THREE ESSAYS ON HOUSEHOLD FINANCE, CLIMATE CHANGE, AND CORPORATE FINANCE

by

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A dissertation submitted to the faculty of
The University of North Carolina at Charlotte
in partial fulfillment of the requirements
for the degree of Doctor of Philosophy in
Business Administration

Charlotte

2022

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ABSTRACT

Chen Shen. THREE ESSAYS ON HOUSEHOLD FINANCE, CLIMATE CHANGE, AND CORPORATE FINANCE (Under the direction of DR. Yongqiang Chu and DR. Dolly King)

My dissertation studies the social impact of finance including payday loans, climate change, and corporate hedging policies. The first paper studies payday loans. Police departments located in states allowing payday lending report 14.34% more property crimes than the police departments located in states not allowing payday lending. I also find that the police departments located in counties bordering states allowing payday lending report more property crimes. Those results are driven by the financial pressure induced by payday loans. Furthermore, the impact of payday lending concentrates in areas with a higher proportion of the minority population.

In the second paper, using a large sample over the period 1986 to 2017, we show that companies with higher exposure to climate change risk induced by sea-level rise (SLR) tend to acquire firms that are unlikely to be directly affected by SLR. We find that acquirers with higher SLR exposure experience significantly higher announcement-period abnormal stock returns. Post-merger, analyst forecasts become more accurate and environmental-related as well as overall ESG scores improve.

In the third essay, we examine the effect of the shareholder-creditor conflict on the corporate hedging policy. Using the mergers between the firm's shareholders and creditors as an exogenous shock, we find a causal positive relation between reduced shareholder-creditor conflict and corporate hedging behavior. Specifically, we find that the treated firms who experience mergers between their shareholder and creditors are not only more likely to use financial instruments to hedge but also hedge more in terms of the notional value of the hedging contracts. Consistent with the argument that the shareholder-creditor conflict often becomes

exaggerated when the firm is in financial distress, we find that the effect is stronger for financially distressed firms.

ACKNOWLEDGEMENTS

I would like to begin by expressing my sincere gratitude to my dissertation chair, Professor Yongqiang Chu, and Professor Tao-Hsien Dolly King. It has been an exceptionally long journey and you have been there all the way providing countless hours of support and motivation. You have challenged me to work harder and made me a better researcher in the process. I am going to miss our weekly meetings, but I look forward to the future working with you on our current and, yet, undiscovered projects.

I would also like to personally thank Professor David Mauer for all his support throughout the Ph.D. program. One of my main regrets is that I did not get the opportunity to work more closely with you on research. Some of my most enjoyable memories of the Ph.D. program are when we batted around ideas during your advanced corporate class. I sincerely hope that the opportunity arises to collaborate with you in the future. Again, thank you for all your help!

Lastly, I want to thank Professor Victor (Zitian) Chen for agreeing to serve on my committee. You were one of my very first instructors in the Ph.D. program, and I have enjoyed all the times when we have gotten to talk over the last five years. I know that you have a busy schedule, but you didn’t hesitate when I asked if you would mind serving on my committee. I just want you to know that I appreciate that. Also, I regret that I never got to take your course in Management. Thanks again!

DEDICATION

I would like to dedicate this work to my mom who is in heaven. I did it! Mom.

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INTRODUCTION

This dissertation consists of three essays on corporate finance, household finance, and climate change. The essays study how payday lending affects property crimes, how sea level rising (SLR) risk affects corporate mergers and acquisitions (M&A), and how reduced shareholder-creditor conflict affects the corporate's hedging behavior.

In the first essay (Job market paper), I study the effect of state-level changes in payday lending regulation on property crimes. I find that the police departments located in states allowing payday lending report 14.34% more property crimes than police departments located in states not allowing payday lending. I also find that the police departments located in counties bordering states allowing payday lending report more property crimes. Those results are driven by the financial pressure induced by payday loans. Furthermore, the impact of payday lending concentrates in areas with a higher proportion of the minority population.

Identifying the causal impact of payday lending is challenging because it is often difficult to isolate the exogenous variation in payday lending access (Gathergood et al., 2019). For example, payday lenders often locate their stores in low-income areas (Bhutta, 2014). To mitigate this concern, I follow Morgan et al. (2012) and study how the number of property crimes changes as the state switches from allowing to prohibiting payday lending, or vice versa. I find that police department located in states allowing payday lending report 14.34% more property crimes than police departments located in states not allowing payday lending.

To ensure that the difference-in-differences (DID) estimation is not driven by the pre-existing trend differences between treated and control states, I perform the dynamic analysis for the effect of payday lending laws on the number of property crimes. I find that the effects of payday lending on property crimes become larger and statistically significant after the state starts to allow payday lending. These results suggest that the DID regression estimation is unlikely to be driven by the pre-existing differences between the treated and control states.

To identify the channel through which payday lending increases property crimes, I conduct a placebo test by replacing the dependent variable with the violent crimes. The rationale behind this test is that if payday lending affects crimes through the non-financial channel(s), then that channel(s) is likely to increase violent crimes as well. Nevertheless, I find that payday lending does not affect violent crimes. This result confirms that payday lending increases property crimes by imposing more financial pressure on its borrowers.

The DID regression delivers unbiased estimates of the effect of payday lending if payday lending law changes are uncorrelated with changes in unobserved determinants of crimes. The natural question for the baseline regression is whether the state legislators target payday lending and crimes at the same time. To mitigate this concern, I use the same measure employed by Melzer (2021) and the state×year fixed effects to reduce the concern that political forces jointly affect payday laws and crimes. I find that The agencies located within 30 miles of a state allowing payday lending report 18.41% more property crimes than agencies located further away.

In the set of cross-sectional tests, I find that the impact of payday lending on property crimes is stronger in areas subject to low economic conditions such as low household income, low income per capita, high unemployment rate, and high property rate. Last, to explore who are the real victims of the property crimes induced by payday lending, I split my sample based on the proportion of minority populations. I find that the effect of payday lending on property crimes is stronger in the areas subject to a higher proportion of the African American population. This result suggests that African American communities suffer more from the negative social impact (Induce more property crimes) of payday lending on property crimes.

My paper is the first one to provide the effect of payday lending on property crimes on a nationwide level with clear identification strategies. Unlike previous literature, my paper suggests that payday lending increases all types of property crimes. This result may act as an alarm to the people who are considering using payday loans to solve their financial difficulties. Also, I explore the channel through which payday lending affects property crimes – the financial pressure induced by payday loans.

In the second essay, we study the effect of sea-level rise (SLR) risk on corporate mergers and acquisitions (M&A). We are interested in the SLR for two reasons: First, sea-level rise and its acceleration are among the most severe impacts of climate change and global warming. The scientific community has reached a consensus that SLR is a serious environmental risk that will disrupt household and business activities in the long run. It is estimated that a 1.8 meter (i.e., roughly 6 feet) SLR would make areas currently populated by 6 million Americans uninhabitable (Hauer et al., 2016). Businesses with commercial properties or operations in low-lying coastal areas may find it increasingly difficult to ensure their assets, making SLR a relevant long-term business risk. Second, accurate forecasting of future sea-level rise is challenging, which makes studying how firms manage such a significant yet uncertain risk particularly urgent and time relevant.

Using a large sample over the period 1986 to 2017, we show that companies with higher exposure to climate change risk induced by sea-level rise (SLR) tend to acquire firms that are unlikely to be directly affected by SLR. We find that acquirers with higher SLR exposure experience significantly higher announcement-period abnormal stock returns. Post-merger, analyst forecasts become more accurate, and environmental-related as well as overall ESG scores improve.

We test our hypotheses using a multi-step approach. Our first set of analyses investigates how exposure to SLR risk affects the likelihood of mergers. Because SLR represents an uncertain yet significant long-term operational risk, we hypothesize that firms exposed to SLR are more (less) likely to become acquirers (targets) in a merger deal. Using the similar methodology employed by Bena and Li (2014), we find that if the firm is subject to the inundation risk, the firm is 4.0% more likely to acquire a firm subject to no inundation risk.

We next turn to examine how the market reacts to acquisition announcements made by the acquiring firms that are exposed to different levels of SLR risk. Consistent with the notion that the market rewards firms for diversifying away from their SLR risk, we find that acquirers exposed to SLR risk experience significantly higher announcement-period abnormal stock returns. The results hold after controlling for a large number of the firm as well as merger deal characteristics. Importantly, we find that the positive announcement effect concentrates on deals in which the target firms are not exposed to SLR risk, suggesting it is driven by the diversification of SLR risk. Furthermore, we also find that the positive announcement return is more pronounced for firms with more analyst coverage.

The identification challenge of the SLR effect on M&A is that the association between premerger SLR and post-merger outcomes could be due to the endogenous selection of firms those subject to the SLR, rather than to the impact of SLR on the post-merger outcome. To address such concerns, we exploit a quasi-experiment. Specifically, following Bena and Li (2014), we employ a control sample of withdrawn bids that failed for reasons exogenous to the SLR of either merger partner. In this case, the assignment of firm pairs to the treatment sample (completed deals) versus the control deals sample can be treated as random with the respect to the outcomes that we examine. We find that the combined firms after the merger experience more financial analysts' coverage and smaller analysts' forecast variations. Our findings, show improvement in the merger outcomes postmerger for deals with premerger acquirers subject to SLR compared to the average outcome.

Our study makes several contributions to the literature. First, this paper expands our understanding of how environmental and climate change risks influence various market participants and underlying assets. Second, our study contributes to a large literature on mergers and acquisitions. Empirical studies on mergers and acquisitions largely focus on either the determinants of mergers or the sources of synergistic gains.

In the third essay, we examine the effect of the shareholder-creditor conflict on the corporate hedging policy. Using the mergers between the firm’s shareholders and creditors as an exogenous shock, we find a causal positive relation between reduced shareholder-creditor conflict and corporate hedging behavior. Specifically, we find that the treated firms who experience mergers between their shareholder and creditors are not only more likely to use financial instruments to hedge but also hedge more in terms of the notional value of the hedging contracts. Consistent with the argument that the shareholder-creditor conflict often becomes exaggerated when the firm is in financial distress, we find that the effect is stronger for financially distressed firms.

The decision to become the dual-holder of the firm is endogenous. For instance, the unobserved factors on the firm level could affect the firm's hedging policy. To mitigate this concern, we follow the identification strategy developed by Chu (2018) and Yang (2019). We implement a DID regression based on the quasi-natural experiment of the mergers of the financial institutions that may generate plausibly exogenous variation in the presence of dual-holders. Based on this methodology, we find that the treated firms are 4.5% more likely to hedge than control firms after the merger between the shareholders and creditors.

To address the identification challenge of DID, we follow Chu (2018) to perform a dynamic analysis. We find that After the merger, the coefficient estimates on $Treat \times Year_k$ becomes greater and statistically significant. These results suggest that the baseline DID estimation is unlikely to be driven by the pre-existing differences between the treated and control firms.

Lastly, we follow Bakke et al. (2016) to construct a quantitative measure of the firm's hedging policy-*Notional value of hedging contracts*. To quantify hedging behavior, we hand-collect financial derivatives positions and operational hedging contracts from 10-K, 10-Q, and Proxy statement filings on the SEC EDGAR System. Firms usually disclose derivative positions in item 7A of 10-K (sometimes disclose this information in other items). We find that the firm that experiences a merger between shareholders and creditors has a notional value 105% higher than the contract used by the firm that does not experience a merger between shareholders and creditors.

This paper contributes to the literature by providing another factor that affects corporate hedging behavior-dual ownership. Our paper also contributes to the literature on the effect of dual-holders by using mergers between institutional shareholders and lenders to the same firms as exogenous shocks to identify firms with institutional dual-holders.

CHAPTER 1: THE IMPACT OF PAYDAY LENDING ON CRIMES

1. Introduction

Does borrowing at high-interest rates do more harm than good to the borrowers? The classical economic theory predicts that borrowing would make the borrowers at least weakly better off as consumers reveal their preferences by borrowing. The behavioral model suggests that borrowing does not necessarily improve the borrowers' financial welfare if the borrowers are irrational (Carrell and Zinman, 2014). Policymakers and borrower rights advocate groups often argue that restricting access to expensive credit protects the borrowers' interests (Zinman, 2010). Payday lending is one of the controversial and expensive credits that receive mixed responses from borrowers and policymakers (e.g., Melzer, 2011, Skiba and Tobacman, 2011, Morgan and Strain, 2008, Morse, 2011.). The payday lending literature has focused primarily on the borrowers' financial welfare but overlooked the social impacts of payday lending. In this paper, I study how payday lending affects crimes. I find that payday lending increases property crimes.

Access to payday lending could affect crime through several different channels. The social disorganization theory (Kubrin et al., 2011) suggests that payday loan stores decrease guardianship against crime by introducing strangers to the neighborhoods. Also, the presence of payday loan stores shows a sign of physical disorder and economic distress in the neighborhood (Kubrin et al., 2011; Lee et al., 2013).

The routine activity theory (Kubrin and Hipp, 2016) suggests that payday loan stores install a large volume of cash in the neighborhood that attracts burglary and robbery and the cash income from such offenses often facilitates drug consumption. The literature documents the positive relationship between cash and crimes. Wright et al. (2017) find that the electronic benefit transfer

(EBT) program[1] had a negative and significant effect on the overall crime rate and specifically for burglary, assault, and larceny crimes.

Finally, because of the high annual percentage rate (APR) and the single-payment structure, payday loan borrowers often find it necessary to renew their contracts when their loans mature because of the difficulty to repay the entire balance. Each time a loan is renewed, the borrower incurs relatively high fees, the burden of which over time exacerbates the borrowers' financial difficulties. The financial strain theory (Kubrin et al., 2011) suggests that financially distressed payday loan borrowers may become crime offenders. For instance, personal indebtedness increases crime (McIntyre and Lacombe, 2012), and neighborhoods subject to higher interest rates have more property crimes (Garmaise and Moskowitz, 2006).

Identifying the causal impact of payday lending is challenging because it is often difficult to isolate the exogenous variation in payday lending access (Gathergood et al., 2019). For example, payday lenders often locate their stores in low-income areas (Bhutta, 2014). To mitigate this concern, I follow the literature and exploit the plausibly exogenous variation generated by the state laws prohibiting or allowing payday lending (Melzer, 2011; Carrell and Zinman 2014).

I collect the property crime data from the Uniform Crime Reporting (UCR) Program. The UCR Program collects statistics on the number of offenses[2] known to law enforcement. I choose the sample period from 1985 to 2014 because that is the complete dataset offered by the UCR program. Using the difference-in-differences specification, I find that the agencies[3] located in states allowing payday lending report 14.34% more property crimes than agencies located in states not allowing payday lending, which translates into approximately 270 property crimes per

1 Electronic benefit transfer (EBT) is an electronic system that allows state welfare departments to issue benefits via a magnetically encoded payment card used in the United States. It reached nationwide operations in 2004. The average monthly EBT payout is $125 per participant.

[2] These offenses including murder and non-negligent homicide, rape, robbery, aggravated assault, burglary, motor vehicle theft, larceny-theft, and arson.

[3] The UCR program refers police department as agency.

agency per year. Breaking down the type of property crimes, agencies located in states allowing payday lending report 13.88%, 14.91%, and 14.22% more burglary, larceny-theft, and motor theft crimes than agencies located in states not allowing payday lending, those numbers represent approximately 68 burglary crimes, 132 larceny-theft crimes, and 37 motor theft crimes. Also, the results are consistent when replacing the state and year fixed effects with the agency and year fixed effects.

To identify the channel through which payday lending increases property crimes, I conduct a placebo test by replacing the dependent variable with the violent crimes. The rationale behind this test is that if payday lending affects crimes through the non-financial channel(s), then that channel(s) is likely to increase violent crimes as well. Nevertheless, I find that payday lending does not affect violent crimes. This result confirms that payday lending increases property crimes by imposing more financial pressure on its borrowers.

One concern is that the results of the difference in differences analysis could be driven by the trend differences between states allowing and not allowing payday lending. However, such an effect is likely to show up even before the passing of laws allowing payday lending. I, therefore, conduct a dynamic analysis of the effect of payday lending on property crimes. I find that the coefficient estimates are statistically insignificant before the treated states allow payday lending, suggesting that the effect of payday lending on crimes is not driven by the pre-existing differences between states allowing and not allowing payday lending.

A more subtle concern is that the unobservable state characteristics could drive the payday lending laws and local crime simultaneously. For example, state-level budget problems could motivate the states to adopt laws allowing payday lending, and at the same time, the worsening budget problems could also impact crime rates. To ensure that the effects of payday lending on property crimes are not driven by the state-level factors that are correlated with the laws, I follow Melzer (2011) to construct an alternative measure for payday lending, an indicator equal to one not only for the states allowing payday lending but also for the agencies located in counties bordering with a state allowing payday lending. After controlling for the state×year fixed effects to account for the contemporaneous local shocks at the state level, I find that the agencies located near a state allowing payday lending report 18.41% more property crimes than the agencies located further away, suggesting that the effect of payday lending on crimes is not driven by the state-level unobservable factors.

To further identify whether payday lending affects property crimes through the financial pressure channel, I split my sample based on the local economic conditions. I find that the impact of payday lending on property crimes is stronger in areas subject to low economic conditions such as low household income, low income per capita, high unemployment rate, and high property rate. I find that the effects of payday lending on crimes are stronger in 3 out of 4 sub-samples associated with lower economic conditions. Barth et al. (2015) suggest that borrowers who have limited access to banks are likely to use more payday loans. If their argument is valid, I predict that people will use more payday loans in the areas subject to fewer banks. It is reasonable to assume that the effect of payday lending on crimes is stronger in such areas. To test this assumption, I split my sample based on the number of commercial bank branches. I find that the effects of payday lending on crimes are similar in both sub-samples, suggesting that the effect of payday lending on crimes does not change with accessibility to banks. Last, to explore who are the real victims of the property crimes induced by payday lending, I split my sample based on the proportion of minority populations. I find that the effect of payday lending on property crimes is stronger in the areas subject to a higher proportion of the African American population. This result suggests that African American communities suffer more from the negative social impact (Induce more property crimes) of payday lending on property crimes.

Payday lending could not only affects the borrowers' financial welfare[4]but also affects other aspects of borrowers' life. For example, payday lending could cause psychological and health problems, such as chronic stress, which could motivate the borrowers to engage in criminal activities (Pew Charitable Trusts, 2016). Using the payday loan stores data in 2013, Barth et al. (2020) find that the presence of payday lenders may help reduce property crimes as well as personal bankruptcies. Nevertheless, their results may suffer from the endogeneity issue because they do not isolate the exogenous variation in payday lending access. Also, their results may be biased because they only use one year of data. The reason is that they may overlook some unobserved factors that only exist in 2013 that increase property crimes and payday loan stores simultaneously. Cuffe (2013) finds that the access to payday lending in some counties of payday lending prohibiting states (Massachusetts, New Jersey, and New York) induces more larceny, fraud, and forgery crimes. Nevertheless, his results are restricted to the three states in the Northeastern region of the United States. Also, he does not find any impact of payday lending on burglary and other types of property crimes. Hynes (2012) investigates the relationship between payday loans' legality and bankruptcy from 1998 to 2009. He reports that payday lending decreases property crimes; However, his results may suffer from the endogeneity issue because he fails to control for the unobservable state characteristics that could drive the payday lending laws and local crimes simultaneously. Also, he does not include the crime data before 1998

4 The literature has explored the relationship between payday lending and household financial welfare. Skiba and Tobacman (2011) find that successful first-time payday borrowing often results in additional loans and interest payments in the future. Campbell, Martinez-Jerez, and Tufano (2012) find that payday lending increases involuntary bank account closures. Melzer (2011) finds that payday lending leads to increased difficulty in paying the mortgage, rent, and utility bills. Fitzpatrick and Coleman-Jensen (2014) find that payday loans help protect some households from food insecurity. Karlan and Zinman (2010) find that restricting access to payday lending cause deterioration in the overall financial conditions of households.

which is publicly available. Xu (2016) studies the effect of payday lending on neighborhood crime rates in Chicago, Illinois. She finds that the property crime rate declined by 1.77% in the first year after the adoption of the new law and 1.49% in the second year. Just as Cuffe (2013) does, her study only focuses on a specific region, which does not provide the overall effect of payday lending on the national level.

Because of the data limitation (Geographic and/or time horizon) and problematic identifications, the literature fails to provide a robust estimation of payday lending on property crimes on the national level. My paper is the first one to provide the effect of payday lending on property crimes on a nationwide level with clear identification strategies. Unlike previous literature, my paper suggests that payday lending increases all types of property crimes. This result may act as an alarm to the people who are considering using payday loans to solve their financial difficulties. Also, I explore the channel through which payday lending affects property crimes – the financial pressure induced by payday loans.

My paper also contributes to the literature on property crime by providing another cause for property crimes - payday lending. Previous studies focus on the impact of households' financial welfare on property crimes. Harries (2006) finds that both property and violent crimes were moderately correlated with population density, and these crimes largely affected the same blocks. Using the data from the 2000 British Crime Survey and the 1991 UK census small area statistics, Tseloni (2005) finds that both household and area characteristics, as well as selected interactions, explain a significant portion of the variation in property crimes. Howsen and Jarrell (1987) find that the level of poverty, the degree of tourism, the presence of police, the unemployment rate, and the apprehension rate affect property crimes. Kelly (2006) finds that violent crimes and property crimes are positively influenced by the percentage of female-headed families and by population turnover, and negatively related to the percentage of the population aged 16-24. Sampson (1985) and Patterson (1991) argue that absolute and relative poverty link to property crime only through their association with family and community instability. Drug enforcement also affects property crimes. Benson and Rasmussen (1992) find that the resource reallocations accompanying strong drug law enforcement lead to more property crimes. Besides the households' financial welfare and drug enforcement, law enforcement also plays a role in property crimes. Sjoquist (1973) finds that an increase in the probability of arrest and conviction and an increase in the cost of crime (punishment) both result in a decrease in the number of property crimes. Last, other factors such as temperature also affect property crimes. Cohn and Rotton (2000) find that more crimes were reported during summer than in other months.

The remainder of the paper proceeds as follows. Section 2 describes the data and construction. Section 3 details the empirical strategy and the identifying assumptions. Section 4 provides the main results on the effects of payday lending on property crimes and addresses the identification challenges. Section 5 extends the analysis by comparing the number of crimes reported by police departments located near a state that allows payday lending with the number of crimes reported by police departments located further away. Section 6 presents some cross-sectional tests on the relationship between payday lending and property crimes. Section 7 concludes.

2. Sample construction and variable definitions

I collect the crime data from the Uniform Crime Reporting (UCR) program. The UCR Program collects data on the number of offenses known to law enforcement. The crime data is obtained from the data received from more than 18,000 cities, universities and colleges, counties, states, tribals, and federal law enforcement agencies voluntarily participating in the program.

These offenses include murder and non-negligent homicide, rape, robbery, aggravated assault, burglary, motor vehicle theft, larceny-theft, and arson. They are serious crimes that occur with regularity in all areas of the country. My sample period starts from 1985 to 2014. I choose this period because this is the complete property crime dataset on the agency level collected by the UCR program.

2.1. The dependent variable

The UCR program reports eight crimes including murder and non-negligent homicide, rape (legacy & revised)[5], robbery, aggravated assault, burglary, motor vehicle theft, larceny-theft, and arson. In this paper, I mainly focus on the number of property crimes, that is, the sum of burglary, larceny-theft, and motor theft crimes, reported by the agencies. My sample is a panel dataset with agency-year level observations. The dependent variables are the natural logarithm of the number of property crimes (*Ln (No. of property crimes)*), the natural logarithm of the number of burglary crimes (*Ln (No. of burglary crimes)*), the natural logarithm of the number of larceny-theft crimes (*Ln (No. of larceny-theft crimes)*), and the natural logarithm of the number of motor theft crimes (*Ln (No. of motor theft crimes)*).

2.2. State Laws of Payday Lending

Some states have laws that effectively prohibit payday lending by imposing binding interest rate caps on payday loans or consumer loans. Some other states explicitly outlaw the practice of payday lending. For example, Georgia prohibits payday loans under racketeering laws in 2005. New York and New Jersey prohibit payday lending through criminal usury statutes. Arkansas's state constitution capped loan rates at 17 percent annual interest in 2005. Maine caps interest at 30 percent but permits tiered fees that result in up to 261 percent annual rates for a two-week

[5] rape statistics prior to 2013 have been reported according to the historical definitions, identified on the tool as "Legacy Rape". Starting in 2013, rape data may be reported under either the historical definition, known as "legacy rape" or the updated definition, referred to as "revised."

$250 loan. Oregon permits a one-month minimum term payday loan at 36 percent interest less a $10 per $100 borrowed initial loan fees in 1998. Just as many other laws in the United States, the payday lending law also varies in states. These laws are generally well-enforced, if not always perfectly enforced (King and Parrish 2010), and hence provide a good source of variation in the availability of payday loans across states and over time. I list the detailed information on state legislation for payday lending in table A.1. of the appendix. I define the main independent variable, $Allowed_{it}$, to be one if state i's law does not prohibit the standard payday loan contract in year t, and zero otherwise.

2.3. State level and county level control variables

I include several state-level and county-level control variables that correlate with property crimes from several sources. At the state level, I collect GDP per capita, household income, unemployment rate, and poverty rate from the Federal Reserve Bank of St. Louis. To control for the state-level political influences on the legislation, I add dummy variables indicating whether the majority of the statehouse/state senate is controlled by the Democratic party. I also add a dummy variable indicating whether the governor belongs to the Democratic party. I collect those data from Ballotpedia.[6] County-level control variables, such as population, personal income, income per capita, and the number of job opportunities offered, are collected from the current population survey of the United States Census Bureau. I collect the data for minority populations at the county level from the National Bureau of Economic Research (NBER). To match the county-level control variables with the agency-year level observations, I first identify which county the agency is located in, and then match the property crimes reported by the agency with the counties' federal information processing standards (FIPS) code. I use the FIPS code to match the county-level control variables with the agency-level property crime data.

[6] https://ballotpedia.org/Main_Page.

Table 1 provides descriptive statistics for the agency-year observations sample. All variables are winsorized at the 1% and 99% levels.[7] I have 100,775 agency-year observations. The average number of *Property crimes* reported by agencies is 2,180. Among all categories of property crimes, the most reported crime is larceny-theft. The average of *Larceny-theft crimes* is 1,420.

Table 2 reports the univariate comparison of the dependent variables between the agency-year observations allowing payday lending and the agency-year observations not allowing payday lending. The former reports a higher number of property crimes. For example, the difference in the average number of property crimes between the agency-year observations allowing payday lending and the agency-year observations not allowing payday lending is 94. The difference in the median number of property crimes between those two groups is approximately 153.

3. Identification strategy

The controversy over payday lending has led to considerable variation in the state laws governing the industry. Using those differences, I define an indicator $Allowed_{it}$, to be one if state i's law does not prohibit the standard payday loan contract in year t, and zero otherwise. Because my baseline regressions include state and year fixed effects, the variation that identifies the effect of $Allowed_{it}$ comes from states that switch from allowing to prohibiting payday credit or vice versa. $Allowed_{it}$ will deliver unbiased estimates of the effect of payday lending as long as the political economy behind changes in $Allowed_{it}$ does not separately influence or respond to, property crimes. In another word, my identification assumption is that payday law changes are uncorrelated with the changes in unobserved determinants of property crimes. This assumption is valid because states make changes to payday lending laws for reasons other than fighting against the crimes. For example, Minnesota starts allowing payday lending in 1995 because of the

[7] The results are consistent if I use unwinsorized data.

Consumer Small Loan Lender Act (https://www.revisor.mn.gov/statutes/cite/47.60) which intends to avoid residents from borrowing money from the unlicensed lender. North Carolina bans payday lending in 2001 because of the predatory nature of such loans - inducing financial pressure on the borrowers in North Carolina.

Following Morgan et al. (2012), I study how the number of property crimes changes as the state switches from allowing to prohibiting payday lending, or vice versa. To mitigate the concern that my results are driven by the differences between states allowing and not allowing payday lending, I construct a propensity score-matched sample. Specifically, I proceed as follows. First, I create a panel dataset that contains the state-year level data including the number of crimes, the dummy variable $Allowed_{it}$, and several control variables that are correlated with the number of crimes. Second, I define the treated group as those state-year observations allowing payday lending and the control group as those state-year observations not allowing payday lending. In this sample, 754 (49.346%) state-year observations allow payday lending, and 776 (50.654%) state-year observations do not allow payday lending. Third, I run a Probit regression to estimate the propensity score (P-score) for receiving the treatment for each observation as follows,

$$Prob(Allowed_{it}) = \alpha_t + \beta_1 X_{it} + \varepsilon_{it} \ (1) \ ,$$

where $Allowed_{it}$ is a dummy to be one if state i allow payday lending in year t, and zero otherwise. The vector X_{it} is a set of state-level control variables that includes the natural logarithm of the state's population (*Ln (state population)*), the natural logarithm of GDP per capita (*Ln (GDP per capita)*), the natural logarithm of household income (*Ln (Household income)*), *Unemployment rate*, *Poverty rate*, the percentage of the minority population, the natural logarithm of the number of crimes (*Ln (No. of crimes)*), a dummy variable equals one if the Democratic party controls the statehouse, a dummy variable equals one if the Democratic party controls the senate, and a dummy variable indicates a Democratic governor. I include year-fixed effects in this model and cluster the standard error at the state level. I report the marginal effects in Table 3. The standard errors are reported in the parentheses. After the Probit regression. I match the treated states with the control states by using the closest P-score in the year that the treated states started to allow payday lending. This matching process is conducted without any replacement. The control state not only includes the observations from states that never allow payday lending but also includes observations from states that allow payday lending outside the ten years window (-5, +5) around the matching treated state's payday lending adoption year.

This matching process generates 24 pairs of the treated-control states. I then use the corresponding agency-year level observations of the treated-control states to test the effects of payday lending on property crimes. I choose the ten years window, that is, from five years before to five years after the treated states start allowing payday lending. Table 4 provides descriptive statistics for the matched sample used for this estimation. The sample consists of 37,695 agency-year observations. The average number of *Property crimes* is 2,401.12, which is comparable to the average number of *Ln(property crime)* in the full agency-year observations sample. I then estimate the effect of payday lending on property crimes with the following specification,

$$Property\ crime_{it} = \alpha_{s(i)} + \alpha_t + \beta_1 Treat_i \times Post_t + \beta_2 Treat_i + \beta_3 Post_t + \gamma X_{st} + \delta Z_{ct} + \varepsilon_{it} \quad (2),$$

where $Property\ crime_{it}$ is *Ln (No. of property crimes)* reported by agency i in year t. $Treat_i$ equals one if the agency is located in the treated states, and zero otherwise. $Post_t$ equals one for years after the treated state's payday lending adoption year, and zero otherwise. The vector X_{st} includes state-level control variables, such as the *Ln (Household income)*, *Poverty rate*, *Unemployment rate,* and *Ln (GDP per capita)*. The vector Z_{ct} includes county-level control variables, such as *Ln (population)*, *Ln (income per capita), Ln (personal income*), *Ln (No. of jobs)*, and the percentage of the minority population. a dummy variable equals one if the Democratic party controls the statehouse, a dummy variable equals one if the Democratic party controls the senate, and a dummy variable indicates the Democratic governor. $\alpha_{s(i)}$ is the state (agency) fixed effects that control for any time-invariant factors across the state (agency) that are correlated with payday lending laws. α_t is the year fixed effects. Following Petersen (2009), I cluster the robust standard errors at the state level because the payday lending laws vary at the state level. Under this specification, $\beta_1$captures the effect of payday lending laws on property crimes.

4. Results

4.1. Baseline difference-in-differences regressions

Table 5 reports the difference-in-differences results for estimating equation (2). I control for state-fixed effects and year-fixed effects in panel A. The dependent variables are *Ln (No. of property crimes), Ln (No. of burglary crimes), Ln (No. of larceny-theft crimes),* and *Ln (No. of motor theft crimes)* in columns (1) to (8). The odd and even number columns provide the results without and with the control variables. I include the results without the control variables because if those variables are affected by the treatment themselves then including them produces biased estimates (Angrist and Pischke, 2008). I include the results with control variables to ensure that my results are robust. The standard errors are reported in the parentheses. In panel A, the coefficient estimates of *Treat×Post* in columns (1) and (2) are positive and statistically significant at the 10% and 5% levels. Based on the estimates in column (2), agencies located in states allowing payday lending report 14.34% more property crimes than agencies located in states not allowing payday lending do, which amounts to approximately 270 property crimes based on the average number of property crimes. The coefficient estimate on *Treat×Post* in column (4) is positive and statistically significant at the 5% level, suggesting that the agencies located in states allowing payday lending report 13.88% more burglaries, which translates into approximately 68 burglary crimes based on the average number of burglary crimes. The coefficient estimates on *Treat×Post* in columns (5) and (6) are positive and significant at the 5% and 1% levels, suggesting that the agencies located in states allowing payday lending report 14.91% (or 132) more larceny-theft crimes than the agencies located in the states not allowing payday lending do. The coefficient estimate on *Treat×Post* in column (8) is positive and significant at the 5% level, suggesting that agencies located in states allowing payday lending report 14.22% (or 37) more motor theft crimes than agencies located in states not allowing payday lending.

In panel B, I replace the state-fixed effects with the agency-fixed effects. I find that the coefficient estimates of *Treat×Post* are still statistically significant in columns (1) and (2). In terms of economic significance, based on column (2), agencies located in states allowing payday lending report 6.40% more property crimes than agencies located in states not allowing payday lending do. This is equivalent to 127 property crimes.

To identify the channels through which the payday lending laws affect property crimes, I perform a placebo test that replaces *Ln (No. of property crimes)* with *Ln (No. of violent crimes)* in equation (2) as the dependent variable. If payday lending affects property crimes through some non-financial channel(s), payday lending should increase violent crimes. If payday lending affects crimes through the social disorganization or routine activities channel, then the borrowers and the payday loan lenders are likely the victims of some violent crimes associated with payday lending. The reason is that offenders of such crimes often use violence to receive cash that facilitates the consumption of drugs and other bad behaviors. Under this scenario, I predict that payday lending increases violent crimes. Nevertheless, if payday lending affects property crimes through the financial channel, then the borrowers are likely the offenders of property crimes. This is because they are trying to fix their financial problems by breaking the law. Under this scenario, the borrowers are more likely to avoid committing violent crimes because they don't want to solve one problem by creating a new and more serious problem.

Table 6 presents the results of the placebo test. The dependent variables are *Ln (No. of violent crimes), Ln (No. of murder crimes), Ln (No. of rape crimes), Ln (No. of robbery crimes),* and *Ln (No. of aggravated assault crimes)*. In contrast to the coefficient estimates on *Treat×Post* in Table 5, the coefficient estimates on *Treat×Post are much smaller and statistically insignificant in all columns (except for column (5) in panel A), suggesting that the increases in property crimes are driven by the financial pressure induced by payday loans.*

4.2. Identification challenges

The consistency of the difference-in-differences estimation depends on the parallel trend assumption, that is, the outcome variables should have parallel trends in the absence of treatment. To ensure that the difference-in-differences estimation is not driven by the pre-existing trend differences between treated and control states, I perform the dynamic analysis for the effect of payday lending laws on the number of property crimes. Specifically, I interact each event year dummy with the treated state dummy, that is, I estimate the following,

$$Property\ crime_{it} = \alpha_s + \alpha_t + \alpha_j + \sum_{k=-3}^{k=3} \beta_k Treat \times Year_k + \gamma X_{st} + \delta Z_{ct} + \varepsilon_{it} \quad (3),$$

where all variables are defined the same as those in equation (2), except for $Year_k$, which is a dummy variable equal to one if the observation is k years after the states allowing payday lending, and zero otherwise. α_i is the state (agency) fixed effects. α_t is the year fixed effects. α_j is the payday lending adoption year fixed effects. In this model, $\beta_k\ 's$ captures the difference between the effect of payday lending on property crimes in year k and the effect of payday lending on property crimes in four and five years before the states started to allow payday lending.

If the effect of payday lending on property crimes is not driven by the pre-existing differences between the treated and control states, I expect $\beta_k's$ to be small for k less than zero, and $\beta_k's$ to be positive for k greater than zero. However, if the difference-in-differences estimates are driven by the pre-existing differences between the treated and control states, the $\beta_k's$ could be positive for some k less than zero.

Figure 1 plots the coefficient estimates and their 95% confidence intervals of $Treat \times Year_k$. The dependent variable is *Ln (No. of property crimes)*. I find that $\beta_k's$ are all small and statistically insignificant for k less than zero, but they become larger and statistically significant for some k greater than zero. These results suggest that the difference-in-differences regression estimation is unlikely to be driven by the pre-existing differences between the treated and control states.

5. Counties close to states legalizing payday lending

The baseline regression delivers unbiased estimates of the effect of payday lending if payday lending law changes are uncorrelated with changes in unobserved determinants of crimes. The natural question for the baseline regression is whether the state legislators target payday lending and crimes at the same time. For example, state-level budget problems could motivate the states to adopt laws allowing payday lending, and at the same time, the worsening budget problems could also impact crime rates. Also, the baseline regression results may be biased by unobserved factors at the state level. To mitigate this concern, I use the same method as Melzer (2021). First, I construct an alternative measure – an indicator called $Access\_X\_Y_{ct}$. The $Access\_X\_Y_{ct}$ equal to one if the center of the county c is located within X and Y miles of a state allowing payday lending in year t, and zero otherwise. I use the state×year fixed effects to reduce the concern that political forces jointly affect payday laws and crimes, as there is little reason to believe that legislators in nearby states directly influence the number of crimes outside of their state. Furthermore, to the extent that political decisions are correlated among adjacent states, the state×year fixed effects in the regressions prevent this source of variation from affecting the $Access\_X\_Y_{ct}$ coefficients. Second, to ensure that the effects of payday lending on crime are not driven by the state-level factors that are correlated with the payday lending laws, I include the state×year fixed effects to account for contemporaneous local shocks at the state level.

I compare the number of property crimes reported by agencies located near a state allowing payday lending with the number of crimes reported by agencies located further away from the state allowing payday lending. In particular, I estimate the following specification,

$$Proeprty\ crime_{it} = \alpha_{s\times t} + \beta_1 Access\_X\_Y_{ct} + \gamma Z_{ct} + \varepsilon_{it}\ (4),$$

For example, *Access_0_30*$_{ct}$ equals one if the center of a county is located 30 miles or less from a state allowing payday lending, and zero otherwise. *Access_30_40*$_{ct}$ equals one if the center of a county is located between 30 and 40 miles from a state allowing payday lending, and zero otherwise. The omitted variable is *Access_40_plus*. The $Access\_X\_Y_{ct}$ *measure* varies within the state year, but only in states prohibiting payday lending. In other words, if the state-year allows payday lending, $Access\_X\_Y_{ct}$ *equals one for sure. If the state-year does not allow payday* *lending, then the* $Access\_X\_Y_{ct}$ *can be one for some agencies located in the county which is close to a state allowing payday lending. Within the state year*, the effect of $Access\_X\_Y_{ct}$ *on crimes* is identified by comparing the number of property crimes reported by the agencies near a state allowing payday lending with those reported by the agencies located further away from states allowing payday lending.

I use *Access_0_30*$_{ct}$ and *Access_30_40*$_{ct}$ as the independent variables and *Ln(No. of property crimes), Ln(No. of burglary crimes)*, *Ln(No. of larceny-theft crimes)*, and *Ln(No. of motor theft crimes)* as the dependent variables to estimate equation (4). *Access_0_30*$_{ct}$ is an effective measure of payday lending because the borrowers who reside in states prohibiting payday lending but have access to payday lenders use payday loans. Considerable pieces of evidence suggest that people cross into payday allowing states to get loans. Spiller (2006) documents that Massachusetts residents travel to New Hampshire to get loans. Appelbaum (2006) documents the build-up of payday loan stores along the South Carolina-North Carolina border to serve customers from North Carolina, which prohibits payday lending. Those papers also document that payday lenders cluster at such borders, as one would expect if they face demand from across the border. Therefore, I include *Border*, a dummy variable indicating whether the center of the county is located within 25 miles of the state border, in equation (4). *Border* controls for general differences between counties near a state border and other counties.

Table 7 presents the results of estimating equation (4). The coefficient estimates on *Access_0_30*$_{ct}$ in columns (1) and (2) are positive and statistically significant at the 5% and 10% levels. The coefficient estimates on *Access_30_40*$_{ct}$ are statistically insignificant in all columns, suggesting that the impact of payday lending on property crimes decreases with the distance between states allowing payday lending. The agencies located within 30 miles of a state allowing payday lending report 18.41% more property crimes than agencies located further away. Overall, the results suggest two things. First, the effect on property crimes is not driven by the political forces that jointly affect payday laws and crimes. Second, payday lending not only affects property crimes in states allowing payday lending but also affects property crimes in counties that share a border with the state(s) allowing payday lending.

Next, I perform the dynamic analysis for the effect of $Access\_0\_30_{ct}$ on property crimes. To do this, I create event year dummies for $Access\_0\_30_{ct}$ around the year (5 years before to 3 and more years after) states start allowing payday lending. Figure 2 plots the coefficient estimates and their 95% confidence intervals for *Access_0_30*. The $\beta_k{'}s$ are small and statistically insignificant for all k less than zero, but they become larger and statistically significant for some k greater than zero, suggesting that the results of table 7 are not driven by the pre-existing differences between a pair of border-sharing counties (One locates in a state allowing payday lending and the other does not).

6. The cross-sectional tests

6.1. Local economic conditions

To further identify whether payday lending affects property crimes through the financial pressure channel, I split the propensity score matched-sample into two subsamples-the poor economic condition subsample and the wealthy economic condition subsample. I then re-estimate equation (2) and test the difference between the coefficient estimates of *Treat×Post* in the poor economic condition subsample and the wealthy economic condition subsample.

Table 8 presents the results of estimating equation (2) for the poor economic condition and the wealthy economic condition subsamples. Panels A, B, C, and D split the sample based on the state-year median household income, income per capita, unemployment rate, and poverty rate. I find that the coefficient estimates on *Treat×Post are positive and statistically significant in low household income, low-income per capita, high unemployment rate, and high poverty rate subsamples.*

The differences between the coefficient estimates of Treat×Post are statistically significant for household income, income per capita, and employment rate subsamples. These results further suggest that the impact of payday lending laws on property crimes is driven by the financial pressure induced by payday loans.

6.2. The availability of other lenders

The lack of access to formal financing could also serve as another channel to induce people to use more payday loans. For example, the lack of commercial banks motivates borrowers to use more payday loans (Barth et al., 2015). Also, Payday loan storeowners are likely to establish their businesses in areas with fewer commercial banks (Pew Charitable Trusts, 2016). If those arguments are valid, I predict that people will use more payday loans in the areas subject to fewer banks. Therefore, if the financial distress induced by payday loans motivates payday loan borrowers to engage in property crimes, this effect should be stronger in areas with fewer commercial banks.

I collect the number of commercial bank branches at the county level from the Federal Deposit Insurance Corporation (FDIC). I split the propensity score matched-sample based on the state-year median number of commercial bank branches. If the lack of commercial banks motivates borrowers to use more payday loans, then the financial pressure induced by payday loans is going to increase for the borrowers. Under this case, I expect to find a stronger effect of payday lending on property crimes in the subsample subject with fewer banks. *If the lack of* *banks does not motivate people to use more payday loans, then the financial pressure would not change. In this case, the effect of payday lending on crimes should be similar in both areas.*

Table 9 presents the results of estimating equation (2) on the higher number of commercial bank branches and the lower number of commercial bank branch subsamples. The coefficient estimate on *Treat×Post is positive and significant in column (1) of both subsamples.* The coefficient estimate in the lower *commercial bank* branches subsample is slightly greater than that in the higher *commercial bank* branches subsample (0.126 vs 0.115). Nevertheless, the difference between those two coefficient estimates is small and statistically insignificant, suggesting that the lack of commercial banks does not motivate people to use more payday loans.

6.3. Who are the real victims of the effect of payday lending

Stegman and Faris (2003) and King, Li, Davis, and Ernst (2005) find that payday lenders are likely to concentrate on the areas subject to the higher minority population. Also, the extensive literature on discrimination in credit markets (Boucher, Barham, and Carter, 2005) suggests that African Americans and other minorities have less access to the lenders such as commercial banks.

To test whether the minority population suffers more from the impact of payday lending on property crimes, I split the propensity score-matched sample into two subsamples - a higher minority population subsample and a lower minority population subsample and then re-estimate the equation (2). If the literature suggests that payday lenders clustered in the minorities' communities, then I would expect that the effect of payday lending on property crimes is stronger *in higher minority population subsamples.*

Table 10 presents the results of estimating equation (2) on a higher minority population and lower minority population subsamples. Panels A, B, C, and D split the sample based on the state-year median of the proportion of the African American, Native American, Asian American, and Latino American populations. I find that the differences between the coefficient estimates *of Treat×Post* in panels A and D *are positive. Also*, the difference between the coefficient estimates *of Treat×Post* in panels A is statistically significant. *The panels B and C suggest that coefficient estimates of Treat×Post are positive and significant in the lower minority population subsamples. The difference* between the coefficient estimates *of Treat×Post is statistically significant in panel C.*

These results indicate that the impact of payday lending on property crimes is larger in the African American communities. To explain this result, Stegman (2007) finds that payday lenders cluster in African American communities. The California Department of business oversight (DBO, 2016) shows that payday loan stores in the state are disproportionately located in heavily African American neighborhoods. Also, the financial institutions do not treat their African American clients equally because the commercial banks use credit scores as a primary determinant of loan approval. Since the average African American has lower credit scores than the average White American (Ards and Myers, 2001; Ross and Yinger 2002; Federal Reserve Board 2007), the African Americans' likelihood of getting a loan denied is higher. To explain the results in Panel C for Asian Americans. Sun (1998) reports that Asian American families are likely to save a higher proportion of their income. Therefore, payday loan store owners are less likely to establish their businesses in those communities because the demand is lower.

7. Conclusion

This paper studies the impact of state-level payday lending regulations on property crimes in the United States. Consistent with the financial strain theory, evidence from the difference-in-differences regressions show that legalizing payday lending increases property crimes. On average, the agencies located in states allowing payday lending report 13.65% more property crimes than the agencies located in states not allowing payday lending do. Nevertheless, this impact does not hold for violent crimes because the effect is driven by the borrowers' financial pressure. In other words, payday lending increases property crimes mainly by financial distress.

To strengthen my identification strategy, I conduct a dynamic analysis of the effect of payday lending on property crimes. My results suggest that the difference-in-differences regressions are unlikely to be driven by the pre-existing differences between treated and control states. To account for contemporaneous local shocks at the state level, I create an alternative measure following Melzer (2011) and include state×year fixed effects. My results still hold. Last, I perform several cross-sectional tests to identify the heterogeneity of the adverse effect of payday lending on property crimes. My results confirm that (1) payday lending laws have an impact on property crimes through the financial pressure channel. (2) Compared with White Americans, minorities such as African Americans are the real victims of the adverse impact of payday lending.

The payday loans industry makes large amounts of money from people who live close to the financial edge. The policy question is whether those borrowers should be able to take out high-cost loans repeatedly, or whether they should have a better alternative. Critics of payday lenders, including the Center for Responsible Lending, claim that the loans could become a debt trap for people who live paycheck to paycheck. Nevertheless, if the industry's critics devote themselves to stopping payday lenders from capitalizing on the financial troubles of low-income borrowers, they should look for ways to make suitable forms of credit available. Perhaps a solution to payday lending could come from reforms that are more moderate to the payday lending industry, rather than attempts to close them. Some evidence suggests that smart regulation can improve the business for both lenders and consumers. In 2010, Colorado reformed its payday-lending industry by reducing the permissible fees, extending the minimum term of a loan to six months, and requiring that a loan be repayable over time, instead of coming due all at once. Pew reports that half of the payday stores in Colorado closed, but each remaining store almost doubled its customer volume, and now payday borrowers are paying 42 percent less in fees and defaulting less frequently, with no reduction in access to credit.

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Appendix A. Variable description

Variable	Definition (data source)
Crime rates	
Ln(Property crime)	Natural logarithm of the number of property crimes (uniform crime report)
Ln(Burglary)	Natural logarithm of the number of burglary crimes (uniform crime report)
Ln(Larceny theft)	Natural logarithm of the number of larceny-theft crimes (uniform crime report)
Ln(Motor theft)	Natural logarithm of the number of motor theft crimes (uniform crime report)
Ln(Violent crime)	Natural logarithm of the number of violent crimes (uniform crime report)
Ln(Murder)	Natural logarithm of the number of murder crimes (uniform crime report)
Ln(Rape)	Natural logarithm of the number of rape crimes (uniform crime report)
Ln(Robbery)	Natural logarithm of the number of robbery crimes (uniform crime report)
Ln(Assault)	Natural logarithm of the number of assault crimes (uniform crime report)
Payday lending access	
Allowed	The dummy variable equals one if the agency locate in the state allowing payday lending, and zero otherwise
Treat	The dummy variable equals one if the agency is located in the treated states, and zero otherwise
Post	The dummy variable equals one if the year is greater than or equal to the first adoption year of the treated states, and zero otherwise
Access_x_y	The dummy variable equals one if the center of the county is located within X and Y miles of a state that allows payday lending, and zero otherwise
Border	The dummy variable equals one if the center of the county is located 25 miles from a state border, and zero otherwise
Payday border	The dummy variable equals one if the county is located in a range of 15 miles from a state that allows payday lending, and zero otherwise
State characteristics	
Ln(GDP per capita)	Natural logarithm of GDP per capita (Federal Reserve Bank of St Louis)
Ln(Household income)	Natural logarithm of median household income (Federal Reserve Bank of St Louis)
Poverty rate	The ratio of the number of people (in a given age group) whose income falls below the poverty line (Federal Reserve Bank of St Louis)
Unemployment	The share of the labor force that is jobless, expressed as a percentage (Federal Reserve Bank of St Louis)
State characteristics	
Ln(Population)	Natural logarithm of county population (U.S Census Bureau)
Ln(Income per capita)	Natural logarithm of county income per capita (U.S Census Bureau)
Ln(Personal income)	Natural logarithm of county personal income (U.S Census Bureau)
Ln(No. of jobs)	Natural logarithm of the number of jobs offered in each county (U.S Census Bureau)
Native American	The proportion of the Native American population (NBER)
African American	The proportion of the African American population (NBER)
Asian American	The proportion of the Asian American population (NBER)
Latino American	The proportion of the Latino American population (NBER)
Democratic House	The dummy variable equals one if the majority of the statehouse is held by the democratic party (Ballotpedia)
Democratic senate	The dummy variable equals one if the majority of the senate is held by the democratic party (Ballotpedia)
Democratic governor	The dummy variable equals one if the governor is a Democratic party member (Ballotpedia)

Table A.1. State Laws Regard to Payday Loan

The sample starts in 1985 and ends in 2014. Many states have laws that effectively prohibit payday lending by imposing binding interest rate caps on payday loans or consumer loans. Some other states explicitly outlaw the practice of payday lending. These laws prohibiting or discouraging payday lending are generally well-enforced, if not always perfectly enforced (King and Parrish 2010), and hence provide a good source of variations in the availability of payday loans across states and time. My primary sources of those laws are the laws themselves such as statutes, superseded statutes, and session laws.

Table A.1.

Classifying payday lending laws, 1985–2014

State	Permitted at the start of the sample?	Change 1		Change 2		Change 3	
		Year	Type	Year	Type	Year	Type
AK	No	2004	Yes				
AL	No	1998	Yes				
AZ	No	2000	Yes	2006	No		
AR	No	1999	Yes	2001	No	2005	Yes
CA	No	1997	Yes				
CO	Yes						
CT	No						
DC	No	1998	Yes	2007	No		
DE	No	1987	Yes				
FL	Yes						
GA	No	2001	Yes	2005	No		
HI	No	1999	Yes				
ID	No	2001	Yes				
IL	No	2000	Yes				
IN	No	1990	Yes				
IA	No	1998	Yes				
KS	No	1991	Yes	2005	No		
KY	No	2009	Yes				
LA	No	1990	Yes				
ME	Yes						
MD	No						
MA	No						
MI	No	2005	Yes				
MN	No	1995	Yes				
MS	No	1998	Yes				
MO	No	2002	Yes				
MT	No	1999	Yes				
NE	No	1993	Yes				
NV	Yes						
NH	No	2003	Yes				
NJ	No						
NM	Yes						
NY	No						
NC	No	1997	Yes	2001	No		
ND	Yes	1997	No	2001	Yes		
OH	No	1995	Yes				
OK	No	2003	Yes				
OR	No	1998	Yes				
PA	No						
RI	No	2001	Yes				
SC	No	1998	Yes				
SD	No	1990	Yes				
TN	No	1990	Yes				
TX	No	2001	Yes	2005	No		
UT	No	1999	Yes				
VT	Yes	2001	No				
VA	No	2002	Yes	2005	No	2009	Yes
WA	No	1995	Yes	2005	No		
WV	No						
WI	Yes						
WY	No	1996	Yes				

Table 1 Descriptive statistics

This table reports the summary statistics of the variables used in this paper. The variables are the number of property crimes, the number of burglary crimes, the number of larceny crimes, the number of motor theft crimes, the number of violent crimes, the number of murder crimes, the number of rape crimes, the number of robbery crimes, the number of assault crimes; Allowed, dummy equals one if the state law does not prohibit the standard payday loan contract, and zero otherwise; Allowed_x_y, dummy equals one if the center of the county is located within X and Y miles of a state allowing payday lending, and zero otherwise; Border, dummy variable indicating whether the center of the county is located within 25 miles of the state border; The GDP per capita; The household income; The Poverty rate, percentage of household income below the federal poverty line; Unemployment, The share of the labor force that is jobless, expressed as a percentage; The county population; The county income per capita; The county personal income; The number of jobs offered in each county; Native American, The proportion of Native American population; African American, The proportion of African American population; Asian American, The proportion of Asian American population; Latino American, The proportion of Latino American population

Variable	N	Mean	Std. Dev	P25	Median	P75
Panel A. Crime						
Property crime	100,775	2,179.654	8,602.477	367	720	1,584
Burglary crime	100,775	500.418	1,979.748	70	154	359
Larceny theft crime	100,775	1,419.979	5,059.971	254	502	1,091
Motor theft crime	100,775	260.329	1,749.651	17	42	117
Violent crime	100,775	311.257	2,177.549	22	58	163
Murder	100,775	3.822	28.918	0	0	2
Rape	100,775	18.554	70.022	2	5	14
Robbery	100,775	108.644	1,120.3	3	10	35
Assault	100,775	186.715	1,139.556	13	37	107
Panel B. Payday lending regulation						
Allowed	100,775	0.468	0.499			
Access_0_30	100,775	0.501	0.500			
Access_30+	100,775	0.072	0.258			
Border	100,775	0.381	0.486			
Panel C. State-level characteristics						
GDP per capita	100,775	35,145.36	12,955.61	23,865	34,131	44,239
Household income	100,775	40,491.48	11,071.85	31,496	40,379	48,294
Poverty rate	100,775	0.128	0.032	0.107	0.127	0.155
Unemployment	100,775	6.129	1.920	4.800	5.800	7.200
Democratic	100,775	0.582	0.493			
Democratic Senate	100,775	0.523	0.499			
Democratic Governor	100,775	0.323	0.484			
Panel D. County-level characteristics						
Population	100,775	680,357.4	1406,684	84,789	250,432	694,808
Income per capita	100,775	29,869.9	12,706.79	19,995	27,741	37,098
Personal income	100,775	22,100	42,800	1,967	6,824	22,900
No. of jobs	100,775	401,990.8	819,316.6	42,452	133,250	411,682
White American	100,775	0.841	0.119	0.774	0.870	0.935
Native American	100,775	0.008	0.014	0.002	0.004	0.008
Asian American	100,775	0.033	0.040	0.007	0.018	0.039
African American	100,775	0.108	0.108	0.023	0.066	0.144
Latino American	100,775	0.103	0.143	0.019	0.045	0.138

Table 2 Uni-variate comparison

This table reports the univariate comparison for the sample. Panel A reports the univariate comparison of crimes between agencies located in states allowing payday lending and agencies located in states not allowing payday lending. Panel B reports the univariate comparison of control variables at the county level between agencies located in states allowing payday lending and agencies located in states not allowing payday lending. Panel C reports the univariate comparison of control variables on state-level between agencies located in states allowing payday lending and agencies located in states not allowing payday lending. The sample contains all crime information in the UCR program database originated during the calendar years 1985 through 2014.

	Allowed=1		Allowed=0		Difference	
	N=41,015		N=59,760			
Panel A. Crime	Mean	Median	Mean	Median	Mean	Median
Property crime	2,257.280	813.000	2,163.230	660.000	94.050***	153.000***
Burglary	505.804	142.000	498.107	171.000	7.698**	-29.000**
Larceny theft	1,496.730	576.000	1,392.950	459.000	103.780***	117.000***
Motor Theft	268.174	48.000	255.883	39.000	12.290**	9.000**
Violent crime	314.874	71.000	314.286	51.000	0.588***	20.000***
Murder	4.036	1.000	3.679	0.000	0.357*	1.000*
Rape	20.792	7.000	17.368	4.000	3.424	3.000
Robbery	118.553	12.000	97.416	9.000	21.138**	3.000**
Assault	196.263	45.000	182.866	33.000	13.397*	12.000*

Table 3 Probit model regression

I run a Probit model regression to get a propensity score (P-score) for receiving treatment for each observation. The model is displayed as follows, $Allowed_{it} = \alpha_t + \beta_1 \times X_{it} + Year\ fixed\ effect + \varepsilon_{it}$ (1) , where *Allowed* equals one if state i allow payday lending in year t, and zero otherwise. X_{it} are state-level control variables such as the natural logarithm of the state population (*Ln(state population)*), the natural logarithm of GDP per capita (*Ln(GDP per capita)*), the natural logarithm of household income (*Ln(Household income)*), *unemployment rate*, *poverty rate*, the proportion of minorities' population on the state level, the natural logarithm of crimes, and Democratic, a dummy to be 1 if the majority of the statehouse is controlled by the Democratic party. I include year-fixed effects in this model. I report the marginal effects. The standard errors are reported in the parentheses. Significance at 1%, 5%, and 10% levels are indicated by ***, **, and *, respectively.

	Allowed
	(1)
Ln(population)	-0.542***
	(0.644)
Ln(GDP per capita)	0188
	(0.953)
Ln(Household income)	-1.029**
	(1.467)
Unemployment	-0.016
	(0.059)
Poverty	-2.471
	(6.518)
White American	-82.527
	(229.733)
Native American	-83.474
	(230.763)
Asian American	-82.646
	(229.608)
African American	-83.061
	(229.739)
Ln(No. of crimes)	0.469***
	(0.568)
Democratic	-0.065
	(0.236)
Democratic Senate	0.024
	(0.224)
Democratic Governor	-0.009
	(0.149)
Year fixed effects	Yes
No. of observations	1,400
Pseudo R-squared	0.249

Table 4 Descriptive statistics for the matched sample

This table reports the summary statistics of the variables used in this paper. The variables are the number of property crimes, the number of burglary crimes, the number of larceny crimes, the number of motor theft crimes, the number of violent crimes, the number of murder crimes, the number of rape crimes, the number of robbery crimes, the number of assault crimes; Allowed, dummy equals one if the state law does not prohibit the standard payday loan contract, and zero otherwise; Allowed_x_y, dummy equals one if the center of the county is located within X and Y miles of a state allowing payday lending, and zero otherwise; Border, dummy variable indicating whether the center of the county is located within 25 miles of the state border; The GDP per capita; The household income; The poverty rate, percentage of household income below the federal poverty line; Unemployment, The share of the labor force that is jobless, expressed as a percentage; The county population; The county income per capita; The county personal income; The number of jobs offered in each county; Native American, The proportion of Native American population; African American, The proportion of African American population; Asian American, The proportion of Asian American population; Latino American, The proportion of Latino American population; Democratic, a dummy to be 1 if the majority of the statehouse is controlled by the Democratic party.

Variable	N	Mean	Std. Dev	P25	Median	P75
Panel A. Crime						
Property crime	37,695	2,401.116	9,242.779	384	788	1,749
Burglary crime	37,695	546.835	2,040.211	74	170	403
Larceny theft crime	37,695	1,538.184	5,368.235	264	536	1,182
Motor theft crime	37,695	316.140	2,012.753	20	49	143
Violent crime	37,695	361.086	2,526.395	25	67	184
Murder	37,695	4.540	32.899	0	1	2
Rape	37,695	19.637	74.478	2	6	15
Robbery	37,695	128.246	1,217.265	3	12	42
Assault	37,695	220.316	1,363.641	15	45	125
Panel B. Payday lending regulation						
Treat	37,695	0.511	0.499			
Post	37,695	0.562	0.496			
Panel C. State-level characteristics						
GDP per capita	37,695	32,139.56	9,269.338	24,787	31,490	38,816
Household income	37,695	38,194.51	8,582.36	31,855	37,715	44,005
Poverty rate	37,695	0.133	0.031	0.110	0.131	0.158
Unemployment	37,695	5.791	1.715	4.700	5.500	6.500
Democratic	37,695	0.686	0.464			
Democratic Senate	37,695	0.542	0.499			
Democratic Governor	37,695	0.373	0.482			
Panel D. County-level characteristics						
Population	37,695	817,430.9	1,720,942	85,473	260,812	781,265
Income per capita	37,695	27,110.65	10,391.72	19,495	25,012	32,227
Personal income	37,695	22,100	42,800	1,967	6,824	22,900
No. of jobs	37,695	473,111.3	976,935.5	41,922	141,083	456,522
White American	37,695	0.845	0.115	0.495	0.866	0.991
Native American	37,695	0.008	0.014	0.002	0.004	0.008
Asian American	37,695	0.034	0.040	0.007	0.018	0.042
African American	37,695	0.105	0.108	0.780	0.067	0.933
Latino American	37,695	0.107	0.142	0.021	0.047	0.140

Table 5 Baseline difference-in-differences

This table reports the OLS estimation results of $Crime_{it} = \alpha_i + \alpha_t + \beta_1 \times Treat_i \times Post_t + \beta_2 \times Treat_i + \beta_3 \times Post_t + \gamma \times X_{it} + \varepsilon_{it}$. The dependent variable in Columns (1) and (2) is Ln(property crimes), the dependent variable in Columns (3) and (4) is Ln(Burglary crimes), the dependent variable in Columns (5) and (6) is Ln(larceny crimes), and the dependent variable in Columns (7) and (8) is Ln(Motor theft crimes). Treat equals one if the agency is a treated state, and zero otherwise. Post equals one if the agency-year observation is after the payday lending adoption. All regressions include year effects and state (agency) fixed effects. Standard errors are clustered by state. Significance at 1%, 5%, and 10% levels are indicated by ***, **, and *, respectively.

	Ln(Property crime)	Ln(Property crime)	Ln(Burglary)	Ln(Burglary)	Ln(Larceny)	Ln(Larceny)	Ln(Motor theft)	Ln(Motor theft)
Panel A.	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treat×Post	0.100**	0.134***	0.112**	0.130**	0.104**	0.139***	0.096	0.133**
	(0.045)	(0.043)	(0.050)	(0.053)	(0.042)	(0.039)	(0.066)	(0.056)
Treat	-0.119***	-0.144***	-0.088**	-0.104***	-0.134***	-0.159***	-0.139***	-0.170***
	(0.036)	(0.031)	(0.038)	(0.034)	(0.038)	(0.031)	(0.048)	(0.041)
Post	-0.076**	-0.098***	-0.055	-0.067*	-0.079**	-0.106***	-0.138***	-0.143***
	(0.034)	(0.030)	(0.034)	(0.034)	(0.035)	(0.029)	(0.043)	(0.036)
Ln(Population)		-2.411***		-2.216***		-2.694***		-1.223
		(0.863)		(0.762)		(0.908)		(1.057)
Ln(GDP per capita)		0.816**		0.685*		0.778*		0.901**
		(0.402)		(0.374)		(0.416)		(0.419)
Ln(Household income)		0.411		0.336		0.417		0.389
		(0.251)		(0.246)		(0.255)		(0.320)
Ln(Income per capita)		-2.482***		-2.596***		-2.494***		-2.272*
		(0.819)		(0.752)		(0.833)		(1.159)
Ln(Personal income)		1.650*		1.559*		1.786*		1.182
		(0.869)		(0.782)		(0.898)		(1.145)
Unemployment rate		0.062***		0.071***		0.055***		0.082***
		(0.019)		(0.017)		(0.019)		(0.019)
Poverty rate		0.886**		0.984**		0.793		0.317
		(0.423)		(0.423)		(0.471)		(0.557)
Ln(No. of jobs)		0.973***		0.847***		1.095***		0.493***
		(0.108)		(0.116)		(0.109)		(0.124)
African American		1.008**		1.053**		0.832*		1.809***
		(0.471)		(0.518)		(0.474)		(0.588)
Asian American		-0.630		-1.684**		-0.448		1.690
		(0.893)		(0.751)		(1.033)		(1.208)
Native American		-0.743		-0.149		-1.267		-0.612
		(1.293)		(1.812)		(0.977)		(2.350)
Democratic House		0.001		-0.014		0.006		-0.025
		(0.022)		(0.022)		(0.023)		(0.030)
Democratic Senate		-0.111***		-0.113***		-0.112***		-0.149***
		(0.036)		(0.034)		(0.037)		(0.041)
Democratic Governor		-0.006		-0.002		-0.006		0.015
		(0.027)		(0.026)		(0.027)		(0.032)
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
No. of observations	42,445	37,695	42,445	37,695	42,445	37,695	42,445	37,695

Adjusted R-squared	0.132	0.215	0.202	0.255	0.107	0.191	0.139	0.287

	Ln(Property crime)	Ln(Property crime)	Ln(Burglary)	Ln(Burglary)	Ln(Larceny)	Ln(Larceny)	Ln(Motor theft)	Ln(Motor theft)
Panel B.	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treat×Post	0.088**	0.064*	0.105**	0.074*	0.091**	0.063**	0.084	0.043
	(0.040)	(0.035)	(0.045)	(0.043)	(0.036)	(0.031)	(0.062)	(0.047)
Treat	-0.085***	-0.071***	-0.062**	-0.044	-0.097***	-0.079***	-0.107**	-0.079**
	(0.028)	(0.023)	(0.028)	(0.027)	(0.030)	(0.022)	(0.045)	(0.034)
Post	-0.069**	-0.031	-0.052*	-0.015	-0.073**	-0.033*	-0.126***	-0.057*
	(0.029)	(0.018)	(0.029)	(0.023)	(0.030)	(0.018)	(0.041)	(0.029)
Ln(Population)		0.858***		0.679*		1.021***		1.090**
		(0.217)		(0.341)		(0.227)		(0.537)
Ln(GDP per capita)		0.316		0.248		0.293		0.320
		(0.300)		(0.311)		(0.311)		(0.333)
Ln(Household income)		0.078		0.098		0.080		0.133
		(0.174)		(0.163)		(0.181)		(0.237)
Ln(Income per capita)		-0.113		-0.173		0.016		-0.571
		(0.186)		(0.363)		(0.180)		(0.502)
Ln(Personal income)		0.144		0.108		-0.018		0.579
		(0.173)		(0.312)		(0.172)		(0.485)
Unemployment rate		0.049***		0.060***		0.041**		0.070***
		(0.015)		(0.015)		(0.016)		(0.016)
Poverty rate		0.704*		0.881*		0.625		0.204
		(0.386)		(0.452)		(0.400)		(0.544)
Ln(No. of jobs)		-0.005		-0.036		0.025		-0.154
		(0.129)		(0.131)		(0.134)		(0.180)
African American		8.573***		5.253		9.856***		1.539
		(3.123)		(3.579)		(3.198)		(3.276)
Asian American		-4.777***		-4.334***		-4.655***		-6.499***
		(1.024)		(1.509)		(1.157)		(2.007)
Native American		-3.750		-8.598*		1.292		-11.950
		(4.214)		(4.854)		(3.733)		(7.360)
Democratic		-0.002		-0.014		0.003		-0.027
		(0.021)		(0.021)		(0.021)		(0.024)
Democratic Senate		-0.064**		-0.079**		-0.060**		-0.090***
		(0.026)		(0.029)		(0.027)		(0.026)
Democratic Governor		0.000		-0.009		0.002		0.001
		(0.018)		(0.020)		(0.018)		(0.022)
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Agency fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
No. of observations	42,445	37,298	42,445	37,298	42,445	37,298	42,445	37,298
Adjusted R-squared	0.92	0.929	0.92	0.926	0.909	0.919	0.921	0.929

Table 6 Placebo tests

This table reports the OLS estimation results of $Crime_{it} = \alpha_i + \alpha_t + \beta_1 Treat_i \times Post_t + \beta_2 Treat_i + \beta_3 Post_t + \gamma X_{it} + \varepsilon_{it}$. The dependent variable in Columns (1) and (2) is Ln(Violent crimes), the dependent variable in Columns (3) and (4) is Ln(Murder crimes), the dependent variable in Columns (5) and (6) is Ln(Rape crimes), the dependent variable in Columns (7) and (8) is Ln(Robbery crimes), and the dependent variable in Columns (9) and (10) is Ln(Assault crimes). Treat equals one if the agency is a treated state, and zero otherwise. Post equals one if the agency-year observation is after the payday lending adoption. All regressions include year effects and state (agency) fixed effects. Standard errors are clustered by state. Significance at 1%, 5%, and 10% levels are indicated by ***, **, and *, respectively.

	Ln(Violent crime)	Ln(Violent crime)	Ln(Murder)	Ln(Murder)	Ln(Rape)	Ln(Rape)	Ln(Robbery)	Ln(Robbery)	Ln(Assault)	Ln(Assault)
Panel A.	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Treat×Post	0.057	0.039	-0.035	-0.036	0.065*	0.047	0.000	0.040	0.050	0.019
	(0.044)	(0.042)	(0.028)	(0.024)	(0.036)	(0.033)	(0.059)	(0.047)	(0.044)	(0.042)
Treat	-0.081*	-0.069**	0.011	0.010	-0.071*	-0.056*	-0.039	-0.062**	-0.087*	-0.067
	(0.043)	(0.034)	(0.016)	(0.014)	(0.037)	(0.029)	(0.035)	(0.030)	(0.050)	(0.042)
Post	-0.042	-0.043	0.040**	0.038**	-0.013	-0.033	0.009	-0.021	-0.048	-0.039
	(0.035)	(0.033)	(0.015)	(0.016)	(0.022)	(0.021)	(0.039)	(0.028)	(0.040)	(0.037)
Control variables	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
No. of observations	42,108	37,358	42,108	37,358	42,108	37,358	42,108	37,358	42,108	37,358
Adjusted R-squared	0.179	0.243	0.119	0.187	0.118	0.162	0.160	0.310	0.200	0.244
	Ln(Violent crime)	Ln(Violent crime)	Ln(Murder)	Ln(Murder)	Ln(Rape)	Ln(Rape)	Ln(Robbery)	Ln(Robbery)	Ln(Assault)	Ln(Assault)
Panel B.	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Treat×Post	0.044	-0.009	-0.037	-0.032	0.055	-0.012	-0.014	0.012	0.040	-0.028
	(0.045)	(0.038)	(0.029)	(0.020)	(0.036)	(0.037)	(0.061)	(0.042)	(0.046)	(0.044)
Treat	-0.041	-0.001	0.028***	0.017	-0.046	0.010	0.004	-0.009	-0.051	0.009
	(0.028)	(0.028)	(0.009)	(0.013)	(0.031)	(0.032)	(0.030)	(0.028)	(0.034)	(0.034)
Post	-0.036	0.009	0.039**	0.037**	-0.012	0.019	0.019	0.016	-0.043	0.017
	(0.034)	(0.026)	(0.016)	(0.015)	(0.023)	(0.025)	(0.040)	(0.022)	(0.039)	(0.032)
Control variables	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Agency fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
No. of observations	42,108	36,961	42,108	36,961	42,108	36,961	42,108	36,961	42,108	36,961
Adjusted R-squared	0.912	0.919	0.802	0.805	0.836	0.839	0.930	0.934	0.877	0.887

Table 7 Account for contemporaneous local shocks at the state level

This table reports the OLS estimation results of $Crime_{it} = \alpha_i + \alpha_t + \beta_1 Access + \gamma X_{it} + \varepsilon_{it}$ (4). The dependent variable in Columns (1) and (2) is Ln(property crimes), the dependent variable in Columns (3) and (4) is Ln(Burglary crimes), and the dependent variable in Columns (5) and (6) is Ln(larceny crimes), and the dependent variable in Columns (7) and (8) is Ln(Mother theft crimes). $Access\_X\_Y_{ct}$ is a county-level indicator that equals one if the center of the county is located within X and Y miles of a state allowing payday lending and zero otherwise. All regressions include state×year fixed effects. Standard errors are clustered by state. Significance at 1%, 5%, and 10% levels are indicated by ***, **, and *, respectively.

	Ln(Property crime)	Ln(Property crime)	Ln(Burglary crime)	Ln(Burglary crime)	Ln(Larceny crime)	Ln(Larceny crime)	Ln(Motor theft crime)	Ln(Motor theft crime)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Access_0_30	0.174**	0.169*	0.062	0.060	0.049	0.075**	0.027	0.040
	(0.084)	(0.097)	(0.047)	(0.052)	(0.067)	(0.033)	(0.104)	(0.076)
Access_30_40	0.062	0.076	0.073	0.083	-0.059	0.023	-0.059	0.027
	(0.144)	(0.140)	(0.120)	(0.117)	(0.130)	(0.089)	(0.158)	(0.107)
Border		0.101		0.053		0.012		0.162**
		(0.083)		(0.060)		(0.053)		(0.064)
Control variables	No	Yes	No	Yes	No	Yes	No	Yes
State×Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
No. of observations	122,714	100,603	122,750	100,639	122,732	100,621	122,734	100,623
Adjusted R-squared	0.137	0.216	0.193	0.251	0.118	0.194	0.168	0.296

Table 8 Financial pressure

This table reports the OLS estimation results of $Crime_{it} = \alpha_i + \alpha_t + \beta_1 \times Treat_i \times Post_t + \beta_2 \times Treat_i + \beta_3 \times Post_t + \gamma \times X_{it} + \varepsilon_{it}$. for subsample split by the state-median of household income, income per capita, employment rate, and poverty rate. The dependent variable in Columns (1) and (2) is Ln(property crimes), the dependent variable in Columns (3) and (4) is Ln(Burglary crimes), the dependent variable in Columns (5) and (6) is Ln(larceny crimes), and the dependent variable in Columns (7) and (8) is Ln(Motor theft crimes). Treat equals one if the agency is a treated state, and zero otherwise. Post equals one if the agency-year observation is after the payday lending adoption. All regressions include year effects and state (agency) fixed effects. Standard errors are clustered by state. Significance at 1%, 5%, and 10% levels are indicated by ***, **, and *, respectively.

Panel A.	Ln(Property crime)	Ln(Property crime)	Diff in (1)	Panel B.	Ln(Property crime)	Ln(Property crime)	Diff in (1)
Household income	Low	High		Income per capita	Low	High	
	(1)	(2)	(3)		(1)	(2)	(3)
Treat×Post	0.347***	-0.067*	0.414***	Treat×Post	0.169***	0.074	0.095*
	(0.056)	(0.036)	P=0.0003		(0.052)	(0.057)	P=0.0943
Treat	-0.213***	0.014		Treat	-0.165***	-0.123***	
	(0.068)	(0.039)			(0.035)	(0.035)	
Post	-0.224***	0.037		Post	-0.098***	-0.085*	
	(0.044)	(0.041)			(0.03)	(0.044)	
Control variables	Yes	Yes		Control variables	Yes	Yes	
Year fixed effects	Yes	Yes		Year fixed effects	Yes	Yes	
State fixed effects	Yes	Yes		State fixed effects	Yes	Yes	
No. of observations	17,097	20,598		No. of observations	17,392	20,303	
Adjusted R-squared	0.185	0.248		Adjusted R-squared	0.248	0.216	
Panel C.	Ln(Property crime)	Ln(Property crime)	Diff in (1)	Panel D.	Ln(Property crime)	Ln(Property crime)	Diff in (1)
Unemployment	Low	High		Poverty	Low	High	
	(1)	(2)	(3)		(1)	(2)	(3)
Treat×Post	0.094**	0.201**	-0.107*	Treat×Post	0.039	0.207**	-0.168
	(0.040)	(0.075)	P=0.081		(0.047)	(0.092)	P=0.201
Treat	-0.044	-0.237***		Treat	-0.038	-0.153	
	(0.030)	(0.074)			(0.053)	(0.113)	
Post	-0.071***	-0.181***		Post	-0.041	-0.119	
	(0.019)	(0.057)			(0.037)	(0.081)	
Control variables	Yes	Yes		Control variables	Yes	Yes	
Year fixed effects	Yes	Yes		Year fixed effects	Yes	Yes	
State fixed effects	Yes	Yes		State fixed effects	Yes	Yes	
No. of observations	21,231	16,464		No. of observations	20,424	17,271	
Adjusted R-squared	0.213	0.198		Adjusted R-squared	0.197	0.188	

Table 9 Access to commercial banks

This table reports the OLS estimation results of $Crime_{it} = \alpha_i + \alpha_t + \beta_1 Treat_i \times Post_t + \beta_2 Treat_i + \beta_3 Post_t + \gamma X_{it} + \varepsilon_{it}$. For subsample split by the state-median of no. of bank branches. The dependent variable in Columns (1) and (2) is Ln(property crimes. Treat equals one if the agency is a treated state, and zero otherwise. Post equals one if the agency-year observation is after the payday lending adoption. All regressions include year effects and state (agency) fixed effects. Standard errors are clustered by state. Significance at 1%, 5%, and 10% levels are indicated by ***, **, and *, respectively.

	Ln(Property crime)	Ln(Property crime)	Diff in (1)
Bank branches	Low	High	
	(1)	(2)	(3)
Treat×Post	0.126**	0.115	0.014
	(0.054)	(0.070)	P=0.129
Treat	-0.147***	-0.130***	
	(0.038)	(0.046)	
Post	-0.108***	-0.088*	
	(0.037)	(0.047)	
Control variables	Yes	Yes	
Year fixed effects	Yes	Yes	
State fixed effects	Yes	Yes	
No. of observations	14,799	22,896	
Adjusted R-squared	0.234	0.201	

Table 10 Minority population

This table reports the OLS estimation results of $Crime_{it} = \alpha_i + \alpha_t + \beta_1 \times Treat_i \times Post_t + \beta_2 \times Treat_i + \beta_3 \times Post_t + \gamma \times X_{it} + \varepsilon_{it}$. for subsample split by the state-median of the proportion of minority populations. The dependent variable in Columns (1) and (2) is Ln(property crimes. Treat equals one if the agency is a treated state, and zero otherwise. Post equals one if the agency-year observation is after the payday lending adoption. All regressions include year effects and state (agency) fixed effects. Standard errors are clustered by state. Significance at 1%, 5%, and 10% levels are indicated by ***, **, and *, respectively.

Panel A.	Ln(Property crime)	Ln(Property crime)	Diff in (1)	Panel B.	Ln(Property crime)	Ln(Property crime)	Diff in (1)
African American	High	Low		Native American	High	Low	
	(1)	(2)	(3)		(1)	(2)	(3)
Treat×Post	0.146***	0.097*	0.049*	Treat×Post	0.089	0.095*	-0.006
	(0.048)	(0.051)	P=0.086		(0.062)	(0.052)	P=0.471
Treat	-0.171***	-0.124***		Treat	-0.103**	-0.084*	
	(0.030)	(0.034)			(0.044)	(0.049)	
Post	-0.101***	-0.099**		Post	-0.053	-0.065**	
	(0.029)	(0.038)			(0.041)	(0.032)	
Control variables	Yes	Yes		Control variables	Yes	Yes	
Year fixed effects	Yes	Yes		Year fixed effects	Yes	Yes	
State fixed effects	Yes	Yes		State fixed effects	Yes	Yes	
No. of observations	19,839	17,856		No. of observations	17,943	20,816	
Adjusted R-squared	0.183	0.245		Adjusted R-squared	0.261	0.184	

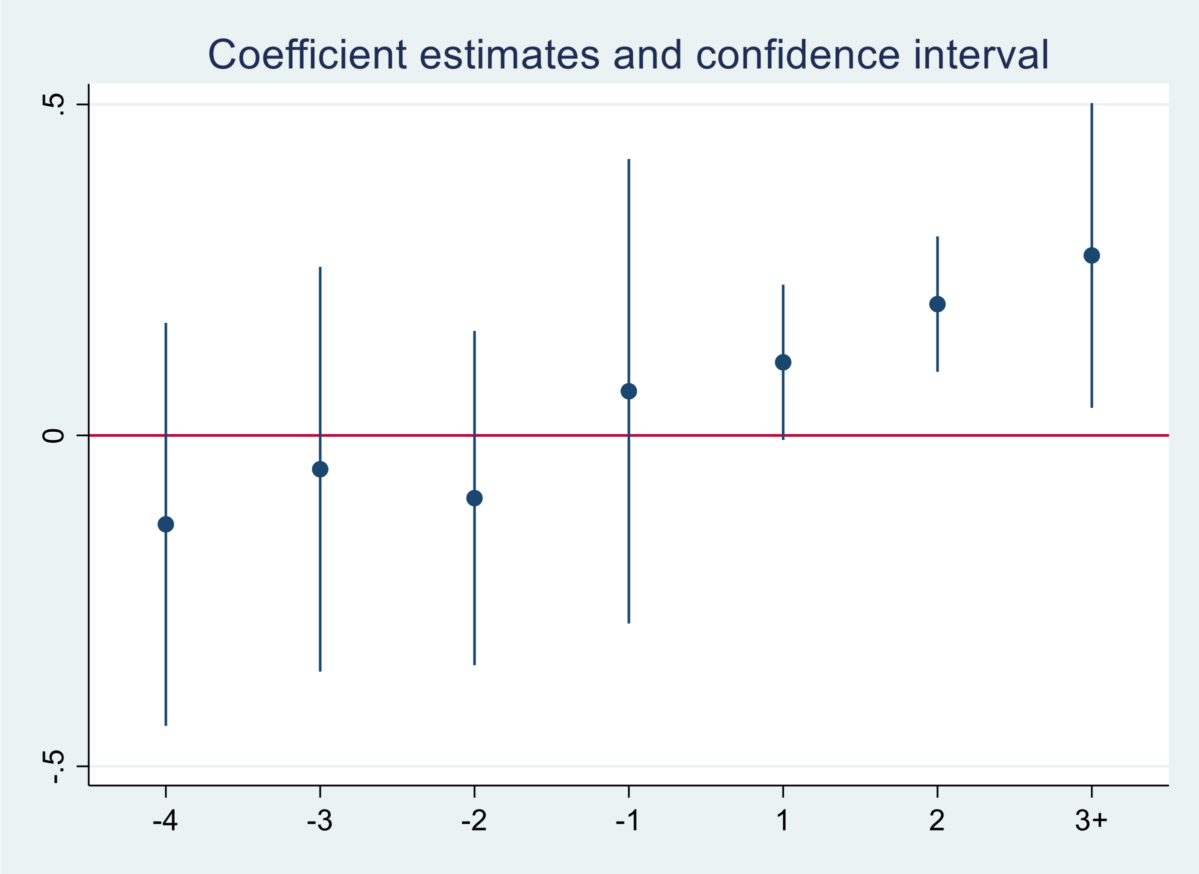
Panel C.	Ln(Property crime)	Ln(Property crime)	Diff in (1)	Panel D.	Ln(Property crime)	Ln(Property crime)	Diff in (1)
Asian American	High	Low		Latino American	High	Low	
	(1)	(2)	(3)		(1)	(2)	(3)
Treat×Post	0.03	0.158**	-0.128**	Treat×Post	0.118*	0.093*	0.025
	(0.044)	(0.062)	P=0.047		(0.063)	(0.050)	P=0.243
Treat	-0.057	-0.163***		Treat	-0.150***	-0.112***	
	(0.039)	(0.054)			(0.041)	(0.030)	
Post	-0.027	-0.102**		Post	-0.078*	-0.085**	
	(0.031)	(0.038)			(0.043)	(0.033)	
Control variables	Yes	Yes		Control variables	Yes	Yes	
Year fixed effects	Yes	Yes		Year fixed effects	Yes	Yes	
State fixed effects	Yes	Yes		State fixed effects	Yes	Yes	
No. of observations	20,788	17,971		No. of observations	16,956	20,739	
Adjusted R-squared	0.223	0.211		Adjusted R-squared	0.168	0.272	

Figure 1 – Equation (3)

Figure 1 plots the coefficient estimates and their 95% confidence intervals of $Treat \times Year_k$ for equation (3). $Property\ crime_{it} = \alpha_i + \alpha_t + \alpha_j + \sum_{k=-3}^{k=3} \beta_k \times Treat \times Year_k + \gamma \times X_{it} + \varepsilon_{it}$ (3), The dependent variable is *Ln (No. of property crimes)*. all variables are defined the same as those in equation (2), except for *Year*, which equals one if the $Year_k$ is k years after the adoption year, and zero otherwise. All regressions include year effects and state (agency) fixed effects. Standard errors are clustered by state. Significance at 1%, 5%, and 10% levels are indicated by ***, **, and *, respectively.

Figure 2 - Equation (4)

Figure 2 plots the dynamic coefficient estimates and their 95% confidence intervals of *Access_0_30*. The dependent variable is *Ln (No. of property crimes)*. All variables are defined the same as those in equation (2). All regressions include State×year fixed effects. Standard errors are clustered by state. Significance at 1%, 5%, and 10% levels are indicated by ***, **, and *, respectively.



CHAPTER 2: MANAGING CLIMATE CHANGE RISKS: SEA LEVEL RISE AND MERGERS AND ACQUISITIONS

1. Introduction

"*The evidence on climate risk is compelling [for] investors to reassess core assumptions about modern finance*"

-Laurence D. Fink

(Founder and Chief Executive Officer of BlackRock)

The last four decades have witnessed the most significant climate changes as well as the ever-increasing awareness of the severe consequences that such changes bring about. However, according to the Wall Street Journal, most companies "are underestimating how climate-related risks, such as extreme weather and changing consumer views on environmental issues, could affect their companies' bottom lines, and they need to make climate risk assessments a bigger priority".[8] In this paper, we study how companies engage in mergers and acquisitions to manage and diversify away one important long-run climate risk: risks associated with sea-level rise (SLR).

We focus on SLR for two reasons: First, sea level rise and its acceleration are among the most severe impacts of climate change and global warming. The scientific community has reached a consensus that SLR is a serious environmental risk that will disrupt household and business activities in the long run. It is estimated that a 1.8 meter (i.e., roughly 6 feet) SLR would make areas currently populated by 6 million Americans uninhabitable (Hauer et al., 2016). Businesses with commercial properties or operations in low-lying coastal areas may find it

[8] https://www.wsj.com/articles/cfos-are-underestimating-the-financial-risks-of-climate-change-executives-say-11560276836

increasingly difficult to ensure their assets, making SLR a relevant long-term business risk.[9] Second, accurate forecasting of future sea-level rise is challenging, which makes studying how firms manage such a significant yet uncertain risk particularly urgent and time relevant.

We posit that firms exposed to significant SLR risk diversify away from such risks by acquiring other firms that are unlikely to be affected by SLR and that such diversifying actions are rewarded by the market. We also conjecture that post-merger, acquiring firms that are ex-ante subject to SLR risks experience an improvement in their information environment because diversifying away the SLR risk removes an important source of forecast uncertainty. Also, we expect that for these acquisitions, the combined firms' Environmental, Social, and Corporate Governance (ESG) score should improve post-merger.

We test our hypotheses using a multi-step approach. Our first set of analyses investigates how exposure to SLR risk affects the likelihood of mergers. Because SLR represents an uncertain yet significant long-term operational risk, we hypothesize that firms exposed to SLR are more (less) likely to become acquirers (targets) in a merger deal. We find evidence consistent with this hypothesis: relative to a group of potential acquirers (targets) in the same industry and similar in size (as well as a book-to-market ratio), firms more exposed to SLR is significantly more (less) likely to be an acquirer (target) in a merger deal.

We next turn to examine how the market reacts to acquisition announcements made by the acquiring firms that are exposed to different levels of SLR risk. Consistent with the notion that the market rewards firms for diversifying away their SLR risk, we find that acquirers exposed to SLR risk experience significantly higher announcement-period abnormal stock returns. The results hold after controlling for a large number of the firm as well as merger deal

[9] https://www.theguardian.com/sustainable-business/2016/mar/18/sea-level-rises-flooding-business-household-risk-uk

characteristics. Importantly, we find that the positive announcement effect concentrates on deals in which the target firms are not exposed to SLR risk, suggesting it is driven by the diversification of SLR risk. Furthermore, we also find that the positive announcement return is more pronounced for firms with more analyst coverage.

Our final set of tests focuses on two firm outcomes after the merger. In the first test, we examine whether firms' information environment improves after a high-SLR risk firm acquires another firm. SLR risk poses a significant challenge and long-run risk for firms' operational decisions, which likely creates information uncertainty. And the risk is lowered and diversified away after an SLR-affected firm acquires another firm, and hence the information uncertainty should decrease after the merger. Consistent with this conjecture, we find that post-acquisition, analyst coverage, as measured by the number of analysts, increases, and analyst forecast dispersion, as measured by the range and standard deviation of analysts' forecast, declines. Our second test examines the ESG score (both the individual component related to the environment and the overall score) post-merger. The idea is that if a merger helps a firm reduce its SLR risk exposure, its ESG score, which assesses its relative performance, commitment, and effectiveness in areas including carbon emissions and environmental, should increase after the merger. We find results broadly consistent with this conjecture.

Our study makes several contributions to the literature. First, this paper expands our understanding of how environmental and climate change risks influence various market participants and underlying assets. Prior studies find that institutional investors consider climate risk as an important source of risk for their portfolios (Krueger, Sautner, and Starks, 2018). For instance, mutual fund investors gravitate towards funds with favorable (low) carbon designation and funds cater to clients by tilting their portfolios towards low fossil fuel and low carbon risk holdings (Bolton and Kacperczyk, 2020). Besides, investors pay a premium to green bonds that use the proceeds for environmental purposes (Baker, Bergstresser, Serafeim, and Wurgler, 2018), and the bond markets start to price SLR risks as early as 2011 (Goldsmith-Pinkham et al., 2020). In the real estate market, however, the evidence is somewhat mixed: while some studies find that real estate prices are heavily influenced (Bernstein, Gustafson, and Lewis, 2019), others find that there is minimal price impact (Murfin and Spiegel, 2020). Acemoglu, Akcigit, Hanley, and Kerr (2016) take a unique approach to model the competing choices that firms face between clean and dirty technologies, and provide empirical evidence that such choices are largely influenced by taxes and subsidies. Bansal, Kiku, and Ochoa (2016) find that equity portfolios with high exposure to climate risk carry a positive risk premium. Adopting a more quantitative approach, Giglio, Maggiori, Rao, Stroebel, and Weber (2018) estimate long-run discount rates for valuing investments in climate-change abatement, while Barnett, Brock, and Hansen (2020) highlight the challenges of modeling climate-change risk due to uncertainty. On the corporate side, Jiang, Li, and Qian (2020) find firms' cost of long-term loans increases with SLR risk. Our study contributes to this literature by providing direct evidence on how firms respond to SLR risk in the market for corporate control.

Second, our study contributes to a large literature on mergers and acquisitions. Empirical studies on mergers and acquisitions largely focus on either the determinants of mergers or the sources of synergistic gains. While many factors such as stock overvaluation (e.g., Shleifer and Vishny, 2003; Rhodes-Kropf and Viswanathan, 2004), economic, regulatory, and technological shocks (e.g., Harford, 2005; Mitchell and Mulherin, 1996) lead to merger waves, mergers' synergistic gains range from better resource allocation and product differentiation (e.g., Lichtenberg and Siegel, 1987; Healy, Palepu, and Ruback, 1992; McGuckin and Nguyen, 1995;

Maksimovic and Phillips, 2001; Schoar, 2002; Hoberg and Phillips, 2010; Maksimovic, Phillips, and Prabhala, 2011; Li, 2013), interest tax shields (Devos, Kadapakkam, and Krishnamurthy, 2009; Fee, Hadlock, and Pierce, 2012), improvements in product quality (Sheen, 2014), to improvements in structured management practices (Bai, Jin, and Serfling, 2021). This paper contributes to this literature by providing systematic evidence that SLR risks are a significant factor that affects the merger likelihood and that markets value mergers that diversify away from such long-run risks.

The remainder of the paper proceeds as follows. Section 2 develops the testable hypotheses. Section 3 discusses the data. We present our empirical findings in Section 4 and conclude in Section 5.

2. Hypothesis Development

2.1 Merger Likelihood

Why would the likelihood of mergers be correlated with environmental risks? On one hand, previous research finds that mergers are clustered by industries and by time and are often motivated by economic, regulatory, and technological shocks (Harford, 2005). Because climate changes are gradual and slow, it is reasonable to expect merger decisions to be unrelated to environmental risks. On the other hand, environmental risks pose a unique challenge for today's companies. "Investors, analysts, research firms, and companies are putting more emphasis on how climate issues ranging from rising sea levels to record heatwaves will affect profits and revenues in the United States and what companies are doing to address those risks."[10] In the context of managing risks associated with rising sea levels, one immediately effective method is to acquire businesses in geographical locations that are not affected by such environmental risks. At the same time, it is expected that businesses located in areas severely impacted by rising sea

[10] https://www.insurancejournal.com/news/national/2019/11/15/548563.htm

levels are very difficult to be sold in the market for corporate assets, as these environmental risks are difficult to diversify away and quite salient for any potential acquirers. As a result of these considerations, we expect the merger likely to be correlated with the risks of sea-level rise. We summarize the above arguments in their null and alternative forms in the following hypotheses:

H1: The likelihood of a firm becoming an acquirer (target) in a merger deal is not correlated with the firm's exposure to the risk of sea-level rise.

H1a: The likelihood of a firm becoming an acquirer (target) in a merger deal is positively (negatively) correlated with the firm's exposure to the risk of sea-level rise.

H1b: Firms subject to SLR risk are more likely to acquire firms subject to no SLR risk.

2.2 Cumulative Abnormal Returns (CAR) around Merger Announcements

Following a similar logic, if environmental risks are slow-moving and do not affect firms' business strategies or day-to-day operations, investors may not reward acquisitions that diversify away the exposure to these risks. On the other hand, if climate changes associated with sea level rises do pose serious operational and business risks, one should expect the market to view acquisitions that diversify such risks as value improvement. We test these competing predictions by investigating abnormal cumulative returns (CAR) around merger announcements. Below, we summarize these predictions in our second set of hypotheses:

H2: The cumulative abnormal return of the acquiring firms around merger announcements is not correlated with the exposure to risks associated with the sea level rise.

H2a: The cumulative abnormal return of the acquiring firms around merger announcements is positively correlated with the exposure to risks associated with the sea level rise.

2.3 Merger Duration

Mergers and acquisitions are important corporate actions that often involve approvals from not only shareholders of the acquiring and the target firms, but also various regulatory agencies. Because of such complexities, not all mergers are completed in the end and many fail during the process. These complexities range from access to financing to regulatory approvals. To the extent that mergers motivated by SLR risk help reduce the combined firms' environmental risk exposure and operational uncertainty, these mergers could have a better chance of receiving approval from various parties involved.

Consistent with this conjecture, Li, Xu, Mclver, Wu, and Pan (2020) find that heavy polluters' green M&A is associated with greater access to resources and reduced financing constraints and tax liabilities. We posit that mergers motivated by SLR risk are shorter in duration, as measured by the length of time between the merger announcement date and merger completion date. We summarize this prediction in its null and alternative forms below:

H3: The duration of the merger is not related to the acquirer's SLR risk exposure pre-merger

H3a: The duration of the merger is negatively related to the acquirer's SLR risk exposure pre-merger.

2.4 Post Merger Analyst Coverage

Existing evidence on how analyst forecasts change surrounding a merger is mixed. Consistent with the notion that mergers often lead to more complex business entities, Haw, Jung, and Ruland (1994) find that forecast accuracy decreases sharply after mergers. Wu and Zang (2009) find that analysts with good earnings forecast performance experience higher turnover during mergers. However, Tehranian, Zhao, and Zhu (2013) find that analyst covering the target company before the merger provides more accurate earnings forecasts and more optimistic stock recommendations and growth forecasts for the merged firms than do the remaining acquirer analysts.

If SLR poses a significant yet unforeseeable long-run risk for firms' operations before the mergers, such a risk should be alleviated after the merger, making it easier for analysts to analyze such companies' operations and forecast their performance and value. This reduction in business risk should be particularly pronounced for firms with high SLR-risk exposure pre-merger.

Therefore, we expect the number of analysts covering the firm to increase, meanwhile the range and standard deviation of forecasts to decline post-merger. The null hypothesis is that the change in firms' information environment does not depend on the acquiring firm's pre-merger SLR risk exposure. These lead to our next hypothesis:

H4: Post-merger, the change in the information environment of the combined firm does not depend on firms' pre-merger SLR risk exposure.

H4a: Post-merger, the information environment of the combined firm improves significantly more for firms with high SLR risk exposure before the merger, as measured by greater analyst coverage, smaller range, and lower standard deviation of analyst forecast.

2.5 Post Merger ESG Score

To the extent that SLR risks are long-term, uncertain, but rather salient, we also expect firms' environmental, social, and governance (ESG) scores to improve after the completion of mergers that help reduce firms' SLR risk. This leads to our final hypothesis, stated in its null and alternative form:

H5a: Post-merger, the ESG score of the combined firm improves does not change significantly for firms with high SLR risk exposure before the merger.

H5a: Post-merger, the ESG score of the combined firm improves significantly more for firms with high SLR risk exposure before the merger.

3. Data, Sample Selection, and Empirical Methodology

3.1 Sample Construction

We obtain data on mergers and acquisitions (M&A) deals announced between 1986 and 2017 from the Thomson Reuters Financial Securities Data Company (SDC) database. [114] We end our sample in 2017 because we require a minimum of three years' data to investigate the long-term performance after the merger. We consider only the deals in which the acquiring firms end up with 100% of the shares of the target firms or subsidiaries after the completion of the deal. Also, we require that the acquiring firms control less than 50% of the shares of the target firms before the deal announcement. Some further filtering criteria include: (1) the transaction is completed with a deal value larger than $1 million; (2) neither the acquirer nor the target firm is from the regulated sector (SIC codes 4900-4999) or the financial sector (SIC 6000–6999); (3) a public or private U.S. firm or a non-public subsidiary of a public or private firm is acquired; and (4) the acquirer is covered by Compustat/CRSP. These filters yield 23,827 deals where financial information on acquirers is available and 3,052 deals where financial information on both acquirers and target firms is available. For our post-merger analyses, we supplement this data set with information on analyst coverage from Institutional Brokers' Estimate System (I/B/E/S) and data on corporate environmental, social, and governance scores from the MSCI ESG database.

To build our sample for merger likelihood, we follow Bena and Li (2014) and construct two matched samples: the industry and size-matched sample and the industry, size, and M/B

[4] Our sample period begins in 1986 because information on M&As in SDC is less reliable before mid-1980s.

ratio matched sample. To construct the first sample, for each acquirer (target firm) of a deal announced in year t, we find up to five matching acquirers (matching target firms) by industry—where the industry definitions are based on the narrowest SIC grouping that includes at least five firms—and by size from Compustat/CRSP in year t − 1 that are neither an acquirer nor a target firm in the three years before the deal. [5] The purpose of this matching process is to capture the clustering of merger activities not only in time, but also by the industry (Andrade, Mitchell, and Stafford, 2001, Harford, 2005).

To construct the second matched sample, for each acquirer (target) of a deal in year t, we find up to five matching acquirers (matching targets)—first matched by industry, and then matched on the propensity scores estimated using the size and M/B ratios—from Compustat in year $t-1$ that is neither an acquirer nor a target firm in the three years before the deal. We add the M/B ratio to our matching characteristics because the literature argues that it captures growth opportunities (Andrade, Mitchell, and Stafford, 2001), and overvaluation (Shleifer and Vishny, 2003, Rhodes-Kropf and Viswanathan, 2004), and asset complementarity (Rhodes-Kropf and Robinson, 2008).

3.2 Firm Exposure to the Risk of Rising Sea Lines

We follow Bernstein et al. (2019) to determine whether a firm's headquarters would be inundated given the projected sea line using the NOAA (National Oceanic and Atmospheric Administration) SLR database (Marcy et al., 2011). Specifically, the NOAA hosts shapefiles that provide the latitudes and longitudes of polygon vertices that will be inundated following a 0-10 foot increase in the local sea line, where the 0-foot map pinpoints the current shoreline based

[5] Our sample size is relatively smaller than the number of deals multiplied by six (23,827×6=142,962) because for some deals, we cannot find up to five matching acquirers.

on the level of local mean higher high water.[6] We then calculate the shortest distance between the zip code of a firm's headquarters and the polygons of the projected sea line; and then construct a binary variable, *Inundated6ft*, that takes the value of one if six feet or less of SLR would flood the firm's headquarters (i.e., the minimum distance is 0) and zero otherwise (i.e., the minimum distance is greater than 0).[7] In a robustness check, we also consider *Inundated3(10)ft*, which takes the value of one if the firm's headquarters would be submerged by three (ten) feet or less of SLR and zero otherwise.

3.3 Dependent and Other Control Variables

Our analyses consist of three main tests: merger likelihood, short-term market response to the merger, and long-term firm performance post-merger tests. For our first set of analyses, our main dependent variable, *Event Firm*$_{im,t}$ is an indicator variable that equals one if firm *i* is the *actual* acquirer (target) in deal *m*, and zero otherwise. For tests on short-term market reactions, our main dependent variable is the acquirer stock's cumulative abnormal return (CAR) around the merger announcement date over various estimation windows. Finally, to examine the long-term firm performance, we use several variables related to analysts' forecasts and the corporate ESG scores as the main dependent variables to gauge changes in the operating efficiency and sustainable growth of the combined firm.

Throughout our analyses, we control for a host of observable firm characteristics for both the acquiring firms and the target firms, including *Firm size*, *Market-to-book* (M/B), *Leverage*, *Dividend payer*, *Ln(Total Sales)*, *ROA*, *R&D*, *Capex*, *Quick ratio*, *Non-debt tax shield*, and *Cash*

[6] The dataset is publically available at https://coast.noaa.gov/slrdata/. The original data is an ArcGIS geodatabase, and we convert it to shapefiles that retain SLR layers.
[7] The zip codes of firms' headquarters are extracted from Professor Bill McDonald's augmented 10-X header data at https://sraf.nd.edu/data/augmented-10-x-header-data/. The US zip code latitude and longitude information is obtained from https://public.opendatasoft.com/explore/dataset/us-zip-code-latitude-and-longitude/information/.

flow volatility. We also include merger deal characteristics such as deal value (*Logarithm of Deal value)*, whether it is a diversifying merger *(Diversify deal)*, and method of payment *(All cash deal)* in the market reaction section. Also, to rule out the possibility that the results are driven by the differences between the firms located near the coast and the firms located far away from the coast, we include a dummy variable *Coast*, which equals one if the firm's headquarters is within 50 miles range of the coast. All continuous variables are winsorized at the 1% and 99% levels. Detailed definitions are provided in the appendix.

3.4 Descriptive Statistics

Table 1 presents descriptive statistics for the industry and size-matched sample, and the industry, size, & M/B ratio matched sample. We report statistics for acquirers (real and matched) in panels A and B. We report statistics for targets (real and matched) in panels C and D. We find that, in general, acquirer firms are more exposed to the SLR risk than target firms. For example, the mean of *Inundated6ft* is 0.074 and 0.072 for acquirer firms. The mean of *Inundated6ft* is 0.05 and 0.037 for target firms. This result suggests a positive/negative correlation between the SLR risk and the probability of being an acquirer/target. Also, acquirer firms tend to have higher total assets, leverage, total sales, and return on assets (ROA) than target firms. The acquiring firms have lower Market-to-book (M/B), R&D, capital expenditure (scales on sales), non-debt tax shield, quick ratio, and cash flow volatility than the target firms. Overall, our samples are similar to those used in other studies such as Bena and Li (2014), Moeller, Schlingemann, and Stulz (2004), and Lee, Mauer, and Xu (2018).[8]

[8] We also conduct the test for the differences of firm characteristics between real acquirers (targers) and matched acquirers (targers). We find that for the targets, the differences are neither statistically nor economically significant. For the acquires, some firm characteristics have significantly differences because the power of the test (our sample size for acquires is 100,364 and 83,293), but we do not observe economic significance.

3.5 Empirical Methodology

Our empirical exercise proceeds in three steps. First, we test whether exposure to SLR risk affects the probability of a firm becoming an acquirer (a target). To this end, we run conditional logistic, logistic, and ordinary least squares (OLS) regressions. Specifically, we estimate the following specification:

$$Event\ Firm_{im,t=}a + \beta_1 SLR\ Risk_{im,t-1} + \beta_2 \boldsymbol{X}_{i,t-1} + \rho_{t\times s} + \psi_k(\text{or } \nu_m) + e_{im,t}, \quad (1)$$

where *i, m, k, t,* and *s* index firm, deal, industry, year, and state respectively. $Event\ Firm_{im,t}$ equals one if firm *i* is the acquirer (target) in deal *m*, and zero otherwise. The key independent variable $SLR\ Risk_{im,t-1}$ is a dummy variable equal to one if firm i's headquarters would be inundated if sea-level rises by 6 feet, and zero otherwise. We include the following firm-level characteristics ($\boldsymbol{X}_{i,t\text{-}1}$) measured in year *t-1* to account for firms' observable characteristics on profitability, financial position, and other attributes: *Firm Size*, *Market-to-book* (M/B), *Leverage*, *Dividend payer*, *Ln (Total Sales)*, *ROA*, *R&D*, *Capex*, *Quick Ratio*, *Non-debt Tax Shield*, and *Cash Flow Volatility*. We include industry fixed effects to account for time-invariant industry characteristics. In some of our tests, we also include deal-fixed effects to control for any time-invariant differences among deals. State×Year fixed effects are also included to control for transitory statewide factors such as state-level economic conditions. In some tests, we cluster standard errors by the zip code level to account for serial correlation within the zip code over time. (Bertrand, Duflo, and Mullainathan, 2004; Petersen, 2009). In other tests, we also cluster the standard errors at the deal level (Bena and Li, 2014). All detailed information about the cluster and fixed effects are listed at the bottom of each table[12].

[12] We cluster the standard errors for a robustedness check in untabluted results. The results are consistent and available upon the request.

Our second set of analyses studies whether acquirers' cumulative abnormal return (CAR) around acquisition announcements is correlated with the SLR exposure of the acquiring firms. Specifically, we estimate the following cross-sectional regression:

$$Acquirer\ CAR_{im,t} = a + \beta_1 SLR\ Risk_{im,t-1} + \beta_2 \boldsymbol{X}_{i,t-1} + \beta_2 Deal\ Character_{m,t-1} + \rho_{t\times s} + \psi_k + e_{im,t}, \tag{2}$$

where the dependent variable is the acquirer CAR around different windows surrounding the merger announcements. All the other variables are defined analogously to Equation (1).

Our third set of analyses investigates how various firm-level outcomes change in the aftermath of acquisitions. We focus on two aspects of the combined firm: analyst forecasts and environment-related scores. We estimate the following regression models:

$$Firm\ Outcome_{i,t} = a + \beta_1 Post - Merger_{i,t} + \beta_2 Post - Merger_{i,t} \times SLR\ Risk_i + \beta_3 \boldsymbol{X}_{i,t-1} + \rho_t + \psi_i + \nu_s + e_{im,t}, \tag{3}$$

where the dependent variables are various firm-level outcomes. $Post - Merger_{im,t-1}$ is an indicator variable equal to one after the merger, and zero otherwise. $SLR\ Risk_i$ is the SLR risk firm i is exposed to. Note that this is time-invariant and firm-specific, so the main effect on $SLR\ Risk$ is absorbed by the firm fixed effects. All the other variables are defined analogously to Equation (1).

4. Empirical Results

Our empirical results proceed in three steps: In Section 4.1, we examine the relation between SLR risk and merger likelihood. Section 4.2 studies acquiring firms' announcement returns and how these market reactions are associated with firms' ex-ante SLR exposure. Finally, we investigate firm outcomes post-merger that are related to the information environment and firms' overall ESG ratings in Section 4.3.

4.1 SLR and Merger Probability

4.1.1 SLR and Acquirer Probability

We test our first set of hypotheses (i.e., H1 and H1a) by investigating whether SLR is an important determinant of a firm engaging in a merger. If SLR risks are significant business risks that firms attempt to diversify away through acquisitions, we expect the SLR risk to be positively correlated with a firm's likelihood of becoming an acquirer, and negatively correlated with a firm's likelihood of being a target firm. To operationalize these tests, we estimate Equation (1) on the two matched samples: the industry and size-matched and the industry, size, and M/B ratio matched samples.

We present the results of these exercises in Table 2. Panels A and B present the results based on the industry and size-matched sample and industry, size, and M/B ratio matched samples, respectively. Columns 1-3 of Panel A present the coefficient estimates from the conditional logit and logit models, while columns 4-7 display the results using the OLS regression model. Overall, throughout various empirical specifications, we find that the coefficient estimates on SLR risk (*Inundated6ft*) are positive and statistically significant, suggesting that firms with higher SLR risk are more likely to become an acquirer, even after controlling for a variety of firm characteristics. In terms of economic significance, column 1 predicts that if the firm is subject to the inundation risk, then the firm is 4.1% more likely to become an acquirer than the firm not subject to the inundation risk. Although the magnitudes become smaller in columns 4-7 when the linear probability model (i.e., OLS) is employed, the positive relation between SLR risk and the probability of becoming an acquirer stays positive and economically meaningful.

Panel B of Table 2 reports the results on the industry, size, and M/B ratio matched sample. The findings are broadly consistent with those in Panel A. The coefficient estimates, as well as the economic significance, become larger, which is most likely due to the better-matched sample that ensures that the control firms and treated firms are comparable across more dimensions. For instance, column 1 predicts that if the firm is subject to the inundation risk, the firm is 13.1% more likely to become an acquirer than the firm not subject to the inundation risk. Our results are robust to further controlling for the county fixed effects that absorb the impact of within-county time-invariant variables (see Online Appendix, Table A.1).

4.1.2 SLR and Target Probability

To examine whether SLR is correlated with firms' propensity to become a target in a merger deal, we estimate a similar set of models to examine the likelihood of any given firm becoming a target and present these results in Table 3. Similar to Table 2, we present the results estimated on the two matched samples in Panels A and B separately. Overall, the results show a significant negative relationship between firms' SLR risk as proxied by *Inundated6ft* and their probability of becoming a target. For instance, column 1 of Panel A shows a negative and statistically significant coefficient of -0.205 on *Inundated6ft*. In terms of economic significance, if the firm is subject to the inundation risk, then the firm is 4.7% less likely to become a target than the firm subject to no inundation risk. Panel B of Table 3 repeats these exercises on the industry, size, and M/B matched sample and shows an overall similar pattern as in Panel A. in terms of economic significance, if the firm is subject to the inundation risk, then the firm is 5.3% less likely to become a target than the firm subject to no inundation risk. Once again, our findings are robust to further controlling for the county fixed effects that absorb the impact of within-county time-invariant variables. We present these additional results in Table A.2.

Taken together, the results in Tables 2 and 3 show that risks associated with sea-level rise (SLR) are strongly correlated with firms' probability to be an acquirer or a target in a merger deal. In particular, the evidence is consistent with the risk diversification interpretation: firms exposed to high levels of SLR risk tend to become acquirers, which allows them to diversify such environmental risks away through buying other firms. Similarly, firms exposed to high levels of SLR risk tend to have a lower probability of becoming target firms in a merger deal, which is consistent with the notion that SLR risk poses additional uncertainties that make a firm less attractive as a target in an acquisition.

4.1.3 SLR and the Merger Pair

We next refine our analyses and examine whether high-SLR-risk acquirers are more likely to buy low-SLR-risk firms. We employ the same matching methodology in earlier sections and for each acquirer (target), we find up to five control acquirers and targets. Next, we follow Bena and Li (2014) to conduct the following test

$$Merger\ pair_{i,jm,t=}a + \beta_1 SLR\ Risk_{i,jm,t-1} + \beta_2 \boldsymbol{X}_{i,j,t-1} + \rho_{t\times s} + \psi_k(\text{or } \nu_m) + e_{i,jm,t}, \quad (4),$$

Where $Merger\ pair_{i,jm,t}$ equals one if the matching pair is the real merger deal, and zero otherwise. $SLR\ Risk_{i,jm,t-1}$ equals one if the acquirer/target is subject to SLR risk/no SLR risk. We include the firm characteristics of both the acquirers and the targets. We also control for a variety of fixed effects. The results are reported in Table 4.

Note that the sample size of this table drops from that of table 2. The reason is that we only include deals that contain both public acquirers and public targets (for matching purposes). Using various empirical specifications, we find that the coefficient estimates on $SLR\ Risk_{i,jm,t-1}$ are positive and statistically significant, suggesting that firms with SLR risk are indeed more likely to acquire firms subject to no SLR risk, even after controlling for a variety of firm characteristics of both the acquirers and the targets. The estimate in column 1 of panel A suggests that if the firm is subject to the inundation risk, the firm is 4.0% more likely to acquire a firm subject to no inundation risk. Although the magnitudes become smaller in columns 4-7 when the linear probability model (i.e., OLS) is employed, the positive relationship between the $SLR\ Risk_{i,jm,t-1}$ and the probability of a merger stays positive and economically meaningful. In panel B, we report the results of the same test by using the industry, size, and M/B matched sample. The results are similar to those in panel A[13].

4.2 What Are the Market Reactions to the Mergers?

In this section, we investigate how the market responds to merger announcements made by acquirers with high exposure to SLR before the merger. Cumulative abnormal returns (CAR) around the merger announcement periods provide a clean estimation of the market's reception of the news announcement and the underlying wealth effects (Li and Prabhala, 2007). If SLR indeed poses a significant business risk, we expect the market to react positively to acquisition announcements that reduce such risks.

To test this conjecture, we estimate Equation (2) in which the dependent variable is the acquirer announcement-period cumulative abnormal return around various windows, and the main independent variable is the acquirer's SLR risk. We focus on several event windows starting from three days before the acquisition announcement to three days after. The longest window of examination is (-3, +3) while the shortest window is (-1, +1). To estimate the

[13] We conduct the sub-sample tests for the effect of SLR risk on the probability of merger. We break the sample into two parts (1986-2006 & 2007-2018). We believe that since the realease of the documentary "An Inconvenient Truth" in 2006, people are focusing more on the climate change issue. We find that the effects are stronger in the sub-sample from 2007 to 2018. The results are avaible up request.

cumulative announcement return, we first use the Fama-French (1993) three factors model and daily stock returns in the estimation window of (−255, −46) to estimate the factor loadings, which are then applied to returns during the event window to estimate the announcement CARs. Because the stock return analyses use the sample of actual acquisition announcements, we have a different sample size than the one used in the analysis of the probability of a merger. We present the basic summary statistics on the acquirer announcement-period CAR around different event windows in Table 5. The mean value for the CARs across all event windows is slightly above 1%, with a significant cross-sectional variation. The distribution of the CARs is heavily skewed to the right, resulting in a significantly higher mean value compared with the median.

When estimating the model presented in Equation (2), we control for a variety of deal-level characteristics and acquirer characteristics. The deal characteristics include the size of the deal (*Ln(Deal value)*), method of payment (*All-Cash Deal*) whether the target is public (*Tpublic*), and whether the acquirer and the target are in the same industry (*Diversify Deal*). We include the same set of acquirer characteristics as in Equation (1). The key parameter of interest is $\beta_1$in Equation (2), which captures the short-term market reaction to the acquirer's exposure to SLR risk.

The results are presented in Table 6. We find that the coefficient estimates of *Inundated6ft* are positive and statistically significant in columns 1-9. The coefficient estimate on *Inundated6ft* in column 1 suggests that acquirers whose headquarters would be inundated if sea-level rise by 6 feet has 0.531% higher CARs than other acquirers over a two-day window around the announcement. Also, the coefficient estimates on *Inundated6ft* increase with the days of the event window of the CARs. Overall, this evidence is consistent with H2a, suggesting that the market rewards acquirers that diversify away from their SLR risk through acquisitions.

Moreover, in Table A.3, we consider the target SLR risk exposure and re-estimate Table 6 by further controlling for *(Inundated6ft × Target's Inundated6ft),* the interaction of the acquirer the target firms' inundation dummies. The coefficient of this interaction term is consistently negative and statistically significant in several specifications, suggesting the positive market response to merger announcements is muted when the target firm also has considerable exposure to SLR risk.

4.2.1 CAR, SLR, and Analyst Coverage

Our results so far indicate that SLR is an important form of environmental risk that affects the market's assessment of a merger deal. We expect such a relationship to be more pronounced for firms with greater information transparency, thus making the SLR risk more salient and in a way, "under the spotlight".

The use of analyst coverage to capture information transparency is ubiquitous in prior studies. For instance, Sibilkov, Straska, and Waller (2013) find that acquirers in M&A transactions are more likely to hire investment banks that provide analyst coverage for the acquirer before the transaction. Kolasinski and Kothari (2008) find that analysts affiliated with acquirer advisors upgrade acquirer stocks around M&A deals. Finally, Li, Lu, and Lo (2019) find that analyst coverage promotes market efficiency by reducing information asymmetry in the M&A process.

To the extent that analyst coverage promotes market efficiency through the timely and more accurate incorporation of value-relevant information, we expect the positive relation between acquirers' SLR risk and the acquirer announcement-period CAR to be stronger if the acquirer receives more attention (i.e., higher analyst coverage) before the merger. To test this, we create an interaction term between the *Number of Analysts* (collected from I/B/E/S of Thomson Reuters) with SLR risk (*Inundated6ft*) and re-estimate equation (2). We posit that the coefficient on this interaction term, *Inundated6ft* × *No. of Analysts*, to be positive and significant.

In Table 7, we present the results of this exercise. Regardless of the window used around the announcement, the coefficient estimates on the interaction term between SLR risk and the number of analysts are all positive and statistically significant. As reported in the online appendix (Table A.4), we find analyst coverage is conducive to strong positive market reaction only when the target firms are not directly exposed to SLR risk.

4.3 Impact of SLR risk on Duration of Merger

In this section, we test Hypothesis 3 and investigate whether SLR-induced mergers have a shorter duration. As discussed in Section 2.3, prior studies (e.g., Li, Xu, Mclver, Wu, and Pan, 2020) find that heavy polluters' green M&As are associated with access to resources, and reduced financing constraints and tax liabilities, suggesting improved legitimacy of green M&As. We, therefore, expect mergers involving firms with greater SLR risk to close faster.

To test this, we re-estimate Equation (2) but with a change: we replace the dependent variable with the merger deal's duration, which is defined as the number of days between the deal announcement date and the deal completion date.

We present the results of this exercise in Table 8. We regress the duration of the merger deal (Column 1) and the natural logarithm of the duration (Column 2) on SLR risk proxied by *Inundated6ft*, controlling for deal characteristics, acquirer characteristics, year fixed effects, and industry fixed effects. The coefficient estimate on *Inundated6ft* is negative and statistically significant. An acquirer with inundation risk closes the merger deal almost four days faster than another acquirer without inundation risk. Overall, the evidence in Table 8 is consistent with Hypothesis 3a that mergers that reduce the preexisting SLR risk go through faster. One caveat of this result is that we cannot pin down the exact reason why such mergers go through faster. It might be due to a green light effect that all involved stakeholders and external regulatory bodies tend to view acquisition attempts that diversify away environmental risks more favorably.

4.4 Long-term Firm Outcomes

4.4.1 SLR and Post-Merger Analyst Forecast

In this section, we examine how the post-merger information environment of the combined firm changes and whether this change is related to the acquiring firm's pre-merger exposure to SLR risk. If SLR indeed represents a significant uncertainty that makes the valuation and forecast of business activities and enterprise value difficult, we should see a significant reduction in such uncertainty when a high-SLR risk acquirer diversifies such risks away through a merger deal.

We focus on various measures of analyst forecast to proxy for firms' information environment. Specifically, we hypothesize that if SLR risk introduces uncertainty into the valuation of the acquirer pre-merger, we should observe analyst forecast accuracy to improve after the merger if the acquire is exposed to high SLR risk before the merger.

To test the hypotheses, we estimate Equation (3) in Section 3.4. Specifically, we regress several measures of analyst forecast accuracy on the post-merger dummy ($Post - Merger_{i,t}$), the interaction between the post-merger dummy and SLR risk ($Post - Merger_{i,t} \times SLR\ Risk_i$), and other firm characteristics. For dependent variables, we include the *number of analysts*, the logarithm of the *number of analysts*, the *Range,* and the *Standard deviation* of the analyst forecast. A detailed explanation of those variables is listed in the appendix.

We present the results of these exercises in Table 9. Panel A provides the summary statistics for this test and panel B provides the results. The estimation window is ten years (-5, +5) around the merger year. Columns 1 through 4 examine whether the change in the number of analysts post-merger is related to acquirers' pre-merger SLR exposure. The coefficient estimates on the interaction term are all positive and statistically significant in three out of the four columns, suggesting that SLR risk is positively associated with the number of analysts post-merger. In contrast, column 6 shows that if an acquirer has a high SLR risk exposure before the merger, the post-merger analyst forecast range experiences a 2.9% improvement (i.e., reduction) relative to the stock price. Similarly, the negative and marginally significant coefficient estimate of -0.015 on the interaction term in column 8 suggests that acquirers with high SLR risk exposure experience a greater reduction in the standard deviation of analyst forecast post-merger. Together, these results are consistent with Hypothesis 4a and suggest that diversifying away SLR risk through acquisitions does help the firm achieve a more transparent information environment. Furthermore, Table A.5 shows that the results are driven by M&A deals of which targets are not directly exposed to SLR risk.

4.4.2 Quasi-Experiment of post-merger outcome using failed merger bids

In examining the post-merger outcomes in Table 9, we implicitly condition on mergers being completed. To the extent that mergers are more likely to be completed if the expected benefits associated with the acquisition are more likely to be realized, our results might overstate the true effect of SLR on post-merger firm outcomes.

To mitigate this endogeneity concern, we exploit a quasi-experiment of failed merger bids (Seru, 2014; Bena and Li, 2014), which are merger attempts that have been announced publicly but are eventually withdrawn due to reasons ranging from differences in corporate culture to disagreement over target valuation. The important consideration for this empirical design is that the underlying reasons for the withdrawal of the merger attempt be unrelated to SLR risks as well as firm outcomes post-merger, i.e., number of analysts and analyst forecast range and standard deviations. This condition is likely satisfied because analysts are not considered a stakeholder of either the target or the acquiring firm in a merger deal.

Specifically, to implement this empirical strategy, we follow Bena and Li (2014) and compare a treated sample of completed mergers to a control sample of withdrawn merger bids. In this case, the assignment of firm pairs to the treatment sample (completed deals) versus the control sample can be treated as random.

To form the control sample, we begin with 1,654 withdrawn bids with non-missing firm-level information in the Compustat/CRSP announced over the period 1986 to 2017. We then examine for each withdrawn bid, excluding those bids that could fail due to the SLR of either merger partner. Next, we form a treatment sample of the completed deals from 1986 to 2017 just as we mentioned in Section 3.1. We require that the acquirer-target industry pairs that match industry pairs of the bids in the control sample. By doing that, we ensure that the treatment and control samples are similar along key dimensions relevant for M&As—industry composition and time clustering.

We then directly investigate the treatment effect of a merger on the post-merger outcome. We estimate the following regression using a panel data set that contains completed and withdrawn deals:

$$Firm\ Outcome_{i,t} = a + \beta_1 Post - Merger_{i,t} \times Complete_j \times SLR\ Risk_i + \beta_2 Post - Merger_{i,t} \times SLR\ Risk_i + \beta_3 Post - Merger_{i,t} + \beta_4 Complete_j + \beta_5 Post - Merger_{i,t} \times Complete_j + + \beta_6 \boldsymbol{X}_{i,t-1} + \rho_t + \psi_i + \nu_s + e_{im,t},$$

(5)

Where all variables are defined as the same as those in the previous sections. $Complete_j$ equals one if the deal j is complete, and zero otherwise. Table 10 provides the results for equation (5). We use the same set of dependent variables as those in Table 9. The estimation window is ten years (-5, +5) around the merger year. The coefficient estimate of interest is β_1 in front of the triple interaction term (Inundated6ft×Post×Complete). We find that β_1 are positive and statistically significant at 5% for all columns in this table. These results suggest that findings that the combined firms after the merger experience more financial analysts coverage and smaller analysts' forecast variations. Overall, this evidence supports H4a that SLR has a causal impact on aspects of analyst coverage post-merger.

4.4.3 SLR and Post-Merger ESG Score

The final set of our analyses focuses on whether firms' ESG score improves after the SLR risk is diversified away through a merger. To some extent, this exercise serves as a validation test to provide additional evidence on whether SLR risk reduction through a merger helps a firm achieve a better ESG score, which covers many aspects related to carbon emissions and the environment as well as sustainability.

We estimate Equation (3) using the ESG index – both the composite score and the component related specifically to the environment – obtained from the MSCI ESG KLD database as the dependent variables. We present the results of these exercises in Table 11. Panel A provides the summary statistics for this test and Panel B provides the results. We estimate both an Ordinary Least Squares specification (columns (1) – (2)) and an ordered probit model (columns (3) – (4)). The estimation window is ten years (-5, +5) around the merger year. Note that despite the relatively small sample size, we find that acquirers with high SLR risk exposure before the acquisition tend to experience a significantly higher increase in the overall ESG scores and the environment-related score in the aftermath of the merger. These results indicate that after the merger, the combined firms have a higher impact the on Environment, Society, Governance, and sustainable growth, particularly so for those mergers that are SLR-induced in the first place.

4.5 Robustness Tests

4.5.1 Alternative Thresholds of SLR Risk

In this section, we examine the robustness of our first set of findings using alternative thresholds of SLR risk. In particular, we examine whether our documented effect that high SLR risk exposure is positively (negatively) correlated to a firm's propensity to become an acquirer (target) is due to the specific threshold of 6ft of inundation we used in our main specifications.

To this end, we re-estimate Equation (1) but replace the measure of SLR risk using *inundated3ft* and *inundated10ft* and present these results in Table 12 and Table 13. Overall, the positive (negative) relationship between SLR risk and acquirer (target) probability remains economically meaningful and statistically significant. These results reassure that our documented effects are not due to the specific empirical proxies we use.

4.6 Remarks and Caveats

Our results in this paper suggest that firms actively manage environmental risks induced by SLR through acquiring other firms not exposed to such risks and that the market appears to reward such acquisition attempts with higher acquirer announcement-period returns. To the extent that SLR risks are uncertain and slow-changing in nature, we believe that the effect of SLR on merger likelihood is likely causal. In addition, because of the short window of investigation in the announcement-period CAR tests, these tests provide a clean assessment of SLR risks by the market. However, our tests on post-merger firm outcomes are likely to be subject to endogeneity concerns that cannot be fully addressed. We hereby remind our readers to interpret our results with these caveats in mind.

5. Conclusion

Effective management of environmental risks has become central to the long-term sustainability and success of modern businesses. While there are many types of environmental risks, the inundation risks associated with sea level rises that are both uncertain and significant pose a unique challenge for firms.

In this paper, we develop and test the hypothesis that firms manage the sea level rise risk through acquisitions. Using a comprehensive sample of publicly traded firms between 1986 and 2017, we find that in the cross-section, firms exposed to high SLR risk have a higher probability of becoming acquirers but a significantly lower probability of becoming targets. Also, we find that the market rewards acquisitions made by firms with high SLR risk exposure, as we observe a significant and positive relationship between the acquirers' cumulative announcement return and pre-merger SLR risk. We also find that this positive relation is more pronounced for firms with higher analyst coverage. Finally, we find that SLR-induced mergers tend to complete faster, and that post-merger, the combined firm experiences a greater increase in analyst coverage, forecast precision, and ESG score when the acquiring firm has a high SLR exposure before the merger.

While our results provide some of the first systematic evidence on how environmental risks associated with sea level rises shape and influence firm behavior in the market for corporate control, many other aspects of such interaction remain unexplored. A deeper understanding of how different types of environmental risks differentially affect firms' investment, financing, and operational policies remains a fruitful area for future research.

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Appendix. Variable description

Variable	Definition (data source)
Merger Variables	
Acquirer	Dummy variable equals one if the firm is the acquirer in the merger deal, and zero otherwise (Thompson Financial Securities Data Company (SDC database).
Target	Dummy variable equals one if the firm is the target in the merger deal, and zero otherwise (Thompson Financial Securities Data Company (SDC database).
Merger pair	Dummy variable equals one for the real merger deal, and zero otherwise (SDC database).
Acquirer's CAR	Acquirer (target) firm cumulative abnormal stock returns from two days before to multiple days after the merger announcement date (i.e., days −2, 0, and + 1, where day 0 is the merger announcement day). Using CRSP value-weighted market returns, we estimate market model parameters over the period from 255 days before to 46 days before the merger announcement date. Abnormal stock return is computed as a firm's raw stock return minus the predicted return from the market model (Eventus).
Duration	The number of days to complete the merger deal ((SDC database).
Ln(Deal value)	The natural logarithm of merger deal value (SDC database).
Cash deal	Dummy variable equals one if the deal is financed with cash, and zero otherwise (SDC database).
Tpublic	A dummy variable equals one if the target firm is public, and zero otherwise
Diversifying deal	Dummy variable equal to one when each of the merging firms is either single- or multi-segment and have at least one segment in the different industry based on 3-digit SIC (4-digit NAICS) code, and zero otherwise (SDC database).
SLR variables	
Inundated6ft	A dummy variable equals one if the firm headquarter would be inundated if sea level rise by 6 feet, and zero otherwise.
Inundated3ft	A dummy variable equals one if the firm headquarter would be inundated if the sea level rise by 3 feet, and zero otherwise.
Inundated10ft	A dummy variable equals one if the firm headquarter would be inundated if sea level rise by 10 feet, and zero otherwise.
SLR risk	A dummy variable equals one if the acquirer/target is subject to SLR risk/no SLR risk, and zero otherwise.
Coast	A dummy variable equals one if the firm headquarters is located within 50 miles of the coastline.
Firm characteristics	
Firm size	Natural logarithm of total book assets (AT) at the fiscal year-end immediately before the merger (asset sale) announcement date (Compustat).
Leverage	The ratio of long-term debt (DLTT) plus short-term debt (DLC) to total book assets (AT) at the fiscal year-end immediately before the merger (asset sale) announcement date (Compustat).
Market-to-book	The market-to-book ratio of a firm's assets at the fiscal year-end immediately before the merger (asset sale) announcement date, where the market value of assets is estimated as the book value of assets plus the difference between the market and book values of equity (AT + PRCC_F ×CSHO −CEQ) (Compustat).
Dividend payer	A dummy variable equals one if the firm pays a dividend at the fiscal year-end immediately before the merger (asset sale) announcement date, and zero otherwise (Compustat).
Ln(Total Sales)	Natural logarithm of total sales (SALE) at the fiscal year-end immediately before the merger (asset sale) announcement date (Compustat).
ROA	The ratio of operating income before depreciation (OIBDP) to total book assets (AT) at the fiscal year-end immediately before the merger (asset sale) announcement date (Compustat).
R&D	The ratio of research and development expense (XRD) to total book assets (AT) at the fiscal year-end immediately before the merger (asset sale) announcement

	date (Compustat).
Capex	The ratio of capital expenditures (CAPX) to total sales (SALE) at the fiscal year-end immediately before the merger (asset sale) announcement date (Compustat).
Non-debt tax shield	The ratio of depreciation (DP) to total assets (AT) at the fiscal year-end immediately before the merger (asset sale) announcement date (Compustat).
Quick ratio	The ratio of current assets minus inventory (ACT-INVT) to current liability (LCT) at the fiscal year-end immediately before the merger (asset sale) announcement date (Compustat).
Cash flow volatility	The seasonally adjusted standard deviation of EBITDA from year t to year t+4. at the fiscal year-end immediately before the merger (asset sale) announcement date, and zero otherwise (Compustat).
Analyst coverage	
No. of analysts	The number of analysts following the firm's earnings per share (I/B/E/S of Thomson Reuters).
Range	The range of analysts' earning per share estimates scaled by the stock price (I/B/E/S of Thomson Reuters).
Standard deviation	The standard deviation of the analyst's earning per share estimates scaled by the stock price (I/B/E/S of Thomson Reuters).
ESG	
ESG score	The MSCI ESG (environmental, social, and governance) score, which covers aspects related to Environment, society, corporate governance, as well as sustainability (MSCI KLD database).

Table 1. Descriptive Statistics

This table reports summary statistics of the acquirers and the target firms. We use two different control samples as pools of potential merger participants. First, we form the industry and size-matched control sample. Specifically, for each acquirer (target) firm of a deal announced in year t, we use the nearest neighbor match to find up to five matching acquirers (matching targets) by the narrowest SIC grouping and by Firm size from Compustat in year t-1 that are neither an acquirer nor a target in the three years before the deal. Second, we form the industry, size, and M/B matched control sample. For each acquirer (target) of a deal in year *t*, we find up to five matching acquirers (matching targets)—first matched by industry, and then matched on the propensity scores estimated using the size and M/B ratios—from Compustat in year t−1 that is neither an acquirer nor a target firm in the three years before the deal. Definitions of the variables are provided in the Appendix. *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

Panel A. Acquirer-Industry and size-matched	N	Mean	Std. Dev	Median	Panel B. Acquirer-Industry, size, and M/B matched	N	Mean	Std. Dev	Median
Variable					Variable				
Event firm (Acquirer)	100,364	0.196	0.397	0.000	Event firm (Acquirer)	83,293	0.192	0.393	0.000
Sea level rising risk									
Inundated3ft	100,364	0.036	0.186	0.000	Inundated3ft	83,293	0.024	0.154	0.000
Inundated6ft	100,364	0.074	0.261	0.000	Inundated6ft	83,293	0.072	0.259	0.000
Inundated10ft	100,364	0.090	0.286	0.000	Inundated10ft	83,293	0.087	0.281	0.000
Coast	100,364	0.451	0.477	0.000	Coast	83,293	0.497	0.483	0.167
Firm-level characteristics									
Firm size	100,364	5.499	2.116	5.487	Firm size	83,293	5.827	2.292	5.854
Leverage	100,364	0.250	0.243	0.203	Leverage	83,293	0.237	0.238	0.197
Market-to-book	100,364	2.158	1.754	1.578	Market-to-book	83,293	2.143	1.961	1.529
Dividend payer	100,364	0.300	0.458	0.000	Dividend payer	83,293	0.440	0.492	0.000
Ln(Total Sales)	100,364	5.421	2.204	5.483	Ln(Total Sales)	83,293	5.903	2.322	6.111
Return on assets	100,364	0.075	0.213	0.115	Return on assets	83,293	0.102	0.180	0.124
R&D	100,364	0.103	0.379	0.000	R&D	83,293	0.054	0.160	0.000
Capex	100,364	0.131	0.320	0.041	Capex	83,293	0.089	0.167	0.036
Non-debt tax shield	100,364	0.049	0.036	0.041	Non-debt tax shield	83,293	0.049	0.037	0.041
Quick ratio	100,364	2.102	2.322	1.393	Quick ratio	83,293	1.960	2.138	1.363
Cash flow volatility	100,364	0.077	0.095	0.046	Cash flow volatility	83,293	0.069	0.089	0.042
Panel C. Target-Industry and size-matched	N	Mean	Std. Dev	Median	Panel D. Target-Industry, size, and M/B matched	N	Mean	Std. Dev	Median
Variable					Variable				
Event firm (Target)	9,183	0.183	0.386	0.000	Event firm (Target)	9,183	0.187	0.370	0.000
Sea level rising risk									
Inundated3ft	9,183	0.022	0.146	0.000	Inundated3ft	9,099	0.010	0.094	0.000
Inundated6ft	9,183	0.050	0.218	0.000	Inundated6ft	9,099	0.037	0.187	0.000
Inundated10ft	9,183	0.067	0.250	0.000	Inundated10ft	9,099	0.055	0.227	0.000
Coast	9,183	0.430	0.489	0.000	Coast	9,099	0.411	0.470	0.000
Firm-level characteristics									
Firm size	9,183	5.000	1.792	4.859	Firm size	9,099	5.752	2.034	5.618
Leverage	9,183	0.228	0.245	0.162	Leverage	9,099	0.222	0.218	0.181
Market-to-book	9,183	2.284	1.882	1.659	Market-to-book	9,099	2.083	1.587	1.540
Dividend payer	9,183	0.232	0.422	0.000	Dividend payer	9,099	0.378	0.477	0.000
Ln(Total Sales)	9,183	4.872	1.931	4.854	Ln(Total Sales)	9,099	5.732	2.090	5.682
Return on assets	9,183	0.049	0.227	0.107	Return on assets	9,099	0.099	0.168	0.128
R&D	9,183	0.296	1.274	0.008	R&D	9,099	0.090	0.274	0.002
Capex	9,183	0.172	0.462	0.047	Capex	9,099	0.113	0.213	0.043
Non-debt tax shield	9,183	0.051	0.039	0.041	Non-debt tax shield	9,099	0.050	0.032	0.044
Quick ratio	9,183	2.666	3.066	1.612	Quick ratio	9,099	2.238	2.411	1.460
Cash flow volatility	9,183	0.086	0.092	0.056	Cash flow volatility	9,099	0.073	0.083	0.045

Table 2. SLR and Likelihood of Becoming an Acquirer

This table reports coefficient estimates from conditional logit, logit, and OLS models in equation (1). The dependent variable is equal to one for the acquirer, and zero for the matched acquirers that form the control group. The key independent variable is *inundated6ft*, a dummy variable equal to one if the firm's headquarters would be inundated if the sea level rise by six feet, and zero otherwise. Panel A presents the baseline specification for the industry and size-matched sample. Panel B presents the baseline specification for the industry, size, and M/B ratio matched sample. Definitions of the variables are provided in the Appendix. All specifications include fixed effects that are listed at the bottom of the table. Robust standard errors (clustered at the deal or zip code level) are reported in parentheses; *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively. We report the marginal effects below the robust standard error in columns 1, 2, and 3.

	Acquirer	Acquirer	Acquirer	Acquirer	Acquirer	Acquirer	Acquirer
Panel A.	Conditional logit	Logit	Logit	OLS	OLS	OLS	OLS
Industry & size matched	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Inundated6ft	0.206***	0.184***	0.186*	0.047***	0.046**	0.032***	0.032*
	(0.038)	(0.036)	(0.106)	(0.008)	(0.021)	(0.006)	(0.019)
	0.041	0.029	0.029				
Firm size	0.685***	-0.116***	-0.116***	0.100***	0.100***	-0.019***	-0.019***
	(0.027)	(0.014)	(0.035)	(0.004)	(0.009)	(0.002)	(0.006)
	0.012	-0.018	-0.018				
leverage	0.166***	0.195***	0.196**	0.034***	0.035**	0.026***	0.027*
	(0.043)	(0.039)	(0.085)	(0.008)	(0.015)	(0.006)	(0.014)
	0.003	0.031	0.031				
market-to-book	0.126***	0.122***	0.124***	0.028***	0.028***	0.022***	0.022***
	(0.005)	(0.004)	(0.012)	(0.001)	(0.003)	(0.001)	(0.002)
	0.002	0.019	0.020				
Dividend payer	-0.256***	-0.173***	-0.177***	-0.050***	-0.050***	-0.030***	-0.031***
	(0.023)	(0.020)	(0.049)	(0.004)	(0.009)	(0.003)	(0.008)
	-0.004	-0.027	-0.028				
Ln(Sales)	0.303***	0.205***	0.206***	0.060***	0.059***	0.034***	0.034***
	(0.017)	(0.015)	(0.037)	(0.003)	(0.007)	(0.002)	(0.006)
	0.005	0.032	0.032				
ROA	0.544***	0.102*	0.115	0.083***	0.086***	0.008	0.009
	(0.072)	(0.057)	(0.126)	(0.012)	(0.024)	(0.008)	(0.019)
	0.009	0.016	0.018				
R&D	-0.031	-0.095***	-0.095	-0.000	-0.001	-0.013***	-0.013
	(0.041)	(0.034)	(0.062)	(0.007)	(0.011)	(0.005)	(0.009)
	-0.001	-0.015	-0.015				
Capex	0.541***	0.311***	0.318***	0.106***	0.107***	0.050***	0.051***
	(0.037)	(0.033)	(0.085)	(0.008)	(0.018)	(0.006)	(0.015)
	0.009	0.049	0.050				
Quick ratio	0.043***	0.043***	0.043***	0.009***	0.009***	0.007***	0.007***
	(0.005)	(0.004)	(0.008)	(0.001)	(0.002)	(0.001)	(0.001)
	0.001	0.007	0.007				
Non debt tax shield	-7.012***	-4.995***	-5.024***	-1.215***	-1.210***	-0.718***	-0.720***
	(0.344)	(0.303)	(0.608)	(0.055)	(0.099)	(0.041)	(0.080)
	-0.121	-0.786	-0.790				
Cash flow volatility	-3.067***	-2.655***	-2.677***	-0.488***	-0.490***	-0.343***	-0.345***
	(0.157)	(0.143)	(0.271)	(0.022)	(0.040)	(0.016)	(0.032)
	-0.053	-0.418	-0.421				
Coast	-0.009	0.006	-0.006	-0.002	-0.003	-0.000	-0.001
	(0.027)	(0.026)	(0.058)	(0.005)	(0.010)	(0.004)	(0.009)
	0.000	0.001	-0.001				
Cluster	Deal	Deal	Zip code	Deal	Zip code	Deal	Zip code
Fixed effects	Deal, State	State, Year, Industry	State, Year, Industry	Deal, State×Year	Deal, State×Year	Industry, State×Year	Industry, State×Year

No. of observations	91,823	100,273	100,273	99,559	99,559	100,364	100,364
R-squared				0.138	0.140	0.061	0.062
Pseudo R-squared	0.087	0.032	0.032				

	Acquirer	Acquirer	Acquirer	Acquirer	Acquirer	Acquirer	Acquirer
Panel B.	Conditional logit	Conditional logit	Conditional logit	OLS	OLS	OLS	OLS
Industry, size, & M/B matched	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Inundated6ft	0.586***	0.542***	0.557**	0.081***	0.082*	0.077***	0.080**
	(0.056)	(0.055)	(0.247)	(0.009)	(0.042)	(0.008)	(0.037)
	0.131	0.056	0.058				
Ln(firm size)	0.366***	0.359***	0.362***	0.044***	0.044***	0.041***	0.041***
	(0.025)	(0.023)	(0.080)	(0.003)	(0.010)	(0.003)	(0.009)
	0.082	0.037	0.037				
leverage	-0.440***	-0.390***	-0.387**	-0.047***	-0.047**	-0.036***	-0.035*
	(0.062)	(0.060)	(0.162)	(0.007)	(0.020)	(0.008)	(0.018)
	-0.098	-0.040	-0.040				
market-to-book	0.009	0.000	-0.002	0.003**	0.002	0.000	0.000
	(0.009)	(0.007)	(0.023)	(0.001)	(0.003)	(0.001)	(0.003)
	0.002	0.000	0.000				
Dividend payer	-0.727***	-0.690***	-0.696***	-0.089***	-0.089***	-0.085***	-0.086***
	(0.029)	(0.028)	(0.084)	(0.003)	(0.011)	(0.003)	(0.010)
	-0.162	-0.072	-0.072				
Ln(Sales)	-0.300***	-0.253***	-0.256***	-0.035***	-0.036***	-0.027***	-0.028***
	(0.025)	(0.024)	(0.080)	(0.003)	(0.010)	(0.003)	(0.009)
	-0.067	-0.026	-0.026				
ROA	1.549***	1.456***	1.461***	0.164***	0.164***	0.135***	0.136***
	(0.120)	(0.121)	(0.272)	(0.011)	(0.032)	(0.011)	(0.027)
	0.345	0.151	0.151				
R&D	0.547***	0.692***	0.686**	0.063***	0.063	0.085***	0.085**
	(0.120)	(0.115)	(0.315)	(0.015)	(0.045)	(0.015)	(0.036)
	0.122	0.072	0.071				
Capex	0.071	0.028	0.022	0.017	0.015	0.018	0.017
	(0.104)	(0.101)	(0.291)	(0.014)	(0.039)	(0.014)	(0.035)
	0.016	0.003	0.002				
Quick ratio	0.015**	0.016**	0.015	0.003***	0.003	0.004***	0.003
	(0.007)	(0.006)	(0.020)	(0.001)	(0.003)	(0.001)	(0.003)
	0.003	0.002	0.002				
No debt tax shield	1.175**	0.721	0.783	0.165***	0.175	0.129**	0.138
	(0.468)	(0.443)	(1.435)	(0.056)	(0.174)	(0.054)	(0.167)
	0.262	0.075	0.081				
Cash flow volatility	1.184***	1.427***	1.435***	0.158***	0.159**	0.161***	0.162***
	(0.178)	(0.173)	(0.462)	(0.024)	(0.066)	(0.023)	(0.060)
	0.264	0.148	0.149				
Coast	-0.010	-0.020	-0.022	-0.002	-0.002	0.003	0.003
	(0.037)	(0.037)	(0.133)	(0.004)	(0.017)	(0.005)	(0.014)
	-0.002	-0.002	-0.002				
Cluster	Deal	Deal	Zip code	Deal	Zip code	Deal	Zip code
Fixed effects	Deal, State	State, Year, Industry	State, Year, Industry	Deal, State×Year	Deal, State×Year	Industry, State×Year	Industry, State×Year
No. of observations	80,132	83,178	83,178	83,057	83,057	83,293	83,293
R-squared				0.117	0.118	0.086	0.087
Pseudo R-squared	0.068	0.051	0.051				

Table 3. SLR and Likelihood of Becoming a Target

This table reports coefficient estimates from conditional logit, logit, and OLS models in equation (1). The dependent variable is equal to one for the target, and zero for the matched targets that form the control group. The key independent variable is *inundated6ft*, a dummy variable equal to one if the firm's headquarters would be inundated if the sea level rise by six feet, and zero otherwise. Panel A presents the baseline specification for the industry and size-matched sample. Panel B presents the baseline specification for the industry, size, and M/B ratio matched sample. Definitions of the variables are provided in the Appendix. All specifications include fixed effects that are listed at the bottom of the table. Robust standard errors (clustered at the deal or zip code level) are reported in parentheses; *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively. We report the marginal effects below the robust standard error in columns 1, 2, and 3.

	Target	Target	Target	Target	Target	Target	Target
Panel A.	Conditional logit	Logit	Logit	OLS	OLS	OLS	OLS
Industry & size matched	(1)	(2)	(3)	(4)	(5)	(6)	(7)
inundated6ft	-0.205*	-0.252**	-0.250	-0.044*	-0.054*	-0.037**	-0.046*
	(0.122)	(0.122)	(0.160)	(0.023)	(0.029)	(0.019)	(0.025)
	-0.047	-0.039	-0.039				
Target Control variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Cluster	Deal	Deal	Zip code	Deal	Zip code	Deal	Zip code
Fixed effects	Deal, State	State, Year, Industry	State, Year, Industry	Deal, State×Year	Deal, State×Year	Industry, State×Year	Industry, State×Year
No. of observations	8,092	9,180	9,180	9,049	9,049	9,183	9,183
R-squared				0.218	0.217	0.145	0.147
Pseudo R-squared	0.044	0.039	0.039				

	Target	Target	Target	Target	Target	Target	Target
Panel B.	Conditional logit	Logit	Logit	OLS	OLS	OLS	OLS
Industry, size, & M/B matched	(1)	(2)	(3)	(4)	(5)	(6)	(7)
inundated6ft	-0.609***	-0.513**	-0.511*	-0.061***	-0.060**	-0.054**	-0.054*
	(0.194)	(0.200)	(0.264)	(0.021)	(0.027)	(0.021)	(0.030)
	-0.053	-0.049	-0.048				
Target control variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Cluster	Deal	Deal	Zip code	Deal	Zip code	Deal	Zip code
Fixed effects	Deal, State	State, Year, Industry	State, Year, Industry	Deal, State×Year	Deal, State×Year	Industry, State×Year	Industry, State×Year
No. of observations	7,889	8,950	8,950	9,087	9,087	9,099	9,099
R-squared				0.135	0.136	0.168	0.168
Pseudo R-squared	0.104	0.087	0.086				

Table 4 SLR and Merger pair

This table reports coefficient estimates from conditional logit, logit, and OLS models in equation (4). The dependent variable is equal to one for the real merger deal, and zero for the matched deal. The key independent variable is $SLR\ Risk_{i,jm,t-1}$ equals one if the acquirer/target is subject to SLR risk/no SLR risk. Panel A presents the baseline specification for the industry and size-matched sample. Panel B presents the baseline specification for the industry, size, and M/B ratio matched sample. Definitions of the variables are provided in the Appendix. All specifications include fixed effects that are listed at the bottom of the table. Robust standard errors (clustered at the deal or zip code level) are reported in parentheses; *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively. We report the marginal effects below the robust standard error in columns 1, 2, and 3.

Panel A.	Pair	Pair	Pair	Pair	Pair	Pair	Pair
	Conditional logit	Logit	Logit	OLS	OLS	OLS	OLS
Size and industry matched	(1)	(2)	(3)	(4)	(5)	(6)	(7)
SLR	0.206*	0.118**	0.119*	0.030*	0.031	0.024**	0.025*
	(0.117)	(0.059)	(0.067)	(0.018)	(0.021)	(0.012)	(0.013)
	0.040	0.022	0.022				
Acquirer control	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Target control	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Cluster	Deal	Deal	Zip	Deal	Zip	Deal	Zip
Fixed effects	Deal, State	State, Year, Industry	State, Year, Industry	Deal, State×Year	Deal, State×Year	Industry, State×Year	Industry, State×Year
No of observations	12,308	13,432	13,357	13,109	13,031	13,166	13,091
R-squared				0.162	0.163	0.086	0.088
Pseudo R-squared	0.052	0.018	0.019				

Panel B.	Pair	Pair	Pair	Pair	Pair	Pair	Pair
	Conditional logit	Logit	Logit	OLS	OLS	OLS	OLS
Size, industry, and mtb matched	(1)	(2)	(3)	(4)	(5)	(6)	(7)
SLR	0.454**	0.274**	0.286*	0.047*	0.047	0.033**	0.034**
	(0.202)	(0.127)	(0.162)	(0.024)	(0.031)	(0.014)	(0.016)
	0.102	0.028	0.029				
Acquirer control	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Target control	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Cluster	Deal	Deal	Zip	Deal	Zip	Deal	Zip
Fixed effects	Deal, State	State, Year, Industry	State, Year, Industry	Deal, State×Year	Deal, State×Year	Industry, State×Year	Industry, State×Year
No of observations	8,562	9,333	9,273	9,261	9,196	9,270	9,206
R-squared				0.162	0.163	0.099	0.100
Pseudo R-squared	0.074	0.042	0.042				

Table 5. Sample and Summary Statistics for Acquirer Announcement Return Regressions

This table presents the descriptive statistics for acquirers' cumulative abnormal stock returns around the merger deal announcement date. We use the Fama-French (1993) three factors model and daily stock returns in the window (−255, −46) to estimate the deal announcement CARs. We use the Center for Research in Security Prices (CRSP) value-weighted index as the benchmark portfolio. We also provide some deal characteristics descriptive statistics in this table.

Acquirers' CARs (%) and deal characteristics						
Variable	N	Mean	STD	P25	Median	P75
(-1, +1)	18,645	1.263	7.611	-2.29	0.475	3.99
(-1, +2)	18,645	1.266	8.343	-2.7	0.534	4.47
(-1, +3)	18,645	1.171	8.949	-3.11	0.504	4.71
(-2, +1)	18,645	1.299	8.15	-2.63	0.496	4.45
(-2, +2)	18,645	1.3	8.84	-2.98	0.552	4.81
(-2, +3)	18,645	1.21	9.394	-3.37	0.496	5
(-3, +1)	18,645	1.35	8.701	-2.97	0.514	4.75
(-3, +2)	18,645	1.348	9.354	-3.25	0.536	5.1
(-3, +3)	18,645	1.261	9.883	-3.64	0.538	5.35
Ln(Deal value)	18,645	3.719	1.763	2.379	3.523	4.88
Cash deal	18,645	0.302	0.459	0	0	1
Tpublic	18,645	0.144	0.35	0	0	0
Diversify	18,645	0.403	0.491	0	0	1
Number of analysts	15,253	8.707	8.216	3	6	12
Duration	18,645	57.11	88.95	0	32	78
Ln(Duration)	18,645	3.964	1.035	3.401	4.025	4.66

Table 6. Market Reaction – Acquirers' Announcement Period Returns

This table reports coefficient estimates for OLS regressions in equation (2) for acquirers. We use the Fama-French (1993) three factors model and daily stock returns in the window (−255, −46) to estimate the deal announcement CARs. We use the Center for Research in Security Prices (CRSP) value-weighted index as the benchmark portfolio. The key independent variable is *inundated6ft*, a dummy variable equal to one if the firm's headquarters would be inundated if the sea level rise by six feet, and zero otherwise. The deal Characteristics include Ln(Deal value), All cash deal, and Diversify deal. Event firm characteristics are *Firm size*, *Market-to-book* (M/B), *Leverage*, *Dividend payer*, *Ln (Total Sales)*, *ROA*, *R&D*, *Capex*, *Quick ratio*, *Non-debt tax shield*, and *Cash flow volatility*. All specifications include fixed effects that are listed at the bottom of the table. Robust standard errors (clustered at the zip level) are reported in parentheses; *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

CARs	[-1,+1]	[-1,+2]	[-1,+3]	[-2,+1]	[-2,+3]	[-3,+1]	[-3,+1]	[-3,+2]	[-3,+3]
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Inundated6ft	0.494*	0.654**	0.730**	0.672**	0.828***	0.925***	0.647**	0.806**	0.900***
	(0.292)	(0.317)	(0.330)	(0.297)	(0.308)	(0.320)	(0.323)	(0.327)	(0.340)
Ln(deal value)	0.493***	0.494***	0.490***	0.534***	0.533***	0.529***	0.583***	0.578***	0.570***
	(0.058)	(0.063)	(0.066)	(0.061)	(0.066)	(0.068)	(0.063)	(0.067)	(0.070)
All cash deal	0.416***	0.400***	0.374***	0.270**	0.262*	0.230	0.207	0.192	0.161
	(0.121)	(0.134)	(0.144)	(0.129)	(0.141)	(0.149)	(0.140)	(0.151)	(0.159)
Diversify deal	0.132	0.011	0.008	0.162	0.049	0.039	0.152	0.035	0.023
	(0.134)	(0.150)	(0.162)	(0.147)	(0.163)	(0.174)	(0.161)	(0.175)	(0.187)
Cluster	Zip	Zip	Zip	Zip	Zip	Zip	Zip	Zip	Zip
Fixed effects	Industry, State×Year	Industry, State×Year	Industry, State×Year	Industry, State×Year	Industry, State×Year	Industry, State×Year	Industry, State×Year	Industry, State×Year	Industry, State×Year
No. of observations	18,645	18,645	18,645	18,645	18,645	18,645	18,645	18,645	18,645
R-squared	0.099	0.094	0.090	0.096	0.092	0.088	0.095	0.093	0.088

Table 7. Cross-sectional Tests on the Level of Analysts' Coverage

This table reports coefficient estimates for the cross-sectional test on the level of analysts' coverage. We use the Fama-French (1993) three factors model and daily stock returns in the window (−255, −46) to estimate the deal announcement CARs. We use the Center for Research in Security Prices (CRSP) value-weighted index as the benchmark portfolio. The key independent variable is *(Inundated6ft × No. of analysts)*. Where *Number of analysts* is the number of analysts following the firm's earnings per share before the deal announcement. The deal Characteristics include Ln(Deal value), All cash deal, and Diversify deal. Event firm characteristics are *Firm size*, *Market-to-book* (M/B), *Leverage*, *Dividend payer*, *Ln (Total Sales)*, *ROA*, *R&D*, *Capex*, *Quick ratio*, *Non-debt tax shield*, and *Cash flow volatility*. All specifications include fixed effects that are listed at the bottom of the table. Robust standard errors (clustered at the zip level) are reported in parentheses; *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

CARs	[-1,+1]	[-1,+2]	[-1,+3]	[-2,+1]	[-2,+2]	[-2,+3]	[-3,+1]	[-3,+2]	[-3,+3]
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Inundated6ft× No. of analysts	0.055**	0.062**	0.057**	0.050**	0.056**	0.050*	0.058**	0.063***	0.058**
	(0.024)	(0.025)	(0.027)	(0.024)	(0.025)	(0.026)	(0.024)	(0.024)	(0.025)
Inundated6ft	-0.010	0.018	0.066	0.253	0.264	0.352	0.143	0.169	0.222
	(0.464)	(0.475)	(0.522)	(0.492)	(0.496)	(0.514)	(0.504)	(0.490)	(0.515)
No. of analysts	0.025*	0.030*	0.043***	0.027*	0.033**	0.046***	0.037**	0.043**	0.055***
	(0.014)	(0.015)	(0.017)	(0.015)	(0.016)	(0.018)	(0.016)	(0.017)	(0.018)
Deal control variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Acquirer control variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Cluster	Zip	Zip	Zip	Zip	Zip	Zip	Zip	Zip	Zip
Fixed effects	Industry, State×Year	Industry, State×Year	Industry, State×Year	Industry, State×Year	Industry, State×Year	Industry, State×Year	Industry, State×Year	Industry, State×Year	Industry, State×Year
No. of observations	15,253	15,253	15,253	15,253	15,253	15,253	15,253	15,253	15,253
R-squared	0.103	0.096	0.091	0.097	0.091	0.087	0.094	0.090	0.085

Table 8. Effect of SLR on the Duration of Mergers

This table presents the results for estimating equation (2). The dependent variables are deal duration, the number of days between the deal announcement date and the deal completion date, and the natural logarithm of deal duration. The key independent variable is *inundated6ft*, a dummy variable equal to one if the firm's headquarters would be inundated if the sea level rise by six feet, and zero otherwise. The deal Characteristics include Ln(Deal value), All cash deal, and Diversify deal. Event firm characteristics are *Firm size*, *Market-to-book* (M/B), *Leverage*, *Dividend payer*, *Ln (Total Sales)*, *ROA*, *R&D*, *Capex*, *Quick ratio*, *Non-debt tax shield*, and *Cash flow volatility*. All specifications include fixed effects that are listed at the bottom of the table. Robust standard errors are reported in parentheses; *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

	Duration	Ln(Duration)
	(1)	(2)
Inundated6ft	-3.941*	-0.091**
	(2.303)	(0.046)
Deal control variables	Yes	Yes
Acquirer control variables	Yes	Yes
Cluster	Zip code	Zip code
Year fixed effects	Yes	Yes
Industry fixed effects	Yes	Yes
No. of observations	18,914	18,914
R-squared	0.215	0.162

Table 9. Effect of SLR on the Long-term Forecast After the Mergers?

This table presents the results for estimating equation (4). Panel A provides the sample used for this test. Panel B provides the results. Odd and even number columns report the regression results without and with control variables. The estimation window is (-5, +5) around the merger year. The dependent variables include the number of analysts following the acquirer's earnings per share (EPS), the natural logarithm of one plus the number of analysts following the acquirer's EPS, range of estimated EPS scaled by the stock price, and standard deviation of estimated EPS scaled by the stock price. The key independent variable is the *Inundated6ft×post merger*. Where *post-merger* equals one if the year is greater or equals to the merger year. Event firm characteristics are *Firm size*, *Market-to-book* (M/B), *Leverage*, *Dividend payer*, *Ln (Total Sales)*, *ROA*, *R&D*, *Capex*, *Quick ratio*, *Non-debt tax shield*, and *Cash flow volatility*. All specifications include fixed effects that are listed at the bottom of the table. Robust standard errors (clustered at the zip level) are reported in parentheses; *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

Panel A.			Analyst test summary statistics			
Variable	N	Mean	STD	P25	Median	P75
Inundated6ft	14,838	0.051	0.220			
Post	14,838	0.645	0.478			
No. of analysts	14,838	2.589	2.905	1	1	3
Ln(No. of analysts)	14,838	1.100	0.530	0.693	0.693	1.386
Range	14,838	0.049	1.266	0	0	0.011
Standard deviation	14,838	0.029	0.891	0	0	0.005

Panel B.	No of analysts	No of analysts	Ln(No of analysts)	Ln(No of analysts)	Range	Range	Standard deviation	Standard deviation
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Inundated6ft×post	0.520*	0.444*	0.080*	0.057	-0.019*	-0.029**	-0.010	-0.015**
	(0.290)	(0.269)	(0.046)	(0.043)	(0.012)	(0.013)	(0.007)	(0.008)
Post	0.054	0.056	0.010	0.010	-0.024	-0.025	-0.017	-0.017
	(0.067)	(0.063)	(0.013)	(0.014)	(0.022)	(0.024)	(0.016)	(0.017)
Acquirer control variables	No	Yes	No	Yes	No	Yes	No	Yes
Cluster	Zip code	Zip code	Zip code	Zip code	Zip code	Zip code	Zip code	Zip code
Fixed effects	Firm, Year, State	Firm, Year, State	Firm, Year, State	Firm, Year, State	Firm, Year, State	Firm, Year, State	Firm, Year, State	Firm, Year, State
No. of observations	15,567	14,838	15,567	14,838	15,488	14,793	15,488	14,793
R-squared	0.767	0.803	0.720	0.751	0.791	0.793	0.793	0.794

Table 10- Quasi-Experiment of post-merger outcome using failed merger bids

This table presents the results for estimating equation (5). Odd and even number columns report the regression results without and with control variables. The estimation window is (-5, +5) around the merger year. The dependent variables include the number of analysts following the acquirer's earnings per share (EPS), the natural logarithm of one plus the number of analysts following the acquirer's EPS, range of estimated EPS scaled by the stock price, and standard deviation of estimated EPS scaled by the stock price. The key independent variable is the *Inundated6ft×Post×Complete*. Where *post-merger* equals one if the year is greater or equals to the merger year. *Complete* equals one if the deal is complete. Event firm characteristics are *Firm size*, *Market-to-book* (M/B), *Leverage*, *Dividend payer*, *Ln (Total Sales)*, *ROA*, *R&D*, *Capex*, *Quick ratio*, *Non-debt tax shield*, and *Cash flow volatility*. All specifications include fixed effects that are listed at the bottom of the table. Robust standard errors (clustered at the zip level) are reported in parentheses; *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

	No of analysts	No of analysts	Ln(No of analysts)	Ln(No of analysts)	Range	Range	Standard deviation	Standard deviation
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Inundated6ft×Post×Complete	1.576**	1.156**	0.149**	0.092**	-0.021**	-0.022**	-0.068**	-0.070**
	(0.652)	(0.575)	(0.072)	(0.045)	(0.009)	(0.010)	(0.033)	(0.034)
Inundated6ft×Post	-0.305	-0.315	-0.027	-0.028	0.018***	0.020***	0.021	0.015
	(0.699)	(0.235)	(0.051)	(0.018)	(0.007)	(0.008)	(0.022)	(0.023)
Inundated6ft×Complete	-1.699***	-1.101***	-0.156***	-0.115***	0.018**	0.016*	0.007	0.008
	(0.416)	(0.176)	(0.048)	(0.019)	(0.009)	(0.009)	(0.047)	(0.053)
Post×Complete	0.658***	0.225	0.099***	0.047***	0.013***	0.010**	0.010	0.003
	(0.188)	(0.161)	(0.021)	(0.017)	(0.005)	(0.005)	(0.014)	(0.015)
Post	0.123	0.100	0.010	0.012	-0.009**	-0.006	-0.006	-0.009
	(0.179)	(0.181)	(0.019)	(0.017)	(0.004)	(0.004)	(0.009)	(0.010)
Complete	-0.519***	-0.097	-0.061***	-0.025	-0.009***	-0.007**	-0.011	-0.003
	(0.159)	(0.174)	(0.016)	(0.019)	(0.003)	(0.004)	(0.020)	(0.021)
Acquire control variables	No	Yes	No	Yes	No	Yes	No	Yes
Cluster	Zip code	Zip code	Zip code	Zip code	Zip code	Zip code	Zip code	Zip code
Fixed effects	Firm, Year, State	Firm, Year, State	Firm, Year, State	Firm, Year, State	Firm, Year, State	Firm, Year, State	Firm, Year, State	Firm, Year, State
No. of observations	37,454	32,831	37,454	32,831	37,485	35,010	37,485	35,010
R-squared	0.825	0.881	0.783	0.853	0.498	0.508	0.560	0.555

Table 11. Effect of SLR on corporate ESG Scores Post Merger

This table presents the results for estimating equation (4). Panel A provides the sample used for this test. Panel B provides the results. Columns 1 and 2 use OLS estimation. Columns 3 and 4 use ordered probit estimation. The estimation window is (-5, +5) around the merger year. The dependent variables are the ESG score and the specific component of the ESG score that assessing environmental performance. The definitions are listed in the appendix. The key independent variable is *Inundated6ft* × *Post*. Where *Post* equals one if the year is greater or equals to the merger year. Event firm characteristics are *Firm size*, *Market-to-book* (M/B), *Leverage*, *Dividend payer*, *Ln (Total Sales)*, *ROA*, *R&D*, *Capex*, *Quick ratio*, *Non-debt tax shield*, and *Cash flow volatility*. All specifications include fixed effects that are listed at the bottom of the table. Robust standard errors (clustered at the zip code level) are reported in parentheses; *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

Panel A.			ESG test summary statistics			
Variable	N	Mean	STD	P25	Median	P75
Inundated6ft	4,403	0.051	0.219	0	0	0
Post	4,403	0.645	0.478	0	1	1
ESG	4,403	-0.254	1.697	-1	0	0
Env_diff	4,392	-0.016	0.583	0	0	0

Panel B	ESG	Env_diff	ESG	Env_diff
	OLS	OLS	Ordered Probit	Ordered Probit
	(1)	(2)	(3)	(4)
Inundated6ft*Post	0.493**	0.099**	0.394**	0.217*
	(0.224)	(0.049)	(0.175)	(0.123)
Inundated6ft	-0.394*	-0.087*	-0.262	-0.199
	(0.228)	(0.046)	(0.173)	(0.125)
Post	0.008	-0.006	0.028	-0.017
	(0.079)	(0.031)	(0.056)	(0.079)
Control variables	Yes	Yes	Yes	Yes
Cluster	Zip code	Zip code	Zip code	Zip code
Fixed effects	Industry, Year, State	Industry, Year, State	Industry, Year, State	Industry, Year, State
No. of observations	4,301	4,392	4,303	4,392
R-squared	0.280	0.223		
Pseudo R-sq			0.087	0.135

Table 12. Which Firms Are Acquirers – Alternative Measures of SLR Risks

This table reports coefficient estimates from conditional logit and logit models in equation (1). The dependent variable is equal to one for the acquirer, and zero for the matched acquirers that form the control group. The key independent variable is *inundated3(10)ft*, a dummy variable if the firm's headquarter would be inundated if the sea level rise by three(ten) feet, and zero otherwise. Panel A presents the baseline specification for industry and size-matched sample. Panel B presents the baseline specification for industry, size, and M/B ratio matched sample. Definitions of the variables are provided in the Appendix. All specifications include fixed effects that are listed at the bottom of the table. Robust standard errors (clustered at the deal or zip code level) are reported in parentheses; *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

	Acquirer	Acquirer	Acquirer	Acquirer
Panel A.	Conditional logit	Logit	Conditional logit	Logit
Size & Industry matched	(1)	(2)	(3)	(4)
inundated3ft	0.248***	0.259**		
	(0.052)	(0.102)		
inundated10ft			0.145***	0.112
			(0.034)	(0.095)
Acquirer control variables	Yes	Yes	Yes	Yes
Cluster	Deal	Zip code	Deal	Zip code
Fixed effects	Deal, State	State, Year, Industry	Deal, State	State, Year, Industry
No. of observations	91,823	100,273	91,823	100,273
Pseudo R-squared	0.085	0.031	0.085	0.031

	Acquirer	Acquirer	Acquirer	Acquirer
Panel B.	Conditional logit	Logit	Conditional logit	Logit
Size, Industry & M/B matched	(1)	(2)	(3)	(4)
inundated3ft	1.287***	1.393***		
	(0.094)	(0.328)		
inundated10ft			0.142***	0.244
			(0.050)	(0.222)
Acquirer control variables	Yes	Yes	Yes	Yes
Cluster	Deal	Zip code	Deal	Zip code
Fixed effects	Deal, State	State, Year, Industry	Deal, State	State, Year, Industry
No. of observations	76,310	83,178	76,310	83,178
Pseudo R-squared	0.070	0.059	0.065	0.056

Table 13. Which Firms Are Targets – Alternative Measures of SLR Risks

This table reports coefficient estimates from conditional logit and logit models in equation (1). The dependent variable is equal to one for the target, and zero for the matched targets that form the control group. The key independent variable is I*nundated3(10)ft*, a dummy variable if the firm's headquarter would be inundated if the sea level rise by three(ten) feet, and zero otherwise. Panel A presents the baseline specification for industry and size-matched sample. Panel B presents the baseline specification for industry, size, and M/B ratio matched sample. Definitions of the variables are provided in the Appendix. All specifications include firm and year or deal fixed effects. All specifications include fixed effects that are listed at the bottom of the table. Robust standard errors (clustered at the deal or zip code level) are reported in parentheses; *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

	Target	Target	Target	Target
Panel A.	Conditional logit	Logit	Conditional logit	Logit
Industry & size matched	(1)	(2)	(3)	(4)
Inundated3ft	-0.458***	-0.465**		
	(0.178)	(0.181)		
Inundated10ft			-0.027	-0.076
			(0.110)	(0.176)
Target control variables	Yes	Yes	Yes	Yes
Cluster	Deal	Zip code	Deal	Zip code
Fixed effects	Deal, State	State, Year, Industry	Deal, State	State, Year, Industry
No. of observations	8,092	9,180	8,092	9,180
Pseudo R-squared	0.045	0.039	0.044	0.039

	Target	Target	Target	Target
Panel B.	Conditional logit	Logit	Conditional logit	Logit
Industry, size & M/B matched	(1)	(2)	(3)	(4)
Inundated3ft	-0.628**	-0.454		
	(0.271)	(0.441)		
Inundated10ft			-0.559***	-0.498*
			(0.177)	(0.279)
Target control variables	Yes	Yes	Yes	Yes
Cluster	Deal	Zip code	Deal	Zip code
Fixed effects	Deal, State	State, Year, Industry	Deal, State	State, Year, Industry
No. of observations	7,889	8,950	7,889	8,950
Pseudo R-squared	0.102	0.086	0.104	0.087

Online Appendix

Table A.1 Which Firms Are Acquirers–further controlling for county fixed effects?

This table reports coefficient estimates from OLS models in equation (1). The dependent variable is equal to one for the acquirer, and zero for the matched acquirers that form the control group. The key independent variable is *inundated6ft*, a dummy variable equal to one if the firm's headquarters would be inundated if the sea level rise by six feet, and zero otherwise. Panel A presents the baseline specification for the industry and size-matched sample. Panel B presents the baseline specification for the industry, size, and M/B ratio matched sample. Definitions of the variables are provided in the Appendix. All specifications include fixed effects that are listed at the bottom of the table. Robust standard errors (clustered at the deal or zip level) are reported in parentheses; *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

	Acquirer	Acquirer	Acquirer	Acquirer
Panel A.	OLS	OLS	OLS	OLS
Industry & size matched	(1)	(2)	(3)	(4)
Inundated6ft	0.041***	0.041*	0.037***	0.037**
	(0.009)	(0.023)	(0.008)	(0.019)
Acquirer control variables	Yes	Yes	Yes	Yes
Cluster	Deal	Zip	Deal	Zip
Fixed effects	Deal, County, Year	Deal, County, Year	Deal, County, Year	Deal, County, Year
No. of observations	99,559	99,559	100,364	100,364
R-squared	0.151	0.151	0.063	0.063

	Acquirer	Acquirer	Acquirer	Acquirer
Panel B.	OLS	OLS	OLS	OLS
Industry, size, & M/B matched	(1)	(2)	(3)	(4)
Inundated6ft	0.123***	0.123***	0.092***	0.092**
	(0.012)	(0.040)	(0.010)	(0.037)
Acquirer control variables	Yes	Yes	Yes	Yes
Cluster	Deal	Zip	Deal	Zip
Fixed effects	Deal, County, Year	Deal, County, Year	Deal, County, Year	Deal, County, Year
No. of observations	83,057	83,057	83,293	83,293
R-squared	0.229	0.229	0.129	0.129

Table A.2 Which Firms Are Targets–further controlling for county fixed effects?

This table reports coefficient estimates from OLS models in equation (1). The dependent variable is equal to one for the target, and zero for the matched targets that form the control group. The key independent variable is *inundated6ft*, a dummy variable equal to one if the firm's headquarters would be inundated if the sea level rise by six feet, and zero otherwise. Panel A presents the baseline specification for the industry and size-matched sample. Panel B presents the baseline specification for the industry, size, and M/B ratio matched sample. Definitions of the variables are provided in the Appendix. All specifications include fixed effects that are listed at the bottom of the table. Robust standard errors (clustered at the deal or zip level) are reported in parentheses; *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

	Target	Target	Target	Target
Panel A.	OLS	OLS	OLS	OLS
Industry & size matched	(1)	(2)	(3)	(4)
inundated6ft	-0.046*	-0.046	-0.050**	-0.050*
	(0.026)	(0.034)	(0.021)	(0.028)
Target control variables	Yes	Yes	Yes	Yes
Cluster	Deal	Zip	Deal	Zip
Fixed effects	Deal, County, Year	Deal, County, Year	Deal, County, Year	Deal, County, Year
No. of observations	9,049	9,049	9,183	9,183
R-squared	0.154	0.154	0.082	0.082

	Target	Target	Target	Target
Panel B.	OLS	OLS	OLS	OLS
Industry, size, & M/B matched	(4)	(5)	(6)	(7)
inundated6ft	-0.090***	-0.090***	-0.074***	-0.074***
	(0.023)	(0.034)	(0.020)	(0.028)
Target control variables	Yes	Yes	Yes	Yes
Cluster	Deal	Zip	Deal	Zip
Fixed effects	Deal, County, Year	Deal, County, Year	Deal, County, Year	Deal, County, Year
No. of observations	9,087	9,087	9,099	9,099
R-squared	0.253	0.253	0.156	0.156

Table A.3 Short-term market reaction – Cross-sectional variation on the target's SLR risk

This table reports coefficient estimates for OLS regressions in equation (2) for acquirers. The main independent variable is *Inundated6ft × (Target Inundated6ft)*, which interacts with the acquirer of the target firms' inundation dummies. We use the Fama-French (1993) three factors model and daily stock returns in the window (−255, −46) to estimate the deal announcement CARs. We use the Center for Research in Security Prices (CRSP) value-weighted index as the benchmark portfolio. The deal Characteristics include Ln(Deal value), All cash deal, and Diversify deal. Event firm characteristics are *Firm size*, *Market-to-book* (M/B), *Leverage*, *Dividend payer*, *Ln (Total Sales)*, *ROA*, *R&D*, *Capex*, *Quick ratio*, *Non-debt tax shield*, and *Cash flow volatility*. All specifications include fixed effects that are listed at the bottom of the table. Robust standard errors (clustered at the zip level) are reported in parentheses; *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

	[-1,+1]	[-1,+2]	[-1,+3]	[-2,+1]	[-2,+2]	[-2,+3]	[-3,+1]
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Inundated6ft × (Target Inundated6ft)	-1.154	-1.330	-1.311	-1.563	-1.706	-1.638	-2.038
	(1.150)	(1.065)	(1.095)	(1.155)	(1.085)	(1.112)	(1.294)
Inundated6ft	0.183	0.728	0.591	0.322	0.823*	0.683	0.330
	(0.476)	(0.491)	(0.513)	(0.461)	(0.474)	(0.501)	(0.523)
Target Inundated6ft	0.230	0.303	0.396	0.219	0.294	0.393	0.212
	(0.404)	(0.423)	(0.458)	(0.436)	(0.451)	(0.483)	(0.463)
Deal control variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Acquirer control variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Cluster	Zip	Zip	Zip	Zip	Zip	Zip	Zip
Fixed effects	Industry, State×Year	Industry, State×Year	Industry, State×Year	Industry, State×Year	Industry, State×Year	Industry, State×Year	Industry, State×Year
No. of observations	6,946	6,946	6,946	6,946	6,946	6,946	6,946
R-squared	0.057	0.056	0.053	0.057	0.056	0.053	0.058

	[-3,+2]	[-3,+3]	[0,+1]	[0,+2]	[0,+3]	[-1,0]	[-2,0]
	(8)	(9)	(10)	(11)	(12)	(13)	(14)
Inundated6ft × (Target Inundated6ft)	-2.272*	-2.332*	-1.488	-1.717*	-1.730*	-0.321	-0.997
	(1.197)	(1.258)	(1.051)	(0.996)	(1.038)	(0.811)	(0.810)
Inundated6ft	0.898*	0.747	0.478	1.058**	0.923*	0.158	0.311
	(0.530)	(0.553)	(0.421)	(0.440)	(0.473)	(0.346)	(0.362)
Target Inundated6ft	0.300	0.402	0.440	0.459	0.589	0.117	0.099
	(0.474)	(0.503)	(0.376)	(0.405)	(0.447)	(0.300)	(0.332)
Deal control variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Acquirer control variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Cluster	Zip	Zip	Zip	Zip	Zip	Zip	Zip
Fixed effects	Industry, State×Year	Industry, State×Year	Industry, State×Year	Industry, State×Year	Industry, State×Year	Industry, State×Year	Industry, State×Year
No. of observations	6,946	6,946	6,946	6,946	6,946	6,946	6,946
R-squared	0.057	0.054	0.051	0.051	0.048	0.051	0.050

Table A.4 "Under the spotlight" – Target subject to vs Target not subject to SLR risk

This table reports coefficient estimates for the cross-sectional test on the level of analysts' coverage. The estimation window is (-5, +5) around the merger year. Panel A includes the deal with the target subject to SLR risk (i.e., *Target Inundated6ft=1*). Panel B includes the deal with the target not subject to SLR risk (i.e., *Target Inundated6ft=0*). We use the Fama-French (1993) three factors model and daily stock returns in the window (−255, −46) to estimate the deal announcement CARs. We use the Center for Research in Security Prices (CRSP) value-weighted index as the benchmark portfolio. The key independent variable is *inundated6ft* × *Number of analysts*. Where *Number of analysts* is the number of analysts following the firm's earnings per share before the deal announcement. The deal Characteristics include Ln(Deal value), All cash deal, and Diversify deal. Event firm characteristics are *Firm size*, *Market-to-book* (M/B), *Leverage*, *Dividend payer*, *Ln (Total Sales)*, *ROA*, *R&D*, *Capex*, *Quick ratio*, *Non-debt tax shield*, and *Cash flow volatility*. All specifications include fixed effects that are listed at the bottom of the table. Robust standard errors (clustered at the zip level) are reported in parentheses; *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

Panel A.	[-1,+1]	[-1,+2]	[-1,+3]	[-2,+1]	[-2,+2]	[-2,+3]	[-3,+1]	[-3,+2]	[-3,+3]
Targets subject to SLR risk	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Inundated6ft × No. of analysts	0.004	-0.024	-0.088	0.047	0.006	-0.062	0.101	0.061	0.005
	(0.122)	(0.141)	(0.149)	(0.132)	(0.150)	(0.160)	(0.142)	(0.163)	(0.173)
Inundated6ft	-3.300	-2.155	-1.382	-3.997	-2.528	-1.736	-5.472*	-4.166	-3.684
	(2.583)	(3.150)	(3.350)	(2.873)	(3.430)	(3.669)	(2.904)	(3.469)	(3.746)
No. of analysts	0.026	0.084	0.111	0.016	0.074	0.108	-0.025	0.035	0.061
	(0.096)	(0.116)	(0.125)	(0.105)	(0.125)	(0.132)	(0.107)	(0.123)	(0.134)
Deal control variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Acquirer control variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Cluster	Zip	Zip	Zip	Zip	Zip	Zip	Zip	Zip	Zip
Fixed effects	Industry, State×Year	Industry, State×Year	Industry, State×Year	Industry, State×Year	Industry, State×Year	Industry, State×Year	Industry, State×Year	Industry, State×Year	Industry, State×Year
No. of observations	549	549	549	549	549	549	549	549	549
R-squared	0.364	0.342	0.350	0.370	0.336	0.358	0.392	0.359	0.368
Panel B.	[-1,+1]	[-1,+2]	[-1,+3]	[-2,+1]	[-2,+2]	[-2,+3]	[-3,+1]	[-3,+2]	[-3,+3]
Targets not subject to SLR risk	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Inundated6ft × No. of analysts	0.078**	0.042	0.025	0.093***	0.057*	0.040	0.088**	0.050	0.033
	(0.031)	(0.032)	(0.036)	(0.032)	(0.034)	(0.037)	(0.037)	(0.038)	(0.041)
Inundated6ft	-0.410	0.599	0.722	-0.593	0.390	0.509	-0.548	0.521	0.652
	(0.695)	(0.745)	(0.784)	(0.718)	(0.774)	(0.802)	(0.835)	(0.882)	(0.914)
No. of analysts	0.019	0.048*	0.060**	0.016	0.047	0.059*	0.035	0.065**	0.077**
	(0.023)	(0.027)	(0.028)	(0.027)	(0.030)	(0.031)	(0.028)	(0.030)	(0.032)
Deal control variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Acquirer control variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Cluster	Zip	Zip	Zip	Zip	Zip	Zip	Zip	Zip	Zip
Fixed effects	Industry, State×Year	Industry, State×Year	Industry, State×Year	Industry, State×Year	Industry, State×Year	Industry, State×Year	Industry, State×Year	Industry, State×Year	Industry, State×Year
No. of observations	5,386	5,386	5,386	5,386	5,386	5,386	5,386	5,386	5,386
R-squared	0.155	0.155	0.152	0.156	0.156	0.152	0.157	0.156	0.150

Table A.5 Analyst coverage – Target subject to vs Target not subject to SLR risk

This table presents the results for estimating equation (4). The estimation window is (-5, +5) around the merger year. Panel A includes the deal with the target subject to SLR risk (i.e, *Target Inundated6ft=1*). Panel B includes the deal with the target subject to no SLR risk (i.e, *Target Inundated6ft=0*). Odd and even number columns report the regression results without and with control variables. The dependent variables include the number of analysts following the acquirer's earnings per share (EPS), the natural logarithm of one plus the number of analysts following the acquirer's EPS, range of estimated EPS scaled by the stock price, and standard deviation of estimated EPS scaled by the stock price. The key independent variable is *inundated6ft×Post-merger*. Where *post-merger* equals one if the year is greater or equals to the merger year. The deal Characteristics include Ln(Deal value), All cash deal, and Diversify deal. Event firm characteristics are *Firm size*, *Market-to-book* (M/B), *Leverage*, *Dividend payer*, *Ln (Total Sales)*, *ROA*, *R&D*, *Capex*, *Quick ratio*, *Non-debt tax shield*, and *Cash flow volatility*. Robust standard errors (clustered at the zip level) are reported in parentheses; *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

Panel A. **Target subject to SLR risk**	No of analysts	No of analysts	Ln(No of analysts)	Ln(No of analysts)	Range	Range	Standard deviation	Standard deviation
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Inundated6ft × post	0.218	0.239	-0.044	-0.065	-0.013	-0.013	-0.007	-0.007
	(0.657)	(0.602)	(0.123)	(0.136)	(0.010)	(0.012)	(0.006)	(0.007)
Post	0.666	0.276	0.097	0.023	0.001	0.005	-0.000	0.003
	(0.437)	(0.342)	(0.067)	(0.075)	(0.009)	(0.011)	(0.005)	(0.006)
Acquirer control variables	No	Yes	No	Yes	No	Yes	No	Yes
Cluster	Zip	Zip	Zip	Zip	Zip	Zip	Zip	Zip
Fixed effects	Firm, Year, State	Firm, Year, State	Firm, Year, State	Firm, Year, State	Firm, Year, State	Firm, Year, State	Firm, Year, State	Firm, Year, State
No. of observations	460	397	460	397	458	397	458	397
R-squared	0.722	0.814	0.715	0.773	0.521	0.568	0.521	0.563

Panel B. **Target not subject to SLR risk**	No of analysts	No of analysts	Ln(No of analysts)	Ln(No of analysts)	Range	Range	Standard deviation	Standard deviation
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Inundated6ft × post	0.478*	0.528*	0.115*	0.119**	-0.369	-0.308	-0.188	-0.153
	(0.278)	(0.290)	(0.060)	(0.059)	(0.313)	(0.241)	(0.156)	(0.115)
Post	0.019	-0.022	-0.003	-0.015	0.598	0.505	0.266	0.229
	(0.111)	(0.117)	(0.020)	(0.021)	(0.734)	(0.535)	(0.367)	(0.252)
Acquirer control variables	No	Yes	No	Yes	No	Yes	No	Yes
Cluster	Zip	Zip	Zip	Zip	Zip	Zip	Zip	Zip
Fixed effect	Firm, Year, State	Firm, Year, State	Firm, Year, State	Firm, Year, State	Firm, Year, State	Firm, Year, State	Firm, Year, State	Firm, Year, State
No. of observations	5,249	4,553	5,249	4,553	5,218	4,553	5,218	4,553
R-squared	0.796	0.833	0.752	0.788	0.731	0.912	0.740	0.923

CHAPTER 3: SHAREHOLDER-CREDITOR CONFLICT AND HEDGING POLICY: EVIDENCE FROM A LENDER-SHAREHOLDER MERGER

1. Introduction

The shareholders and creditors have different preferences for the firm policy because they have different claim orders on the firm's assets (Jensen and Meckling, 1976; Myers, 1977). A large number of studies analyze the relation between shareholder-creditor conflict and various firm policies. For example, Billett, King, and Mauer (2004) use the bond price reaction to measure the conflict and test the relation between mergers and acquisitions. Chu (2018) finds a negative impact of the decreased shareholder-creditor conflict on dividend policies. Because of the limited liability, the shareholders enjoy the most or all of the upset benefits of the risky project. On the other hand, the creditors have to bear the downside costs associated with taking on risky projects. In particular, the equity carries the feature of a call option on the firm's total assets (Merton, 1974). The call option feature not only leads to a positive relationship between the equity value and firm risk but also leads to a negative relation between the firm's debt and the risk. Therefore, managers, who act in the interest of the firm's shareholders, will adopt risky projects as long as the gain in equity value exceeds the loss in debt value. Although creditors realize the risk-shifting incentive and charge higher interests, these protections are not efficient in eliminating the existence of risk-shifting in equilibrium (Dichev and Skinner 2002).

One of the firm policies that can directly reflect the risk preference is the firm's hedging. This paper attempts to shed the light on the effect of shareholder-creditor conflicts on corporate hedging. Building on Jiang, Li, and Shao (2010), we examine this question by using a unique context to evaluate both debt and equity holdings of a company by the same institutional investor (hereafter referred to as "dual holders"). Institutional dual-holders, due to their large stakes in both a firm's equity and debt, have strong incentives and abilities to internalize the conflicts of interest between the shareholders and creditors. By analyzing the extent to which the firms are owned by such dual-holders, we can find the shareholder-creditor conflicts' effect on the firm's hedging policy without directly measuring such conflicts, which is known to be very difficult.

To test the dual holders' effect on the firm's hedging policy. We use a similar approach as Jiang, Li, and Shao (2010) do. We examine the relationship between corporate hedging and whether the firms have dual-holders. We find that dual holders reduce the conflict between shareholders and creditors. We also find that the reduced conflict leads the dual holding firms to hedge more than the otherwise identical firms. In terms of economic significance, on average, the dual holding firms are 1.5% more likely to use financial instruments to hedge.

While the results of the Ordinary least squares (OLS) regression proves the existence of shareholder-creditor conflicts and dual holders' roles in corporate hedging. Nevertheless, it is difficult for us to provide a causal relationship between dual ownership and the corporate hedging policy. Specifically, the decision to become the dual-holder of the firm is endogenous. For instance, financial institutions may choose to become the firm's dual-holder if the firm hedges because this behavior protects the interests of debt-holders. To mitigate this concern, we follow the identification strategy developed by Chu (2018). Specifically, we implement a difference-in-differences (DID) regression based on the quasi-natural experiment of the mergers and acquisitions of the financial institutions that may create a plausibly exogenous variation for the dual-holders. We build the sample of mergers between lenders and shareholders as follows. First, we collect all mergers between financial institutions from the Statistics Data Corporation (SDC) Platinum Financial Securities Data. Second, we closely matched the acquirer and target names with the lenders' names in the SDC DealScan. We also match shareholders' names in Thomson Reuters 13F. Finally, we find companies that are borrowers of the merger lender and whose stock is held by the merged institutional shareholders. Following Chu (2018), we filter the samples below. We require institutional investors to hold more than 1% of all shares at the time of the merger. We also require lenders to allocate more than 10% of the loan at start-up. We treat these companies as the treated group. For each company in the treatment group, we found the closest controlling firm by matching firm size, leverage, price-to-book ratio, sales growth, ROA (return on assets), R&D (research on development), capital expenditure, firm age, tangibility, Non-debt tax shield (NDTS), Altman Z-Score. We also require that the controlling company had outstanding bank loans at the time of the merger.

We then perform the DID regression. We find that the treated firms hedge more than the control firms after the merger between their shareholders and creditors. In terms of the economic significance, we find that after the merger, the treated firms are 4.5%, 4.9%, and 3.9% more likely to use financial derivatives to hedge against the overall risk, interest rate risk, and foreign exchange risk.

Just as with other DID regressions, We need to make sure that the main results are not driven by the differences between treated and control groups before the treatment. The parallel trends assumption states that, although treatment and comparison groups may have different levels of the outcome before the start of treatment, their trends in pre-treatment outcomes should be the same. We follow Chu (2018) to test the dynamics effect of the mergers and acquisitions between the shareholders and creditors on a firm's hedging policy. We find that the effects of mergers and acquisitions on the firm's hedging policy are statistically and economically insignificant before the mergers between the shareholder and creditor. Also, the effects of mergers and acquisitions on the firm's hedging policy are statistically and economically significant after the mergers between the shareholder and creditor. This result indicates that the main regression results are satisfied with the parallel trend assumption.

To find out the heterogeneity of the effect of reduced shareholder-creditor conflict on the firm's hedging policy, we also test whether the effect of the mergers and acquisitions is stronger for firms in financial difficulties. To test this, we create sub-samples based on the firm's Leverage ratio and run the DID regressions. We find that the positive and significant effects of the merger on the firm's hedging policy are stronger for firms in the higher Leverage sub-sample. This result further shows that the mergers affect the firm's hedging policy through the channel of reducing conflict between the shareholders and creditors.

Next, to further justify the validity of our results. We hand-collect the commodity-related hedge positions of firms from the oil and gas industry (SIC code: 1300-1399) for our sample period and re-run the DID regression by replacing the dummy dependent variable that indicates hedging activities with the notional value of the hedging contracts. We find that after the merger, the treated firms hedge more against the commodity-related risks. In terms of the economic significance, we find that after the merger, the notional value of the hedging contracts used by the treated firm increases 78.24% more than that of the control firms.

The literature has explored why firms hedge and provided several explanations. Bartram, Brown, and Fehle (2004) report that 65% of U.S. firms use derivatives to hedge, which indicates that hedging is one of the widely adopted firm policies that impact the overall firm risk. Nance, Smith, and Smithson (1993) find that firms that hedge face more convex tax functions have less coverage of fixed claims, have more growth options in their investment opportunity set, and employ fewer hedging substitutes. Campello, Lin, Ma, and Zou (2011) find that hedgers pay lower interest spreads and are less likely to have capital expenditure restrictions in their loan agreements. These favorable financing terms, in turn, allow hedgers to invest more. Haushalter (2000) finds that the likelihood of hedging is related to economies of scale in hedging costs and the basis risk associated with hedging instruments. Campbell and Kracaw (1990) state that hedging could decrease the agency's cost of finance by controlling the project risk. Chen and King (2014) find consistent results showing that hedging is associated with a lower cost of public debt. Lin, Phililips, and Smith (2008) also document a positive relation between *Leverage* and hedging. Aretz and Bartram (2010) try to link the agency conflict to hedging policy and point out that the complex relations between hedging and other corporate policies make it difficult to examine the agency theories. Nevertheless, direct empirical evidence on the impact of agency conflict on hedging is still understudied.

One potential reason for lacking direct empirical evidence is the difficulty to measure the conflict between shareholders and creditors. From another perspective, we contribute by investigating the relation between dual ownership which directly measures shareholder-creditor conflict, and hedging.

Our paper also contributes to the effect of dual holders literature. Cheng, Cheng, Weng, and Yan (2021) find that such firms are less likely to provide management forecasts and disclose fewer voluntary 8-K items. Jiang, Li, and Shao (2010) suggest that syndicated loans with dual holders' participation have bank loan interests that are 18–32 bps lower than those without. Yang (2019) finds that firms held by dual-holders generate fewer but more valuable patents. Chu (2018) finds that firms have conservative payout policies when the conflict between shareholders and creditors is reduced. Chava, Wang, and Zou (2018) find that firms with dual ownership are less likely to have capital expenditure restrictions in loan contracts, and the relationship varies in predicted ways between the monitoring needs of borrowers and the monitoring capacity of dual owners.

The rest of the paper is structured as follows. Section 2 develops our main hypothesis. Section 3 describes the data sample and our identification strategy. Section 4 presents our main results. Section 5 reports our notional value results and Section 6 concludes.

2. Hypothesis

Hedging reduces the agency costs between shareholders and creditors and increases the firm value (e.g., Dobsen and Soenen, 1993). The literature provides some evidence of the negative relation between shareholder-creditor conflict and the hedging policy. Campbell and Kracaw (1990) find that hedging decreases agency costs by controlling the project risk. Chen and King (2014) find that hedging is associated with a lower cost of debt. On the other hand, hedging reduces the agency's cost by controlling the underinvestment incentive and by increasing the proportion of future states in which equity holders are the residual claimants (e.g., Bessembinder, 1991). Last, hedging increases the firm value by securing value-increasing changes in contracting terms with the creditors (e.g., Bessembinder, 1991). Therefore, to reduce the agency cost of debt and increase firm value, firms are more likely to hedge.

Institutional dual-holders, given their claims in both equity and debt of the company, have the incentive to serve as a commitment mechanism and mitigate the conflicts of interest between shareholders and creditors. If the institutions simultaneously hold significant positions in equity and debt, then these institutions will have incentives to monitor and prevent managers from opportunistic behaviors at the expense of creditors. Therefore, we should expect that the presence of significant dual ownership will (at least partly) reduce the conflict between shareholders and creditors. Consistent with this argument, Jiang, Li, and Shao (2010) discover that dual-holders exhibit longer investment horizons and stronger lending relationships with borrowing firms, indicating that they monitor intensively on both equity and debt sides. Therefore, firms with dual holders are more likely to hedge.

The above arguments lead to the following empirical predictions: firms are more likely to engage in hedging activities compared to similar firms without significant dual ownership.

Hypothesis 1: All else equal, firms with institutional dual-holders use more financial hedge instruments in general than those without dual-holders.

In addition, the risk-shifting incentive and underinvestment incentive are more pronounced for financially distressed firms (Jensen and Meckling, 1976). Eisdorfer (2008) finds evidence of risk-shifting behavior in financial distress firms. Due to the higher level of conflict between shareholders and creditors, the firms will face a higher agency cost of debt. Therefore, the impact of agency conflict on hedging is more pronounced for firms under financial distress. Accordingly, we have the following hypothesis:

Hypothesis 2: The effect of institutional dual-holders on hedging is more pronounced for financial distress firms.

3. Sample construction and the identification strategy

3.1 OLS regression sample construction

We construct our sample from companies that reported 10-K filings from 1994 to 2020. The sample starts from 1994 to 2020. We use this period because 1994 is the year that the DealSacan database's loan information became reliable. We use the same procedure as Jiang, Li, and Shao (2010) do to discover dual-holders. Wee obtain firm-year level financial variables from Compustat. Based on the above sample's Central Index Key (CIK) number, We identify firms' hedging behavior by examining the 10-K filings and the proxy statements from the Electronic Data Gathering, Analysis, and Retrieval (EDGAR) website. In particular, we perform a keyword search for derivatives uses. The keywords are listed in the Appendix. We then define the dummy variable, hedging, to be one in a given year for firms holding a hedging position or having a detailed description of their hedging policy in the year. The company holds derivatives for trading or speculating purpose and does not count as a hedger for the given year. To determine whether the company hold derivatives for trading purpose, we read each firm's 10-K forms. For instance, Target mentions that "During 2008, we hold certain 'pay floating' interest rate swaps with a combined notional amount of $3,125 million for cash proceeds of $160 million, which are classified within other operating cash flows in the Consolidated Statements of Cash Flows.

We had no derivative instruments designated for trading purposes." in its 2009 10-K form. This procedure produces a sample of 39,139 firm-year observations.

3.2 Variable definition and summary statistics

In addition to the dummy variable indicating whether the firm hedges, we use three other variables to measure the firm's hedging policy for different purposes. Interest rate hedging, foreign exchange hedging, and commodity hedging. Interest rate hedging is a dummy variable equal to 1 if the firm annually uses financial instruments to hedge interest rate risk but not for trading purposes, and zero otherwise. Foreign exchange hedging is a dummy variable equal to 1 if the company annually uses financial instruments to hedge currency exchange rate risk but not for trading purposes, and zero otherwise. A commodity hedge is a dummy variable that is equal to 1 if the firm annually uses financial instruments to hedge commodity price risk, but not for trading purposes, and zero otherwise.

We also include the following firm-level characteristics: firm size - natural log of total assets (AT), leverage - sum of current liabilities and long-term debt as measured by total assets (AT) (DLC+DLTT), market - book ratio - Market value of total assets (PRCC_F×CSHO-CEQ+AT) divided by book value of total assets (AT), ROA-Earnings before interest (EBITDA) divided by total assets (AT), R&D-R&D expenses (XRD) divided by Total Assets (AT), Capital Expenditure - Capital Expenditure (CAPX) divided by Total Sales (SALE), Non-Debt Tax Shield - Ratio of Depreciation (DP) to Total Assets (AT)), Altman's Z-score, Cash Holding Volume, Tangibility - Total Property, Plant and Equipment (PPENT) as measured by Total Assets (AT), and Company Age - the natural logarithm of the number of years a company has existed in Compustat.

Table 1 provides the summary statistics for variables used in our baseline OLS regressions. About 57% of firm-year observations report hedging activities, among which 45%, 38%, and 16% report interest rate, foreign exchange, and commodity hedging activities. These numbers are similar to those reported in Lin, Phillips, and Smith (2008) and Chen and King (2014)

3.2 Difference-in-differences regression sample construction

We construct samples for DID regression following Chu (2018). We start with all mergers between companies in the financial industry from 1998 to 2017. First, we use the 1998 consolidated sample because we need four years of data to consolidate the company's shareholders and creditors. We end the sample in 2017 because we need data for four years after the merger of the company's shareholders and creditors. The reason we ended 2017 is that for some companies like Walmart, 2017 was also fiscal 2016. Second, we collect the lender's information from DealScan. We then match the lender's name with the acquirer's name or the name of the target in the merger. to match the name of the acquirer. Specifically, we match not only the names of lenders directly involved in merger transactions but also the names of the parent companies of the lenders and acquirers. We also use business addresses from both databases to facilitate matching. After this step, we have in the DealScan database all mergers that the acquirer or target can match with the lender.

Third, we collect the names of institutional investors from Thomson Reuter's 13F database. After collecting the names of the institutional investors, we matched the names of the institutional investors with the acquirer and target names that were not matched in the previous step. As we did with the names in the DealScan database, we matched not only the names of the companies directly involved but also the names of their parent acquirers. After matching names, there were 511 mergers between lenders in the DealScan database and institutional investors in the Thomson Reuters 13F database.

Fourth, we identified treated companies. First, we identified all companies that had outstanding loans from the merger lender at the time of the merger. Next, we asked the merged institutional investors to hold company stock at the end of the quarter before the merger. Following Jiang, Li, and Shao (2010), we restrict lenders from holding more than 10% of the original loan and institutional shareholders from holding more than 1% of all outstanding shares of the company. For companies that have been identified as processing enterprises many times, we only keep the time of the company's first processing. Next, we removed all companies in the financial and utilities industries and companies that were missing key variables. In the end, the program provided us with 455 processed companies.

Then we use the following procedure to find the controlling company. First, we excluded all companies that had been consolidated. Second, we require the holding company to also have outstanding bank loans at the time of the merger. Third, we perform propensity score matching to identify a list of control firms. Specifically, we perform Probit regression to estimate propensity scores. The dependent variable for Probit regression is a dummy variable equal to 1 for treated firms and zero otherwise. Independent variables include company size, leverage, price-to-book ratio, sales growth, ROA, R&D, capital expenditures, company age, tangibility, non-debt tax shield, and Altman's Z-score. We included year-fixed effects in the Probit regression and clustered standard errors at the firm level. We report the marginal effects and standard errors of the Probit regression results in Table 2. After Probit regression, each observation receives a propensity score (p-score). Then, for each treated firm, we retain the closest control firm based on their propensity score. In addition, due to the limited number of control companies, to ensure better matching quality, we use one-to-one matching replacement. This matching process yielded a sample of 398 control firms.

In our DID regression, we use the window of t-4 to t+4, that is, four years before to four years after the mergers.

Table 3 provides the summary statistics for the sample used in the DID regression. that the statistics of *Hedge*, *Interest rate hedge*, *Foreign exchange hedge*, and *Commodity hedge* are similar to those presented in Table 1.

4. Main results

4.1 OLS results of the effect of the dual-holders on the corporate hedging

We first provide OLS regression results on the effect of dual-holders on the corporate hedging policies for all firms with DealScan loans outstanding.

We then estimate the following regression:

$$Y_{it} = \alpha_i + \alpha_t + \beta Dual_{it} + \gamma X_{it-1} + \varepsilon_{it}, \qquad (1)$$

where Y_{it} are dummy variables equal one if the firm-year uses the financial instrument to hedge but not for trading purposes, and zero otherwise; $Dual_{it}$ is a dummy variable equals one if the firm-year has a dual-holder, and zero otherwise; α_i is the firm fixed effects; $\alpha_t$is the year fixed effects. Following Petersen (2009), we cluster the standard errors at the firm level.

Table 5 provides the results from estimating Equation (1). All independent variables are lagged by one year. We provide results without/with firm characteristics in odd/even number columns. From columns (1) to column (2), we use *Hedge* as the dependent variable. In both columns, the coefficient estimates on $Dual_{it}$ are both statistically and economically significant, consistent with the argument that dual holders reduce the shareholder-creditor conflict and hence decrease firms' risk-taking incentives. In columns (3) and (4), we present the results for interest rate hedging. The coefficient estimates remain positive and statistically significant. The results for the exchange rate in columns (5) and (6) are similar. However, the coefficient estimates on *Dual* for commodity hedging in columns (7) and (8) are small and statistically insignificant. Based on the estimate in column (2), firms with dual-holders are 1.5% more likely to use financial derivative instruments to hedge. Columns (4) and (6) suggest that firms with dual-holders are 2.6% and 1.9% more likely to use financial derivative instruments to hedge the firm's interest rate and foreign exchange risk.

4.1 Baseline difference-in-differences results

There is an obvious endogeneity problem in the OLS regression results. In particular, dual holding and hedging decisions may be driven by unobservable firm characteristics, such as risk management policies and firm financial policies. The results can also be driven by reverse causality, that is, firms that hedge is more likely to attract dual holders. To reduce this concern, we rely on mergers between shareholders and lenders to generate reasonable exogenous changes in dual holdings and examine the impact of mergers on firm hedging policies in DID settings.

When lenders and institutional investors in the same company merge, there is less conflict of interest between creditors and shareholders. Lenders often lend to multiple borrowers at the same time, and institutional shareholders often hold stock in hundreds of companies. Therefore, they are less likely to make merger decisions based on factors relevant to a small group of companies. Therefore, a merger of a lender with an institutional shareholder is likely to satisfy both the subordination and exclusion conditions.

To identify the effect of mergers between shareholders and creditors on the corporate hedging policy, we estimate the difference-in-differences specification as follows:

$$Y_{it} = \alpha_i + \alpha_t + \beta_1 Treat_i \times Post_{it} + \beta_2 \times Post_{it} + \gamma X_{it-1} + \varepsilon_{it}, \qquad (2)$$

Where Y_{it} are dummy variables that equal one if the firm-year uses the financial instrument to hedge but not for trading purposes, and zero otherwise. $Treat_i$ is a dummy variable equals one if the firm is treated, and zero otherwise. $Post_{it}$ is a dummy variable that equals one if the firm-year observation is after the announcement of the merger between the firm's shareholder and creditor. α_i is the firm fixed effects. $\alpha_t$is the year fixed effects. X_{it-1} is a vector of control variables including *Firm size*, *Leverage*, *Market-to-book ratio*, *Sales growth*, *ROA*, *R&D*, *Capital expenditure*, *Firm age*, *Tangibility*, *Non-debt tax shield*, and the *Altman's Z-score*. In the baseline DID regressions, $Treat_i$ is subsumed by the firm fixed effects. The coefficient estimate of $Treat_i \times Post_{it}$ captures the marginal effect of the mergers on the corporate hedging policy. We cluster the standard errors by the firm (Petersen, 2009).

We report the results of the DID regressions in Table 6. The odd and even number columns report the results without and with firm-level control variables. We estimate Equation (2) without and with control variables because the control variables may also be affected by the treatment. Similar to the OLS results in table 5, the DID coefficient estimates are all positive and statistically significant, except for columns (7) and (8) for commodity hedging.

Taking the estimate in column (2), treated firms are 4.5% more likely to hedge than control firms after the merger between the shareholders and creditors. Based on the results in columns (4) and (6), we find that treated firms are 4.9% and 3.9% more likely to hedge interest rate and foreign exchange rate risk than control firms after the merger. To the extent that the mergers are exogenous, these results suggest that dual holding is likely to have a causal effect on corporate hedging decisions.

4.2 Placebo test of hedging activities

In the hypothesis section, we suggest that If the institutions simultaneously hold significant positions in equity and debt, then these institutions will have incentives to monitor and prevent managers from opportunistic behaviors at the expense of creditors. In other words, dual-holding firms are more likely to hedge for risk managing purposes. To test this, we conduct a placebo test. Specifically, we run the regression for equation (2) by replacing the dependent variable with a dummy equal to one if the firm-year uses derivatives instruments for trading purposes. If the coefficient estimates of $Treat_i \times Post_{it}$ are statistically and economically insignificant, then we suggest that dual-holding firms are more likely to hedge for risk managing purposes. We provide the results in table 7.

From columns (1) to (8) of table 7, we find that the coefficient estimates of $Treat_i \times Post_{it}$ are statistically and economically insignificant, suggesting that dual-holding firms are more likely to hedge for risk managing purposes. In other words, the reduced conflict between shareholders and creditors affects the firm's hedging behavior through the risk-managing channel.

4.3 Addressing identification challenges

A common concern for DID estimation is that the results could be driven by systematic differences between treated and control firms, that is, the outcome variables, hedging policy in this case, for treated and control firms may have different trends in the absence of treatment. To mitigate this concern, we conduct an event study to examine the timing of the effect. We use the sample of treated and control firms fives years before the merger between shareholder and creditor and five years after the merger between shareholder and creditor (t-5 to t+5). Specifically, we estimate the following specification,

$$Y_{it} = \alpha_i + \alpha_t + \alpha_j + \sum_{k=-4}^{k=4} \beta_k Treat \times Year_k + \gamma X_{it-1} + \delta Z_{ct} + \varepsilon_{it}\ , \qquad (3)$$

Where Y_{it} is the dummy variable for corporate hedging; $Year_k$ is a dummy variable equal to one if the observation is k years after the merger between the firm's shareholder and creditor, and zero otherwise; α_i is the firm fixed effects; α_t is the year fixed effects; and α_j is the merger deal year fixed effects. In this equation, $\beta_k{}'s$ capture the difference between the effect of the merger between shareholders and creditors on corporate hedging policy in year k and the effect of the merger between shareholders and creditors on corporate hedging policy in four years before the merger between the firm's shareholders and creditors. If the baseline DID results are indeed driven by the mergers, instead, the coefficient estimates of $Treat \times Year_k$ should be close to zero for k less than zero, and become positive for k greater than zero. Nevertheless, if our baseline DID results are driven by the pre-existing differences between the treated and control firms, the effect is likely to show up before the merger (k less than zero).

We plot the coefficient estimates and their corresponding 95% confidence intervals in Figure 1. The coefficient estimates on $Treat \times Year_k$ are small and statistically insignificant before the mergers between shareholders and creditors. After the merger, the coefficient estimates on $Treat \times Year_k$ becomes greater and statistically significant. These results suggest that the baseline DID estimation is unlikely to be driven by the pre-existing differences between the treated and control firms.

4.4 Financial distress and the effects of the mergers

When a company is in financial distress, the conflict of interest between shareholders and creditors intensifies. Therefore, aligning the interests of shareholders and creditors through mergers should have a greater impact on companies that are already in financial distress. In this section, we test this conjecture to further support the argument that the main outcome is driven by a reduction in conflict of interest between shareholders and creditors.

We split the DID sample based on whether the firm's *Leverage* immediately before the merger is above or below the industry (FF48)-year median. We then re-estimate Equation (2) on those two sub-samples and present the results in table 8. Panels A and B report the results for using the high-*Leverage* and low *Leverage* sub-samples. For the high

Leverage sub-sample in panel A, except for column (8), all the coefficient estimates on $Treat_i \times Post_{it}$ are positive and significant. In contrast, all the coefficient estimates on $Treat_i \times Post_{it}$ are small and statistically insignificant for the low *Leverage* sub-sample. Also, the difference in coefficient estimates in column (1) vs (2) is statistically significant. In terms of economic significance, the treated firms that have above-median *Leverage* are 5.8% more likely to hedge than the treated firms that have below-median *Leverage* after the merger between shareholders and creditors.

In panel B, we report the sub-sample results divided by the industry-year median of *Distance to Default*. We construct the *Distance to Default* following Bharath and Shumway (2008). We find that the coefficient estimate of $Treat_i \times Post_{it}$ is positive and statistically significant for the low *Distance to Default* sub-sample. The difference between coefficient estimates in low and high sub-samples is statistically significant. For economic significance, the treated firms that have an above-median *Distance to Default* are 4.3% less likely to hedge than the treated firms that have a below-median *Distance to Default* after the merger between shareholders and creditors.

Overall, the table shows that when a company is in financial distress, the alignment of shareholder and creditor interests has a stronger effect on mitigating shareholder-creditor conflict. The results thus support the argument that mergers influence firm hedging policies through their effects on shareholder-creditor conflicts.

5. The effect of the merger on the national value of hedge

In previous sections, we use dummy variables to capture the extensive margin of firms' hedging policy. These variables are, however, unable to capture the intensity of hedging. In this section, we follow Bakke et al. (2016) to construct a quantitative measure of the firm's hedging policy-*Notional value of hedging contracts*. Because we have to hand collect the notional values of the hedging contracts, we only focus on firms in the oil and gas industry in this analysis. Considering the labor and time efficiency, we mainly focus on the effect of dual-holders on *Notional value* for the firms in the oil and gas industry-an industry regularly using financial instruments to hedge the firm's commodity price risk. To collect this data, first, we get all the oil and gas firms (SIC code:13XX) from the sample used to test equation (2) from 1998 to 2017. We choose SIC 13XX firms, as these firms are relatively homogeneous in their exposure to commodity prices and use similar hedging strategies (see Jin and Jorion, 2006). These industry characteristics help minimize the problems of omitted variables or spurious correlations, or both, that we would face if we did a cross-industry study or focused on a more complex heterogeneous industry.

To quantify hedging behavior, we hand-collect financial derivatives positions and operational hedging contracts from 10-K, 10-Q, and Proxy statement filings on the SEC EDGAR System. Firms usually disclose derivative positions in item 7A of 10-K (sometimes disclose this information in other items). In the oil and gas industry, firms typically report their use of oil and gas derivative contracts clearly (most times in tabulated format). Firms also report fixed price delivery operational hedging contracts in item 7A and management footnotes. We collect the contract type (forward, future, call, put, swap, etc.), amount sold in the future (firms sometimes provide these figures on a per-day basis and sometimes in aggregate), and price of the commodity in the agreement. After we collect that information, we multiply the price of the commodity by the number of contracts outstanding in the fiscal year to get the *Notional value of hedging contracts*.

5.2 The effect of the merger on the notional values of hedging contracts

Once we get the *Notional value of hedging contracts*, we run the regression on two different levels-contract-year level and the firm-year level. First, we provide the summary statistics of the sample for the test of mergers between shareholders and creditors on the *notional value of hedging contracts* in table 9. Panel A reports the summary statistics for the contract-year level. Panel B reports the summary statistics for the firm-year level. Based on panel A, we find that 409 commodity hedge contracts are outstanding for oil and gas firms from 1998 to 2017 including forward, future, call, put, swap, etc. The mean of the contract's *Notional value* is 12,259.24 thousand dollars. When we aggregate our data on the firm-year level. We find 105 firm years have outstanding hedging contracts, the average *Notional value* on the firm-year level is 72,335.86 thousand dollars.

Next, we perform the DID regressions for the effect of mergers between shareholders and creditors on the *notional value of hedging contracts*. Specifically, we re-run equation (2) by replacing the dummy dependent variables with the *notional value of hedging contracts* divided by the firm's total assets. We report the results in table 10. Panels A and B report the results on the contract-year level and firm-year level.

We find that the coefficient estimates of $Treat_i \times Post_{it}$ are statistically significant in columns (2) of panels A and B. In terms of economic significance, take the column (4) of panel B as an example, the contract used by the firm that experiences a merger between shareholders and creditors has a notional value almost 50% higher than the contract used by the firm that does not experience a merger between shareholders and creditors.

Overall, the results of this table are consistent with those of the previous tables, suggesting that the reduced conflict of interest between shareholders and creditor motivates firms to engage more in hedging activities.

6. Conclusion

This paper examines the impact of shareholder-creditor conflict on a firm's hedging policy. Using mergers between corporate shareholders and creditors as an exogenous shock, we find a positive causal relationship between shareholder-creditor conflict reduction and corporate hedging behavior. Specifically, we find that treated firms that experience shareholder and creditor consolidation are not only more likely to hedge using financial instruments, but also hedge more in terms of the notional value of the hedge contract. Consistent with the argument that shareholder-creditor conflicts are often exaggerated when firms are in financial distress, we find that the impact on financial distress firms is stronger.

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Appendix: Keyword list
Interest rate derivatives:
"interest rate swaption" or "interest rate futures" or "interest rate option" or "interest rate agreement" or"forward rate agreement" or "interest rate floor" or "basis swap" or "Interest rate derivative" or "Interest rate hedging" or "Interest rate swap" or "Interest rate contract" or "Interest rate cap" or "Interest rate collar" or "Interest rate protection" or "Interest rate lock" or "Interest rate forward" or "Hedge interest rate risk using derivative" or "Mitigate our interest rate risk" or "Mitigates its interest rate risk" or "Mitigate interest rate risk" or "Manage our interest rate risk" or "Manage its interest rate risk" or "Manage interest rate risk" or "Hedge interest rate risk" or "Hedge our interest rate risk" or "Hedge its interest rate risk"

and not "Does not use interest rate derivative" and not "Does not utilize interest rate derivative" and not "Did not have any interest rate swap" and not "No interest rate derivative" and not "No interest rate swap" and not "Did not have any interest rate derivative" and not "Did not have any interest rate contract" and not "Does not hedge its interest rate risk" and not "Does not utilize interest rate contract" and not "Does not use any derivative contracts to hedge its interest rate risk" and not "No material interest rate risk" and not "Does not use derivative financial instruments, such as interest rate swap" and not "No open interest rate derivatives" and not "Manages its interest rate risk exposure by maintaining a mix of" and not "Manages interest rate risk exposure by maintaining a mix of" and not "Interest rate hedging master agreement" and not "Means any interest rate swap" and not "Do not use interest rate derivative" and not "The company may enter into certain foreign currency and interest rate derivative" and not "The company may enter into interest rate derivative" and not "The company may enter into interest rate swap" and not "The company may also enter into certain foreign currency and interest rate derivative" and not "The company may also enter into interest rate derivative" and not "The company may also enter into interest rate swap" and not "No outstanding currency swap, interest rate derivative" and not "Liabilities under interest rate swap" and not "Changes in fair value of interest rate swap" and not "No interest rate contract" and not "Termination of interest rate swap"
and not "Termination of related interest swap" and not "Termination of an interest rate swap" and not "no open interest rate derivative" and not "it is not the company policy to enter into derivative financial instruments" and not "it is not the company's policy to enter into derivative financial instruments"

Foreign exchange hedge:
"currency derivative" or "currency futures" or "currency contract" or "exchange forward" or "exchange future" or "exchange swap" or "exchange option" or "exchange contract" or "forward exchange contract" or "exchange agreement" or "currency forward" or "currency option" or "currency rate hedge" or
"foreign exchange forward" or "exchange rate contract" or "foreign exchange derivative" or "foreign exchange contract" or "foreign exchange rate contract" or "forward foreign exchange" or "exchange rate derivative" or "forward currency exchange contract" or "currency swap" or "cross-currency swap" or
"foreign currency hedge contract" or "manage its currency risk" or "manage currency risk" or "manage our currency risk" or "manage its exchange rate risk" or "manage our exchange rate risk" or "manage exchange rate risk" or "hedges its exchange rate risk" or "hedge our exchange

rate risk" or "hedge exchange rate risk" or "foreign currency exchange rates and utilize derivatives" or "forward contract"

and not "no currency forward" and not "no currency option" and not "no foreign exchange forward" and not "no exchange rate contract" and not "no foreign exchange derivative" and not "no foreign exchange contract" and not "no foreign exchange rate contract" and not "no forward foreign exchange" and not "no exchange rate derivative" and not "no foreign currency exchange rate" and not "no currency swap" and not "no cross-currency swap" and not "no foreign currency hedge contract" and not "does not have any exchange rate derivative" and not "does not have currency forward" and not "does not manage our currency risk" and not "does not have any currency derivative" and not "does not have any outstanding foreign exchange derivative" and not "does not have any outstanding exchange rate contract" and not "does not have any outstanding foreign currency forward contract" and not "does not utilize currency derivative" and not "does not use currency derivative" and not "does not utilize foreign currency derivative" and not "does not utilize currency forward" and not "no material exchange rate risk" and not "but continues to monitor the effects of foreign currency exchange rate" and not "no outstanding commodity derivatives, currency swap" and not "no outstanding interest rate derivatives, currency swap" and not "no outstanding interest rate derivatives, foreign exchange contract" and not "not directly subject to foreign currency exchange rate fluctuations" and not "not subject to foreign currency exchange rate fluctuations" and not "do not engage in forward foreign exchange" and not "no foreign currency forward contract" and not "does not currently have any significant foreign currency exposure" and not "it is not the company policy to enter into derivative financial instruments"
and not "it is not the company's policy to enter into derivative financial instruments"

Commodity hedge:
"commodity futures" or "commodities future" or "commodity option" or "commodity derivative" or
"commodity swap" or "commodity swaption" or "commodity agreement" or "derivative commodity instrument" or "manage commodity price risk" or "hedge commodity price" or "manage fuel price risk" or
"hedge fuel price risk" or "natural gas option" or "natural gas swap" or "crude oil hedge" or "oil futures" or "oil contract" or "jet fuel forward" or "gold contract" or "commodity forward" or "manage exposure to fluctuation in commodity prices" or "manage exposure to fluctuations in commodity prices" or "manage exposure to changes in commodity prices" or "manage exposure to change in commodity prices" or "manage electricity cost" or "aluminum forward" or "natural gas forward" or"utilizes commodity futures and options" or "diesel fuel hedge contract" or "fuel hedge"

and not "no commodity futures" and not "no commodities future" and not "no commodity option" and not "no derivative commodity instrument" and not "does not hedge its commodity price risk" and not "do not use any commodity derivative" and not "does not have any commodity derivative outstanding" and not "does not have material commodity price risk" and not "no commodities future contract" and not "has not used derivative commodity instruments" and not "manages commodity price risk through negotiated supply contract" and not "manages commodity price risk through supply contract" and not "manages commodity price risks through negotiated supply contract" and not "manages commodity price risks through supply contract" and not "no outstanding commodity derivative" and not "does not use financial instruments to hedge commodity prices" and not "we do not hold or issue derivatives, derivative commodity instruments" and not "company has not entered into any transactions using derivative financial instruments or derivative commodity instruments" and not "does not use derivative commodity instrument" and not "we do not use a derivative or other financial instruments or derivative commodity instruments to hedge" and not "not utilize derivative financial instruments, derivative commodity instrument" and not "not utilize derivative commodity instrument" and not "it is not the company policy to enter into derivative financial instruments" and not "it is not the company's policy to enter into derivative financial instruments"

Table 1 Summary Statistics

We construct the sample to regress the firm's hedging policy on the dummy variable Dualit, which equals 1 if the firm has dual holders for the year. This sample is from 1994 to 2020. Hedging is a dummy variable equal to 1 if the firm-year uses the financial instrument for hedging but not for trading purposes, and zero otherwise. Interest rate hedging is a dummy variable equal to 1 if the company annually uses financial instruments to hedge interest rate risk rather than for trading purposes, and zero otherwise. Foreign exchange hedging is a dummy variable equal to 1 if the firm annually uses financial instruments to hedge exchange rate risk rather than for trading purposes, and zero otherwise. Commodity hedging is a dummy variable that is 1 if the firm annually uses financial instruments to hedge commodity price risk, but not for trading purposes, and 0 otherwise. Dual is a dummy variable equal to 1 if the company year has dual holders, and zero otherwise. We also include control variables such as company size - natural log of total assets (AT), leverage - the sum of current liabilities and long-term debt as measured by total assets (AT) (DLC+DLTT), market pair - book ratio - total Market value of assets (PRCC_F×CSHO-CEQ+AT) divided by book value of total assets (AT), ROA - earnings before interest (EBITDA) divided by total assets (AT), R&D - research and development expenses (XRD) divided by Total Assets (AT), Capital Expenditure - Capital Expenditure (CAPX) divided by Total Sales (SALE), Non-Debt Tax Shield - Ratio of Depreciation (DP) to Total Assets (AT), Altman's Z Score, Cash Holdings, Tangibility - total property, plant and equipment (PPENT) as measured by total assets (AT), and company age - the natural logarithm of the number of years a company has existed in Compustat.

Variables	N	Mean	STD	P25	P50	P75
Hedge	39,139	0.572	0.413			
Interest rate hedge	39,139	0.452	0.484			
Foreign exchange hedge	39,139	0.382	0.409			
Commodity hedge	39,139	0.156	0.409			
Dual	39,139	0.471	0.499			
Firm size	39,139	7.030	1.851	5.725	6.947	8.252
Leverage	39,139	0.231	0.186	0.080	0.194	0.343
M/B	39,139	1.667	0.957	1.086	1.363	1.878
ROA	39,139	0.110	0.095	0.061	0.110	0.161
R&D	39,139	0.017	0.045	0.000	0.000	0.007
Capex	39,139	0.099	0.213	0.015	0.034	0.079
Non debt tax-shield	39,139	0.042	0.033	0.021	0.036	0.055
Altman's z	39,139	3.463	3.009	1.511	2.847	4.443
Cash	39,139	0.070	0.114	0.009	0.026	0.078
Tangibility	39,139	0.285	0.257	0.071	0.208	0.452
Firm age	39,139	16.729	10.000	8.000	16.000	24.000

Table 2-Probit Regression Results

The dependent variable for Probit regression is a dummy variable equal to 1 for the companies being treated and zero otherwise. Independent variables include company size, leverage, price-to-book ratio, sales growth, ROA, R&D, capital expenditures, company age, tangibility, non-debt tax shield, and Altman's Z-score. We included year-fixed effects in the Probit regression and clustered standard errors at the firm level. We report the marginal effects and standard errors of Probit regression in Table 2 (in parentheses). *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively. We report marginal effects on top and robust standard errors in parentheses.

	Merger
	(1)
Firm size	-0.040***
	(0.033)
Leverage	-0.011
	(0.299)
Market-to-book	-0.007
	(0.054)
Sales growth	0.001***
	(0.001)
ROA	-0.036
	(0.580)
R&D	0.154
	(0.703)
Capex	-0.012
	(0.050)
Firm age	0.038*
	(0.091)
Tangibility	0.054
	(0.211)
Non-debt tax shield	-0.759*
	(1.718)
Altman's Z	-0.002
	(0.018)
Year fixed effects	Yes
No. of Observations	4,661
Pseudo R-squared	0.145

Table 3-Summary Statistics for the DID Sample

Table 3 provides summary statistics for the difference in difference (DID) samples. Hedging is a dummy variable equal to 1 if the firm annually uses the financial instrument for hedging but not for trading purposes, and zero otherwise. Interest rate hedging is a dummy variable equal to 1 if the company annually uses financial instruments to hedge interest rate risk rather than for trading purposes, and zero otherwise. Foreign exchange hedging is a dummy variable equal to 1 if the firm annually uses financial instruments to hedge exchange rate risk rather than for trading purposes, and zero otherwise. Commodity hedging is a dummy variable that is 1 if the firm annually uses financial instruments to hedge commodity price risk, but not for trading purposes, and 0 otherwise. Treat is a dummy variable equal to 1 for the company being treated. Post is a dummy variable that equals 1 if combined.

Variables	N	Mean	Standard deviation	P25	Median	P75
Hedge	7,122	0.370	0.483			
Interest rate hedge	7,122	0.285	0.452			
Foreign exchange hedge	7,122	0.235	0.424			
Commodity hedge	7,122	0.072	0.258			
Treat	7,122	0.525	0.484			
Post	7,122	0.527	0.499			
Firm size	7,122	6.377	1.784	5.202	6.339	7.433
Leverage	7,122	0.335	0.234	0.164	0.315	0.453
M/B	7,122	1.570	0.867	1.062	1.293	1.733
ROA	7,122	0.126	0.093	0.080	0.125	0.175
R&D	7,122	0.011	0.033	0.000	0.000	0.003
Capex	7,122	0.102	0.197	0.018	0.039	0.086
Non debt tax-shield	7,122	0.046	0.031	0.026	0.041	0.058
Altman's z	7,122	3.308	5.647	1.809	2.822	4.034
Cash	7,122	0.070	0.114	0.009	0.026	0.078
Tangibility	7,122	0.334	0.253	0.123	0.274	0.514
Firm age	7,122	2.523	0.776	1.946	2.639	3.219

Table 4-Univariate Comparison

Table 4 provides univariate comparisons between treatment and control companies. Hedging is a dummy variable equal to 1 if the firm annually uses the financial instrument for hedging but not for trading purposes, and zero otherwise. Interest rate hedging is a dummy variable equal to 1 if the company annually uses financial instruments to hedge interest rate risk rather than for trading purposes, and zero otherwise. Foreign exchange hedging is a dummy variable equal to 1 if the firm annually uses financial instruments to hedge exchange rate risk rather than for trading purposes, and zero otherwise. Commodity hedging is a dummy variable that is 1 if the firm annually uses financial instruments to hedge commodity price risk, but not for trading purposes, and 0 otherwise. Treat is a dummy variable equal to 1 for the company being treated. Post is a dummy variable that equals 1 if combined.

	Treat firms		Control firms		Diff	
	N=3,739		N=3,383			
	Mean	Median	Mean	Median	Mean	Median
Hedge	0.385	0.000	0.346	0.000	0.039	0.000
Interest rate hedge	0.299	0.000	0.263	0.000	0.036	0.000
Foreign exchange hedge	0.242	0.000	0.224	0.000	0.018	0.000
Commodity hedge	0.065	0.000	0.084	0.000	-0.019	0.000
Post	0.533	1.000	0.517	1.000	0.017	0.000
Firm size	6.275	6.227	6.543	6.559	-0.268	-0.331
Leverage	0.336	0.313	0.333	0.317	0.003	-0.004
M/B	1.583	1.289	1.548	1.297	0.036	-0.008
ROA	0.126	0.123	0.126	0.127	0.000	-0.005
R&D	0.010	0.000	0.013	0.000	-0.002	0.000
Capex	0.098	0.038	0.108	0.042	-0.010	-0.004
Non debt tax-shield	0.044	0.039	0.048	0.043	-0.004	-0.004
Altman's z	3.420	2.866	3.120	2.718	0.300	0.148
Cash	0.068	0.025	0.074	0.029	-0.006	-0.005
Tangibility	0.327	0.271	0.345	0.284	-0.018	-0.013
Firm age	2.445	2.485	2.652	2.890	-0.207	-0.405

Table 5-OLS Regression Result

Table 5 provides the results of the regression firm's hedging policy on the dummy variable Dualit, which is equal to 1 if the firm-year has dual holders. This sample is from 1994 to 2020. Hedging is a dummy variable equal to 1 if the firm-year uses the financial instrument for hedging but not for trading purposes, and zero otherwise. Interest rate hedging is a dummy variable equal to 1 if the company annually uses financial instruments to hedge interest rate risk rather than for trading purposes, and zero otherwise. Foreign exchange hedging is a dummy variable equal to 1 if the firm annually uses financial instruments to hedge exchange rate risk rather than for trading purposes, and zero otherwise. Commodity hedging is a dummy variable that is 1 if the firm annually uses financial instruments to hedge commodity price risk, but not for trading purposes, and 0 otherwise. Dual is a dummy variable equal to 1 if the company year has dual holders, and zero otherwise.

	Hedge	Hedge
	(1)	(2)
Dual	0.033***	0.015*
	(0.008)	(0.008)
Firm size		0.068***
		(0.007)
Leverage		0.101***
		(0.031)
M/b		0.004
		(0.006)
Sales growth		-0.001
		(0.006)
ROA		0.099**
		(0.045)
R&D		-0.031
		(0.178)
Capex		-0.022
		(0.020)
Firm age		-0.042*
		(0.022)
Tangibility		0.022
		(0.043)
Non-debt tax shield		-0.161
		(0.174)
Altman'z		-0.007***
		(0.002)
Cluster	Firm	Firm
Firm fixed effects	Yes	Yes
Year fixed effects	Yes	Yes
No. of observations	45,421	39,139
Adjusted R-squared	0.504	0.514

Table 6-DID Regression Results

Table 6 provides the results of DID regression. Hedging is a dummy variable equal to 1 if the firm annually uses the financial instrument for hedging but not for trading purposes, and zero otherwise. Interest rate hedging is a dummy variable equal to 1 if the company annually uses financial instruments to hedge interest rate risk rather than for trading purposes, and zero otherwise. Foreign exchange hedging is a dummy variable equal to 1 if the firm annually uses financial instruments to hedge exchange rate risk rather than for trading purposes, and zero otherwise. Commodity hedging is a dummy variable that is 1 if the firm annually uses financial instruments to hedge commodity price risk, but not for trading purposes, and 0 otherwise. Treat is a dummy variable equal to 1 for the company being treated. Post is a dummy variable that equals 1 if combined.

	Hedge	Hedge
	(1)	(2)
Treat*post	0.047**	0.045*
	(0.022)	(0.024)
Post	0.025	-0.016
	(0.021)	(0.021)
Control variables	No	Yes
Year fixed effects	Yes	Yes
Firm fixed effects	Yes	Yes
No. of observations	7,677	7,122
Adjusted R-squared	0.591	0.623

Table 7-Placebo Test

Table 7 provides the results of DID regression. Hedging is a dummy variable equal to 1 if the firm annually uses the financial instrument for trading purposes, and zero otherwise. Interest rate hedging is a dummy variable that is 1 if the company annually trades in interest-related financial instruments, and 0 otherwise. FX hedging is a dummy variable that is 1 if the company annually uses FX-related financial instruments for trading purposes, and 0 otherwise. Commodity hedging is a dummy variable that is 1 if the company year trades using commodity-related financial instruments, and 0 otherwise. Treat is a dummy variable equal to 1 for the company being treated. Post is a dummy variable that equals 1 if combined. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively. We report robust standard errors in parentheses.

	Hedge	Hedge	Interest rate hedge	Interest rate hedge	Foreign exchange hedge	Foreign exchange hedge	Commodity hedge	Commodity hedge
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treat*post	0.034	0.037	0.034	0.044*	0.029	0.033	-0.001	0.013
	(0.022)	(0.024)	(0.022)	(0.024)	(0.020)	(0.022)	(0.013)	(0.014)
Post	0.004	-0.010	-0.003	-0.027	-0.006	-0.012	0.001	-0.011
	(0.020)	(0.021)	(0.022)	(0.024)	(0.019)	(0.021)	(0.014)	(0.014)
Control variables	No	Yes	No	Yes	No	Yes	No	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
No. of observations	7,677	7,122	7,677	7,122	7,677	7,122	7,677	7,122
Adjusted R-squared	0.558	0.584	0.498	0.515	0.514	0.552	0.485	0.505

Table 8-Sub-sample *Leverage* and distance to default

Table 8 provides subsample results for DID regression. Panel A is the high-leverage and low-leverage subsamples. Panel B is the high and low distances to the default subsample. We build the default distance following the KMV-Merton model. Hedging is a dummy variable equal to 1 if the firm annually uses the financial instrument for hedging but not for trading purposes, and zero otherwise. Interest rate hedging is a dummy variable equal to 1 if the company annually uses financial instruments to hedge interest rate risk rather than for trading purposes, and zero otherwise. Foreign exchange hedging is a dummy variable equal to 1 if the firm annually uses financial instruments to hedge exchange rate risk rather than for trading purposes, and zero otherwise. Commodity hedging is a dummy variable that is 1 if the firm annually uses financial instruments to hedge commodity price risk, but not for trading purposes, and 0 otherwise. Treat is a dummy variable equal to 1 for the company being treated. Post is a dummy variable that equals 1 if combined. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively. We report robust standard errors in parentheses.

Panel A	Hedge			Interest rate hedge			Foreign exchange hedge			Commodity hedge		
Leverage	Low	High	Diff	Low	High	Diff	Low	High	Diff	Low	High	Diff
	(1)	(2)	(1) vs (2)	(3)	(4)	(3) vs (4)	(5)	(6)	(5) vs (6)	(7)	(8)	(7) vs (8)
Treat*post	-0.000	0.058*	0.058**	0.035	0.066*	0.031	-0.006	0.066**	0.072**	0.001	-0.011	-0.012
	(0.034)	(0.034)	(0.024)	(0.034)	(0.034)	(0.036)	(0.031)	(0.027)	(0.032)	(0.25)	(0.013)	(0.011)
Post	-0.014	-0.035		-0.078**	-0.044		0.011	-0.013		0.023	0.018	
	(0.030)	(0.029)		(0.033)	(0.031)		(0.028)	(0.028)		(0.020)	(0.019)	
Control variables	Yes	Yes		Yes	Yes		Yes	Yes		Yes	Yes	
Year fixed effects	Yes	Yes		Yes	Yes		Yes	Yes		Yes	Yes	
Firm fixed effects	Yes	Yes		Yes	Yes		Yes	Yes		Yes	Yes	
No. of observations	3,226	3,756		3,226	3,756		3,226	3,756		3,226	3,756	
Adjusted R-squared	0.654	0.630		0.533	0.573		0.633	0.542		0.504	0.545	
Panel B	Hedge			Interest rate hedge			Foreign exchange hedge			Commodity hedge		
Default to distance	Low	High	Diff	Low	High	Diff	Low	High	Diff	Low	High	Diff
	(1)	(2)	(1) vs (2)	(3)	(4)	(3) vs (4)	(5)	(6)	(5) vs (6)	(7)	(8)	(7) vs (8)
Treat*post	0.052**	0.009	-0.043*	0.017	0.034	0.017	0.065***	0.017	-0.048*	0.008	-0.019	-0.027
	(0.023)	(0.025)	(0.023)	(0.024)	(0.026)	(0.017)	(0.023)	(0.024)	(0.015)	(0.014)	(0.023)	(0.037)
Post	-0.024	0.014		-0.006	-0.043		-0.003	0.031		0.008	0.027	
	(0.024)	(0.026)		(0.025)	(0.026)		(0.024)	(0.024)		(0.015)	(0.022)	
Control variables	Yes	Yes		Yes	Yes		Yes	Yes		Yes	Yes	
Year fixed effects	Yes	Yes		Yes	Yes		Yes	Yes		Yes	Yes	
Firm fixed effects	Yes	Yes		Yes	Yes		Yes	Yes		Yes	Yes	
No. of observations	3,298	3,666		3,298	3,666		3,298	3,666		3,298	3,666	
Adjusted R-squared	0.619	0.653		0.539	0.579		0.582	0.582		0.524	0.528	

Table 9 Summary statistics for the notional value sample

Table 9 provides summary statistics for the contract-level and firm-level notional value samples. Notional value is the notional amount ($000) of the hedged contract.

Variable	N	Mean	STD	P25	Median	P75
Notional value	105	72,335.860	74,110.350	4,598.574	28,736.000	164,304.000
Notional value/firm size	105	0.023	0.017	0.011	0.026	0.026
Treat	105	0.321	0.469	0.000	0.000	1.000
Post	105	0.596	0.493	0.000	1.000	1.000
Firm size	105	7.324	1.645	6.121	7.135	8.594
Leverage	105	0.292	0.162	0.168	0.267	0.385
Market-to-book	105	1.512	0.446	1.184	1.397	1.739
ROA	105	0.136	0.192	0.109	0.158	0.205
Capex/sales	105	0.785	0.466	0.462	0.712	1.011
Non debt tax shield	105	0.109	0.051	0.077	0.090	0.133
Altman's Z	105	1.437	1.561	0.622	1.405	2.152
Cash	105	0.024	0.036	0.003	0.010	0.032
Tangibility	105	0.859	0.053	0.832	0.866	0.897
Firm age	105	20.604	12.767	10.000	17.500	31.000

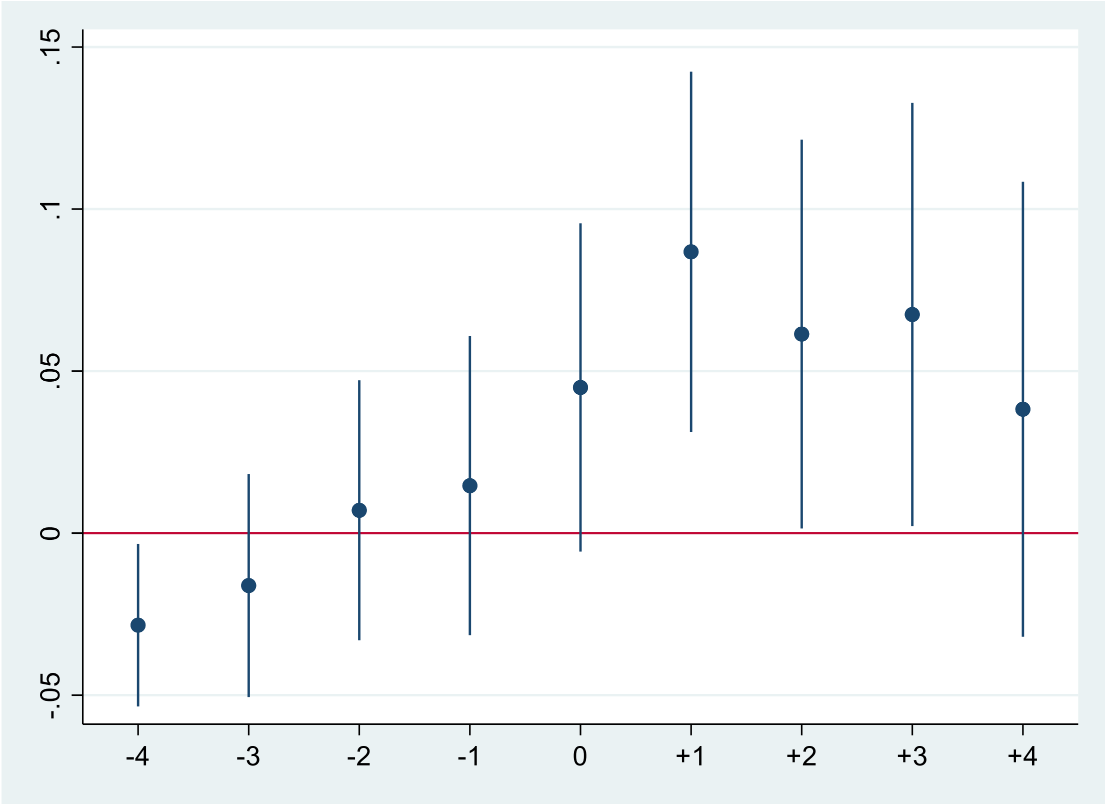
Table 10-Notional value regression results

Table 10 provides the regression results for contract-level and firm-level nominal values. Notional value is the notional amount ($000) of the hedged contract measured by total assets. Treat is a dummy variable equal to 1 for the company being treated. Post is a dummy variable that equals 1 if combined. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively. We report robust standard errors in parentheses.

Panel A.	Notional value/Total assets	Notional value/Total assets
Firm-level	(1)	(2)
Treat*Post	0.004	0.012*
	(0.006)	(0.007)
Post	0.003	-0.003
	(0.007)	(0.008)
Control variables	No	Yes
Year fixed effects	Yes	Yes
Firm fixed effects	Yes	Yes
No. of observations	108	105
Adjusted R-squared	0.668	0.669

Figure 1-Dynamic effects of hedging

We plot the regression results for equation (3). The coefficient estimates and 95% confidence interval of Treat×〖Year〗_k are displayed in this figure. We find that the coefficient estimates of Treat×〖Year〗_k are a little above or below 0.05 before the merger between shareholder and creditor takes place. After the merger, the coefficient estimates Treat×〖Year〗_k increase to approximately 0.1 and remain at a similar level afterward. These results suggest that the baseline DID estimation is unlikely to be driven by the pre-existing differences between the treated and control firms.



Conclusion

My dissertation studies the social impact of finance including payday loans, climate change, and corporate hedging policies. The first paper studies the impact of state-level payday lending regulations on property crimes in the United States. Consistent with the financial strain theory, evidence from the difference-in-differences regressions show that legalizing payday lending increases property crimes. On average, the agencies located in states allowing payday lending report 13.65% more property crimes than the agencies located in states not allowing payday lending do. Nevertheless, this impact does not hold for violent crimes because the effect is driven by the borrowers' financial pressure. In other words, payday lending increases property crimes mainly by financial distress.

In the second paper, we develop and test the hypothesis that firms manage the sea level rise risk through acquisitions. Using a comprehensive sample of publicly traded firms between 1986 and 2017, we find that in the cross-section, firms exposed to high SLR risk have a higher probability of becoming acquirers but a significantly lower probability of becoming targets. Also, we find that the market rewards acquisitions made by firms with high SLR risk exposure, as we observe a significant and positive relationship between the acquirers' cumulative announcement return and pre-merger SLR risk. We also find that this positive relation is more pronounced for firms with higher analyst coverage. Finally, we find that SLR-induced mergers tend to complete faster, and that post-merger, the combined firm experiences a greater increase in analyst coverage, forecast precision, and ESG score when the acquiring firm has a high SLR exposure before the merger.

The third paper examines the impact of shareholder-creditor conflict on a firm's hedging policy. Using mergers between corporate shareholders and creditors as an exogenous shock, we find a positive causal relationship between shareholder-creditor conflict reduction and corporate hedging behavior. Specifically, we find that treated firms that experience shareholder and creditor consolidation are not only more likely to hedge using financial instruments, but also hedge more in terms of the notional value of the hedge contract. Consistent with the argument that shareholder-creditor conflicts are often exaggerated when firms are in financial distress, we find that the impact on financial distress firms is stronger.