value before Flood Re, but the negative effect disappears after the introduction of Flood Re.

Panel B of Table 3.6 employs distance to water (source of water is either river or sea) as another alternative measurement of flood risk. We classify properties located within 100 meters of water as at risk properties. The results are still consistent across different columns. In the preferred specification in column 4, we find that properties located within 100 m of sea or river sell at a discount of 0.8% (*t*-statistics -7.04) before the introduction of Flood Re and this negative effect is mitigated by Flood Re.

In the appendix, we replace the dummy variables of the three categorical measurements of flood risk with continuous measurements, namely duration of flood, flood risk probability and distance to water. We find consistent results. The results in Table A3.4 suggest that the negative effect increase with the severity of flood risk measured by the 3 continuous measurements. In all three continuous measurements, Flood Re mitigates the negative effect of flood risk on property values.

3.5.5 Effect on trade volume

The discount of flood prone property could have an implications on transaction volume. Following the loss aversion consideration in Genesove and Mayer (2001), owners of flood-prone properties may defer selling the flooded properties until the effect of flood fades away over time. If this is the case, we should expect recently flooded properties are less likely to be traded, and this

[33] Details of the survey can be found on https://comresglobal.com/polls/assaad-razzouk-eu-referendum-and- science-poll/.

effect should be mitigated by the introduction of Flood Re. To examine the changes in transaction volume accompanying flood events and the introduction of Flood Re, we follow Bernstein et al. (2019) to expand the original sample into a balanced panel data set (i.e. each property has an observation in each year of the sample period) to estimate the following equation 3.4:

$$\begin{aligned} Trade_{i,g,t} = \beta_0 + \beta_1 Flooded_{i,g,t} + \beta_2 Flooded_{i,g,t} \times PostFloodRe_t + \\ \beta_3 Flashflooded_{i,g,t} + \beta_4 Flashflooded_{i,g,t} \times PostFloodRe_t + \gamma X_{i,g,t} + \delta_{g,t} + \varepsilon_{i,g,t} \end{aligned} \tag{3.4}$$

where $Trade_{i,g,t}$ is the outcome variable, indicating whether the property is traded in year t, $Trade_{i,g,t}$=1 if property i is traded in year t, 0 otherwise. $\delta_{g,t}$ captures the confounding factors affecting the property of being traded in the 3 digit postcode g in year t. Definitions of other variables follow equation 3.2.

In column 1 of Table 3.7, we start with examining if being flooded within the past 4 years reduce the probability of being transacted by estimating equation 3.4 without the interaction terms. Consistent with our expectations, the results show that flooded properties are 0.5% less likely to be transacted (from a base transaction rate of 14.6%). The results also suggest that flash flood does not affect the probability of transaction. We then introduce the interaction terms of $FloodRisk_{i,g,t} \times PostFloodRe_t$. The results are similar irrespective of the inclusion of property control variables (shown in column 2 and 3 of of Table 3.7). The results with control variables are shown in column 3, suggesting that flooded properties are 3.6% less likely to be transacted in the following four years of flood (from a base transaction rate of 14.6%), but Flood Re not only mitigates the negative effect, it increases the transaction probability by 2.4%. The results plausibly reflect the sales of the accumulated properties that were flooded before the introduction of Flood Re.[34]

3.6 Conclusion

In this chapter, we examine how the introduction of a public reinsurance scheme, Flood Re, in the UK affects value and liquidity of properties at flood risk. Our results suggest that Flood Re mitigates the negative effect of flood risk on property prices and transaction volume. We also find that Flood Re has heterogeneous effects on property prices in different areas. The effect on property prices are stronger in urban areas and areas with wealthier, older and less deprived populations. Yet we do not find strong evidence that the effect of Flood Re are different in terms

[34] Apart from the probability of trade, we also find that flooded properties are being traded later than non-flooded properties, and Flood Re completely mitigates this effect. The results are shown in Table A3.5 in the appendix, we temper the interpretation of the results because this test plausibly suffers from reverse causality between the probability of being flooded and the time interval between transactions.

of their climate-related preferences, revealed by voting outcomes in the 2019 general election and the 2016 United Kingdom European Union membership referendum.

Our paper offers two key policy implications. First, the results highlight the transition risk of public policy interventions. Flood Re is planned to phase out in 2039. The flood risk component of property insurance is therefore expected to be fully priced into premiums by that time. Consequently, value of properties at flood risk may experience a sudden adjustment, reflecting the increase in current and future premiums, which can disrupt property and financial markets. Second, our results highlight the plausibly unintended distributional consequences of Flood Re. While Flood Re is expected to help lower-income households, our results suggest that Flood Re has a weak impact in lower income and more deprived areas but a stronger impact in higher income and and less deprived areas. This finding provides an unique insight in examining the effectiveness of Flood Re and the design of future public policies in mitigating climate risk.

There are potential research directions that are beyond the scope of this chapter because of the data limitations. Particularly, future work can identify and differentiate the channels in driving the heterogeneous impacts of Flood Re in different demographic groups. Is it because of the difference in financial sophistication or awareness of future climate risk or local property market structure or other potential channels?

3.7 Tables and figures

Table 3.1: Summary statistics

Variable	N	Mean	Std. Dev.	p5	p95
Panel A: Property variables					
Property price (ln)	1,754,067	12.332	0.618	7.313	19.163
D. Property price (ln)	1,754,067	0.424	0.33	-0.019	1.249
New built$_{t-1}$	1,754,067	0.029	0.169	0	1
Property type:					
Detached	1,754,067	0.233	0.423	0	1
Semi-detached	1,754,067	0.288	0.453	0	1
Terraced	1,754,067	0.319	0.466	0	1
Flat	1,754,067	0.153	0.36	0	1
Other	1,754,067	0.008	0.087	0	1
Tenure:					
Freehold	1,754,067	0.801	0.399	0	1
Leasehold	1,754,067	0.199	0.399	0	1
Panel B: Flood risk variables					
Flooded	1,754,067	0.003	0.059	0	1
Flash-flooded	1,754,067	0.001	0.031	0	1
Risk (L+M+H)	1,754,067	0.109	0.312	0	1
Distance to water (<100 m)	1,754,067	0.075	0.263	0	1
Panel C: Local authority characteristics					
Annual household income	324	42,745.470	8,270.216	32,338.461	57,644.445
Index of Multiple Deprivation	308	19.777	8.012	8.500	34.300
Age	308	42.144	5.094	33.300	50.500
Urban	330	0.727	0.446	0.000	1.000
Education level (%)	324	27.212	7.903	16.900	41.000
Votes for the Green Party (%)	316	2.970	2.007	0.000	5.637
Votes for Brexit (%)	330	54.504	9.963	32.540	68.860

Notes: This table provides descriptive statistics for the variables used in the empirical analysis. Summary statistics of property level variables are presented in Panel A. Panel B summarizes statistics of the measurements of flood risk. Summary statistics of local authorities level variables are shown in Panel C. (ln) denotes that a variable is measured in natural logarithm.

Table 3.2: Effect of flood events and Flood Re on property prices

	1	2	3	4	5
Dependent variable	D. Property price (ln)				
Flooded	-0.009**	-0.018***	-0.015***	-0.021***	-0.016***
	(-2.21)	(-3.14)	(-2.70)	(-3.39)	(-2.97)
Flooded x Post Flood Re		0.015**	0.014**	0.019**	0.018***
		(2.07)	(2.15)	(2.58)	(2.68)
Flash-flooded	0.002	0.001	0.004	0.000	0.004
	(0.34)	(0.09)	(0.57)	(0.01)	(0.49)
Flash-flooded x Post Flood Re		0.003	0.000	0.004	0.001
		(0.20)	(0.02)	(0.30)	(0.08)
3 dig plc X Year FE (current)	Yes	Yes	Yes	No	No
3 dig plc X Year FE (previous)	Yes	Yes	Yes	No	No
3 dig plc X Year FE (current) X Year FE (previous)	No	No	No	Yes	Yes
Built year FE	Yes	Yes	Yes	Yes	Yes
Property controls	No	No	Yes	No	Yes
Observations	1,754,067	1,754,067	1,754,067	1,754,067	1,754,067
R^2	0.761	0.761	0.766	0.788	0.792

Notes: Column 1 of this table presents estimation results of equation 3.2 without the interaction variable, Flood Risk × Post Flood Re. Column 2 and 3 of this table present estimation results of equation 3.2. Column 4 and 5 of this table presents estimation results of equation 3.3. Measurements of flood risk in this table is Flooded and Flash-flooded. The dependent variable in this table is D. Property price (ln) and property control variables include sets of dummy variables indicating property types, forms of tenure and whether the property is new built in the previous transaction. Definitions of variables are detailed in Table A3.1 in the appendix. Standard errors are clustered at local authority district level and the corresponding t-statistics are reported in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Table 3.3: Placebo tests

Panel A (Placebo test: Extension of the SoP in July 2013)	1	2	3	4
Dependent variable	D. Property price (ln)			
Flooded	-0.013**	-0.012**	-0.015**	-0.014**
	(-2.11)	(-1.97)	(-2.15)	(-2.01)
Flooded x Post SoP extension	-0.001	0.002	-0.001	0.003
	(-0.09)	(0.28)	(-0.10)	(0.30)
Flash-flooded	0.001	0.001	0.007	0.007
	(0.10)	(0.09)	(0.58)	(0.58)
Flash-flooded x Post SoP extension	0.002	0.006	-0.003	0.001
	(0.14)	(0.45)	(-0.22)	(0.09)
3 dig plc X Year FE (current)	Yes	Yes	No	No
3 dig plc X Year FE (previous)	Yes	Yes	No	No
3 dig plc X Year FE (current) X Year FE (previous)	No	No	Yes	Yes
Built year FE	Yes	Yes	Yes	Yes
Property controls	No	Yes	No	Yes
Observations	933,566	933,566	933,566	933,566
R^2	0.796	0.801	0.818	0.822

Panel B (Monte Carlo simulations for the role of flood and Flood Re)	1	2
Dependent variable	D. Property price (ln)	
Explanatory variable	Placebo-flooded	Placebo-flooded x Post Flood Re
Rejection rate at the 10% lelvel (2-tailed test)	13.60	11.40
Rejection rate at the 5% lelvel (2-tailed test)	7.30	7.40
Rejection rate at the 1% lelvel (2-tailed test)	2.60	1.80
Mean coefficient (*t*-statistics)	-0.002 (-0.50)	0.003(0.60)

Notes: Column 1 and 2 in Panel A of this table present estimation results of equation 3.2 with the placebo treatment (extension of the SoP). Column 3 and 4 of this table present estimation results of equation 3.3 with the placebo treatment (extension of the SoP). Definitions of variables are detailed in Table A3.1 in the appendix. Standard errors are clustered at local authority district level and the corresponding *t*-statistics are reported in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively. Column 1 (2) of Panel B shows the rejection rates of the null hypothesis of the estimated coefficient of Placebo-flooded (Placebo-flooded x Post Flood Re)=0 at the 10%, 5%, and 1% levels, the mean coefficient and *t*-statistics of the two variables are also presented.

Table 3.4: Effect of Flood Re on property prices (Sample split-demographic characteristics)

	1	2	3	4	5	6	7	8	9	10
Dependent variable	D. Property price (ln)									
Sample split	Annual household income		Index of multiple deprivation		Age		Education		Urban/Rural area	
	≥$p50$	<$p50$	≥$p50$	<$p50$	≥$p50$	<$p50$	≥$p50$	<$p50$	Urban	Rural
Flooded	-0.024***	-0.001	-0.010*	-0.021***	-0.021**	-0.017**	-0.023***	-0.004	-0.022***	-0.003
	(-3.44)	(-0.12)	(-1.76)	(-2.99)	(-2.57)	(-2.37)	(-3.14)	(-0.56)	(-3.54)	(-0.36)
Flooded x Post Flood Re	0.019**	0.007	0.004	0.027***	0.029***	0.009	0.023**	0.008	0.022**	0.008
	(2.38)	(0.70)	(0.43)	(3.46)	(3.29)	(0.78)	(2.47)	(0.88)	(2.57)	(0.91)
Flash-flooded	0.024*	-0.004	-0.001	0.014	0.009	-0.005	0.007	0.005	0.002	0.005
	(1.91)	(-0.43)	(-0.12)	(1.22)	(0.88)	(-0.36)	(0.67)	(0.41)	(0.14)	(0.56)
Flash-flooded x Post Flood Re	-0.015	0.007	0.000	0.002	0.002	-0.002	0.000	-0.004	-0.000	0.010
	(-0.83)	(0.39)	(0.02)	(0.11)	(0.14)	(-0.07)	(0.01)	(-0.19)	(-0.02)	(0.53)
3 dig plc X Year FE (current) X Year FE (previous)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Built year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Property controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Chow test F-statistics	2.44		2.71		2.85		1.21		3.50	
Observations	878,670	842,721	880,974	790,351	742,261	928,234	882,770	835,253	1,330,189	413,634
R^2	0.796	0.790	0.788	0.801	0.791	0.795	0.795	0.793	0.793	0.795

Notes: This table presents estimation results of equation 3.3 based on different sub-samples. Sample in column 1 (2) includes property transactions in local authority districts with higher (lower) average annual income. Sample in column 3 (4) includes property transactions in local authority districts with higher (lower) Index of multiple deprivation. Sample in column 5 (6) includes property transactions in local authority districts with higher (lower) age. Sample in column 7 (8) includes property transactions in local authority districts with higher (lower) education level. Sample in column 9 (10) includes property transactions in local authority districts in urban (rural) area. Measurements of flood risk in this table is Flooded and Flash-flooded. The dependent variable in this table is D. Property price (ln) and property control variables include sets of dummy variables indicating property types, forms of tenure and whether the property is new built in the previous transaction. Definitions of variables are detailed in Table A3.1 in the appendix. The Chow test F-statistic is the F-statistic from a Chow test for equality of the estimated coefficients between the two respective sub-samples. Standard errors are clustered at local authority district level and the corresponding t-statistics are reported in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Table 3.5: Effect of Flood Re on property prices (Sample split-revealed believes)

	1	2	3	4
Dependent variable	D. Property price (ln)			
Sample split	Percentage of vote for the Green Party		Percentage of vote for Brexit	
	$\geq p50$	$<p50$	$\geq p50$	$<p50$
Flooded	-0.016*	-0.017***	-0.007	-0.022***
	(-1.80)	(-2.75)	(-0.84)	(-2.99)
Flooded x Post Flood Re	0.016	0.020**	0.012	0.022**
	(1.30)	(2.41)	(1.07)	(2.37)
Flash-flooded	-0.005	0.016	0.004	0.004
	(-0.43)	(1.42)	(0.36)	(0.36)
Flash-flooded x Post Flood Re	0.012	-0.012	-0.002	0.003
	(0.59)	(-0.70)	(-0.10)	(0.22)
Chow test F-statistics	0.50		1.15	
3 dig plc X Year FE (current)	Yes	Yes	Yes	Yes
Built year FE	Yes	Yes	Yes	Yes
Property controls	Yes	Yes	Yes	Yes
Observations	889,755	850,770	782,499	961,677
R^2	0.798	0.791	0.796	0.791

Notes: This table presents estimation results of equation 3.3 based on different sub-samples. Sample in column 1 (2) includes property transactions in local authority districts with higher (lower) percentage of vote for the Green Party. Sample in column 3 (4) includes property transactions in local authority districts with higher (lower) percentage of vote for Brexit. Measurements of flood risk in this table is Flooded and Flash-flooded. The dependent variable in this table is D. Property price (ln) and property control variables include sets of dummy variables indicating property types, forms of tenure and whether the property is new built in the previous transaction. Definitions of variables are detailed in Table A3.1 in the appendix. The Chow test F-statistic is the F-statistic from a Chow test for equality of the estimated coefficients between the two respective sub-samples. Standard errors are clustered at local authority district level and the corresponding t-statistics are reported in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Table 3.6: Effect of Flood Re on property prices- Alternative measurements of flood risk

Panel A	1	2	3	4
Dependent variable	D. Property price (ln)			
Risk (L+M+H)	-0.006***	-0.004***	-0.006***	-0.004***
	(-4.83)	(-3.24)	(-4.56)	(-3.11)
Risk (L+M+H) x Post Flood Re	0.005***	0.004***	0.004***	0.004***
	(3.74)	(3.67)	(3.55)	(3.45)
3 dig plc X Year FE (current)	Yes	Yes	No	No
3 dig plc X Year FE (previous)	Yes	Yes	No	No
3 dig plc X Year FE (current) X Year FE (previous)	No	No	Yes	Yes
Built year FE	Yes	Yes	Yes	Yes
Property controls	No	Yes	No	Yes
Observations	1,754,067	1,754,067	1,754,067	1,754,067
R^2	0.761	0.766	0.788	0.792
Panel B	**1**	**2**	**3**	**4**
Distance to water (<100m)	-0.012***	-0.008***	-0.013***	-0.008***
	(-11.04)	(-7.17)	(-10.49)	(-7.04)
Distance to water (<100m) x Post Flood Re	0.005***	0.004***	0.005***	0.005***
	(4.31)	(3.99)	(4.46)	(4.08)
3 dig plc X Year FE (current)	Yes	Yes	No	No
3 dig plc X Year FE (previous)	Yes	Yes	No	No
3 dig plc X Year FE (current) X Year FE (previous)	No	No	Yes	Yes
Built year FE	Yes	Yes	Yes	Yes
Property controls	No	Yes	No	Yes
Observations	1,754,067	1,754,067	1,754,067	1,754,067
R^2	0.761	0.766	0.788	0.792

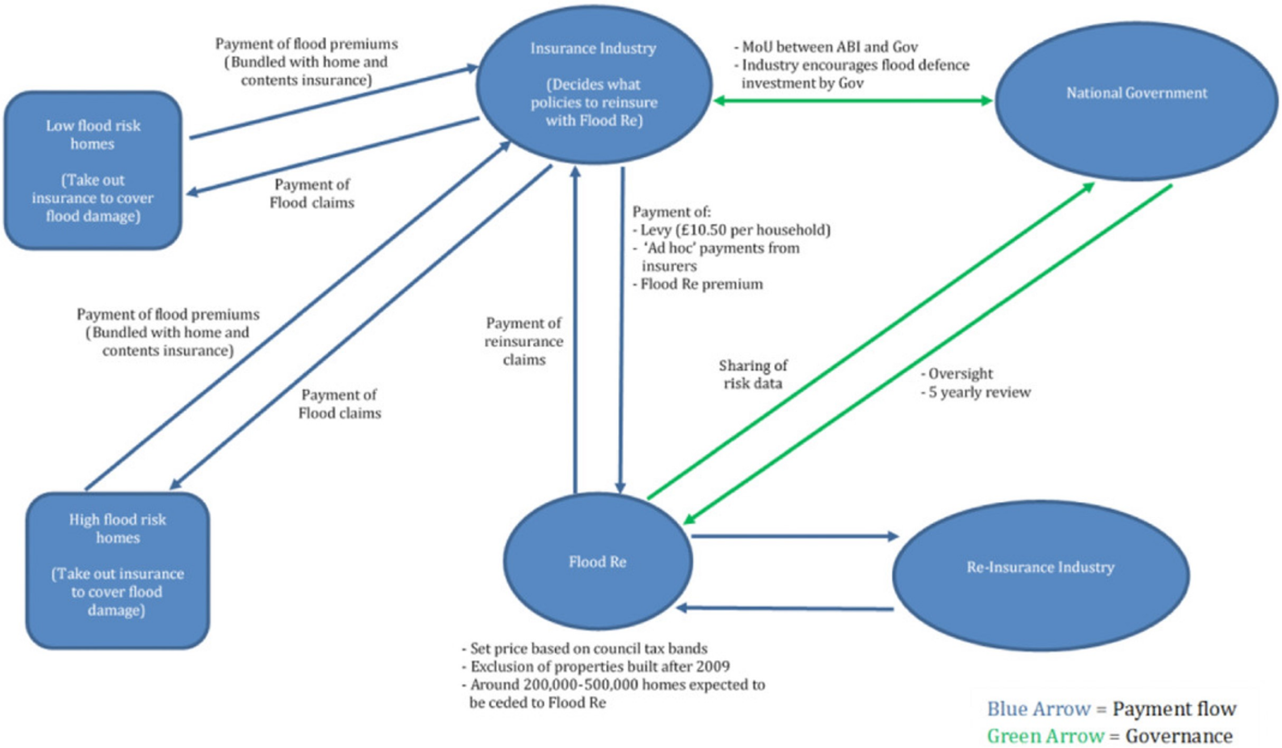
Notes: Column 1 and 2 of this table presents estimation results of equation 3.2. Column 3 and 4 of this table presents estimation results of equation 3.3. Measurement of flood risk is Risk (L+M+H) in Panel A and Distance to water (<100m) in Panel B. The dependent variable in this table is D. Property price (ln) and property control variables include sets of dummy variables indicating property types, forms of tenure and whether the property is new built in the previous transaction. Definitions of variables are detailed in Table A3.1 in the appendix. Standard errors are clustered at local authority district level and the corresponding t-statistics are reported in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Table 3.7: Effect of flood events and Flood Re on transaction volume

	1	2	3
Dependent variable		Trade	
Flooded	-0.005**	-0.036***	-0.036***
	(-2.17)	(-9.81)	(-9.97)
Flooded x Post Flood Re		0.061***	0.060***
		(9.69)	(9.69)
Flash-flooded	0.003	-0.001	-0.002
	(0.81)	(-0.15)	(-0.25)
Flash-flooded x Post Flood Re		0.008	0.008
		(0.87)	(0.85)
3 dig plc X Year FE	Yes	Yes	Yes
Built year FE	Yes	Yes	Yes
Property controls	No	No	Yes
Observations	14,446,899	14,446,899	14,446,899
R^2	0.014	0.014	0.014

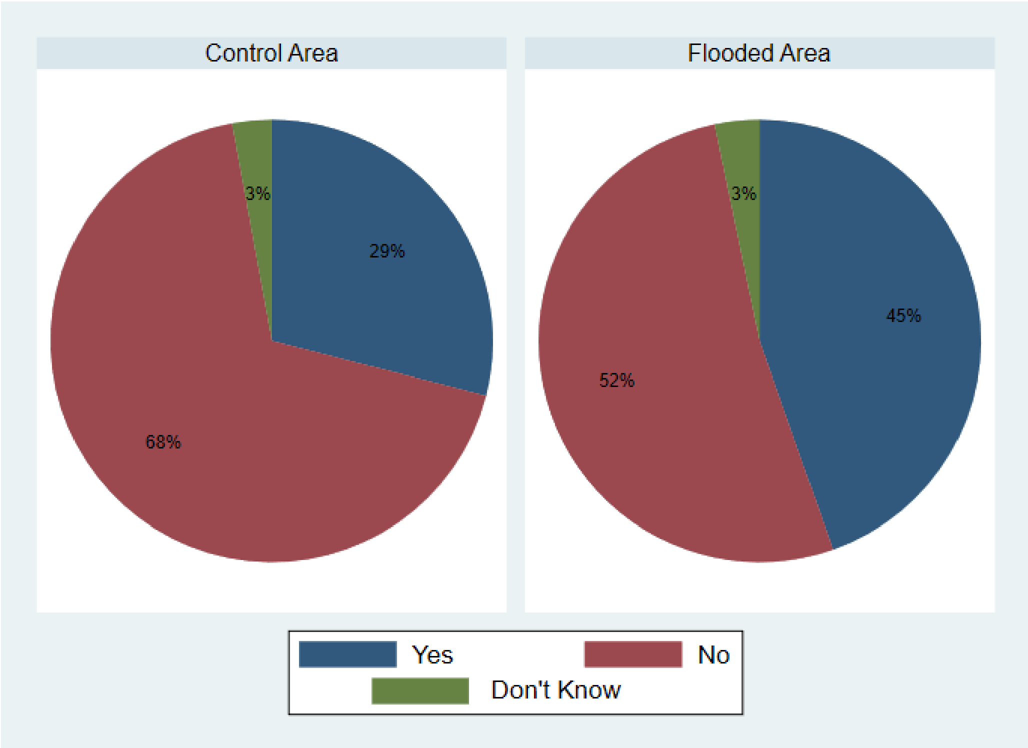
Notes: Column 1 of this table presents estimation results of equation 3.4 without the interaction terms, Flooded × Post Flood Re and Flash-flooded × Post Flood Re. Column 2 and 3 of this table presents estimation results of equation 3.4. The dependent variable in this table is a dummy variable indicates whether the property is traded in the year of observation. Property control variables include sets of dummy variables indicating property types, forms of tenure and whether the property is new built in the previous transaction. Definitions of variables are detailed in Table A3.1 in the appendix. Standard errors are clustered at local authority district level and the corresponding t-statistics are reported in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Figure 3.1: Flood Re mechanism



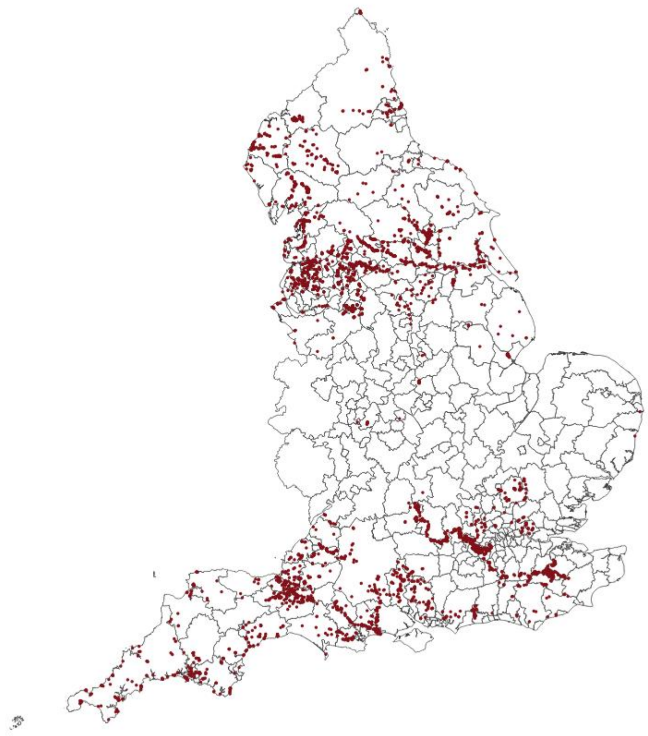
Notes: This figure was produced in Crick et al. (2018), depicting the mechanism of Flood Re and the interplay between different key players of Flood Re.

Figure 3.2: Awareness of Flood Re



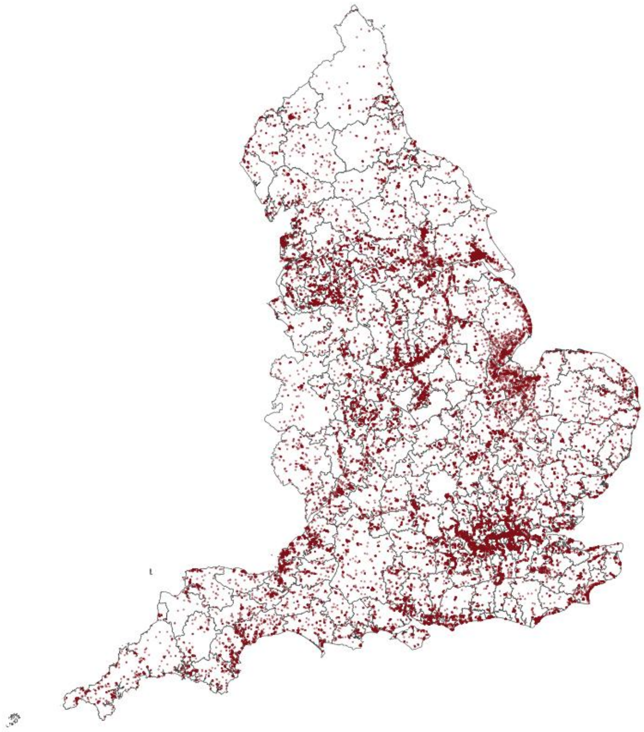
Notes: This figure shows the awareness of Flood Re in flooded area and non-flooded area. The data is based on the survey data of the 2018 Availability and Affordability of Insurance report.

Figure 3.3: Map of flooded 6-digit postcodes



Notes: This figure depicts the 6-digit postcodes of properties experiencing at least one flood event lasting for more than a day in the past four years of transactions.

Figure 3.4: Map of 6-digit postcodes with above no/very low flood risk

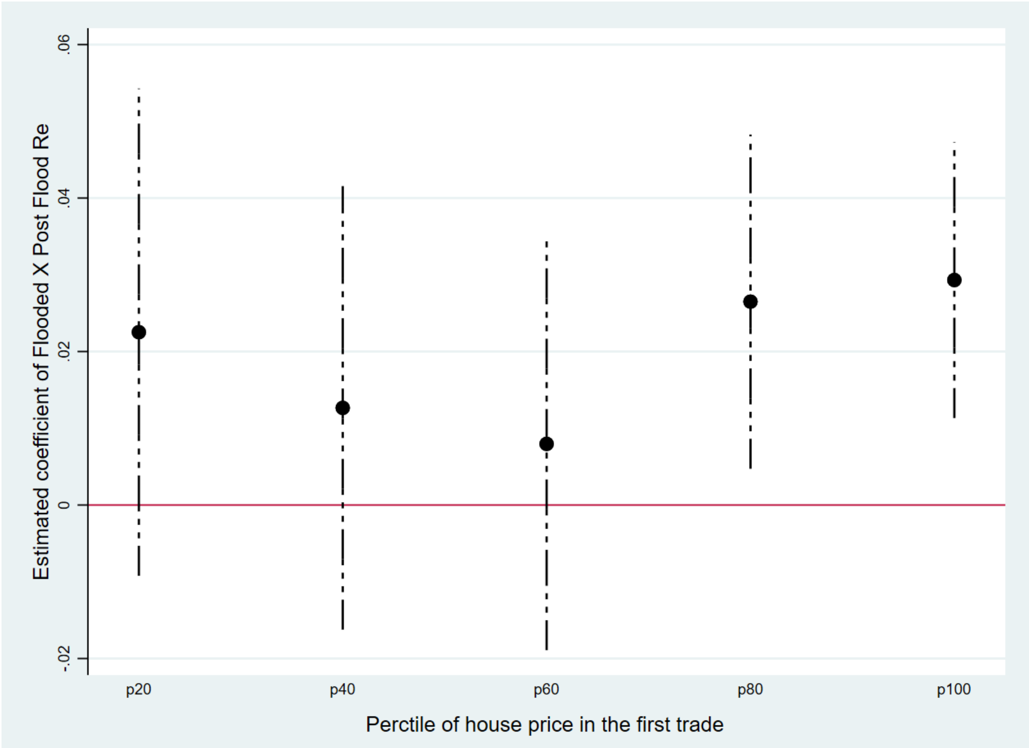


Notes: This figure depicts the 6-digit postcodes of properties at flood risk.

Figure 3.5: Map of 6-digit postcodes within 100 meters to river/sea

Notes: This figure depicts the 6-digit postcodes of properties within 100 meters to river/sea.

Figure 3.6: Effect of Flood Re at different percentiles of the property prices distribution



Notes: Each point in the figure represents the estimated coefficient of Flooded x Post Flood Re of a specific percentile of the property prices (in the first transaction) distribution and each dash line represents the 95% confidence interval of each estimated coefficient. The specification of the estimations follows the specification in column 5 of Table 3.2.

3.8 Appendix

Table A3.1: Variable definitions

Variable	Definition	Source
Property price (ln)	Natural logarithm of property price.	HM Land Registry Price Paid Data
D. Property price (ln)	First difference of the natural logarithm of property price.	HM Land Registry Price Paid Data
New built	A dummy variable=1 if the property is newly built in the previous transaction, 0 otherwise.	HM Land Registry Price Paid Data
Detached	A dummy variable=1 if the property is a detached house, 0 otherwise.	HM Land Registry Price Paid Data
Semi-detached	A dummy variable=1 if the property is a semi-detached house, 0 otherwise.	HM Land Registry Price Paid Data
Terraced	A dummy variable=1 if the property is a terraced house, 0 otherwise.	HM Land Registry Price Paid Data
Flat	A dummy variable=1 if the property is a flat, 0 otherwise.	HM Land Registry Price Paid Data
Other	A dummy variable=1 if the property is other property type, 0 otherwise.	HM Land Registry Price Paid Data
Freehold	A dummy variable=1 if the legal ownership of property is freehold, 0 otherwise.	HM Land Registry Price Paid Data
Leasehold	A dummy variable=1 if the legal ownership of property is leasehold, 0 otherwise.	HM Land Registry Price Paid Data
Trade	A dummy variable=1 if the property is transacted in the year of observation, 0 otherwise.	HM Land Registry Price Paid Data
Flooded	A dummy variable=1 if the property only experiences flood event last for more than a day four years before the transaction, 0 otherwise.	EA Recorded Flood Outlines
Flash-flooded	A dummy variable=1 if the property only experiences flood event last for a day four years before the transaction, 0 otherwise.	EA Recorded Flood Outlines
Risk (L+M+H)	A dummy variable=1 if the property is in a 6-digit postcode classified as at least low risk, 0 otherwise.	EA Flood Map
Distance to water ($<$100 m)	A dummy variable=1 if the property is within 100 meters of river or sea, 0 otherwise.	Authors' calculation
Annual household income	Average annual household income of local authority district in 2019.	Office of National Statistics
Index of multiple deprivation	Index of multiple deprivation of local authority district in 2019.	Office of National Statistics
Age	Average age of the households per local authority district in 2019.	Office of National Statistics
Education	Proportion of population with level 4 or above qualification (e.g. degree with honours and postgraduate certificate) in local authority district in 2019.	Office of National Statistics
Urban	A dummy variable=1 if the local authority district is urban area, 0 otherwise.	Office of National Statistics
Percentage of votes for the Green Party	Percentage of votes for the Green Party in the 2019 United Kingdom general election of local authority district.	House of Common
Percentage of votes for Brexit	Percentage of votes for Brexit per local authority district.	Data.gov.uk

Table A3.2: Awareness of Flood Re

	1	2
Dependent variable	Awareness of Flood Re	
Flooded	0.154**	-0.340*
	3.23	-2.14
Flooded x Age:		
35-54		0.361*
		(2.11)
>55		0.455**
		(2.89)
Flooded x Income level:		
26,000-41,599		-0.053
		(-0.39)
>41,600		0.163
		(1.25)
Flooded x Tax band:		
C-D		0.023
		(0.17)
E-H		0.019
		(0.11)
Age:		
35-54		-0.303
		(-1.79)
>55		-0.275
		(-1.49)
Income level:		
26,000-41,599		0.110
		(0.90)
>41,600		-0.021
		(-0.16)
Tax band:		
C-D		-0.048
		(-0.46)
E-H		-0.091
		(-0.85)
Observations	772	455
R^2	0.020	0.041

Notes: This table shows the heterogeneity in the awareness of Flood Re among the respondents in the survey of the 2018 Availability and Affordability of Insurance report. The dependent variable in this table is a dummy variable indicating whether the respondent is aware of Flood Re. Standard errors are clustered at region level and the corresponding t-statistics are reported in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Table A3.3: Correlation of local authority variables

Variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)
(1) Annual household income	1						
(2) Index of multiple deprivation	-0.699	1					
(3) Age	-0.02	-0.446	1				
(4) Education level	0.757	-0.514	-0.148	1			
(5) Urban	0.001	0.304	-0.582	-0.007	1		
(6) Percentage of votes for the Green Party	0.055	-0.077	-0.022	0.189	-0.063	1	
(7) Percentage of votes for Brexit	-0.534	0.213	0.394	-0.889	-0.129	-0.251	1

Notes: This table shows the correlation matrix of local authority variables.

Table A3.4: Effect of Flood Re on property prices-Continuous measurements of flood risk

Panel A	1	2	3	4
Dependent variable	D. Property price (ln)			
Flood duration (in 100 days)	-0.026***	-0.023***	-0.027***	-0.024**
	(-2.84)	(-2.82)	(-2.61)	(-2.59)
Flood duration x Post Flood Re	0.018	0.020*	0.021*	0.023**
	(1.62)	(1.90)	(1.76)	(2.04)
3 dig plc X Year FE (current)	Yes	Yes	No	No
3 dig plc X Year FE (previous)	Yes	Yes	No	No
3 dig plc X Year FE (current) X Year FE (previous)	No	No	Yes	Yes
Built year FE	Yes	Yes	Yes	Yes
Property controls	No	Yes	No	Yes
Observations	1,754,067	1,754,067	1,754,067	1,754,067
R^2	0.761	0.766	0.788	0.792
Panel B	1	2	3	4
Dependent variable	D. Property price (ln)			
Flood risk mid-point	-0.002***	-0.001**	-0.002***	-0.001**
	(-3.95)	(-2.50)	(-3.78)	(-2.45)
Flood risk mid-point x Post Flood Re	0.002***	0.001***	0.002***	0.002***
	(2.98)	(2.82)	(2.78)	(2.61)
3 dig plc X Year FE (current)	Yes	Yes	No	No
3 dig plc X Year FE (previous)	Yes	Yes	No	No
3 dig plc X Year FE (current) X Year FE (previous)	No	No	Yes	Yes
Built year FE	Yes	Yes	Yes	Yes
Property controls	No	Yes	No	Yes
Observations	1,754,067	1,754,067	1,754,067	1,754,067
R^2	0.761	0.766	0.788	0.792
Panel C	1	2	3	4
Dependent variable	D. Property price (ln)			
Distance to water (in 1000 meters)	-0.003***	-0.005***	-0.002***	-0.005***
	(-4.31)	(-7.13)	(-3.49)	(-6.28)
Distance to water x Post Flood Re	0.009***	0.008***	0.008***	0.008***
	(13.24)	(12.42)	(12.24)	(11.49)
3 dig plc X Year FE (current)	Yes	Yes	No	No
3 dig plc X Year FE (previous)	Yes	Yes	No	No
3 dig plc X Year FE (current) X Year FE (previous)	No	No	Yes	Yes
Built year FE	Yes	Yes	Yes	Yes
Property controls	No	Yes	No	Yes
Observations	1,754,067	1,754,067	1,754,067	1,754,067
R^2	0.761	0.766	0.788	0.792

Notes: Column 1 and 2 of this table presents estimation results of equation 3.2. Column 3 and 4 of this table presents estimation results of equation 3.3. The continuous measurement of flood risk are Flood duration (in 100 days) in Panel A, Flood risk mid-point in Panel B and Distance to water (in 1,000 meters) in Panel C. The dependent variable in this table is D. Property price (ln) and property control variables include sets of dummy variables indicating property types, forms of tenure and whether the property is new built in the previous transaction. Definitions of variables are detailed in Table A3.1 in the appendix. Standard errors are clustered at local authority district level and the corresponding t-statistics are reported in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Table A3.5: Effect of flood events and Flood Re on days since last trade

	1	2	3
Dependent variable	Days since last trade (ln)		
Flooded	0.006***	0.076***	0.076***
	(3.47)	(19.96)	(19.92)
Flooded x Post Flood Re		-0.131***	-0.131***
		(-13.46)	(-13.43)
Flash-flooded	0.006**	0.070***	0.070***
	(2.09)	(13.18)	(13.27)
Flash-flooded x Post Flood Re		-0.115***	-0.115***
		(-9.24)	(-9.26)
3 dig plc X Year FE (current)	Yes	Yes	No
3 dig plc X Year FE (previous)	Yes	Yes	No
3 dig plc X Year FE (current) X Year FE (previous)	No	No	Yes
Built year FE	Yes	Yes	Yes
Property controls	Yes	Yes	Yes
Observations	1,754,067	1,754,067	1,754,067
R^2	0.939	0.939	0.939

Notes: Column 1 and 2 of this table present estimation results of equation 3.2 with the dependent variable measuring the natural logarithm of the number of days since the last transaction, column 1 present estimation results without the interaction term, Flooded × Post Flood Re and Flash-flooded × Post Flood Re. Column 3 of this table presents estimation results of equation 3.3 with the dependent variable measuring the natural logarithm of the number of days since the last transaction. Measurements of flood risk in this table is Flooded and Flash-flooded. The dependent variable in this table is D. Property price (ln) and property control variables include sets of dummy variables indicating property types, forms of tenure and whether the property is new built in the previous transaction. Definitions of variables are detailed in Table A3.1 in the appendix. Standard errors are clustered at local authority district level and the corresponding t-statistics are reported in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively

CHAPTER 4

CONCLUDING REMARKS

4.1 Summary of key findings

I conclude the thesis by summarizing the key findings in the three previous chapters. Chapter 1 exploits the existence of a private deposit insurance scheme that protects deposits above the FDIC insurance coverage limit in Massachusetts, the chapter shows that banks whose deposits are federally and privately insured obtain more deposits, expand lending, and remain prudent in the mortgage origination process during the subprime crisis, in contrast to banks whose deposits are only federally insured.

Chapter 2 employs mass shootings as exogenous shocks to examine the impact of having lending relationships with gun manufacturers on bank deposits and bank value. This chapter finds that branches operated by such banks, particularly for branches located near the mass shootings, experience lower deposit growth in the years of mass shootings. However, this chapter finds no evidence that the mass shootings affect such banks' market value.

Chapter 3 exploits the introduction of a public reinsurance scheme, named Flood Re, in England which reduces current and future insurance premiums of properties at the risk of flooding. This chapter document that this policy increases the value and transaction volume of flood-prone properties. The effect on property value is especially strong in urban areas and areas with wealthier households.

4.2 Policy contributions

The findings of this thesis are timely and important for policy makers. The findings in Chapter 1 suggest that depositors can exploit differences in deposit insurance coverage, implying that countries with lower deposit insurance coverage may experience deposit outflows during crises. Therefore, harmonizing deposit insurance schemes under a European Deposit Insurance Scheme has potential to mitigate potentially destabilizing deposit outflows. The findings also suggest that banks that have better access to deposits are less vulnerable to short-term funding shocks which mitigates adverse effects on their lending activities, highlighting the synergies between deposits and lending.

In Chapter 2, the results highlight the importance of corporate social responsibility (CSR) and environmental, social and governance (ESG) in the banking industry. In particular, the results underline the potential negative effect of funding perceptually unsustainable and unethical industries on bank deposit funding. Therefore, the rule proposed by the Office of the Comptroller of the Currency (OCC), prohibiting large banks from denying lending to controversial industries, may lead to potential deposit loss of the related banks.

Chapter 3 highlights the potential transition risk of public policy interventions addressing climate risk. The reinsurance scheme, Flood Re, is planned to phase out in 2039 and flood risk component is expected to be fully priced into the premiums by that time, value of flood-prone properties may therefore experience an adjustment of property value. The results also highlight the unexpected distributional impacts of Flood Re, in terms of its weak impact on property price in more deprived areas.

4.3 Limitations and future works

This thesis is definitely not without limitations. In Chapter 1 and 2, we employ the Summary of Deposits (SoD) to examine branch deposits. While the detailed geographical information of branches in the SoD is beneficial for the identification strategies, the SoD only documents total deposits of each branch annually. Therefore, the chapters are constrained to differentiate the effects on different types of deposits. For example, is the effect of the MA-DIF could only be found on deposits over the FDIC coverage limit in Chapter 1? In Chapter 2, is the reduction of affected banks' deposits after mass shootings originated from individual or corporate depositors? Another constraint comes from the reporting interval of the SoD, the annual data poses challenges in ruling out confounding events in the same years of the shocks. Chapter 1 mitigates the limitations of the SoD by employing Call Reports, which provides more detailed quarterly bank-level data. However, the same strategy cannot be applied in Chapter 2, because the identification strategy in the chapter builds on examining whether the effect of the shootings on the related banks diminishes with the increasing distance between branches and shooting incidents.

Another potential limitation of Chapter 2 is the identification of banks having lending relationship with gun manufacturers. While the chapter employs a well-publicized public letter written by a Chicago Mayor to identify such banks, we cannot rule out that there were less well-publicized sources accusing other banks of funding gun manufacturers. The challenge lies in the lack of a credible and publicly accessible data source in evaluating the relationship between banks and gun manufacturers. Not until recently, a campaign called "Is Your Bank Loaded" reveals its ranking of the 15 largest banks (by consolidated assets) in the U.S. in terms of their relationship with the gun industry. The ranking is extensively quoted by activist groups, such as Gun Down America, to encourage depositors to boycott banks supporting the gun industry. Therefore, future works extending from Chapter 2 include employing this source of information in identifying "loaded" banks and examine the effect of more recent high-profile mass shootings on bank deposits.

Chapter 3 is constrained in explaining the heterogeneous effects of Flood Re in different areas. While we find that the effect of the scheme is stronger in urban areas and wealthier areas, we cannot fully disentangle different channels in driving the results. Therefore, my coauthors and I are actively seeking more granular data, for example the proprietary mortgage data at the Bank of England which would help identifying the characteristics (e.g. income level) of home buyers and sellers, in disentangling the different channels in the near future.

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