RICE UNIVERSITY

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A THESIS SUBMITTED
IN PARTIAL FULFILLMENT OF THE
REQUIREMENTS FOR THE DEGREE

Doctor of Philosophy

APPROVED, THESIS COMMITTEE

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ABSTRACT

Essays on Finance and Law

by

Mengming Michael Dong

This dissertation contains three chapters that study topics in financial economics and how finance affects legal outcomes. In the first chapter, I study how access to consumer litigation funding (CLF) impacts outcomes in the legal system. CLF provides cash advances to consumer plaintiffs in pursuing a tort complaint. Using exogenous variations in access to CLF resulting from staggered state law changes in the U.S., I find evidence that restricting CLF causes a decline in the number of tort lawsuits filed with courts by 18.7 percent. Such finding contrasts the theoretical predictions in the literature that CLF induces settlement, and the contrast could be due to the fact that, in reality, defendants hardly ever know whether the plaintiff is funded by CLF, which reduces the incentives of the defendants to settle. Hence, the incentives of the plaintiff to further fight the case due to less financial constraints could play a bigger role than the incentives of the defendants to settle. I also find that restricting CLF increases the proportion of liability rulings at trial that favor the plaintiffs by 5.9 percentage points but does not change the portion of lawsuits that go to trial. Overall, these findings suggest that access to CLF either incentivizes plaintiffs to file lawsuits that are less likely to be successful in the court or induces moral hazards of plaintiffs during the trial process.

In the second chapter, I examine the conflicting evidence in the finance literature

on whether the equity markets underreact or overreact to liquidity shocks. Using comprehensive stock-level news data, I find that the markets underreact to liquidity shocks, whether there is contemporaneous public news or not. Furthermore, when there is public news released contemporaneously, the price discovery process of liquidity shocks does not get any faster. In certain tests, the drift is actually significantly larger. This shows that even though public news reveals more information to investors and draws more investor attention, it does not help them incorporate liquidity shocks into prices. Such findings are consistent with the notion that liquidity level overall is rather difficult for an average investor to grasp. Information environment and investor inattention are not the market frictions that result in the markets' underreaction to liquidity shocks.

In the third chapter, my coauthors and I explore the impacts of early-life hardship experiences on corporate leaders' Corporate Social Responsibility (CSR) decisions and address this question by exploiting the Down-to-the-Countryside Movement in China from 1956 to 1978. This movement is mandatory and is an extreme early-life hardship for the people involved. We find that the chairmen of the board and CEOs who were sent to the countryside and mountains have significantly less CSR practice in their companies than their peers who were not sent. We argue that corporate leaders have less CSR practice because they believe that they have already suffered enough in life and have developed an in-depth aversion to social contribution. Such results are especially strong for chairmen in state-owned enterprises (SOEs) and CEOs in non-state-owned companies (non-SOEs) because chairmen have more decision power in SOEs and CEOs have more decision power in non-SOEs. Moreover, this negative association is aggravated by corporate leaders' college education and the corporate governance structure of the enterprise they lead.

Acknowledgments

I would like to thank my Heavenly Father, my Lord, my Savior, and my closest friend, Jesus Christ, for helping me walk through this Ph.D. journey. I hope that my talents and training can be used by Him as a good vessel to mentor students, help others, and glorify His Kingdom.

I would like to thank my family for their unconditional love, support, and encouragement during this Ph.D. process.

I am so grateful and honored to complete my dissertation under the mentoring and support of my dissertation chair, Dr. Gustavo Grullon. Dr. Grullon has been a great mentor to me and guided me not only in this dissertation but also through this profession. His help and encouragement have helped me greatly in becoming a good research scholar and teacher.

I am also very grateful to my dissertation committee member Dr. Yuhang Xing, who has been a great mentor to me as well. Dr. Yuhang Xing has given me lots of excellent guidance and support along the journey. I also thank my dissertation committee member Dr. Hülya Eraslan who has provided valuable advice and support.

The list of people who I want to thank is endless as there are so many other people who helped me greatly throughout this Ph.D. process. Even though your names have not been mentioned here, I sincerely thank you all for your generous and kind assistance.

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Consumers' Financial Constraints, Lawsuit Decisions, and the Civil Justice System

1.1 Introduction

Financial constraints, whether labeled liquidity or credit constraints, have a broad effect on the decisions of economic agents and play an essential role in various sectors of the economy. On the one hand, from a firm's perspective, financial constraints have multiple impacts on corporate policies, including investments (Almeida and Campello, 2007), cash holdings (Almeida, Campello, and Weisbach, 2004), capital structure (Leary, 2009), and innovation (Cornaggia et al., 2015). On the other hand, from a household's perspective, numerous studies have explored how credit or income constraints affect both economic and social decision-making, such as education (Johnson, 2013; Becker and Tomes, 1979, 1986; Mayer, 2019), marriage (Charles and Stephens, 2004), location of residence, driving (Giulietti, Tonin, and Vlassopoulos, 2018), labor supply (Crawford and Meng, 2011), consumption and savings (Agarwal and Qian, 2014; Hsieh, 2003; Jappelli and Pistaferri, 2010), debt holding (Duca and Rosenthal, 1993), and credit card usage (Gross and Souleles, 2002).

This paper focuses on how credit constraints affect a particular type of economic and social decisions, i.e., lawsuit decision. As an innovative form of credits to consumers, Consumer litigation funding (CLF), provides a unique setting to address this question. CLF provides non-recourse cash advances to consumer plaintiffs during tort[1] claims to smooth their daily consumption, and CLF companies only receive payment when consumers get compensations from the claims. CLF was initiated in the United States in the late 1990s, and legislatures have been debating on whether or not to regulate the industry. Legislatures continue the debate mainly because we know little about the implications of the CLF industry.

CLF plays a role in the litigation decisions of consumers during a tort claim in the following way. When an individual is involved in an injury, they usually file a claim with an insurance company[2], and it can take up to many years for these claims to be resolved. Also, in cases where the consumer loses vital income due to injuries or property damages, or have extensive medical expenses, they can become financially constrained. Legal costs are typically not part of the constraints, because if the consumer hires a lawyer for the claim, the legal fees are covered upfront by law firms. Thus, the main constraints stem from the daily consumption and medical expenses of the consumer since insurance will not typically help them cover those expenses upfront. As a result, the consumer may settle with the insurance company on a small amount in exchange for quick cash rather than further pursue the claim to its actual value. Indeed, many tort claims are settled out of court. Insurance companies, on the other hand, can wait as long as they prefer, since they have a

[1]A tort, in common law jurisdictions, is a civil wrong that causes a claimant to suffer loss or harm resulting in legal liability for the person who commits the tortious act.

[2]In rare cases, the plaintiff files a complaint to an individual defendant, but those cases are usually not funded by CLF.

giant financial backup. In other words, during the bargaining process between the plaintiff and the insurance company, the plaintiff is at a disadvantage due to financial constraints. That is one of the financial frictions that exist in the legal system, and such friction affects the socioeconomic decisions of both consumers and insurance companies.

Theoretically, however, the existing literature is unclear about whether the availability of CLF credit makes a consumer plaintiff more likely to bring the case to the court in equilibrium. On the one hand, when a consumer plaintiff has access to CLF, the consumer's needs for cash from the claim is less urgent. As a result, the plaintiff is more capable of pursuing the case to its full value, and the plaintiffs are then expected to be more likely to bring the case to court (Xiao, 2017a). On the other hand, the lawsuit negotiation is, in fact, a game among four parties: the plaintiff, the defendant, the plaintiff attorney, and the CLF funding company. A theoretical model built by Daughety and Reinganum (2014) predicts that when the plaintiff has private information about damages, the optimal plaintiff-funder CLF contract induces all plaintiff types to make the same demand in a pooling equilibrium, which induces full settlement. When the defendant has private information about the consumer's likelihood of being found liable, the likelihood of settlement is unaffected. Intuitively speaking, if the insurance company knows that the consumer is funded by CLF companies and could further pursue the case to its fullest, the insurance company would be motivated to settle early to avoid the extensive costs of going to courts and trials. With these two conflicting theories, whether access to CLF increases or decreases the likelihood of a consumer plaintiff bringing a tort claim to court remains an open question.

There are very few empirical studies regarding consumer litigation funding. Avraham and Sebok (2018) present statistical facts on the terms and pricing of CLF contracts. Xiao (2017b) documents that CLF increases medical malpractice claim settlement amounts and durations, and that the rates of bankruptcy drop due to access to CLF. None of these empirical studies addresses whether CLF makes a tort case more or less likely to go to court. That is a question legislatures frequently ask and try to answer. This study fills this gap and examines whether CLF brings more or fewer tort lawsuits to court.

I use staggered state laws changes that restricted CLF in the U.S. as an identification strategy and apply a difference-in-difference methodology for statistical inference. Specifically, over the past twenty years, several states in the U.S. restricted CLF through State Supreme Court rulings, state legislation, and state regulations. For instance, in 2003, the Ohio Supreme Court denied the validity of CLF contracts in the Rancman v. Interim Settlement Funding Corp. case, where the CLF plaintiff sued the CLF company. The judge and jury believed that the CLF contract was a violation of Champerty and Maintenance[3] in the common law. A similar scenario occurred in 2005 when the New York Supreme Court imposed an interest rate cap on the CLF contract in the Echeverria v. Estate of Lindner case in 2005 because the judge and jury believed that the CLF contract should follow the state's usury laws. Such state law changes which restricted CLF were adverse shocks to consumers' ac-

[3]Molot (2009) defines "maintenance" as "the provision of support for a lawsuit to which one is not a party," and "champerty" as "a form of maintenance that involves acquiring an interest in the recovery from the lawsuit."

cess to CLF as after these changes, CLF companies withdrew part or all of their funds from those states due to the risks of violating laws or the high cost of capital of their business.[4] These state law changes are plausibly exogenous shocks to consumers' access to CLF because most law changes used in this study are State Supreme Court rulings. The State Supreme Court rulings occurred during the trials of a few individual cases, where the consumer plaintiff sued the CLF company. Decisions at trial provided exogeneity to the changes in the laws. Lobbyists, who usually lobby legislatures, could not influence the decisions of judges or jury at trial. Moreover, these decisions were unlikely to be affected by the historical number of lawsuits because they were mainly made based on a violation of maintenance and champerty of a few particular cases, or a violation of usury laws. For robustness, I limited the instruments to only law changes due to State Supreme Court rulings and have found similar results. I also test the parallel trends and run placebo tests to ensure that the rate of change in the number of lawsuits ex-ante is not what drives the results. The robustness tests confirmed the findings.

In the difference-in-difference analysis, I have found that in the three years following the restriction of CLF in some states, for every 10,000 people in a county in treated states, 1.28 fewer tort lawsuits were filed with the courts, compared with the counties in neighboring states with similar environmental, economic, and legal conditions. Furthermore, I used the counties in neighboring states which are within 100 miles of the treated counties as 'control counties', and the results were nonethe-

[4] Similar quasi-experiments have also been explored in Xiao (2017a). I further confirmed such behaviors of CLF companies through talking with multiple CLF funders.

less the same if not stronger. When I use the counties within 100 miles as control counties, I find that for every 10,000 people in a county in treated states, 2.71 fewer tort lawsuits were filed with the courts — that is an 18.7 percent drop in the number of tort lawsuits filed with courts. Neighboring counties are good control counties because people commute between them, and any accidents or injuries, which may occur, will often be of similar nature. Moreover, I have run a triple-difference regression to rule out the possibility that it is other law or rule changes, which might affect all general civil cases in the sample periods, that drive the results in Table 2. In the triple-difference analysis, I have compared the difference in the number of civil lawsuits between civil tort lawsuits and civil non-tort lawsuits (third difference), between before and after the law changes (second difference), and between counties in treated states and counties in non-treated states (first difference). Since CLF companies almost only finance civil tort cases as opposed to civil non-tort cases, we should observe a more significant drop in the number of civil tort lawsuits than civil non-tort lawsuits after the law changes. I have found in the triple-difference analysis that the number of civil tort cases experienced a more significant drop (1.70 fewer cases for every 10,000 people in a county) than the civil non-tort cases after the law changes which restricted CLF. I also conduct an event-year analysis to identify which event-years contributed to the treatment effects and find that the effects took place two years after the law changes. I further run placebo tests and a robustness test to make sure that the identified treatment effects did not exist prior to the law changes. Overall, the evidence supports the hypothesis that access to CLF results in consumers being more likely to bring civil tort lawsuits to court. This finding contrasts with Daughety and Reinganum (2014)'s theory, which predicts that CLF should induce settlement in equilibrium in the game among plaintiffs, defendants, and consumer litigation funders. One of the possible reasons for such contrast might be that, in reality, defendants hardly ever know whether or not consumer plaintiffs are funded by CLF, which violates the main assumption in the model in Daughety and Reinganum (2014). When the defendants do not know whether the plaintiff is funded by CLF (or can only predict the probability of the plaintiff being funded), their incentive to settle is significantly reduced. Hence, the incentives of the plaintiff to further fight the case due to less financial constraints could play a bigger role than the incentives of the defendants to settle. However, this paper cannot conclude that the violation of such assumptions would incur a new equilibrium in their model but provides initial empirical evidence that contrasts their model's prediction.

Furthermore, I conduct a difference-in-difference analysis on liability rulings at trial to examine whether more civil tort lawsuits, due to access to CLF, means more access to justice for consumers or more frivolous litigation. I have found that following the state laws that restricted CLF, the proportion of lawsuits going to trial did not change. However, the proportion of trials where the liability ruling favors the plaintiffs significantly increased by 5.9 percentage points. Such findings are consistent with the hypothesis that plaintiff winning rates drop due to access to CLF. There could be multiple explanations for these results. One likely explanation is that CLF incentivizes plaintiffs to file lawsuits that are less likely to be successful in the courts. Another likely explanation is that consumer plaintiffs' behaviors change when funded by CLF. Specifically, consumers with CLF funding behave less seriously and professionally during the litigation process before and at trial than if they have not been financed by CLF companies because they have already received compensations from CLF and are somewhat indifferent about whether or not they would win the case. The plaintiffs' misbehavior before the trial could result in delay or incomplete preparation of evidence, and their misbehavior at trial could encourage bias or prejudice in the decisions of the judge or jury against them. This is a plaintiff's moral hazard story. Both explanations could contribute to the finding that access to CLF causes a drop in the liability rulings favoring the plaintiffs.

Finally, I have also found that the average duration of tort lawsuits, defined as the number of days from the lawsuit filing date to the disposition date, significantly dropped after the state law changes restricting CLF. Such evidence complements the findings in Xiao (2017), where the researcher reveals that the average duration of medical malpractice claims, defined as the number of days from the insurance claim filing date to the ending date, increases due to CLF. The duration in my finding is defined as the number of days from the day of the lawsuit filing with the court to the day of the disposition of the case.

This paper contributes to the finance literature in the following ways. First, it documents how financial constraints affect one of the consumers' socioeconomic decisions: legal decisions. CLF mitigates the financial constraints of consumers, which results in their being more likely to bring claims to court. Moreover, the empirical evidence in this paper helps address the conflicting theories on different economic agents' decisions in equilibrium when consumers have access to CLF. Furthermore, this study shows how financial constraints could, on a large scale, affect one of the economic sectors: the legal sector. The civil justice system is an essential part of the economy, and this paper provides initial evidence that CLF results in higher court caseloads and fewer liability rulings favoring the plaintiff at trial. Finally, this study also contributes to the legal literature by providing empirical evidence regarding the policymakers' debates on the regulation of CLF.

The rest of the paper is organized as follows. Section 2 describes the institutional background of consumer litigation funding in tort claims. Section 3 introduces the theory and hypotheses. Section 4 describes data and variable constructions. Section 5 introduces the identification strategy. Section 6 presents the empirical tests and results. Lastly, Section 7 discusses possible explanations and concludes.

1.2 Institutional Background of Consumer Litigation Funding and Tort Claims

Consumer litigation funding is a non-recourse contract where the CLF company provides cash advances to consumer plaintiffs during the pursuit of a personal injury or tort claim. When a consumer is injured, they usually file a complaint with the defendant, either by themselves or by an attorney representing them. The defendant, in such cases, is usually an insurance company. Part of the business models of these companies is to reduce costs. One of the ways to achieve this goal is to compensate their clients as little as they could. It can take up to two years for insurance claims to be resolved because of the lengthy negotiation on the liability and compensation between the plaintiff and the insurance company. The consumer plaintiff usually does not need to worry about the legal expenses of the claims or lawsuits because most personal injury attorneys represent the client on a contingency fee arrangement. That is, the attorney pays upfront all the legal expenses in pursuing the suit and, in return, receives a portion (usually 30-40 percent) of the awards of the claim only if the case is either settled or resolved in the courts. However, in many situations, an individual loses the ability to work due to the injuries or damage, hence losing income during the claim period. Most individuals struggle with making ends meet or paying for daily needs such as food, utilities, or loans because of their financial constraints during the long period of waiting. Many of them also incur large medical bills due to the injuries. Thus, they sometimes settle with the insurance company with a smaller amount to trade for faster cash. In fact, many tort claims are settled and never go to courts. Insurance companies, on the other hand, can wait as long as they desire, since they have a giant financial backup. In other words, during the bargaining process between the plaintiff and the insurance company, the two parties are not standing on the same level of the financial playground.

Consumer litigation funding lifts the consumers' financial playground and smooths consumers' consumption during such financial distress by offering non-recourse cash advances to finance the plaintiff's daily expenses. Consumers use such cash advances mainly for daily and medical expenses. Since it is a non-recourse transaction, the repayment (principal plus interest and fees) from the plaintiff is only required if the plaintiff receives awards (either in a settlement, summary judgment, or trial). Furthermore, the repayment is only up to the case proceeds amount (after paying the plaintiff's attorneys first). According to Avraham and Sebok (2018), the median actual annual cost is approximately 44% of the amount funded. Attorney representation is required for the consumer to apply for the cash advances. However, not every application for CLF is approved. CLF companies only approve cases where they think an investment is worthwhile. The types of cases funded by CLF are almost all personal injury or tort cases, such as car accidents, medical malpractice, slip and fall, and premises liability.

Given that financial constraints are prevalent in the lives of American middle-class households when they experience adverse financial shocks, CLF plays an important role in the economy. The Fed's New Survey of Household Economics and Decision-Making in 2018 documents that "41% of American households could not cover a $400 emergency expense using cash in 2017". This means that when individuals experience adverse financial shocks, almost half of U.S. households struggle to pay their mortgages, rents, credit card bills, utility bills, medical bills, and even put food on their tables. Financial shocks come from several sources, and among the tops are health conditions, loss of employment, severe personal injuries, and natural disasters. CLF helps fund financial shocks resulting from personal injuries or torts.

How often do personal injuries happen in the U.S.? Personal injuries are extremely prevalent in the U.S. We hear about car accidents, work injuries, slip and fall, and medical malpractice all the time. The largest number of personal injury cases is automobile accidents. The U.S. has one of the heaviest road traffic loads among all countries. According to the National Highway Traffic Safety Administration, in 2015, there were an estimated 19,534 cars involved in fatal crashes and an estimated 1.72 million cars involved in injury crashes in the U.S. Such numbers have not taken account of medical malpractice incidents, slip and fall, premises liability, and many other forms of injuries. With both the financial conditions of U.S. households and the prevalence of injuries, CLF is prevalently needed by U.S. households.

Consumer litigation funding fundamentally differs from payday lending or traditional loans. Payday lending or traditional loans is a recourse loan, where the borrower must pay on the due date of the loan regardless of the situation, while CLF is a non-recourse cash advance, where the borrower only pays if the plaintiff receives an award from the case and up to the proceeding amount. Several studies in the finance literature, such as Melzer (2018), Carvalho, Meier, and Wang (2016), Bhutta (2014), Morse (2011), and Skiba and Xiao (2017), examine whether payday lending brings adverse financial effects to households due to the high-interest costs. Several studies find that consumers are stuck in the vicious cycle of paying interests on payday lending loans (Skiba and Xiao, 2017).

Policymakers and research scholars have been heatedly debating on the regulation of CLF and evaluating the benefits and costs of CLF, but there are barely any empirical studies in this field due to the lack of data (Avraham and Sebok, 2018; Xiao, 2017a). Proponents of CLF argue that CLF provides valuable financial resources that consumers could not find elsewhere, especially when the plaintiffs are unemployed or have reached their credit limits and cannot borrow money from banks or other lending institutions. In contrast, opponents' arguments are twofold. Firstly, opponents are concerned that CLF is a violation of Champerty and Maintenance (Garber, 2010; Martin, 2004). That is, it might discourage plaintiffs from settling and cause frivolous litigation. Secondly, many states are concerned that interest rates charged on the

CLF are too high (over 44% annually) and that the consumers might end up with very little money after they first pay 30-40% of the total proceeds to the attorney and then pay the principal plus interests and fees to the CLF funding companies.

1.3 Theory and Hypotheses

I focus on addressing the following three sets of research questions:

1.3.1 The effects of consumer litigation funding on the number of lawsuits

Hypothesis 1: Consumer litigation funding results in more lawsuits going to courts

In theory, whether CLF makes it more likely for the plaintiff to bring the case to the court in equilibrium is not clear and is up for debate. On the one hand, with the assistance of CLF, a plaintiff is less urgent for cash and is more capable of pursuing the case to its full value. From that perspective, we would expect the plaintiffs to be more likely to bring the case to the courts. On the other hand, the lawsuit negotiation is, indeed, a game between four parties: the plaintiff, the defendant (usually insurance companies), the plaintiff attorney, and the CLF funding company. Daughety and Reinganum (2014) build a theoretical model and find that when the plaintiff has private information about injury and damages, the optimal (plaintiff-funder) loan induces all plaintiff types to make the same demand in a pooling equilibrium, which induces full settlement. When the defendant has private information about her likelihood of being found liable, the likelihood of settlement is unaffected. Intuitively, if the insurance company knows that the consumer is funded by CLF and could further pursue the case to its fullest, it would want to settle early to avoid the extensive costs of going to courts. From that perspective, we would expect the plaintiffs to be less likely to bring the case to the courts. Overall, how the CLF affects the likelihood of plaintiffs bringing the case to the courts remains an open question. I empirically test this hypothesis using county-level novel lawsuit caseload data.

1.3.2 The effects of consumer litigation funding on the liability ruling of lawsuits

Hypothesis 2: Consumer litigation funding results in lower plaintiff winning rates at trial

According to the well-known Fifty-Percent Limit Hypothesis proposed by Priest and Klein (1984), there is a tendency toward 50 percent plaintiff victories, and extra funding should not change the plaintiff winning rates at trial. However, CLF funding might change the quality of the pool of lawsuits filed with the court. On the one hand, CLF helps those with high-quality cases to pursue their lawsuits further that they otherwise would not have the capability to do. From that perspective, some high-quality cases are brought to court and might increase the plaintiff's winning rates at trial. On the other hand, some consumers might simply be more aggressive in pursuing the cases, even when the cases are of lower quality because the longer they drag the case, the more money they can request from the CLF companies. From that viewpoint, the marginal quality cases might be brought to court, which could lower the average quality of the pool of lawsuits at court and hence could lower the plaintiff winning rates at trial. Furthermore, consumers could have moral hazard problems. They already receive the advanced cash from CLF companies and know that even if they lose the case, they do not need to pay the CLF companies anything back. Hence, they do not have a strong incentive to prepare seriously before the trial or act seriously at trial. Such under-preparation or misbehavior could affect the jury or judge's decisions at trial. Therefore, whether CLF enhances or lowers lawsuit outcome is not clear. I empirically test this hypothesis using novel lawsuit outcome data. I measure the outcome of lawsuits by plaintiff winning rates at trial.

1.3.3 The effects of consumer litigation funding on the duration of lawsuits

Hypothesis 3: Consumer litigation funding causes longer duration of lawsuits at the courts

The duration of a lawsuit is defined as the number of days from the date when the lawsuit is filed with the court to the date when it is disposed of by the courts. Whether CLF increases or decreases lawsuit duration is not clear. On the one hand, when consumers are less impatient with cash due to CLF, they want to fight the case to its fullest value. Hence, we should expect the lawsuit to be fought longer rather than settled sooner. On the other hand, according to Daughety and Reinganum (2014)'s prediction, both parties should settle sooner due to CLF in equilibrium. Furthermore, some consumers might soon realize how quickly the interest accrues and might not want to accrue more interests by dragging the lawsuit longer. From that perspective, the duration of cases should be shorter due to access to CLF. I empirically test this hypothesis using novel lawsuit duration data.

1.4 Data and Variable Constructions

The lack of legal data is the main barrier for empirical studies in the area of finance and law, and particularly for consumer litigation funding. The data used in this study mainly comes from four sources. First, I have hand-collected civil lawsuit filing data state by state from thirteen different U.S. States' Court Administrative Offices. I could only obtain data from thirteen states because the other U.S. states do not maintain the detailed data needed for this study. These ten states include about one-fifth of the U.S. counties and population. This dataset covers county-level filing and trial data in ten U.S. states in the period from 2001 to 2018. Second, I have obtained lawsuit liability ruling and duration data from a litigation analytics company Premonition. Premonition collected such data by web crawling each county clerk office's lawsuit docket database. The lawsuit liability ruling data covers the period from 1999 to 2017. Third, I have obtained county demographics and economic indicators from the U.S. Census Bureau from 1990 to 2017. Fourth, law changes restricting CLF, including legislation, regulations, and court rulings, are hand collected by searching legal literature, state legislation, and LexisNexis lawsuit database.

After merging all four datasets, my final sample consisted of 1080 counties from thirteen states during the period from 2001 to 2017. Panel B of Table 1 reports the summary statistics of lawsuit characteristics and county characteristics. Figure 1 draws the maps of counties included in the sample.

[Inserts Figure 1 here]

[Inserts Table 1 here]

1.5 Identification

To identify the causal effects of consumer litigation funding on the number, liability ruling, and duration of lawsuits, I use staggered law changes, including Supreme Court rulings, legislation, and regulations restricting consumer litigation funding as plausibly adverse exogenous shocks to the supply of CLF at county levels. For example, in 2003, the Ohio Supreme Court denied the validity of CLF during the Rancman v. Interim Settlement Corp. case trial due to the violation of champerty. I have also used other state-level court rulings and regulations on interest rates cap as instruments. Some of the law changes used in this study are similar to those used in Xiao (2017). However, the quasi-experiments used in this paper are very different from those of Xiao (2017a) and Xiao (2017b) because I have used both prohibition and interest caps as instruments for robustness instead of only one event as in Xiao (2017). I also did not include disclosure requirements of CLF agreements as instruments as in Xiao (2017) because it is not clear whether disclosure requirement is a positive or negative shock to the supply of CLF.

I have run the DID tests in three different settings. First, in the staggered DID tests, the treated counties are counties in states that took actions restricting CLF and become treated counties after the law changes. The control counties, on the other hand, are counties in states that did not take action restricting CLF. Second, I have used counties in the neighboring states as control counties, where the legal, climatic, and economic environments are similar, except for the absence of regulation or court ruling on consumer litigation funding, and run an event-window DID. Third, I have used counties that are in the neighboring states and that are within 100 miles of the treated counties as control counties and run an event-window DID. Counties within 100 miles of the treated counties are good counterfactuals because people usually commute among neighboring counties and could have accidents or injuries of similar nature. The states and the respective state laws are listed in Panel A of Table 1.

For such law changes to serve as valid instruments for identification, they need to meet two restrictions, relevance restriction and exclusion restriction. Such an instrument meets the relevance restriction because, after the rule changes, CLF companies do not want to take the risks of violating state laws and hence pull their funding and business away from the states that take such actions. From anecdotal evidence, this is indeed the case. For example, after the 2003 Ohio law change, the number of funded cases dropped from tens of thousands to almost zero.[5] Such an instrument meets the exclusion restriction because most law changes used in this study are Supreme Court ruling decisions that happened during the hearing or trial of a

[5]Such anecdotal evidence is collected from talking to multiple CLF market players.

case, which provides certain randomness to the decision making. The decisions of the judges or jury are unlikely to be influenced by lobbyists, who can only lobby legislatures. Furthermore, such decisions are unlikely to be affected by the historical number of lawsuits, because the decisions are mainly made based on the existing laws and statutes, such as violation of maintenance and champerty, or violation of usury laws, and are not based on historical caseloads.

1.6 Empirical Tests

1.6.1 The effects of consumer litigation funding on the number of civil tort lawsuits filed

I have first applied a staggered difference-in-difference methodology to identify the causal effects of consumer litigation funding on the likelihood of consumers bringing cases to the courts. The treated and control counties are dynamic over the full sample periods, i.e., a county transforms from a control group to a treated group after the state takes actions restricting CLF. The law changes are plausibly negatively exogenous shocks to the supply of CLF for treated counties. The main difference-in-difference specification used in this paper to identify the effects of CLF on the number of tort lawsuits filed is as follows:

$Model : Number\ of\ Tort\ Civil\ Lawsuits\ Filed_{i,t}/Population_{i,t} = \beta_0 + \beta_1 Post_{i,t} \times Treatment_{i,t} + X^T\beta_4 + \phi_i + \gamma_t + \epsilon_{i,t}$

where $Number\ of\ Tort\ Civil\ Lawsuits\ Filed_{i,t}/Population_{i,t}$ is the ratio of the number of new civil tort lawsuits filed in county i of year t over the population in county i of year t. $Treatment_{i,t}$ is a dummy variable that equals one if the county is in a treated state, and equals zero otherwise. $Post_{i,t}$ is a dummy variable equal to one in the years following the state's actions of restriction of litigation financing and zero otherwise. $X_{i,t}$ are control variables such as county demographic and economic characteristics. To capture the unobserved time-invariant state or county heterogeneity, I have controlled state fixed effects and county fixed effects in the specification separately. To capture unobserved nationwide shocks that affect all units in the sample, I have controlled year fixed effects.

Table 2 Panel A reports the baseline staggered difference-in-difference regression results. On average, after the restriction of consumer litigation funding, among every 10,000 people, there are 1.32 and 1.28 fewer tort lawsuits filed with the courts when controlling state fixed effects and county fixed effects respectively. The results are statistically significant, and the economic magnitude is non-negligible. This is consistent with the hypothesis that consumer litigation funding increases the likelihood of a tort lawsuit being filed with the courts.

[Inserts Table 2 here]

1.6.2 Robustness and placebo tests

For robustness, I have constructed samples of control counties. First, I look at treated counties and control counties within a five-year event window and measure the likelihood of consumers bringing cases to the courts by examining the number of civil lawsuits filed in a county scaled by the population in a county. Specifically, I have compared the number of civil lawsuits filed in civil jurisdictions in counties in treated states with those jurisdictions in counties in neighboring states, where the legal and economic environments are similar, except for the absence of regulation or court ruling on consumer litigation funding. I also plot the time trend of the number of fatal automobile accidents in Figure A1 in the appendix. Five years before the law changes, the control states and treated states have parallel trends in the number of fatal automobile accidents. This supports the validity that the control counties are a good counterfactual. Second, I use counties that are in neighboring states and that are within 100 miles of the treated counties as control counties. For tests using either control groups, the results are reported in Table 2 Panel B and Penal C, respectively. I have found consistent and even stronger results when the control counties are a better counterfactual.

To rule out the possibility that it might be other law changes affecting all general civil cases in the sample periods that drive the results, I have run a triple-difference regression. Particularly, I have compared the difference in the number of tort and civil non-tort lawsuits scaled by county population (third difference), before and after the law changes (second difference), between counties in treated states and counties in non-treated states (first difference). The rationale behind this test is that, since CLF almost only finance tort cases and not non-tort cases, such a drop in the number of civil lawsuits should mainly be observed for tort cases. The triple-differences regression results are shown in Table 3, where the outcome variables are the number of civil cases of type j in county i of year t scaled by population in county i, and the case type is categorized into tort and non-tort cases based on the definition of tort. I have found that indeed, the number of tort cases dropped significantly more than the non-tort cases after the law changes restricting CLF. On average, after the restriction of CLF, among every 10,000 people, there are 2.24 and 2.21 fewer tort lawsuits filed than non-tort lawsuits filed with the courts between treated counties and control counties, when controlling state fixed effects and county fixed effects, respectively. The results are statistically significant, and the economic magnitude is non-negligible. Overall, the results are consistent with the hypothesis that the availability of CLF results in consumers being more likely to bring lawsuits to the courts, particularly tort lawsuits. In Appendix Table A2, I have also examined, for robustness, whether the number of tort cases dropped more than non-tort cases within treated states. I have found similar results supporting the hypothesis that access to CLF increases the number of tort lawsuits filed in the courts.

[Inserts Table 3 here]

To further test the assumption of parallel trends, I conduct the same difference-in-difference analysis as above, substituting the Post dummy with EventYear dummies. The regression results are reported in Table 4. I find that the treatment effects mainly start after the law changes and continue until the event year four. The treated counties and control counties do seem to have similar trends before the law changes.

[Inserts Table 4 here]

I also conduct placebo tests to rule out the possibility that the difference-in-difference results can also be identified before law changes. I have used the three event-years before the actual law changes years as "placebo law change years" and performed the same difference-in-difference analysis. The placebo test results are reported in Table 5. I have found that as predicted, in the placebo event years, the results in the difference-in-difference tests become statistically insignificant. The placebo test provides supporting evidence that the treatment effects identified in the previous section are not due to the difference in the rate of change of lawsuits between treated and control states. In other words, the placebo test supports the validity of the parallel trend assumption.

[Inserts Table 5 here]

To eliminate the concern that some legislations restricting CLF might be due to lobbying from insurance companies, which could create endogeneity problems on the tests, I also limit the sample to Supreme Court rulings that restricted CLF. Such rulings are not predictable ex-ante, nor can judges be lobbied by insurance companies. The results are reported in Table 6. The results are similar and even stronger.

[Inserts Table 6 here]

1.6.3 The effects of consumer litigation funding on the liability ruling of lawsuits.

To further examine whether the increase in litigation due to CLF means more justice or more frivolous lawsuits, I have conducted a staggering difference-in-difference analysis and look at liability ruling by examining the plaintiff's winning rates at trials. The plaintiff winning rates is one of the best ways to measure the average merits of lawsuits in legal studies.

The main difference-in-difference specification for the outcome of lawsuits used in this section is as follows:

$Model : Tort\ Lawsuit\ Plaintiff\ Winning\ Rate_{i,t} = \beta_0 + \beta_1 Post_{i,t} \times Treatment_{i,t} + X^T\beta_2 + \phi_i + \gamma_t + \epsilon_{i,t}$

where $TortLawsuitPlaintiffWinningRate_{i,t}$ is the ratio of the total number of tort cases where the plaintiff is favored in the trial judgment or verdict over all cases with a summary judgment, trial judgment or verdict, filed in county i of year t. $Treatment_{i,t}$ is a dummy variable that equals one if the county is treated, and equals zero otherwise. $X_{i,t}$ are control variables such as county demographic and economic characteristics.

Table 7 reports the results of the difference-in-difference regressions on the second hypothesis. The results show that plaintiff winning rates significantly increased by 5.3 and 5.9 percentage points for the civil tort cases filed with the courts after the law changes restricting CLF when controlling state fixed effects and county fixed effects, respectively. In other words, CLF results in lower plaintiff winning rates. This is a significant change, both statistically and economically. However, such results are only limited to the cases that go to trial. What if the proportion of cases going to trial also changes, and that is what drives the results? According to the well-known Fifty-Percent Limit Hypothesis proposed by Priest and Klein (1984), CLF should not change the plaintiff winning rates. To empirically identify whether it is more lawsuits going to trial that drives the results, I have examined whether the proportion of lawsuits going to trial experience a significant change after the law changes. The results are reported in Table 8. I find no statistical change in the proportion of lawsuits that go to trials. In other words, even though the proportion of lawsuits going to trial did not change due to CLF, the plaintiff winning rates dropped as a result.

[Inserts Table 7 here]

[Inserts Table 8 here]

There could be three potential explanations for the above results. The first potential explanation is that with CLF smoothing the plaintiff's consumption, the plaintiff is more aggressive in bringing the cases to the courts, where they could have settled before filing the lawsuits. In other words, the average merits of the lawsuit cases drop due to CLF. Hence, the plaintiff winning rate is lower among the pool of cases with lower merits. The second potential explanation is that the average merits of the case do not change, but the defendant, knowing that the plaintiff is funded by CLF, put in more resources and stronger legal teams in fighting the lawsuit. That could also drive the plaintiff winning rates to drop. A third possible explanation is that CLF changes the way the consumer plaintiff behaves during the lawsuit process. From anecdotal evidence provided by personal injury lawyers, many consumers simply do not care about whether they win the case or not and hence act in a sloppy manner before and at trial because they have already received money from the CLF beforehand. They know that even if they lose the cases, they do not owe the CLF companies anything. This paper does not take a stand on which explanation contributes most to the changes in the plaintiff's winning rates, and the mechanisms underneath such findings could benefit significantly from future research.

1.6.4 The effects of consumer litigation funding on the duration of civil tort lawsuits filed.

I further test whether consumer litigation funding affects the average duration of the cases filed with the courts. The duration of a case is defined as the number of days it takes from the day the lawsuit is filed to the day it is disposed of. I have used the same difference-in-difference setting and found that after the law changes of restricting CLF, the average duration of days dropped by 41 days and 55 days, controlling state fixed effects and county fixed effects, respectively. In other words, CLF results in a longer duration of lawsuits filed with the courts. This is a large change economically and might have meaningful inference on how lawsuits are appropriately using the resources of the courts, which ultimately are paid by all taxpayers. One potential explanation for such change in duration is that CLF results in more cases going to trials, as I have documented in the last test, which usually makes the lawsuit process longer due to the lengthy discovery and deposition process. This result echoes the finding of the study of Xiao (2017), where the researcher finds that the average duration of medical malpractice claims increases as a result of CLF. The finding in this study complements the finding in Xiao (2017). The duration in Xiao (2017) is measured as the length of a claim starting from the filing of an insurance claim with the insurance company, whereas the duration in my study is measured as the length of the lawsuit starting from the filing of a lawsuit with the courts.

[Inserts Table 9 here]

1.7 Discussion and Conclusion

Financial constraints have always been a core area of interest in the realm of finance. In this study, I have particularly examined a new asset class, consumer litigation funding (CLF), and explored how access to CLF credits affects the economic and legal decisions of the consumer as well as the civil justice system. I have particularly found that access to CLF results in plaintiff consumers being more likely to bring civil tort lawsuits to court. One possible explanation for this finding is that consumer plaintiffs are less urgent on the cash from the insurance claims because CLF companies provide cash advances to them for daily consumption. Hence, consumers are more willing to pursue the cases to their actual value. Such empirical results contradict the theoretical prediction of Daughety and Reinganum (2014). This conflict may be due to the fact that some primary assumptions in the Daughety and Reinganum (2014) model are challenged in reality. The main challenged assumption that they make is that the insurance company knows whether the plaintiff is funded by CLF or not. In reality, insurance companies or defendants hardly ever know this particular piece of information as these CLF agreements are highly confidential and, thus, they are not supposed to be disclosed to the defendants. When the defendants do not know whether the plaintiff is funded by CLF (or can only predict the probability of the plaintiff being funded), their incentive to settle is significantly reduced. Hence, the incentives of the plaintiff to further fight the case due to less financial constraints could play a bigger role than the incentives of the defendants to settle. Furthermore, in reality, it's not optimal for plaintiffs to voluntarily disclose funding to the insurance company because insurance could use the evidence of funding against the plaintiff at trial. When judges or jury know that the plaintiff is funded with such a high interest rate, it shows that the plaintiff is money hungry and the judges or jury could have less sympathy for the plaintiff's injury. Even though that I could not conclude that the violation of such assumptions would renege the prediction by Daughety and Reinganum (2014), this study provides an initial step to address this question empirically and to motivate future researchers to build a theoretical model that take these assumptions in reality into consideration in the models.

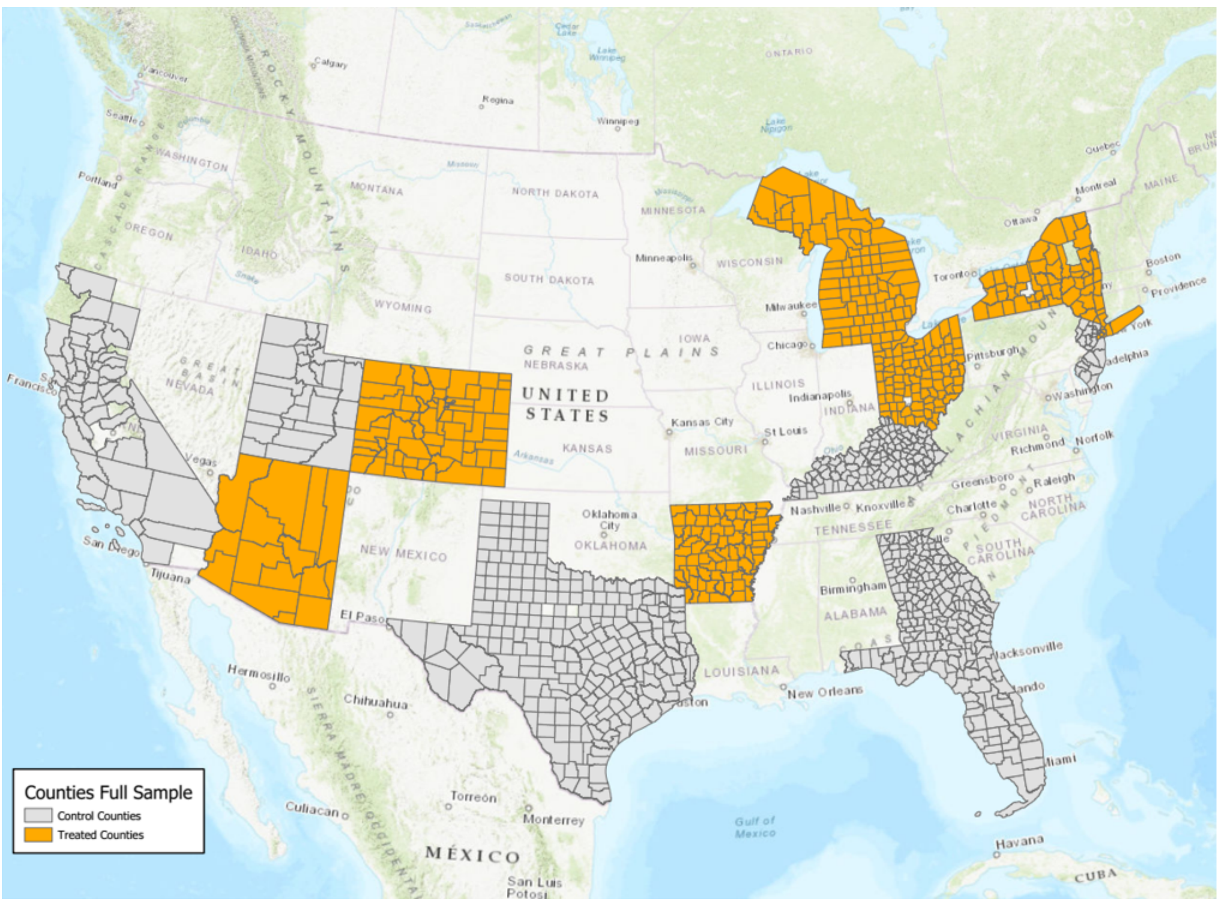
Furthermore, I have asked the question of whether the increase in litigation due to access to CLF means more justice or more aggressiveness of the plaintiff. I have found that consumer plaintiffs' access to CLF results in a smaller proportion of liability rulings favoring the plaintiffs at trial. There are three potential explanations for this finding. One possible explanation is that plaintiffs with access to CLF are more aggressive in bringing lawsuits to trial. Hence, the average merits of the pool of cases at trial drop when plaintiffs have access to CLF. A second possible explanation is that the defendants, knowing that the plaintiffs are more likely to be funded by CLF, put more effort into fighting the lawsuits, which results in lower plaintiff winning rates. A third possible explanation is that CLF fundamentally changes how consumer plaintiffs behave at trial. People who are fed with cash advances by CLF know that even if they lose the case, they do not have to pay the CLF funding company anything back and hence might not care how the trial proceeds and hence misbehave before or at trial. This incurs a moral hazard problem.

Due to the unavailability of claim settlement data, this study does not take a stand on the policy implications of these findings. However, it provides initial evidence on how credit constraints could affect the legal decisions of the consumer as well as the civil justice system. For future research, research scholars could further explore the mechanisms under which the effects of CLF take place and whether access to CLF credit brings more justice to our society.

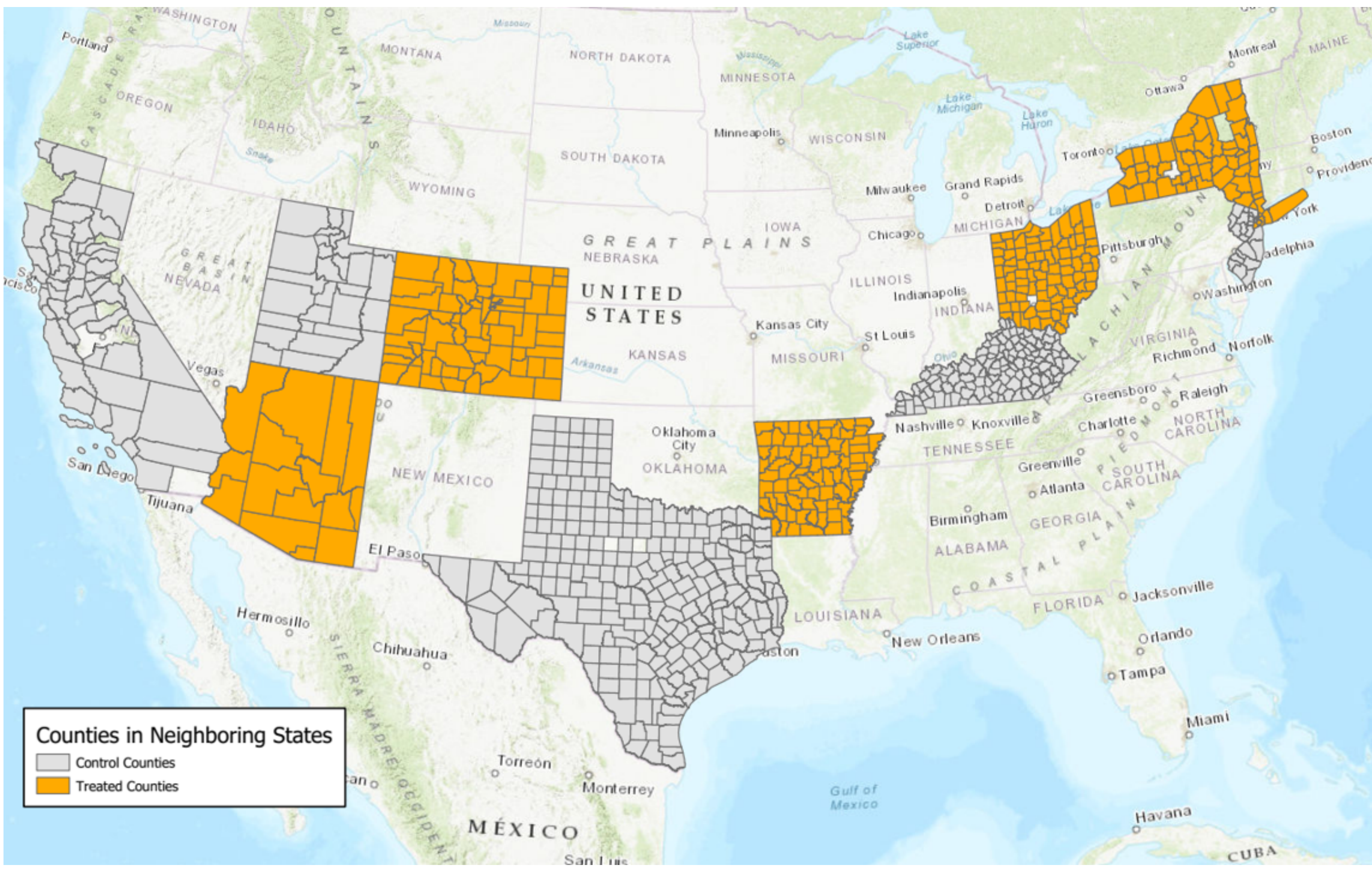
Figure 1.1: Map of Counties in the Sample

This Figure draws the map of counties included in the sample. Panel A draws all counties in the full sample. Panel B draws the treated counties and the counties in the neighboring states of the treated states. Panel C draws the treated counties and the neighboring state counties that are within 100 miles of the treated counties.

Panel A: All Counties in the Sample



Panel B: Treated Counties and Counties in the Neighboring States



Panel C: Treated Counties and Neighboring States Counties within 100 Miles

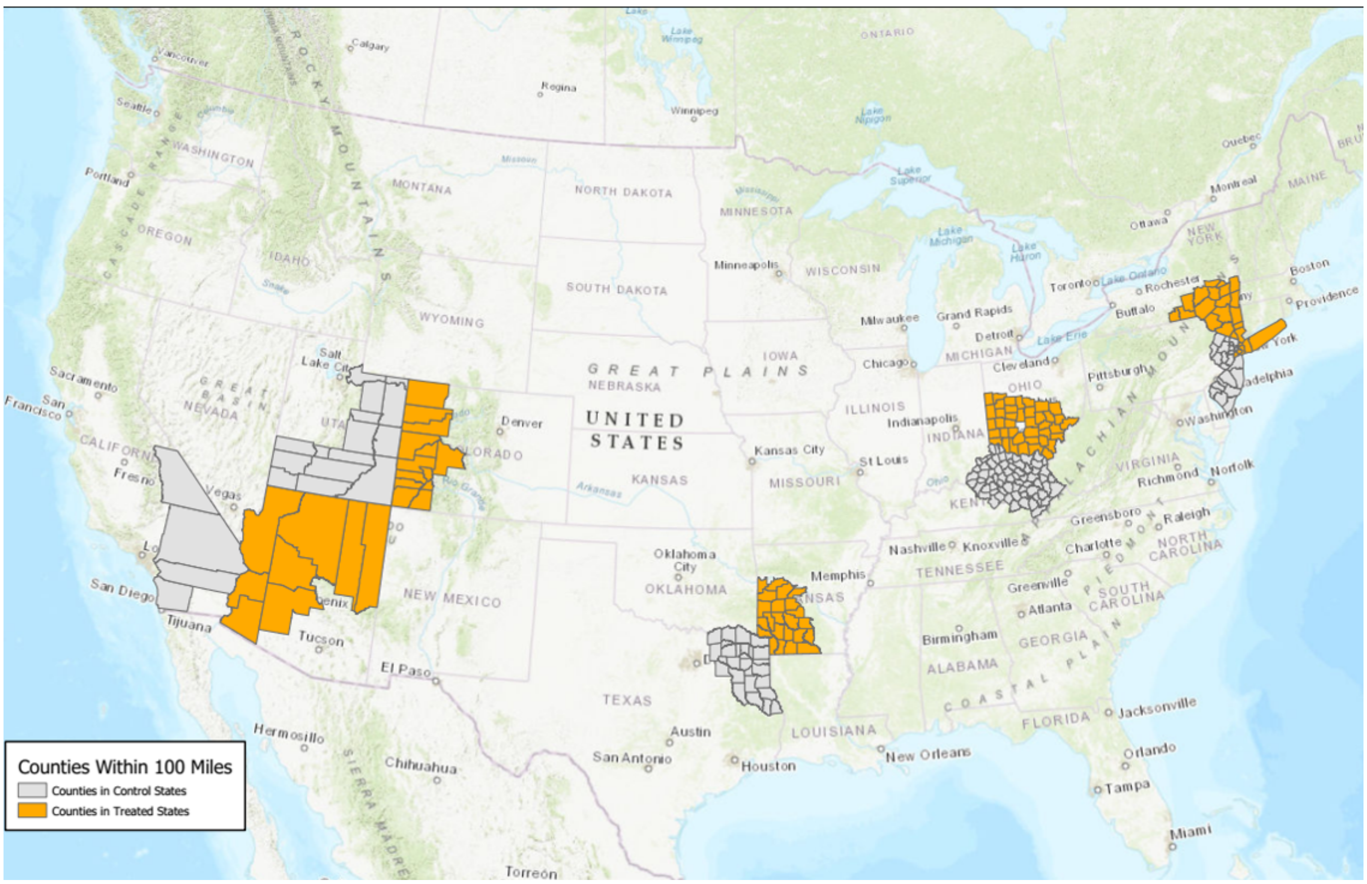


Table 1.1: Descriptive Statistics

This table reports the summary statistics for caseload obtained from the Court System, lawsuit outcome and duration obtained from Premonition, and county demographics and economic factors from U.S. Census Bureau. The sample is from 2000 to 2017. The unit of observation in each panel is a value for each county i in a given year t. Panel A summarizes the lawsuit characteristics, county demographic and economic characteristics for the full sample. In Panel A, New cases filed is the number of new lawsuit cases filed for each county i in year t. A tort case, in common law jurisdictions, is defined as a civil wrong that causes a claimant to suffer loss or harm resulting in legal liability for the person who commits the tortious act. Per capita income is annual personal income per county scaled by the annual county population, measured in 1983 U.S. dollars. Unemployment rate is the number of unemployed population scaled by the labor force in the same county. Panel B summarizes the states and the number of counties included in the sample.

Panel A: Descriptions of The States and Law Changes Included in The Sample

Year	State Actions	Treated State	Citation
2003	Prohibition	Ohio	Rancman v. Interim Settlement Funding Corp., 99 Ohio St.3d 121
2004	Interest Rate Cap/Fees	Michigan	Lawsuit Financial, L.L.C. v. Curry, 261 Mich.App. 579, 683 N.W.2d 233
2005	Interest Rate Cap/Fees	New York	Echeverria v. Estate of Lindner, 801 N.Y.S.2d 233
2013	Interest Rate Cap/Fees	Colorado	Oasis Legal Finance Group, LLC v. Coffman, 361 P.3d 400
2015	Interest Rate Cap/Fees	Arkansas	Arkansas Senate Act (S.B. 882)
2015	Interest Rate Cap/Fees	Arizona	Arizona Senate Act (S.B. 1403)
Control States: Kentucky, Georgia, New Jersey, Utah, Florida, California, Texas			
Total Number of Counties: 1080			

Panel B: Summary Statistics for the Variables

	Mean	Median	Min	Max	Std. Dev.	Count
Num. of Tort Lawsuits Per 1,000 People	1.26	0.96	0.00	65.57	1.43	18229
Num. of Non-Tort Lawsuits Per 1,000 People	1.42	0.87	0.00	73.54	1.90	18229
Pop. - Above 65 Yrs Old	0.16	0.15	0.03	0.57	0.05	18229
Pop. - Below 19 Yrs Old	0.27	0.27	0.08	0.43	0.04	18229
Pop. - White Race	0.87	0.92	0.20	1.00	0.13	18229
Pop. - Female	0.50	0.50	0.27	0.57	0.03	18229
Unemployment Rate	0.07	0.06	0.01	0.30	0.03	18229
Pop. - Labor Force	0.47	0.47	0.19	1.33	0.07	18229
Per Capita Income	15060	14115	5813	71480	4561.94	18229
Economic Growth	0.01	0.01	-0.45	0.80	0.05	18229
Total Population	152600	32196	61	10163507	457918	18229
Plaintiff Winning Rate	0.55	0.56	0.00	1.00	0.32	2325
Num. of Judgment	143.34	16.90	0.10	6396.50	493.07	2325
Average Duration of Case (Days)	428.18	357.43	1.00	2543.25	329.29	1975

Panel C: Covariates Balance Between Treated and Control States During Pre-Treatment Periods

Variables	Treatment	Control	Standardized Diff.	T-Statistic
Tort Lawsuits Per 1,000 People	1.45	1.36	0.06	-1.19
Pop. - Above 65 Yrs Old	0.16	0.15	0.07	-1.45
Pop. - Below 19 Yrs Old	0.26	0.26	-0.01	0.27
Pop. - White Race	0.89	0.88	0.06	-1.35
Pop. - Female	0.50	0.50	0.05	-1.13
Economic Growth	0.01	0.02	-0.03	0.54
Unemployment Rate	0.07	0.07	-0.03	0.64

Table 1.2: The Effects of Consumer Litigation Funding on the Number of Tort Civil Lawsuits Filed: Difference-in-Difference

$Model : Number\ of\ Tort\ Civil\ Lawsuits\ Filed_{i,t}/Population_{i,t} = \beta_0 + \beta_1 Post_{i,t} \times Treatment_{i,t} + X^T\beta_4 + \phi_i + \gamma_t + \epsilon_{i,t}$

This table reports the results of an event-window event-time difference-in-difference analysis of state actions regarding restrictions on litigation financing over the sample period of 2001-2017, comparing number of tort cases between treated states and control states. Treated states are states that banned or restricted consumer litigation funding. Control states are states that did not ban or restrict consumer litigation funding during the respective periods. $Number\ of\ Tort\ Civil\ Lawsuits\ Filed_{i,t}/Population_{i,t}$ is the ratio of the number of tort civil lawsuits filed in county i of year t over the population in county i of year t. A tort case is defined as a civil wrong that causes a claimant to suffer loss or harm resulting in legal liability. $Treatment_{i,t}$ is a dummy variable that equals to one if the county is in a treated state, and equals to zero otherwise. $Post_{i,t}$ is a dummy variable equal to one in the years following the respective treated state's restriction of litigation financing and zero otherwise. Unemployment rate is the number of unemployed population scaled by the labor force in the same county. Economic growth is defined as $Y_{i,t}/Y_{i,t-1}$, where $Y_{i,t}$ is the income per capita in county i of year t, i.e. the annual personal income per county scaled by the annual county population, measured in 1983 U.S. dollars. All standard errors are clustered at county and year level and are reported in the parentheses. Statistical significance is represented according to $*p < 0.10, **p < 0.05, ***p < 0.01$.

Panel A: Staggered Diff-in-Diff: Full Sample

	(1) New Filings/Popul.	(2) New Filings/Popul.
Treatment * Post	-0.132***	-0.128***
	(0.021)	(0.023)
Pop ratio - above 65 yrs old	-0.017	0.004
	(0.034)	(0.037)
Pop ratio - below 19 yrs old	-0.023	-0.001
	(0.065)	(0.076)
Pop ratio - white race	-0.015***	0.032***
	(0.004)	(0.012)
Pop ratio - female	0.045	0.048
	(0.055)	(0.085)
Economic Growth	-0.046	-0.090
	(0.346)	(0.322)
Unemployment rate	0.020**	0.013*
	(0.008)	(0.007)
Pop ratio - labor force	0.009	0.009
	(0.006)	(0.006)
Observations	17445	17445
Year FE	Yes	Yes
State FE	Yes	No
County FE	No	Yes
Within R-Square	0.053	0.056

Panel B: Event Window Diff-in-Diff: Counties in Neighboring States as Control States

	(1) New Filings/Popul.	(2) New Filings/Popul.
Treatment * Post	-0.142***	-0.139***
	(0.025)	(0.022)
Pop ratio - above 65 yrs old	0.003	0.011
	(0.029)	(0.033)
Pop ratio - below 19 yrs old	0.054	0.128
	(0.059)	(0.087)
Pop ratio - white race	-0.016***	0.041**
	(0.003)	(0.016)
Pop ratio - female	-0.016	-0.050
	(0.049)	(0.064)
Economic Growth	0.003	0.017
	(0.866)	(0.793)
Unemployment rate	0.015*	0.006
	(0.009)	(0.011)
Pop ratio - labor force	0.003	-0.010
	(0.010)	(0.010)
Observations	8398	8398
Year FE	Yes	Yes
State FE	Yes	No
County FE	No	Yes
Within R-Square	0.025	0.032

Panel C: Event Window Diff-in-Diff: Counties in the Neighboring States Within 100 Miles of Treated Counties as Control Counties

	(1) New Filings/Popul.	(2) New Filings/Popul.
Treatment * Post	-0.264*	-0.271**
	(0.138)	(0.124)
Pop ratio - above 65 yrs old	-0.100***	0.122**
	(0.024)	(0.056)
Pop ratio - below 19 yrs old	-0.148***	-0.045
	(0.027)	(0.046)
Pop ratio - white race	-0.046***	0.176***
	(0.010)	(0.058)
Pop ratio - female	0.132***	-0.014
	(0.035)	(0.069)
Economic Growth	0.259	0.028
	(0.621)	(0.481)
Unemployment rate	0.049*	0.057**
	(0.029)	(0.024)
Pop ratio - labor force	-0.042***	0.002
	(0.010)	(0.016)
Observations	1259	1259
Year FE	Yes	Yes
State FE	Yes	No
County FE	No	Yes
Within R-Square	0.011	0.020

Table 1.3: The Effects of Consumer Litigation Funding on the Number of Civil Lawsuits Filed: Triple Difference

$Model$: $Number\ of\ Civil\ Lawsuits\ Filed_{i,j,t}/Population_{i,t} = \beta_0 + \beta_1 Treatment_{i,j,t} \times Post_{i,j,t} \times Tort_{i,j,t} + \beta_2 Treatment_{i,j,t} \times Post_{i,j,t} + \beta_3 Treatment_{i,j,t} \times Tort_{i,j,t} + \beta_4 Post_{i,j,t} \times Tort_{i,j,t} + \beta_5 Tort_{i,j,t} + \beta_6 Treatment_{i,j,t} + \beta_7 Post_{i,j,t} + X^T \beta_8 + \phi_i + \gamma_t + \epsilon_{i,j,t}$

This table reports the results of an event-window event-time triple-difference analysis of state actions regarding restrictions on litigation financing over the sample period of 2001-2017, comparing the number of civil cases between treated states and control states (first difference), between before and after respective treated state actions (second difference), and between tort and non-tort civil cases (third difference). Treated states are states that banned or restricted consumer litigation funding. Control states are states that did not ban or restrict consumer litigation funding during the respective periods. A tort case is defined as a civil wrong that causes a claimant to suffer loss or harm resulting in legal liability. $Number\ of\ Civil\ Lawsuits\ Filed_{i,j,t}/Population_{i,t}$ is the ratio of the number of civil lawsuits of type j filed in county i in year t over the population in county i of year t. $Treatment_{i,j,t}$ is a dummy variable equal to one if the county is in a treated state and zero otherwise. $Post_{i,j,t}$ is a dummy variable equal to one in the years following the respective treated state's restriction of litigation financing and zero otherwise. $Tort_{i,j,t}$ is a dummy variable equal to one if the observation is a tort civil case type, and zero if the case is a non-tort civil case type. Unemployment rate is the number of unemployed population scaled by the labor force in the same county. Economic growth is defined as $Y_{i,t}/Y_{i,t-1}$, where $Y_{i,t}$ is the income per capita in county i of year t, measured in 1983 U.S. dollars. All standard errors are clustered at county and year level and are reported in the parentheses. Statistical significance is represented according to $*p < 0.10, **p < 0.05, ***p < 0.01$.

	(1) New Filings/Popul.	(2) New Filings/Popul.
Treatment * Post * Tort	-0.171***	-0.170***
	(0.027)	(0.026)
Treatment * Post	0.087***	0.088***
	(0.009)	(0.010)
Treatment * Tort	-0.060	-0.061
	(0.062)	(0.067)
Post * Tort	0.011	0.010
	(0.022)	(0.020)
Tort	0.531***	0.532***
	(0.050)	(0.054)
Observations	8790	8790
Control Variables	Yes	Yes
Year FE	Yes	Yes
State FE	Yes	No
County FE	No	Yes
Within R-Square	0.107	0.111

Table 1.4: The Effect of Consumer Litigation Funding on the Number of Tort Civil Lawsuits Filed: Parallel Trend Tests

$Model : Number\ of\ Tort\ Civil\ Lawsuits\ Filed_{i,t}/Population_{i,t} = \beta_0 + \beta_1 Treatment_{i,t} \times EventYear(k)\ Dummy_{i,t} + X^T\beta_2 + \phi_i + \gamma_t + \epsilon_{i,t}$

This table reports the results of parallel trend tests in the difference-in-difference analysis of state actions regarding restrictions on consumer litigation funding over the sample period of 2001-2017, examining which event-time years contribute to the treatment effects. Treated states are states that banned or restricted consumer litigation funding and that have minus five event time (Colorado, Arkansas, and Arizona). Control states are states that did not ban or restrict consumer litigation funding during the respective periods (Utah, Texas, and California). $Number\ of\ Tort\ Civil\ Lawsuits\ Filed_{i,t}/Population_{i,t}$ is the ratio of the number of tort civil lawsuits filed in county i of year t over the population in county i of year t. A tort case is defined as a civil wrong that causes a claimant to suffer loss or harm resulting in legal liability. $Treatment_{i,t}$ is a dummy variable that equals to one if the county is in a treated state, and equals to zero otherwise. $EventYear(k)Dummy_{i,t}$ are dummy variables equal to one in the event year k and zero otherwise. All standard errors are clustered at county and year level and are reported in the parentheses. Statistical significance is represented according to $*p < 0.10, **p < 0.05, ***p < 0.01$.

	(1) New Filings/Popul.	(2) New Filings/Popul.
EventYear=-5 X Treatment	-0.001	-0.041
	(0.079)	(0.080)
EventYear=-4 X Treatment	-0.085	-0.076
	(0.077)	(0.069)
EventYear=-3 X Treatment	-0.144*	-0.101
	(0.074)	(0.071)
EventYear=-2 X Treatment	-0.168**	-0.192***
	(0.074)	(0.070)
EventYear=-1 X Treatment	-0.178**	-0.137*
	(0.074)	(0.073)
EventYear=0 X Treatment	-0.222***	-0.259***
	(0.076)	(0.076)
EventYear=1 X Treatment	-0.292***	-0.255***
	(0.074)	(0.075)
EventYear=2 X Treatment	-0.339***	-0.352***
	(0.074)	(0.076)
EventYear=3 X Treatment	-0.236***	-0.235**
	(0.080)	(0.108)
EventYear=4 X Treatment	-0.253***	-0.328***
	(0.082)	(0.115)
Observations	4509	4509
Control Variables	No	Yes
Year FE	No	Yes
State FE	No	Yes
Within R-Square	0.006	0.097

Table 1.5: The Effects of Consumer Litigation Funding on the Number of Tort Civil Lawsuits: Placebo Tests

$Model$: $Number\ of\ Tort\ Civil\ Lawsuits\ Filed_{i,t}/Population_{i,t} = \beta_0 + \beta_1 Post_{i,t} \times Treatment_{i,t} + X^T\beta_2 + \phi_i + \gamma_t + \epsilon_{i,t}$

This table reports the results of placebo tests of an event-window event-time difference-in-difference analysis on state actions regarding restrictions on consumer litigation funding over the sample period of 2001-2017. Treated states are states that banned or restricted consumer litigation funding. Control states are states that did not ban or restrict consumer litigation funding during the respective periods. $Number\ of\ Tort\ Civil\ Lawsuits\ Filed_{i,t}/Population_{i,t}$ is the ratio of the number of tort civil lawsuits filed in county i of year t over the population in county i of year t. A tort case is defined as a civil wrong that causes a claimant to suffer loss or harm resulting in legal liability. $Treatment_{i,t}$ is a dummy variable that equals to one if the county is in a treated state, and equals to zero otherwise. $Post_{i,t}$ is a dummy variable equal to one in the years following placebo event years and zero otherwise. Placebo event years are years before the actual event years where treated states actually had state actions restricting consumer litigation funding. Unemployment rate is the number of unemployed population scaled by the labor force in the same county. Economic growth is defined as $Y_{i,t}/Y_{i,t-1}$, where $Y_{i,t}$ is the income per capita in county i of year t, i.e. the annual personal income per county scaled by the annual county population, measured in 1983 U.S. dollars. All standard errors are clustered at county and year level and are reported in the parentheses. Statistical significance is represented according to $*p < 0.10, **p < 0.05, ***p < 0.01$.

	(1) News Filings/Popul.	(2) News Filings/Popul.
Placebo Year T-1: Treatment * Post	0.015	-0.009
	(0.065)	(0.058)
Placebo Year T-2: Treatment * Post	-0.014	-0.017
	(0.029)	(0.027)
Placebo Year T-3: Treatment * Post	-0.037	-0.038
	(0.029)	(0.029)
Observations	8398	8398
Control Variables	Yes	Yes
Year FE	Yes	Yes
State FE	Yes	No
County FE	No	Yes

Table 1.6: The Effects of Consumer Litigation Funding on the Number of Tort Civil Lawsuits Filed: Supreme Court Rulings

$Model : Number\ of\ Tort\ Civil\ Lawsuits\ Filed_{i,t}/Population_{i,t} = \beta_0 + \beta_1 Post_{i,t} \times Treatment_{i,t} + X^T\beta_4 + \phi_i + \gamma_t + \epsilon_{i,t}$

This table reports results of an staggered difference-in-difference analysis of state actions change regarding restrictions on litigation financing over the sample period of 2001-2017, examining how consumer litigation funding affects lawsuit outcome. Lawsuit outcome is measured by the plaintiff wining rates. Treated states are states that banned or restricted consumer litigation funding during Supreme Court rulings. Control states are states that did not ban or restrict consumer litigation funding during the respective periods. $Tort\ Lawsuit\ Plaintiff\ Wining\ Rate_{i,t}$ is the ratio of the number of tort cases where the plaintiff is favored in the judgment or verdict over all cases with a judgment or verdict, filed in county i of year t. $D_{i,t}$ is a dummy variable equal to one if the county i is in a treated state and year t in in the years following the state's restriction of consumer litigation funding and equal to zero otherwise. Unemployment rate is the number of unemployed population scaled by the labor force in the same county. Economic growth is defined as $Y_{i,t}/Y_{i,t-1}$, where $Y_{i,t}$ is the per capita income in county i of year t, measured in 1983 U.S. dollars. All standard errors are clustered at county and year level and are reported in the parentheses. Significance is represented according to $*p < 0.10, **p < 0.05, ***p < 0.01$.

	(1) New Filings/Popul.	(2) New Filings/Popul.
Treatment * Post	-0.314**	-0.327**
	(0.145)	(0.131)
Pop ratio - above 65 yrs old	-0.107***	0.037
	(0.017)	(0.037)
Pop ratio - below 19 yrs old	-0.133***	-0.032
	(0.019)	(0.040)
Pop ratio - white race	-0.080***	0.152***
	(0.012)	(0.048)
Pop ratio - female	0.168***	0.018
	(0.021)	(0.054)
Economic Growth	-0.144	-0.213
	(0.405)	(0.327)
Unemployment rate	0.070**	0.083***
	(0.033)	(0.029)
Pop ratio - labor force	-0.039***	-0.003
	(0.009)	(0.012)
Observations	1895	1895
Year FE	Yes	Yes
State FE	Yes	No
County FE	No	Yes
Within R-Square	0.014	0.022

Table 1.7: The Effects of Consumer Litigation Funding on Tort Liability Ruling: Difference-in-Difference

$Model : Tort\ Lawsuit\ Plaintiff\ Winning\ Rate_{i,t} = \beta_0 + \beta_1 Post_{i,t} \times Treatment_{i,t} + X^T\beta_2 + \phi_i + \gamma_t + \epsilon_{i,t}$

This table reports results of an staggered difference-in-difference analysis of state actions change regarding restrictions on litigation financing over the sample period of 2001-2017, examining how consumer litigation funding affects lawsuit outcome. Lawsuit outcome is measured by the plaintiff wining rates. Treated states are states that banned or restricted consumer litigation funding. Control states are states that did not ban or restrict consumer litigation funding during the respective periods. $Tort\ Lawsuit\ Plaintiff\ Wining\ Rate_{i,t}$ is the ratio of the number of tort cases where the plaintiff is favored in the judgment or verdict over all cases with a judgment or verdict, filed in county i of year t. $D_{i,t}$ is a dummy variable equal to one if the county i is in a treated state and year t in in the years following the state's restriction of consumer litigation funding and equal to zero otherwise. Unemployment rate is the number of unemployed population scaled by the labor force in the same county. Economic growth is defined as $Y_{i,t}/Y_{i,t-1}$, where $Y_{i,t}$ is the per capita income in county i of year t, measured in 1983 U.S. dollars. All standard errors are clustered at county and year level and are reported in the parentheses. Significance is represented according to $*p < 0.10, **p < 0.05, ***p < 0.01$.

	(1) Plaintiff Winning Rate	(2) Plaintiff Winning Rate
Treatment * Post	0.053***	0.059***
	(0.014)	(0.014)
Pop ratio - above 65 yrs old	1.281***	1.273**
	(0.426)	(0.583)
Pop ratio - below 19 yrs old	2.135***	2.368***
	(0.514)	(0.798)
Pop ratio - white race	-0.085	-0.604**
	(0.116)	(0.282)
Pop ratio - female	0.325	3.011*
	(0.958)	(1.582)
Economic growth	0.001	-0.021
	(0.116)	(0.113)
Unemployment rate	0.001	0.002
	(0.003)	(0.003)
Pop ratio - labor force	-0.329*	-0.333*
	(0.178)	(0.193)
Observations	3387	3387
Year FE	Yes	Yes
State FE	Yes	No
County FE	No	Yes
Within R-Square	0.055	0.059

Table 1.8: The Effects of Consumer Litigation Funding on The Proportion of Tort Lawsuits Going To Trials: Difference-in-Difference

$$Model : Trial\ Rate_{i,t} = \beta_0 + \beta_1 Post_{i,t} \times Treatment_{i,t} + X^T\beta_2 + \phi_i + \gamma_t + \epsilon_{i,t}$$

This table reports results of an staggered difference-in-difference analysis of state actions change regarding restrictions on litigation financing over the sample period of 2001-2017, examining how consumer litigation funding affects the proportion of lawsuits that go to trials. Treated states are states that banned or restricted consumer litigation funding. Control states are states that did not ban or restrict consumer litigation funding during the respective periods. $Trial\ Rate_{i,t}$ is the proportion of tort lawsuits that go to trial in county i of year t. $D_{i,t}$ is a dummy variable equal to one if the county i is in a treated state and year t in in the years following the state's restriction of consumer litigation funding and equal to zero otherwise. Unemployment rate is the number of unemployed population scaled by the labor force in the same county. Economic growth is defined as $Y_{i,t}/Y_{i,t-1}$, where $Y_{i,t}$ is the per capita income in county i of year t, measured in 1983 U.S. dollars. All standard errors are clustered at county and year level and are reported in the parentheses. Significance is represented according to $*p < 0.10, **p < 0.05, ***p < 0.01$.

	(1) Trial Rate	(2) Trial Rate
Treatment * Post	-0.010	-0.014
	(0.008)	(0.009)
Pop ratio - above 65 yrs old	-0.003*	-0.000
	(0.002)	(0.004)
Pop ratio - below 19 yrs old	-0.003	-0.003
	(0.002)	(0.003)
Pop ratio - white race	0.000	-0.000
	(0.000)	(0.003)
Pop ratio - female	0.006***	0.014**
	(0.002)	(0.006)
Economic growth	0.067	0.059
	(0.044)	(0.045)
Unemployment rate	0.001	0.000
	(0.002)	(0.002)
Pop ratio - labor force	-0.003***	-0.004**
	(0.001)	(0.002)
Observations	10145	10145
Year FE	Yes	Yes
State FE	Yes	No
County FE	No	Yes
Within R-Square	0.013	0.014

Table 1.9: The Effects of Consumer Litigation Funding on Tort Lawsuit Duration: Difference-in-Difference

$Model : Tort\ Lawsuit\ Duration_{i,t} = \beta_0 + \beta_1 D_{i,t} + X^T \beta_2 + \phi_i + \gamma_t + \epsilon_{i,t}$

This table reports results of an staggered difference-in-difference analysis of state actions change regarding restrictions on litigation financing over the sample period of 2001-2017, examining how consumer litigation funding affects lawsuit duration. Treated states are states that banned or restricted consumer litigation funding. Control states are states that did not ban or restrict consumer litigation funding during the respective periods. $Tort\ Lawsuit\ Duration_{i,t}$ is the average number of days from the date when the tort lawsuit is filed with the court to the date when the case is disposed in county i of year t. $D_{i,t}$ is a dummy variable equal to one if the county i is in a treated state and year t in in the years following the state's restriction of consumer litigation funding and equal to zero otherwise. Unemployment rate is the number of unemployed population scaled by the labor force in the same county. Economic growth is defined as $Y_{i,t}/Y_{i,t-1}$, where $Y_{i,t}$ is the per capita income in county i of year t, measured in 1983 U.S. dollars. All standard errors are clustered at county and year level and are reported in the parentheses. Significance is represented according to $*p < 0.10, **p < 0.05, ***p < 0.01$.

	(1) Duration (in days)	(2) Duration (in days)
Treatment * Post	-40.547**	-55.372***
	(16.919)	(15.664)
Pop ratio - above 65 yrs old	-373.898	1080.303*
	(446.521)	(589.387)
Pop ratio - below 19 yrs old	594.692	1113.680
	(715.858)	(867.668)
Pop ratio - white race	117.261	1294.019***
	(108.567)	(420.690)
Pop ratio - female	-2276.735**	-4064.403**
	(913.089)	(1673.236)
Economic growth	-103.249	-149.075
	(138.404)	(128.891)
Unemployment rate	1.805	0.226
	(3.790)	(3.536)
Pop ratio - labor force	-235.874	-369.676*
	(186.581)	(224.290)
Observations	3123	3123
Year FE	Yes	Yes
State FE	Yes	No
County FE	No	Yes
Within R-Square	0.105	0.112

— Chapter 2 —

Do Equity Markets Really Underreact or Overreact to Liquidity Shocks?

2.1 Introduction

Modern asset pricing studies commonly agree that illiquidity, as a firm characteristic, should be priced (Amihud and Mendelson, 1986)[1]. If that is the case, investors form their beliefs about the level of liquidity throughout each period, and when the realized liquidity is different from their expectations, investors update their beliefs, and asset prices are updated. If markets are efficient, when there is a positive (negative) liquidity shock, we should expect firms to experience on average high (low) contemporaneous stock returns and average low (high) future stock returns.

In a recent paper, Bali, Peng, Shen, and Tang (2014) find that liquidity shocks (liquidity innovations) are not only positively correlated with current months' returns, but also predict future returns up to six months. They argue that this is evidence that markets are not efficient in incorporating the liquidity shocks into stock prices, because liquidity shocks are considered intangible public information (compared to event-type public news) and market frictions such as investor inat-

[1]There is another stream of literature that argues that liquidity risk, as a risk factor, should be priced (e.g., Pástor and Stambaugh, 2003, etc.). There is evidence for both arguments, and this paper focuses on how the level of illiquidity, as a firm characteristic, correlates the cross-section of stock returns.

tention and stock-level illiquidity contribute to such markets inefficiency. Bali et al. (2014) document a very important market inefficiency phenomenon and propose two market frictions that could result in such inefficiency – investor inattention and market illiquidity. However, the sample of liquidity shocks identified in the study is entangled with public news. In other words, some liquidity shocks have public news in the same months, and some do not. Hence, it is not clear whether the market underreacts to liquidity shocks or to the news, which could cause liquidity shocks. In fact, contradictory evidence against their finding is documented in Savor (2012) and Chan (2003), who consistently find that the equity markets underreact to price movements due to public news, and overreacts to price movements without public news (due to liquidity shocks or investor sentiment shift). In other words, if the price movements are caused by liquidity shocks, then Savor (2012) and Chan (2003)'s findings are consistent with the notion that the markets underreact to public news and overeat to liquidity shocks, which contradicts the conclusions reached by Bali et al. (2014). Therefore, there is still mixed evidence in the finance literature on whether the equity markets underreact or overreact to liquidity shocks.

This paper aims to address the mixed evidence in the finance literature and answer the question as to whether the markets underreact or overreact to liquidity shocks and whether public news mitigates the markets' underreaction or overreaction to liquidity shocks. I study two groups of liquidity shocks. One group (referred to as "no-news group") has public news in the same month and whereas the other group (referred to as "news group") does not. On the one hand, by studying the no-news group, I am able to study the markets' reaction to pure liquidity shocks (not related to public news). This has given us a clean result of how the market processes liquidity shocks and helped clarify the conflicting results between Bali et al. (2014) and Savor (2012). On the other hand, Bali et al. (2014) use firm size, analyst coverage, and institutional holdings as proxies for investor attention. These proxies are commonly used in finance literature. However, when it comes to price discovery, they are highly correlated with a firm's illiquidity level. Such proxies for investor attention could be just proxies for a firm's liquidity level. Larger stocks are more liquid than smaller stocks and are easier to trade. Therefore, the swiftness of the price discovery process of large stocks could simply be due to the fact that those stocks are more liquid and not because they draw more investor attention. In order to study whether investor's inattention contributes to the markets' underreaction to liquidity shocks, a better proxy for investor attention is needed. I use the release of public news as a proxy for investor attention. By comparing the two groups (news group versus the no-news group, I can better address whether investor attention mitigates the markets' underreaction to liquidity shocks. Using the release of public news as a proxy for investor attention is well documented in the finance literature and has empirical support. Both institutional investors and retail investors increase their trading volume during news release periods. For example, Barber and Odean (2008) document that individual investors are net buyers of attention-grabbing stocks, particularly stocks in the news. Yuan (2008) finds that the impact of attention proxied by record-breaking events of the Dow index and front-page articles about the stock market is pervasive across the market. Nofsinger (2001) documents that investors conduct a high degree of trading around news release in the Wall Street Journal, especially earnings and dividend news. Therefore, by using firm-specific news, we can more convincingly address the question of whether investors' inattention contributes to the markets' underreaction to liquidity shocks.

Here is how the testing portfolios are constructed in this study. Within each group (news and no-news), I sort the portfolios into deciles based on liquidity shocks in month t and examine the portfolios' performance in contemporaneous and future months. I find that the market reacts to both groups of liquidity shocks in contemporaneous months, and long-short portfolios sorted on liquidity shocks generate a significant abnormal return of 4.90% for the news group and 2.39% for the no-news group. More importantly, positive (negative) liquidity shocks continue having positive (negative) abnormal returns in the following month. Buying the highest decile stocks and shorting the lowest decile stocks generate a significantly positive abnormal return of 1.27% for the news group and 0.75% for the no-news group. Furthermore, buying the high-low decile news portfolio and shorting the high-low decile no-news portfolio generates a significantly positive alpha of 0.52% for month t+1. This provides initial evidence that market underreacts to both liquidity shocks with and without public news attached, and that in certain tests, the magnitude of the underreaction for the news group is larger.

I further explore if such a difference in underreaction continues for two or more months ahead. I find that in the second month after the liquidity shocks, equal-weighted (EW) high-low portfolios in the no-news group generate a statistically significant average return of 0.508% and an abnormal return of 0.511%. The value-weighted high-low portfolio generates insignificant returns and alphas. The EW high-low portfolios in the news group, on the other hand, generate a statistically significant average return of 0.75% and an abnormal return of 0.91%. The VW high-low portfolios in the news group generate an average return of 0.66% and an abnormal return of 0.75%. This means that for two months ahead, both the news group and the no-news group continue having drifts. The magnitude of the underreaction to liquidity shocks for the news group continues to be larger but it is statistically insignificant. In the third month after liquidity shocks, abnormal returns for both groups are gone. However, buying high-low decile portfolios in the news group and shorting high-low decile portfolios in the no-news group still generate a significantly positive alpha of 0.54% at a 10% significance level. Furthermore, what is further worth noting is that the elasticity of the markets' underreaction to liquidity shocks is much larger for the news group since the average magnitude of liquidity shocks is much smaller for the news group, but the underreaction to the two groups is about the same level. This provides evidence that the market underreacts to liquidity shocks (with or without news in the same month) up to three months, and the magnitude of underreaction is larger when there is public news.

In order to test the robustness of such results, I control for other factors that predict returns by double sorting the stocks into quintile portfolios based on firm size (or book-to-market equity) and liquidity shocks. Within each quintile of firm size (or book-to-market equity), I find significantly positive alphas of the high-low quintile portfolios for both the no-news group and news group in the following month. Moreover, the magnitudes of alphas are not consistently larger for the news group anymore. For example, within the second-lowest market capitalization quintile, the high-low portfolio for the news group generates a statistically significant alpha (1.66%) larger than the no-news group (1.80%). Whereas, within the third market capitalization quintile, the high-low portfolio for the news group generates an alpha (0.94%, insignificant) smaller than the no-news group (1.92%, significant). This means that after controlling for firm size and book-to-market equity, the difference of underreaction to liquidity shocks in the following month becomes trivial. This provides further evidence that the markets underreact to liquidity shocks for both no-news and news groups, and that public news does not mitigate the markets' underreaction to liquidity shocks. Also, What is worth noting is that the underreaction in the following months is larger and more significant in small size and high book-to-market stocks. The underreaction being stronger in smaller size stocks is consistent with Bali et al. (2014)'s argument that illiquidity may contribute to the markets' underreaction to liquidity shocks because smaller stocks tend to have lower liquidity. However, Why the underreaction is stronger in higher book-to-market equity stocks remains an interesting topic for future research.

The main findings in this paper indicate that even though public news reveals more information to investors and reduces information asymmetry (Tetlock, 2010), it does not help investors discover the information related to liquidity shocks any faster. Moreover, as much as public news draws more investors' attention (Barber and Odean, 2008), the markets' underreaction to liquidity shocks does not get any better either. This finding is inconsistent with Bali et al. (2014)'s argument that investor inattention contributes to the markets' underreaction to liquidity shocks.

This paper contributes to the finance literature in several manners. First, this paper is the first one to answer the question as to whether public news mitigates the markets' underreaction to liquidity shocks. The answer is negative. In some cases, public news actually aggravates the markets' underreaction to liquidity shocks. On the one hand, this improves our understanding of how investors process information related to liquidity shocks and public news. On the other hand, this puts into question the argument posed by Bali et al. (2014) that investor inattention contributes to the markets' underreaction to liquidity shocks. One potential reason why Bali et al. (2014) find evidence that supports the investor-attention story is that their proxy for investor attention (such as firm size, intuitional holdings, analyst coverage, etc.) is highly correlated to illiquidity level. Their finding could be due to the stock-level illiquidity. The underlying mechanism of how investors incorporate liquidity shocks is still unclear in the literature.

Second, this paper helps reconcile the differences in the findings between Savor (2012) and Bali et al. (2014). While Bali et al. (2014) find that the market underreacts to liquidity shocks, Savor (2012) finds that the market overreacts to price movements without news, and he claims that such price movements are due to liquidity shocks or investor sentiment shift. This paper confirms the finding in Bali et al. (2014)'s that the markets indeed underreact to liquidity shocks, with or without public information in the same months. Regarding Savor (2012)'s finding of the markets' overreaction to price movement without public news, this paper provides initial evidence that the markets' overreaction documented in Savor (2012) is not to price movements due to liquidity shocks.

Third, this paper reinforces the argument by Tetlock (2010) that when information asymmetry is reduced, liquidity shocks are also reduced. The average magnitudes of liquidity shocks of the top decile and bottom decile are 5.51 and -3.53 for the news group and 12.73 and -12.24 for the no-news group. The news group has a significantly smaller magnitude of liquidity shocks than the no-news group. When informed investors trade based on private information, it incurs a shock to liquidity demand. Less informed investors provide liquidity to accommodate to such shock to liquidity demand. When there is public news released, information asymmetry is reduced, and the less-informed investors are more willing to accommodate such shocks to liquidity demand. Therefore, the price impact was reduced compared with when there is no public news. In other words, the magnitudes of liquidity shocks are smaller.

The rest of the paper is organized as follows. Section 2 presents a review of previous literature. Section 3 describes the sample and data used. Section 4 describes, in detail, the empirical tests and results. Section 5 presents robustness tests and results. Section 6 discusses the potential explanations of the results. Section 7 concludes the study.

2.2 Literature Review

Most asset pricing literature agrees that illiquidity should be priced (Amihud and Mendelson, 1986; Amihud, 2002, etc.). Asset pricing literature also documents that liquidity is both time-varying and persistent. That is, positive (negative) liquidity shocks predict higher (lower) future liquidity. The persistence of liquidity implies that liquidity predicts future returns and co-moves with contemporaneous returns

(Amihud, 2002).

There is plenty of literature documenting the markets' reactions to different types of signals. Regarding the markets' reaction to liquidity shocks, Baker and Stein (2004) build a model that helps explain why increases in liquidity — such as lower bid-ask spreads, a lower price impact of trade, or higher turnover – predict lower subsequent returns in both firm-level and aggregate data. Similarly, Bali et al. (2014) find that the stock markets underreact to stock-level liquidity shocks and provide evidence that investor inattention and illiquidity contribute to the underreaction.

Regarding the markets' reaction to public news, Daniel, Hirshleifer, and Subrahmanyam (1998) propose a theory that shows that overconfidence implies public-event-based return predictability when managerial actions are correlated with stock mispricing. Biased self-attribution adds positive short-lag autocorrelations and short-run earnings drift. Similarly, Barberis, Shleifer, and Vishny (1998) also propose a theory based on psychological facts showing that the market tends to underreact to public news. In empirical work, Chan (2003) finds strong drift after bad news and reversal after extreme price movements unaccompanied by public news, and they are mainly seen in smaller, more illiquid stocks. Similarly, Savor (2012) finds that price movement accompanied by information is followed by drift, while price movement accompanied by no news results in reversals. He interprets it as investors underacting to news about fundamentals and overreacting to other shocks that move stock prices.

Regarding the markets' reaction to other types of signals, Baker and Wurgler (2006) find that when beginning-of-period proxies for sentiment are low, subsequent returns are relatively high for small stocks, young stocks, high volatility stocks, unprofitable stocks, non-dividend-paying stocks, extreme growth stocks, and distressed stocks. Abarbanell and Bernard (1992) present evidence that analysts' forecasts underreact to recent earnings, and conclude that security analysts' behavior is at best only a partial explanation for stock price underreaction to earnings, and may be unrelated to stock price overreactions. Hong and Stein (1999) build a model where they find that if "newswatchers" and "momentum traders" can only implement simple strategies, their attempts at arbitrage must inevitably lead to overreaction at long horizons. Cohen and Lou (2012) find that it takes conglomerate firms' stocks longer for the information to be incorporated into prices.

Overall, there is no literature that studies the interaction between liquidity shocks and public news, and how public news mitigates the markets' underreaction to liquidity shocks. This paper intends to fill in this gap.

2.3 Sample and Data

2.3.1 Data source

Following the existing literature, my sample includes all common stocks traded on the NYSE, NASDAQ, and AMEX exchanges. The sample covers the period from 2001 to 2010. I only include ten years in my sample because of the availability of public news data. The following are descriptions of the data sources of different variables. (1) Stock return and volume data: The Center for Research in Security Prices (CRSP) database. (2) Accounting variables: Compustat database under Wharton

Research Data Services (WRDS). (3) Public news: Key Development in Capital IQ database.

Regarding the public news data, Key Developments in Capital IQ has structured summaries of material news and events that may affect the market value of securities. It monitors over 200 Key Development types, including executive changes, MA rumors, changes in corporate guidance, delayed filings, SEC inquiries, among others. In other words, this database covers important public news that potentially affects a firm's value and furthermore liquidity. Each Key Development observation includes company name, announced date, entered date, modified date, headline, situation summary, event type, and company identifiers (gvkey).

Following Harris (1994), Jegadeesh and Titman (2001), and Bali et al. (2005), at the end of each portfolio formation month, I eliminate stocks with price per share less than $5 or more than $1000 out of liquidity considerations. Daily and monthly stock returns are adjusted for delisting to avoid survivorship bias.[2] Trading volumes are adjusted for institutional features.[3]

2.3.2 Liquidity measures

Following Amihud (2002) and Bali et al. (2014), I use Amihud measure for illiquidity.[4] The illiquidity of stock i in month t, denoted $ILLIQ_{i,t}$, is the average daily ratio of the absolute stock return to the dollar trading volume within each

[2]See Shumway (1997).

[3]See Gao and Ritter (2010).

[4]I also use other liquidity measures such as volume-weighted effective spread from TAQ database, and find similar results as Amihud measure.

month:

$$ILLIQ_{i,t} = Avg\left[\frac{R_{i,d}}{VOL_{i,d}}\right]$$

where $R_{i,d}$ is the daily return and $VOL_{i,d}$ is the daily dollar trading volume for stock i on day d. Every stock in the sample has at least 15 daily return observations in month t, and Amihud illiquidity measure is scaled by 10^6 following standard liquidity literature.

Liquidity shocks are measured in a parsimonious model, where liquidity shock (LIQU) is defined as the negative difference between ILLIQ of month t and its past 12-month average:

$$\mathrm{LIQU}_{i,t} = -\left(ILLIQ_{i,t} - \mathrm{AVGILLIQ}_{i|t-12,t-1}\right)$$

where $AVGILLIQ_{i|t-12,t-1}$ is the mean of illiquidity over the past 12 months. The average can be interpreted as the conditional expectation of illiquidity of month t conditional on the information of past illiquidity information because liquidity shocks are persistent, and the average is the best estimate of current month's illiquidity. A positive (negative) liquidity shock means that there is an increase (decrease) in the liquidity level of the firm relative to its past 12-month average.

2.3.3 News and no-news group

Capital IQ Key Development database documents all material news of common stocks. If a firm i has no public news in month t, the dummy variable $NEWS_{i,t} = 1$ takes a value of 0. Otherwise, it takes the value of 1.

For each month t, I classify stocks into news group ($NEWS_{i,t} = 1$) and no-news group ($NEWS_{i,t} = 0$), regrouping monthly. This separates liquidity shocks that have no public news in a month and liquidity shocks that have news in the same months. Liquidity shocks that have no public news in the same month occur because of trading activities and not of public news. For example, informed investors trading based on private information may incur a liquidity shock. Mutual funds that have a surprising cash outflow are also a liquidity shock to the stocks they are selling. On the other hand, liquidity shocks with public news in the same month are also due to trading activities, but such activities might or might not be related to the public news. We do not have a good understanding as to what types of news are more related to liquidity shocks than others. The correlation between different types of news and liquidity shocks are beyond the scope of this paper but could be an extension of this paper in the future.

2.3.4 Control variables

All control variables are measured as of the end of portfolio formation month t and require a minimum of 15 daily observations for all variables computed from daily data. Following Fama and French (1992, 1993) and Davis, Fama, and French (2000), market beta (BETA) of an individual stock is estimated by running a time-series regression of monthly return observations over the prior 60 months (if not available, with a minimum of 24 months).

$$R_{i,t} - R_{f,t} = \alpha_i + \beta_i^1 \left(R_{m,t} - R_{f,t}\right) + \beta_i^2 \left(R_{m,t-1} - R_{f,t-1}\right) + \varepsilon_{i,t}$$

where $R_{i,t}$, $R_{f,t}$, and $R_{m,t}$ are the monthly returns on stock i, the one-month Treasury bills, and the CRSP value-weighted index, respectively. The stock's market beta is the sum of the slope coefficients of the current and lagged excess market returns:

$$BETA = \widehat{\beta_l^1} + \widehat{\beta_l^2}$$

A stock's size (LNME) is computed as the natural logarithm of the product of price per share and the number of shares outstanding (in a million dollars). The natural logarithm of the book-to-market equity ratio at the end of June of year t, denoted LNBM, is computed as the difference between the book value of the total asset and total debt for the last fiscal year end in t-1, scaled by the market value of equity at the end of December of year t-1.

Momentum (MOM) is the cumulative return of a stock over a period of 11 months ending one month prior to the portfolio formation month (Jegadeesh and Titman, 1993). Short-term price reversal (REV) is the stock return over the prior month (Jegadeesh, 1990). The idiosyncratic volatility of stock i of month t is defined as the standard deviation of the residuals from the regression:

$$R_{i,d} - R_{f,d} = \alpha_i + \beta_i \left(R_{m,d} - R_{f,d}\right) + \gamma_i SMB_d + \delta_i HML_d + \varepsilon_{i,d}$$

where $R_{i,d}$, $R_{f,d}$, and $R_{m,d}$ are the daily returns of stock i, the one-month Treasury bills, and the CRSP value-weighted index, respectively. SMB_d and HML_d are the daily size and book-to-market factors in Fama and French (1993).

2.3.5 Summary statistics

Panel A of Table 1 reports the summary statistics for the full sample. The mean of my sample for most variables is close to Bali et al. (2014)'s sample. This further validates my variable constructions and gives a good comparison between my results and their results.

[Inserts Table 1 here]

Panel B of Table 1 reports the summary statistics of the variables for the news and no-news groups separately. The news group's sample size is a little over twice the size of the no-news group. The mean of most of the variables between the two groups is quite close, except the news dummy (NEWS) and momentum (MOM). It is of more importance to us as to whether there is a huge difference in liquidity shocks between the two groups, and the answer is negative. This gives us a good treatment group and a control group because it shows evidence to a certain level that the liquidity shocks of the two groups are not statistically significantly different.

2.4 Empirical Tests

Before I started the empirical tests and analysis of this paper, I first replicated Table 1, 2, 3, and 6 of Bali et al. (2014)'s paper. The results are similar in terms of both significance and magnitude. This validates the sample used in this paper and the constructions of different variables. Furthermore, it confirms the results of Bali et al. (2014). The replication results are reported in Appendix A.

2.4.1 Research hypotheses

My main hypothesis is that the market underreacts to liquidity shocks less for the news group than for the no-news group. There are two arguments in the literature about how public news helps investors process liquidity shocks related information. One is the investors' attention story. Bali et al. (2014) argue that it is investors' inattention that contributes to markets' underreaction to liquidity shocks. Public news draws more investors' attention (Barber and Odean, 2008). If that is the case, liquidity shocks attached to more news should have a faster and more efficient price discovery process and therefore generate a smaller drift than the no-news group. The other is that public news reveals more information to the investors and reduces information asymmetry. It might reveal some information that is reflected in the liquidity shocks. This is why we would expect the markets to underreact to liquidity shocks less when there is public news.

2.4.2 Contemporaneous portfolio returns

In order to examine the magnitude of drifts between the two liquidity shocks group, I perform a nonparametric analysis and sort the common stocks monthly within each group into decile portfolios based on liquidity shocks. I then compare the performance of high-liquidity shock portfolios to low-liquidity shocks portfolios in contemporaneous and the following months. If the market underreacts to liquidity shocks, the top (bottom) decile liquidity-shock portfolios should generate positive (negative) abnormal returns in current months, and continue to do so in the following months. The performance of high-low (long top decile and short bottom decile) liquidity-shock portfolios should generate a statistically significant abnormal return in contemporaneous and following months.

[Inserts Table 2 here]

Table 2 reports the contemporaneous returns of the decile portfolios. Panel A reports the portfolio performance of the full sample, and high-low equal-weighted (EW) decile-portfolios generate a significant alpha of 3.22% (the alpha for value-weighted (VW) portfolios is 3.01%). This is consistent with Bali et al. (2014)'s finding.

Panel B reports the decile portfolio's performance of the no-news group. High-low EW portfolios generate a significant alpha of 2.39% (the alpha for VW portfolios is 2.42%). The lower decile portfolios do not generate significant abnormal returns, and the majority of the drift comes from the top liquidity shocks portfolios.

Panel C reports the decile portfolio's performance of the news group. High-low EW portfolios generate a significant alpha of 4.90% (the alpha for VW portfolios is 4.33%). Similar to the no-news group, the lower decile portfolios do not generate significant abnormal returns, and the majority of the drift comes from the top liquidity shocks portfolios.

The results in Table 2 demonstrate that for both news group and no-news group, the market reacts to the liquidity shocks in the contemporaneous months. More importantly, Panel B of Table 2 documents the markets' reaction to pure liquidity shocks. This is consistent with the theory that liquidity is priced and that investors update their beliefs about liquidity level when there are liquidity shocks.

What is also worth noting is that in Column 6, the liquidity shocks levels are different between the two groups. The average magnitudes of liquidity shocks of the top decile and bottom decile are 5.51 and -3.53 for the news group and 12.73 and -12.24 for the no-news group. The news group has a significantly smaller magnitude of liquidity shocks than the no-news group. This is consistent with the findings in Tetlock (2010) that when informed investors trade based on private information, it incurs a shock to liquidity demand. Less informed investors provide liquidity to accommodate to such shock to liquidity demand. When there is public news released, information asymmetry is reduced, and the less-informed investors are more willing to accommodate such shocks to liquidity demand. Therefore, the price impact was reduced compared with when there is no public news. In other words, the magnitudes of liquidity shocks are smaller. The elasticity of the markets' underreaction to liquidity shocks is larger in the news group (50.88%) than the no-news group (10.29%). The elasticity can be calculated as the difference of the markets' underreaction between the top and bottom decile portfolios in the following month divided by the difference of magnitudes of liquidity shocks between the top and bottom decile portfolios. It can be interpreted as for one unit of liquidity shock, how much underreaction (measured in abnormal returns) is for each group.

2.4.3 One-month ahead portfolio returns

If the markets are efficient, liquidity related information should be incorporated into prices instantaneously in current months, and we should not observe abnormal returns in the following months. I examine the one-month ahead portfolio returns for both news and no-news group. The findings can be found in Table 3.

[Inserts Table 3 here]

Panel A reports the portfolio returns for the full sample, and it shows that the high-low liquidity shocks portfolios continue generating significantly positive abnormal returns in the next months. This is consistent with the findings in Bali et al. (2014).

Panel B reports the portfolio returns for the no-news group. High-low liquidity shocks EW (VW) portfolios continue generating significantly positive abnormal returns of 0.75% (1.11%) in the next months. This reinforces the evidence that the markets do underreact to liquidity shocks related information. Note that the liquidity shocks here are uncontaminated, i.e., have no public news in current months, and therefore such liquidity shocks are not related to public news.

Panel C reports the portfolio returns of the news group, and high-low liquidity shocks EW (VW) portfolios continue generating significantly positive abnormal returns of 1.27% (1.34%) in the next months. Note that the magnitude of such abnormal return is 0.52% (0.23% for VW portfolios) larger than the no-news group.

We can infer two points from these results. One, the market underreacts to liquidity shocks even when there is public news in the same months. Two, the magnitude of price drift after liquidity shocks is larger when there is public news "attached". In other words, the market underreacts to liquidity shocks even more when there is public news in the same months.

What is worth noting is that the magnitude of liquidity shocks for the news group is smaller (due to a reduction in information asymmetry as discussed above), but the markets' underreaction level is about the same, if not larger. In other words, the elasticity of the markets' underreaction to liquidity shocks when there is public news is much larger than when there is no news.

Up until here, this provides initial evidence against the argument of investors' inattention. If investors' inattention contributes to the markets' underreaction to liquidity shocks, we should expect the price drift after liquidity shocks to be smaller when there is public news, since public news typically draws more investors' attention. On the other hand, such evidence is consistent with the theory of the markets' underreaction to public signals. When there are public signals, information asymmetry is reduced, and ex-ante uninformed investors are less wary of being liquidity providers, and hence are slower in reacting to liquidity shocks. This slows down the price discovery process of liquidity shocks.

2.4.4 Two-month-ahead portfolio returns

I also investigate how long the markets' underreaction to liquidity shocks lasts. In Table 4, I examine the two-month-ahead portfolio returns.

[Inserts Table 4 here]

Panel A shows that the markets continue to react in the following second month to pure liquidity shocks with no news attached. High-low EW portfolios generate a return of 0.58% and an alpha of 0.51%, and high-low VW portfolios generate an insignificant return and alpha.

On the other hand, Panel B shows that the market continues to react in the following month to liquidity shocks with news attached. Both high-low EW and VW portfolios generate a statistically significant return (0.75% and 0.66% respectively) and alpha (0.91% and 0.75% respectively). This further provides evidence that the news group has not only a larger drift (magnitude of underreaction is larger) but also a longer and more robust drift (both EW and VW have drifted up until the second month). This is consistent with the markets' underreaction to the public signal story.

2.4.5 Three-month-ahead portfolio returns

Does the markets' underreaction continue in the third month after the liquidity shocks? I examine the three-month-ahead portfolio return in Table 5. From the results in Panel A, the no-news group high-low portfolios generate an insignificant alpha of 0.15% for EW and 0.22% for VW. Panel B shows that the news group high-low portfolios also generate an insignificant alpha of 0.37% and 0.44%. This shows that the markets' underreaction decays to zero in the third month.

[Inserts Table 5 here]

2.4.6 Long-short the news and no-news portfolios

We have learned from a simple comparison between the magnitudes that the news group has a larger drift than the no-news group up until the second month. I test the significance of such a difference in this section by buying a high-low news group and shorting high-low no-news group portfolios. This has revealed that the drift difference between the news and the no-news group. The results are shown in Table 6.

[Inserts Table 6 here]

It can be seen from Table 6 that for both current months, 1-month ahead, 3-month ahead, the news group has a larger drift difference between high and low liquidity shocks decile portfolios than the no-news group, and the differences are 2.45%, 0.51%, and 0.54% respectively at 10% significance level. Even though the separate drifts for the news group and the no-news group are gone in the third month as shown in Table 5, buying the news group and shorting the no-news group still generate a significantly positive alpha.[5] I also plot the markets' underreaction to liquidity shocks for the new and no-news group in Figure 1, where the markets' underreaction to liquidity shocks is larger when there is news in the same month as the liquidity

[5]Such results are robust using either the Fama-French three-factor model (Fama and French, 1993) or five-factor model (based on Fama and French (2016), and Hou, Xue, and Zhang (2015)). The results reported here are derived using the Fama-French five-factor model.

shocks. This reinforces the evidence that liquidity shocks with news attached make markets' underreaction get even worse than when there is no news attached. All the above evidence confirms the argument of the markets' underreaction to liquidity shocks and does not seem to support the investors' inattention story.

[Inserts Figure 1 here]

2.5 Robustness Test

2.5.1 Bivariate sort portfolios

In order to control other return predicting variables, I double sort the portfolios into quintiles based on liquidity shocks and market capitalization or book-to-market ratio. By doing this, I could at least control for these two variables that are important in explaining the cross-section of stock returns in a non-linear way. The results are shown in Table 7.

[Inserts Table 7 here]

As shown in Panel A1 and A2 in Table 7, within quintile 2 and quintile 3 of market capitalization, high-low liquidity shocks portfolios still generate significantly positive alpha of 1.66% and 1.92%, respectively, in the following months for the no-news group. Within quintile 1 and quintile 2 of market capitalization, high-low liquidity shocks portfolios generate significantly positive alpha of 0.99% and 1.80%.

Similarly, Panel B1 and Panel B2 in Table 7 show that within most of the book-to-market quintiles, particularly the higher quintiles, high-low liquidity shocks portfolios generate significantly positive alphas.

There are two interesting findings from Table 7 that are worth noting. First, there is not much difference in the magnitude of underreactions between no-news group and news group after controlling for size and book-to-market equity. This is still consistent with the argument that public news does not mitigate the markets' underreaction to liquidity shocks. Two, most of the underreaction comes from smaller size firms and high book-to-market firms. There are obviously some correlations between the markets' underreaction to liquidity shocks and size effect or value effect. The underreaction being stronger in smaller size stocks is consistent with Bali et al. (2014)'s argument that illiquidity may contribute to the markets' underreaction to liquidity shocks because smaller stocks tend to have lower liquidity. Why the underreaction is stronger in higher book-to-market equity stocks remains an interesting topic for future research.

2.5.2 Stock-level cross-sectional analysis

The nonparametric analysis at portfolio level is to control for variables in a non-linear way. I also account for other control variables to reinforce the results I find. I control for market beta (BETA), log market capitalization (LNME), log book-to-market ratio (LNBM), momentum (MOM), idiosyncratic volatility (IVOL), and Amihud illiquidity (ILLIQ). I run a Fama-MacBeth (1973) regression to examine whether liquidity shocks explain the cross-sectional variation of stock returns in the following months controlling for other predicting variables. Current month and one-month-ahead stock-level excess returns are separately regressed on the liquidity shock, news dummy, interaction of liquidity shock and news dummy, and other pricing factors:

$$R_{i,t} - R_{f,t} = \alpha_{i,t} + \beta_{i,t+1} LIQU_{i,t} + \gamma_{i,t+1} LIQU_{i,t} \times News_{i,t} + \delta_{i,t+1} News_{i,t} + \theta_{i,t+1} X_{i,t} + \varepsilon_{i,t}$$

$$R_{i,t+1} - R_{f,t+1} = \alpha_{i,t+1} + \beta_{i,t+1} LIQU_{i,t} + \gamma_{i,t+1} LIQU_{i,t} \times News_{i,t} + \delta_{i,t+1} News_{i,t} + \theta_{i,t+1} X_{i,t} + \varepsilon_{i,t+1}$$

where $R_{i,t+1} - R_{f,t+1}$ is the realized excess return on stock i in month t+1, $LIQU_{i,t}$ represents Amihud measure of liquidity shock of stock i in month t. $News_{i,t}$ is the news dummy that equals one if there is public news in month t and 0 otherwise. $X_{i,t}$ is a vector of control variables mentioned above for stock i in month t. Moreover, I include an interaction term of liquidity shock and news dummy.

$\gamma_{i,t+1}$ is the coefficient of interest. If $\gamma_{i,t+1}$ is positive, it means that public news makes the markets underreact to liquidity shocks more. If $\gamma_{i,t+1}$ is negative, it means that public news mitigates the markets' underreaction to liquidity shocks. Table 8 reports the Fama-MacBeth regression results.

[Inserts Table 8 here]

In Table 8, columns 1-2 report the regression results for the no-news group, columns 3-4 reports the regression results for the news group, and columns 5-6 reports the regression results for the full sample.

For both the news group and the no-news group, liquidity shocks can explain the cross-sectional variation of stock returns of current months. Since $\gamma_{i,t+1}$ is significantly positive, the magnitude for liquidity shocks' explanatory power of the current month's returns is larger for the news group. $\gamma_{i,t+1}$ is statistically insignificant, which means that the markets' underreaction to liquidity shocks is indifferent between the news and the no-news group after controlling for other return predicting variables, which is consistent with the results revealed in Table 7.

The standard procedure of Fama-MacBeth regressions treats large firms and small firms equally. Placing greater weight on small firms could generate noise, and it does not reflect the effect of an average dollar. Following Ang, Hodrick, Xing, and Zhang (2009), I also run a value-weighted Fama-MacBeth regression. In the first stage, I run GLS regressions with a diagonal weighting matrix that equals the inverse of the firms' market capitalization along the diagonal. This is analogous to creating value-weighted portfolios. The value-weighted Fama-MacBeth regression results are reported in Table 9. Once again, I find similar results to those in Table 8. This reinforces my findings in this paper that public news does not mitigate the markets' underreaction to liquidity shocks.

[Inserts Table 9 here]

2.5.3 Matching liquidity shocks

In order to further ensure that the treatment group (with news) and control group (without news) have the same liquidity shocks level and that it is not the magnitude of liquidity shocks that drive the results, I also apply another method to categorize the news group and no-news group. I sort the full sample into decile portfolios based on liquidity shocks. Within each decile, I calculate the returns of the portfolios that have news in the current month and the returns of the portfolios that have no-news in the current month. This was done to guarantee that the news and no-news group are in the same liquidity shocks decile. The results are reported in Table 10. I find that the markets underreact to liquidity shocks for both groups in the following month of the liquidity shocks, and the magnitude of underreaction is almost the same. The high-low portfolios generate a significant alpha of 0.99% for the no-news group and 0.97% for the news group.

[Inserts Table 10 here]

2.5.4 Does public news truly draw more investors' attention?

The empirical literature has documented that both institutional investors and retail investors increase their trading volume during news release periods. [6] In order to make sure that this is also the case in my sample, I examine if the share

[6] Examples include Barber and Odean (2008), Yuan (2008), and Nofsinger (2001), etc.

turnover is significantly higher during news release periods. As a robustness test to check the correlation between investor attention and public news release, I simply regress share turnover on news dummy, which is an indicator of whether there is news released in month t. The results are reported in Table 11. Stocks that have news released in month t have significantly higher share turnover, which indicates that there are more trading activities for those stocks during news release periods. This is consistent with the evidence that public news draws more investor attention documented in previous literature. Such evidence supports using public news as a proxy for investors' attention.

2.6 Discussion and Conclusion

This paper addresses two major questions. The first question is whether equity markets underreact or overreact to liquidity shocks. Regarding this point, I have found that markets underreact to liquidity shocks whether or not there is contemporaneous firm news. The second question is whether public news mitigates the markets' underreaction to liquidity shocks. The answer to this was negative. If anything, public news, in fact, aggravates the markets' underreaction. Public news may affect the magnitude of liquidity shocks but does not affect how fast the market incorporates such information into prices.

There are four important notions behind this phenomenon. First, what are liquidity shocks without public news? Liquidity shocks come from trading activities, but there are two major sources of liquidity shocks without public news. One is from informed traders (Tetlock, 2010). When informed traders trade based on private information, this generates a persistent liquidity shock. Another is from funding liquidity shock, which results in market liquidity shock. In other words, either informed trading or noisy trading can cause liquidity shocks. There are many more sources of liquidity shocks without public news, and such sources are beyond the scope of this paper. Our understanding of liquidity could benefit greatly from future research regarding this topic.

Second, why does the market respond to liquidity shocks? Stock-level illiquidity is priced. Hence, every month, investors form an expectation of the liquidity of a stock for next month (in my case, based on the past twelve months' average). Next month, when the true liquidity is realized and if it is different from investors' expectations, investors update their beliefs about expected liquidity. This is the innovation of liquidity, which is defined as liquidity shocks. In each month, they repeat the same action. Hence, the market is incorporating liquidity shocks into prices every single month.

Third, why do the markets underreact to liquidity shocks? Unlike direct and well-defined information event-type public news studied in previous literature, liquidity related information is not well defined, transparent, or tangible; it is also harder for investors to capture and interpret. I argue that liquidity measures like Amihud are mostly studied in academics, and it is rather challenging for an average investor to have a good grasp of how liquidity level changes, especially for the general unsophisticated investors. Furthermore, the fact that there is a market's underreaction might mean that either investors leave money on the table, or there are limits to arbitrage that prevents the free money to be arbitraged away. For example, the long-short portfolios in my study might not be a zero-cost portfolio in reality because of transaction or liquidity costs or short-sale constraints. Hence, it takes a while for the market to fully interpret the information in such signals. As a result, the stock market underreacts to liquidity shocks.

Lastly, is investors' inattention a reason for the markets' underreaction to liquidity shocks? The answer seems to be no. Bali et al. (2014)'s argument that investor inattention can explain the markets' underreaction to liquidity shocks is inconsistent with the findings in this paper. I find that public news, which usually draws more attention from investors, does not mitigate the markets' underreaction to liquidity shocks. Public news may affect the magnitude of liquidity shocks and draw more investors' attention, but given a liquidity shock, public news does not speed up the process of updating the liquidity level into prices. The reason why they have contrast findings in their paper is that their proxy for investors' attention is market capitalization, analyst coverage, and institutional holdings, all of which are highly correlated with firm size and stock-level illiquidity. Larger firms tend to have more analyst coverage, more institutional holdings, and higher liquidity level. It is difficult to disentangle whether it is size effect, illiquidity effect, or "attention" proxy that contributes to the markets' underreaction. Their argument that stock-level illiquidity can partially explain the markets' underreaction seems more reasonable. The underlying mechanism of the markets' underreaction to liquidity shocks remains unclear and requires more exploration in future research.

Another question that deserves the attention of scholars is why the markets' underreaction to liquidity shocks is highly correlated with size and value effects. Moreover, the underlying mechanism of why the market underreacts to liquidity shocks is still unclear. We only know that liquidity related information is intangible compared to event-type public news, and that is probably why the market underreacts to it. However, how investors form their beliefs about liquidity each month and over what horizon, are not well known and remain a heated topic in future finance research.

Figure 2.1: The Market's Underreaction to Liquidity Shocks for News and No-news Group

This figure plots the event-time risk-adjusted returns for equal-weighted portfolios after the monthly liquidity shocks. For each month t , NYSE, AMEX, and NASDAQ stocks are categorized into news and no-news group. Within each group, stocks are sorted into 10 decile portfolios based on Amihud liquidity shock measures (LIQU) using the NYSE breakpoints. LIQU denotes the liquidity shock, defined as the negative Amihud (2002) illiquidity measure (ILLIQ), demeaned using the past 12-month ILLIQ as the mean. The risk-adjusted returns for each decile portfolio plotted in this figure are based on the Fama-French (1993) three-factor benchmark model. The sample period for the results is from January 2001 to December 2010.

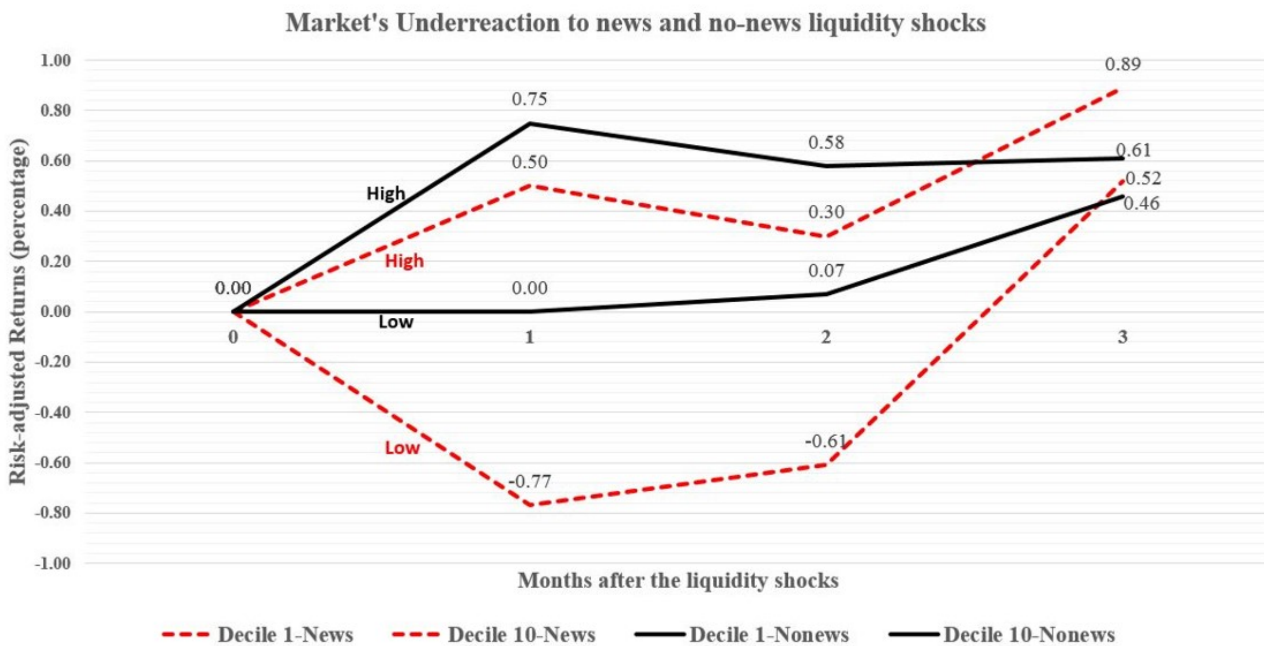


Table 2.1: Summary statistics

Panel A reports the time-series averages of the cross-sectional mean, median, and standard deviation of each variable for the full sample used in this paper. Panel B reports the same statistics for news group and no-news group. All the variables, except for RET, the return in month t+1, are computed for individual stocks at the end of the portfolio formation month (month t). LIQU denotes the liquidity shock, defined as the negative Amihud (2002) illiquidity measure (ILLIQ), demeaned using the past 12-month ILLIQ as the mean. BETA, LNME, and LNBM denote the market beta, the natural logarithm of the market capitalization, and the natural logarithm of the book-to-market equity ratio, respectively. The sample covers the period from January 2001 to December 2010.

Panel A: Full Sample

Variable	Mean	Min	Median	Max	Std. dev.	Obs
RET	1.88	-94.01	0.93	1349.51	14.58	398955
ILLIQ	0.94	0.00	0.01	1088.09	9.19	398974
LIQU	0.27	-1066.94	0.00	1011.81	10.51	398974
NewsAmount	2.66	0.00	2.00	178.00	4.14	398974
NewsDummy	0.71	0.00	1.00	1.00	0.45	398974
BETA	1.17	-6.30	0.95	18.01	1.02	371108
LNME	6.27	-0.40	6.19	13.31	1.91	371108
LNBM	-0.70	-8.86	-0.62	3.28	0.82	371108
MOM	19.93	-97.57	9.59	9857.14	75.17	371108
REV	1.82	-94.01	0.94	1349.51	14.26	371108
IVOL	2.08	0.03	1.71	134.57	1.51	371108

Panel B Full Sample categorized by New and No News Group

Variable	Mean			Min		Median			Max		Std. dev.		Obs	
	News	No News	Diff	News	No News	News	No News	Diff	News	No News	News	No News	News	No News
RET	1.88	1.87	0.01	-94.01	-88.65	1.00	0.81	0.19	1349.51	365.91	14.94	13.67	283291	115664
ILLIQ	0.64	1.67	(1.03)	0.00	0.00	0.00	0.04	(0.04)	636.47	1088.09	8.05	11.48	283309	115665
LIQU	0.25	0.33	(0.08)	-633.20	-1066.94	0.00	0.00	0.00	1011.81	866.62	9.61	12.46	283309	115665
News#	3.74	0.00	3.74	1.00	0.00	3.00	0.00	3.00	178.00	0.00	4.47	0.00	283309	115665
BETA	1.27	0.94	0.33	-6.30	-3.98	0.95	0.72	0.23	18.01	16.99	1.05	0.93	263739	107369
LNME	6.59	5.49	1.10	-0.40	-0.40	6.19	5.40	0.79	13.31	13.31	1.89	1.72	263739	107369
LNBM	-0.75	-0.56	(0.19)	-8.86	-7.22	-0.62	-0.47	(0.15)	3.28	3.27	0.81	0.82	263739	107369
MOM	19.33	21.40	(2.07)	-97.57	-94.94	9.59	11.90	(2.31)	9857.14	8550.00	75.99	73.10	263739	107369
REV	1.83	1.80	0.03	-94.01	-88.65	0.94	0.82	0.12	1349.51	365.91	14.71	13.08	263739	107369
IVOL	2.06	2.11	(0.05)	0.03	0.04	1.71	1.74	(0.03)	134.57	52.84	1.54	1.45	263739	107369

Table 2.2: Contemporaneous returns for portfolios formed on LIQU for nonews and news groups

For month t , NYSE, AMEX, and NASDAQ stocks are categorized into news and no-news group. Within each group, stocks are sorted into 10 decile portfolios based on Amihud liquidity shock measures (LIQU). LIQU is defined as the negative Amihud (2002) illiquidity measure (ILLIQ), demeaned using the past 12-month ILLIQ as the mean. This table reports the equal- and value-weighted monthly contemporaneous returns (month t) and the alpha with respect to the Fama-French (1993) factors for each LIQU portfolio. Columns "LIQU" and "ILLIQ" report the average LIQU and ILLIQ values for each decile portfolio. The last column shows the average market share of each portfolio. The last row shows the differences in monthly returns between high- and low-LIQU decile portfolios and the alphas.Average returns and alphas are defined in monthly percentage terms. Newey-West t-statistics are given in parentheses. The sample covers the period from January 2001 to December 2010. Panel A reports the full sample. Panel B reports no news group. Panel C reports news group.

Panel A Full Sample

	Equal-weighted		Value-weighted				
Decile	RET	Alpha	RET	Alpha	LIQU	ILLIQ	% Mkt. shr.
1(Low)	0.54	-0.31*	-0.33	-1.07***	-2.70	4.09	6.07
	(0.92)	(-1.80)	(-0.50)	(-5.25)			
2	0.22	-0.48**	-0.29	-0.86***	-0.00	0.01	14.59
	(0.34)	(-2.22)	(-0.41)	(-3.68)			
3	0.54	-0.11	0.20	-0.40**	-0.00	0.00	20.51
	(0.83)	(-0.62)	(0.30)	(-2.27)			
4	0.64	-0.01	0.35	-0.25	-0.00	0.00	16.19
	(1.02)	(-0.05)	(0.52)	(-1.47)			
5	0.76	0.09	0.57	-0.02	0.00	0.00	11.91
	(1.31)	(0.62)	(0.90)	(-0.12)			
6	1.06*	0.43***	1.02	0.46***	0.00	0.00	11.33
	(1.82)	(3.62)	(1.63)	(2.84)			
7	1.32**	0.68***	1.20*	0.66***	0.00	0.00	7.75
	(2.34)	(5.43)	(1.98)	(3.72)			
8	1.65***	0.89***	1.51***	0.85***	0.00	0.01	5.69
	(2.98)	(6.79)	(2.70)	(5.45)			
9	2.13***	1.21***	1.97***	1.16***	0.01	0.02	3.11
	(3.80)	(9.53)	(3.56)	(6.53)			
10(High)	3.74***	2.91***	2.82***	1.94***	2.61	0.95	2.84
	(6.46)	(15.26)	(4.89)	(12.67)			
High-Low	3.21***	3.22***	3.15***	3.01***			
	(13.08)	(13.56)	(8.04)	(9.60)			
Obs.	120	120	120	120	120	120	120

Newey-West t-statistics in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Panel B No-News Group

	Equal-weighted		Value-weighted				
Decile	RET	Alpha	RET	Alpha	LIQU	ILLIQ	% Mkt. shr.
1(Low)	0.25	-0.18	-0.31	-0.67*	-12.24	17.20	1.86
	(0.52)	(-0.45)	(-0.65)	(-1.66)			
2	0.39	-0.18	-0.41	-0.92*	-0.13	0.47	11.94
	(0.64)	(-0.31)	(-0.76)	(-1.73)			
3	0.41	-0.17	-0.01	-0.55	-0.01	0.07	21.81
	(0.69)	(-0.30)	(-0.01)	(-0.97)			
4	0.87	0.33	0.67	0.17	-0.00	0.02	22.78
	(1.49)	(0.62)	(1.08)	(0.29)			
5	0.84	0.35	0.73	0.23	0.00	0.02	16.99
	(1.33)	(0.65)	(1.12)	(0.40)			
6	1.39**	0.96*	1.04	0.64	0.01	0.05	10.95
	(2.30)	(1.82)	(1.62)	(1.11)			
7	1.90***	1.43***	1.31*	0.86	0.06	0.14	7.98
	(3.06)	(2.67)	(1.91)	(1.42)			
8	2.46***	1.98***	2.31***	1.85***	0.29	0.39	4.01
	(4.78)	(4.58)	(4.32)	(3.92)			
9	2.06***	1.65***	1.74***	1.32***	1.58	1.34	1.08
	(5.00)	(4.68)	(4.14)	(3.61)			
10(High)	2.68***	2.21***	2.21***	1.75***	12.73	3.75	0.61
	(4.06)	(3.89)	(3.43)	(3.18)			
High-Low	2.43***	2.39***	2.52***	2.42***			
	(6.16)	(6.13)	(5.55)	(5.28)			
Observations	120	120	120	120	120	120	120

Newey-West t-statistics in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Panel C News Group

	Equal-weighted		Value-weighted				
Decile	RET	Alpha	RET	Alpha	LIQU	ILLIQ	% Mkt. shr.
1(Low)	0.91	0.30	-0.25	-0.82	-3.53	5.02	5.59
	(1.54)	(0.59)	(-0.40)	(-1.57)			
2	0.24	-0.36	-0.49	-1.08	-0.01	0.03	16.98
	(0.35)	(-0.53)	(-0.67)	(-1.49)			
3	0.46	-0.16	0.01	-0.58	-0.00	0.00	22.11
	(0.67)	(-0.25)	(0.02)	(-0.84)			
4	0.79	0.24	0.51	-0.04	-0.00	0.00	17.48
	(1.25)	(0.39)	(0.73)	(-0.05)			
5	1.10*	0.57	0.91	0.39	0.00	0.00	13.46
	(1.71)	(0.97)	(1.36)	(0.64)			
6	1.30**	0.77	1.18*	0.66	0.00	0.01	11.07
	(2.01)	(1.32)	(1.79)	(1.11)			
7	2.08***	1.59***	1.88***	1.41**	0.01	0.01	7.00
	(3.21)	(2.83)	(2.87)	(2.50)			
8	2.56***	2.09***	2.15***	1.72***	0.02	0.04	3.67
	(3.95)	(3.61)	(3.37)	(2.98)			
9	4.17***	3.61***	3.38***	2.91***	0.12	0.14	2.18
	(5.74)	(5.68)	(4.91)	(4.83)			
10(High)	5.78***	5.19***	4.20***	3.51***	5.51	1.62	0.46
	(7.41)	(7.77)	(5.97)	(6.22)			
High-Low	4.87***	4.90***	4.44***	4.33***			
	(10.83)	(9.70)	(9.51)	(9.21)			
Observations	120	120	120	120	120	120	120

Newey-West t-statistics in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 2.3: One-month-ahead returns for portfolios formed on liquidity shocks

For month t , NYSE, AMEX, and NASDAQ stocks are categorized into news and no-news group. Within each group, stocks are sorted into 10 decile portfolios based on Amihud liquidity shock measures (LIQU) using the NYSE breakpoints. LIQU denotes the liquidity shock, defined as the negative Amihud (2002) illiquidity measure (ILLIQ), demeaned using the past 12-month ILLIQ as the mean. This table reports the equal- and value-weighted one-month-ahead returns (month t+1) and the alpha with respect to the Fama-French (1993) factors for each liquidity shock decile portfolio. The last row shows the differences in monthly returns between high- and low-liquidity shock decile portfolios and the corresponding alphas. Average returns and alphas are defined in monthly percentage terms. Newey-West t-statistics are given in parentheses. The sample period for the results is from January 2001 to December 2010.

Panel A Full Sample

	Equal-weighted		Value-weighted				
Decile	RET	Alpha	RET	Alpha	LIQU	ILLIQ	% Mkt. shr.
1(Low)	0.22	-0.55***	-0.06	-0.76***	-2.70	4.09	6.03
	(0.33)	(-3.69)	(-0.09)	(-3.59)			
2	0.48	-0.12	0.46	-0.04	-0.00	0.01	14.45
	(0.70)	(-0.59)	(0.73)	(-0.22)			
3	0.59	0.08	0.35	-0.05	-0.00	0.00	20.47
	(0.90)	(0.51)	(0.56)	(-0.32)			
4	0.82	0.26**	0.71	0.22	-0.00	0.00	16.18
	(1.33)	(2.14)	(1.21)	(1.48)			
5	0.76	0.14	0.66	0.08	0.00	0.00	11.91
	(1.15)	(1.08)	(1.03)	(0.54)			
6	0.80	0.15	0.80	0.23	0.00	0.00	11.35
	(1.23)	(1.23)	(1.32)	(1.58)			
7	0.66	0.00	0.65	0.05	0.00	0.00	7.80
	(1.14)	(0.04)	(1.10)	(0.45)			
8	0.73	-0.02	0.58	-0.05	0.00	0.01	5.74
	(1.23)	(-0.15)	(1.02)	(-0.34)			
9	0.91	0.08	0.82	0.09	0.01	0.02	3.15
	(1.63)	(0.69)	(1.48)	(0.55)			
10(High)	1.15**	0.40***	0.94*	0.15	2.61	0.95	2.92
	(2.11)	(2.89)	(1.68)	(1.00)			
High-Low	0.93***	0.95***	1.01**	0.91***			
	(3.28)	(4.53)	(2.58)	(2.88)			

Newey-West t-statistics in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Panel B No-News Group

	Equal-weighted		Value-weighted				
Decile	RET	Alpha	RET	Alpha	LIQU	ILLIQ	% Mkt. shr.
1(Low)	0.17	-0.42	-0.17	-0.76***	-12.24	17.20	1.83
	(0.29)	(-1.48)	(-0.33)	(-3.43)			
2	-0.03	-0.78***	-0.10	-0.81***	-0.13	0.47	11.84
	(-0.04)	(-3.07)	(-0.14)	(-2.71)			
3	0.51	-0.30	0.69	0.06	-0.01	0.07	21.76
	(0.72)	(-1.47)	(1.09)	(0.25)			
4	0.58	-0.15	0.60	-0.02	-0.00	0.02	22.79
	(0.85)	(-0.72)	(0.88)	(-0.10)			
5	0.67	-0.25	0.71	-0.11	0.00	0.02	17.01
	(1.10)	(-1.65)	(1.08)	(-0.53)			
6	0.96	0.07	0.63	-0.19	0.01	0.05	10.98
	(1.51)	(0.43)	(1.00)	(-0.80)			
7	0.94	0.02	0.82	-0.04	0.06	0.14	7.98
	(1.53)	(0.14)	(1.31)	(-0.15)			
8	1.19*	0.48	1.03*	0.34	0.29	0.39	4.08
	(1.91)	(1.48)	(1.72)	(0.96)			
9	1.26***	0.72***	1.17**	0.64**	1.58	1.34	1.10
	(2.70)	(2.69)	(2.43)	(2.00)			
10(High)	0.92	0.33	0.90	0.36	12.73	3.75	0.62
	(1.59)	(0.90)	(1.47)	(0.85)			
High-Low	0.75**	0.75***	1.07***	1.11***			
	(2.45)	(2.68)	(2.92)	(3.18)			
Observations	119	119	120	120	120	120	120

Newey-West t-statistics in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Panel C News Group

	Equal-weighted		Value-weighted				
Decile	RET	Alpha	RET	Alpha	LIQU	ILLIQ	% Mkt. shr.
1(Low)	-0.06	-0.77***	-0.42	-1.09***	-3.53	5.02	5.57
	(-0.09)	(-4.04)	(-0.59)	(-4.61)			
2	0.22	-0.46***	0.03	-0.56**	-0.01	0.03	16.87
	(0.29)	(-2.81)	(0.04)	(-2.45)			
3	0.72	0.19	0.49	0.08	-0.00	0.00	22.01
	(1.10)	(1.11)	(0.79)	(0.40)			
4	0.59	0.02	0.52	-0.01	-0.00	0.00	17.53
	(0.90)	(0.13)	(0.82)	(-0.04)			
5	0.64	-0.02	0.63	0.02	0.00	0.00	13.48
	(0.97)	(-0.19)	(0.96)	(0.13)			
6	0.69	-0.01	0.67	0.02	0.00	0.01	11.10
	(1.07)	(-0.08)	(1.12)	(0.17)			
7	0.68	-0.12	0.54	-0.20	0.01	0.01	7.04
	(1.09)	(-0.95)	(0.87)	(-1.28)			
8	0.90	0.04	0.76	-0.00	0.02	0.04	3.71
	(1.51)	(0.28)	(1.30)	(-0.02)			
9	1.24**	0.43*	0.97	0.16	0.12	0.14	2.20
	(2.06)	(1.80)	(1.64)	(0.75)			
10(High)	1.16**	0.50**	0.94	0.25	5.51	1.62	0.49
	(2.12)	(2.30)	(1.46)	(0.90)			
High-Low	1.22***	1.27***	1.36***	1.34***			
	(3.74)	(4.98)	(2.90)	(3.31)			
Observations	119	119	120	120	120	120	120

Newey-West t-statistics in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 2.4: Two-month-ahead returns for portfolios formed on liquidity shocks

For month t , NYSE, AMEX, and NASDAQ stocks are categorized into news and no-news group. Within each group, stocks are sorted into 10 decile portfolios based on Amihud liquidity shock measures (LIQU). LIQU denotes the liquidity shock, defined as the negative Amihud (2002) illiquidity measure (ILLIQ), demeaned using the past 12-month ILLIQ as the mean. This table reports the equal- and value-weighted two-month-ahead returns (month t+2) and the alpha with respect to the Fama-French (1993) factors for each liquidity shock decile portfolio. The last row shows the differences in monthly returns between high- and low-liquidity shock decile portfolios and the corresponding alphas. Average returns and alphas are defined in monthly percentage terms. Newey-West t-statistics are given in parentheses. The sample period for the results is from January 2001 to December 2010.

Panel A No-News Group

	Equal-weighted		Value-weighted	
Decile	RET	Alpha	RET	Alpha
1(Low)	0.656	0.070	0.560	-0.059
	(1.20)	(0.25)	(1.04)	(-0.23)
2	0.713	-0.074	0.379	-0.325
	(1.03)	(-0.32)	(0.59)	(-1.06)
3	0.931	0.034	0.768	0.048
	(1.47)	(0.16)	(1.23)	(0.23)
4	0.983	0.120	0.850	0.094
	(1.60)	(0.75)	(1.40)	(0.48)
5	1.106*	0.214*	0.948	0.174
	(1.76)	(1.79)	(1.47)	(1.17)
6	1.077*	0.165	0.942	0.044
	(1.72)	(0.87)	(1.41)	(0.18)
7	1.133*	0.284	1.014*	0.199
	(1.97)	(1.58)	(1.89)	(1.20)
8	1.087*	0.231	1.009*	0.150
	(1.84)	(0.78)	(1.67)	(0.52)
9	1.295**	0.644**	1.195**	0.551*
	(2.42)	(2.21)	(2.21)	(1.79)
10(High)	1.164**	0.581*	0.823	0.191
	(2.30)	(1.87)	(1.36)	(0.41)
High-Low	0.508*	0.511**	0.263	0.250
	(1.95)	(2.13)	(0.66)	(0.66)
Observations	120	120	120	120

Newey-West t-statistics in parentheses
*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Panel B News Group

	Equal-weighted		Value-weighted	
Decile	RET	Alpha	RET	Alpha
1(Low)	0.236	-0.608***	0.136	-0.672***
	(0.34)	(-2.86)	(0.20)	(-3.45)
2	0.657	-0.294*	0.546	-0.232
	(0.93)	(-1.87)	(0.86)	(-1.23)
3	0.840	0.103	0.642	0.074
	(1.27)	(0.82)	(1.03)	(0.42)
4	0.743	0.015	0.654	0.037
	(1.14)	(0.12)	(1.08)	(0.24)
5	0.762	-0.016	0.678	-0.028
	(1.21)	(-0.13)	(1.08)	(-0.19)
6	0.563	-0.230**	0.572	-0.079
	(0.89)	(-1.99)	(0.98)	(-0.54)
7	0.742	-0.119	0.676	-0.095
	(1.18)	(-1.01)	(1.12)	(-0.63)
8	0.730	-0.185*	0.789	-0.046
	(1.18)	(-1.88)	(1.30)	(-0.32)
9	0.901	-0.049	0.704	-0.234
	(1.51)	(-0.31)	(1.14)	(-1.16)
10(High)	0.987*	0.304	0.801	0.081
	(1.77)	(1.15)	(1.33)	(0.29)
High-Low	0.751**	0.912***	0.664*	0.753**
	(2.44)	(3.60)	(1.79)	(2.15)
Observations	120	120	120	120

Newey-West t-statistics in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 2.5: Three-month-ahead returns for portfolios formed on liquidity shocks

For month t , NYSE, AMEX, and NASDAQ stocks are categorized into news and no-news group. Within each group, stocks are sorted into 10 decile portfolios based on Amihud liquidity shock measures (LIQU). LIQU denotes the liquidity shock, defined as the negative Amihud (2002) illiquidity measure (ILLIQ), demeaned using the past 12-month ILLIQ as the mean. This table reports the equal- and value-weighted three-month-ahead returns (month t+3) and the alpha with respect to the Fama-French (1993) factors for each liquidity shock decile portfolio. The last row shows the differences in monthly returns between high- and low-liquidity shock decile portfolios and the corresponding alphas. Average returns and alphas are defined in monthly percentage terms. Newey-West t-statistics are given in parentheses. The sample period for the results is from January 2001 to December 2010.

Panel A No-News Group

	Equal-weighted		Value-weighted	
Decile	RET	Alpha	RET	Alpha
1(Low)	0.685	0.460	0.593	0.411
	(1.26)	(1.10)	(1.11)	(0.98)
2	0.638	0.663	0.600	0.644
	(0.97)	(1.22)	(1.03)	(1.34)
3	1.449**	1.436***	1.261**	1.181***
	(2.31)	(2.63)	(2.31)	(2.79)
4	1.071*	1.096**	0.967	0.978*
	(1.72)	(2.04)	(1.58)	(1.85)
5	1.164*	1.231**	0.872	0.964*
	(1.84)	(2.31)	(1.41)	(1.85)
6	0.946	0.980*	0.850	0.861
	(1.48)	(1.75)	(1.33)	(1.57)
7	1.129**	1.115**	0.883*	0.889**
	(2.17)	(2.62)	(1.74)	(2.27)
8	1.279***	1.247***	1.014*	0.989**
	(2.64)	(2.92)	(1.95)	(2.19)
9	1.093**	0.926**	1.236**	1.158**
	(2.33)	(2.45)	(2.51)	(2.61)
10(High)	0.795	0.608	0.740	0.632
	(1.43)	(1.30)	(1.29)	(1.24)
High-Low	0.111	0.148	0.147	0.221
	(0.57)	(0.75)	(0.38)	(0.54)
Observations	120	120	120	120

Newey-West t-statistics in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Panel B News Group

	Equal-weighted		Value-weighted	
Decile	RET	Alpha	RET	Alpha
1(Low)	0.605	0.513	0.337	0.292
	(0.94)	(1.03)	(0.49)	(0.54)
2	0.854	0.969	0.523	0.603
	(1.15)	(1.60)	(0.73)	(1.05)
3	0.934	0.947*	0.683	0.691
	(1.40)	(1.71)	(1.08)	(1.36)
4	0.818	0.832	0.768	0.748
	(1.21)	(1.59)	(1.19)	(1.51)
5	0.933	0.967*	0.752	0.794*
	(1.41)	(1.92)	(1.22)	(1.82)
6	0.710	0.782	0.736	0.781*
	(1.09)	(1.60)	(1.22)	(1.75)
7	0.733	0.770	0.645	0.710
	(1.13)	(1.46)	(0.99)	(1.35)
8	0.833	0.913*	0.705	0.834
	(1.31)	(1.70)	(1.06)	(1.54)
9	0.977	0.975*	0.693	0.747
	(1.62)	(1.85)	(1.10)	(1.40)
10(High)	1.074*	0.886*	0.825	0.727
	(1.80)	(1.84)	(1.32)	(1.30)
High-Low	0.469	0.372	0.488	0.435
	(1.46)	(1.35)	(1.24)	(1.23)
Observations	120	120	120	120

Newey-West t-statistics in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 2.6: Buying high-low news portfolio and shorting high-low no-news portfolio

This table reports risk-adjusted returns of buying high-low liquidity shocks no-news portfolios and shorting position of high-low liquidity shocks news portfolios for the current months and following three months.

	Current Mon		1-Mon		2-Mon		3-Mon	
	EW	VW	EW	VW	EW	VW	EW	VW
Alpha	2.453***	1.931***	0.516*	0.221	0.401	0.504	0.537*	0.466
	(7.86)	(4.31)	(1.77)	(0.50)	(1.38)	(1.17)	(1.79)	(0.84)
Obs	120	120	120	120	120	120	120	120

Newey-West t-statistics in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 2.7: Bivariate sorts controlling for Firm size and Book-to-Mkt Equity ratio

For month t , NYSE, AMEX, and NASDAQ stocks are categorized into news and no-news group. Within each group, stocks are sorted into quintile portfolios based on an market capitalization or Book-to-market equity and then into quintile portfolios of liquidity shock (LIQU) within each control quintile. This table reports the average one-month-ahead returns (month t+1) for each of the 5 by 5 portfolios, the return differences between high- and low-LIQU quintile portfolios within each control variable quintile portfolio and the Fama-French (1993) alphas. The last rows presents the 5-1 average return differences within each LIQU quintile portfolio. Newey-West t-statistics are given in parentheses.

Panel A1: Control for Market Cap (No News Group)

LIQU	ME (Low)	ME 2	ME 3	ME 4	ME 5 (High)
1 (Low)	0.94	-0.82	-0.99**	-0.31	-0.49
	(1.39)	(-1.64)	(-2.34)	(-0.62)	(-1.13)
2	0.64	0.34	-0.33	-0.60*	0.35
	(1.07)	(0.61)	(-0.88)	(-1.80)	(1.18)
3	0.80	1.01**	0.17	-0.27	0.23
	(1.55)	(2.08)	(0.53)	(-1.10)	(1.12)
4	1.81***	0.74*	0.41	-0.03	-0.36
	(3.29)	(1.82)	(1.36)	(-0.12)	(-1.58)
5 (High)	1.77***	0.84*	0.94***	0.32	0.21
	(3.09)	(1.67)	(2.84)	(1.17)	(0.92)
High-Low	0.82	1.66***	1.92***	0.63	0.70
	(1.18)	(3.07)	(4.05)	(1.15)	(1.48)
News Attached	NO	NO	NO	NO	NO

Newey-West t-statistics in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Panel A2: Control for Market Cap (News Group)

1 (Low)	-0.29	-0.82*	-0.61	0.08	-0.05
	(-0.55)	(-1.79)	(-1.13)	(0.16)	(-0.13)
2	-0.36	-0.44	-0.12	0.17	-0.04
	(-0.77)	(-1.23)	(-0.43)	(0.92)	(-0.25)
3	0.40	0.01	-0.17	0.10	0.22*
	(0.85)	(0.04)	(-1.03)	(0.86)	(1.68)
4	0.51	0.81***	-0.15	0.08	0.19*
	(1.28)	(3.18)	(-0.91)	(0.56)	(1.75)
5 (High)	0.71*	0.98***	0.33	0.04	0.18
	(1.76)	(3.02)	(1.47)	(0.22)	(1.10)
High-Low	0.99**	1.80***	0.94	-0.04	0.23
	(2.42)	(4.02)	(1.56)	(-0.07)	(0.50)
News Attached	YES	YES	YES	YES	YES

Newey-West t-statistics in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Panel B1: Control for BE/ME (No News Group)

LIQU	BM (low)	BM 2	BM 3	BM 4	BM 5 (High)
1 (Low)	-0.56	-0.19	-0.21	0.05	0.10
	(-0.88)	(-0.32)	(-0.59)	(0.12)	(0.17)
2	-0.64*	0.11	0.14	-0.30	-0.21
	(-1.69)	(0.33)	(0.42)	(-0.80)	(-0.34)
3	-0.69**	0.14	0.28	0.23	0.89*
	(-2.18)	(0.50)	(1.02)	(0.74)	(1.91)
4	0.30	0.16	0.06	0.85**	1.03**
	(0.87)	(0.52)	(0.21)	(2.29)	(2.53)
5 (High)	0.53	0.86**	1.21***	1.37**	1.78***
	(1.13)	(2.42)	(3.43)	(2.54)	(3.03)
High-Low	1.09	1.05*	1.42***	1.32**	1.68***
	(1.60)	(1.75)	(3.48)	(2.52)	(2.84)
News Attached	NO	NO	NO	NO	NO

Newey-West t-statistics in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Panel B2: Control for BE/ME (News Group)

LIQU	BM (low)	BM 2	BM 3	BM 4	BM 5 (High)
1 (Low)	-0.76	-0.38	-0.36	-0.55	-0.40
	(-1.32)	(-0.84)	(-0.84)	(-1.39)	(-0.83)
2	-0.11	-0.06	0.12	0.18	-0.53
	(-0.33)	(-0.23)	(0.50)	(0.66)	(-1.64)
3	-0.01	0.23	0.23	0.05	0.45
	(-0.04)	(1.40)	(1.45)	(0.37)	(1.63)
4	-0.33	-0.12	0.10	0.28	0.77***
	(-1.08)	(-0.77)	(0.65)	(1.51)	(2.88)
5 (High)	0.13	0.43	0.76***	0.66**	0.99***
	(0.33)	(1.45)	(3.06)	(2.40)	(2.75)
High-Low	0.89	0.81*	1.12**	1.21***	1.39***
	(1.57)	(1.73)	(2.56)	(3.02)	(3.33)
News Attached	YES	YES	YES	YES	YES

Newey-West t-statistics in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 2.8: Stock-level cross-sectional regressions for with and without news group

Monthly excess stock returns are regressed on a set of lagged predictive variables using the Fama and MacBeth (1973) methodology. This table reports the average slope coefficients and Newey-West t -statistics in parentheses. LIQU denotes the liquidity shock, defined as the negativeAmihud (2002) illiquidity measure (ILLIQ), demeaned using the past 12-month ILLIQ as the mean. BETA, LNME, and LNBM denote the market beta, the natural logarithm of the market capitalization, and the natural logarithm of the book-to-market equity ratio, respectively. The sample covers the period from January 2001 to December 2010.

	Return t+1
LIQU	0.032**
	(2.42)
LIQU * NEWS	0.032
	(1.46)
NEWS	0.124
	(1.65)
BETA	-0.032
	(-0.11)
LNME	-0.131***
	(-3.27)
LNBM	-0.010
	(-0.09)
MOM	0.001
	(0.33)
IVOL	-0.199
	(-0.60)
ILLIQ	-0.122***
	(-6.67)
REV	-0.046***
	(-5.93)
Constant	1.586***
	(4.20)
Observations	364936
R-squared	0.09

Robust t-statistics in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 2.9: Stock-level cross-sectional regressions for with and without news group - Weighted Least Square

Monthly excess stock returns are regressed on a set of lagged predictive variables using the value-weighted Fama and MacBeth (1973) methodology. Weighted least square estimation method is applied. The weight is the reciprocal of each stock's market capitalization at the beginning of the month. This table reports the average slope coefficients and Newey-West t -statistics in parentheses. LIQU denotes the liquidity shock, defined as the negativeAmihud (2002) illiquidity measure (ILLIQ), demeaned using the past 12-month ILLIQ as the mean. BETA, LNME, and LNBM denote the market beta, the natural logarithm of the market capitalization, and the natural logarithm of the book-to-market equity ratio, respectively. The sample covers the period from January 2001 to December 2010.

	Return t+1
LIQ-Shock	0.06***
	(2.96)
LIQU * NEWS	0.009
	(0.36)
News	0.116
	(1.54)
BETA	3.17***
	(3.16)
LNME	-0.15***
	(-3.52)
LNBM	-0.008
	(-0.07)
MOM	0.000
	(-0.02)
IVOL	-3.45***
	(-4.34)
ILLIQ	-0.16***
	(-5.20)
REV	-0.05***
	(-5.83)
Constant	1.76***
	(4.21)
R-squared	0.092
Observations	371108

Robust t-statistics in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 2.10: One-month-ahead liquidity-matched portfolio returns

For month t , NYSE, AMEX, and NASDAQ stocks are sorted into 10 decile portfolios based on one of the Amihud liquidity shock measures (LIQU) using the NYSE breakpoints. LIQU denotes the liquidity shock, defined as the negative Amihud (2002) illiquidity measure (ILLIQ), demeaned using the past 12-month ILLIQ as the mean. This table reports the equal- and value-weighted news and no-news portfolios one-month-ahead returns (month t+1) and the alpha with respect to the Fama-French (1993) factors for each liquidity shock decile portfolio. The last row shows the differences in monthly returns between high- and low-liquidity shock decile portfolios and the corresponding alphas. Average returns and alphas are defined in monthly percentage terms. Newey-West t-statistics are given in parentheses. The sample period for the results is from January 2001 to December 2010.

Panel A No-News Group

	Equal-weighted		Value-weighted				
Decile	RET	Alpha	RET	Alpha	LIQU	ILLIQ	% Mkt. shr.
1(Low)	0.20	-0.54***	0.17	-0.52*	-4.40***	6.62***	7.67
	(0.31)	(-3.15)	(0.25)	(-1.98)	(-3.03)	(3.45)	
2	0.61	-0.04	0.58	0.03	-0.00***	0.01***	10.38
	(0.85)	(-0.11)	(0.90)	(0.09)	(-3.39)	(4.20)	
3	0.61	-0.04	0.50	-0.01	-0.00**	0.00***	13.73
	(0.92)	(-0.11)	(0.75)	(-0.03)	(-2.35)	(3.86)	
4	1.17*	0.50*	0.91	0.27	-0.00	0.00***	13.25
	(1.68)	(1.79)	(1.36)	(0.92)	(-0.92)	(3.35)	
5	0.68	-0.10	0.56	-0.10	0.00	0.00***	11.90
	(1.07)	(-0.42)	(0.78)	(-0.29)	(1.59)	(4.91)	
6	0.72	0.06	0.64	0.15	0.00***	0.00***	10.66
	(0.98)	(0.21)	(0.88)	(0.55)	(3.60)	(4.80)	
7	0.64	-0.13	0.71	0.06	0.00***	0.01***	8.93
	(0.92)	(-0.37)	(1.01)	(0.17)	(4.24)	(4.71)	
8	0.18	-0.57**	0.29	-0.39	0.00***	0.01***	7.78
	(0.27)	(-2.11)	(0.39)	(-1.12)	(4.34)	(4.67)	
9	1.00*	0.12	0.93	0.18	0.01***	0.03***	6.84
	(1.77)	(0.53)	(1.63)	(0.71)	(4.40)	(5.25)	
10(High)	1.11**	0.44***	0.96*	0.24	3.59***	1.40***	9.05
	(2.20)	(2.78)	(1.89)	(1.42)	(4.60)	(6.51)	
High-Low	0.91***	0.99***	0.79*	0.76**			
	(2.99)	(4.52)	(1.82)	(2.24)			
Observations	119	119	119	119	119	119	119

Newey-West t-statistics in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Panel B News Group

	Equal-weighted		Value-weighted				
Decile	RET	Alpha	RET	Alpha	LIQU	ILLIQ	% Mkt. shr.
1(Low)	0.13	-0.61***	-0.11	-0.79***	-2.01***	2.99***	5.91
	(0.19)	(-3.37)	(-0.15)	(-3.02)	(-3.08)	(3.50)	
2	0.43	-0.13	0.42	-0.08	-0.00***	0.01***	14.90
	(0.62)	(-0.57)	(0.65)	(-0.29)	(-3.36)	(3.86)	
3	0.55	0.10	0.31	-0.06	-0.00**	0.00***	21.22
	(0.84)	(0.66)	(0.50)	(-0.36)	(-2.34)	(4.10)	
4	0.77	0.24*	0.71	0.23	-0.00	0.00***	16.41
	(1.25)	(1.82)	(1.21)	(1.51)	(-0.92)	(4.60)	
5	0.68	0.08	0.61	0.03	0.00	0.00***	11.86
	(1.02)	(0.53)	(0.94)	(0.18)	(1.59)	(7.78)	
6	0.77	0.13	0.77	0.20	0.00***	0.00***	11.37
	(1.17)	(0.99)	(1.26)	(1.38)	(3.56)	(5.46)	
7	0.66	0.00	0.63	0.03	0.00***	0.00***	7.51
	(1.12)	(0.00)	(1.06)	(0.20)	(4.23)	(6.15)	
8	0.70	-0.02	0.49	-0.11	0.00***	0.01***	5.49
	(1.15)	(-0.17)	(0.83)	(-0.63)	(4.33)	(5.00)	
9	0.76	-0.07	0.67	-0.05	0.01***	0.01***	2.84
	(1.30)	(-0.46)	(1.14)	(-0.28)	(4.33)	(5.60)	
10(High)	1.13*	0.36**	0.95	0.14	2.29***	0.72***	2.48
	(1.97)	(2.49)	(1.61)	(0.84)	(5.65)	(7.22)	
High-Low	1.00***	0.97***	1.06**	0.92***			
	(3.00)	(3.90)	(2.35)	(2.63)			
Observations	119	119	119	119	119	119	119

Newey-West t-statistics in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 2.11: More Investors' Attention During News Release Periods

At monthly level, I regress share turnover on newsdummy, which is an indicator of whether there is public news release in month t. Share turnover of a stock is defined by the monthly trading volume divided by the average number of shares outstanding during that month. Standard errors are adjusted for heteroskedasticity.

	Share Turnover
NewsDummy	0.89***
	(105.40)
Constant	1.10***
	(180.95)
R-Squared	0.02
Obs	398,974

Robust t-statistics in parentheses
*** $p<0.01$, ** $p<0.05$, * $p<0.1$

— Chapter 3 —

Hardship and Virtue: Down-to-the-Countryside Movement and Corporate Social Responsibilities

3.1 Introduction

In recent years, there have been growing scholarly interests in corporate social responsibility (CSR).[1] CSR is defined as actions that appear to further some social goods beyond the interests of the firm and that sometimes are required by law (McWilliams and Siegel, 2001). It deviates from the optimum pecuniary interests of a firm and is difficult to be explained solely by the traditional corporate finance theories.

Many researchers have recently attempted to explore the determinants of CSR by looking beyond the standard neo-classical models. Many psychological factors are proposed to explain the motivations of CSR practice. One major psychological factor proposed is personality traits. Many personality traits are considered to be associated with decision modes (Bandura, 1982; Diener et al., 1984). A series of studies conducted by Snyder and his colleagues presented the concept that individuals might choose situations that allow the expression of their distinctive personality traits

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and values (Ickes et al., 1997; 2010; Snyder, 1983; Snyder and Ickes, 1985). Petrenko et al. (2016) argue that organizations with CEOs that have a high need for attention and are preoccupied with having their positive self-views reinforced would engage in higher levels of corporate social responsibility.

It has been well documented that many factors could affect personality traits, such as family members or early-life experiences. Cronqvist and Yu (2017) find that when a CEO has a daughter, the CSR ratings of his company tend to increase significantly. They explain that the rationale behind this is that the CEOs' daughters would typically shape the CEOs' personality traits and furthermore social preferences. Early-life experiences, especially negative ones (e.g., losing a parent), could also affect the development of personality. (Adcock and Ross, 1983; Belsky, 1981; Erikson and Erikson, 1998; Hunt, 1979; Kitamura and Fujihara, 2003; Rutter, 1980). A considerable number of studies have shed some light on the association between early-life experiences of corporate leaders and corporate decisions. Malmendier et al. (2010) find that CEOs who grow up during the Great Depression are averse to debt and lean excessively on internal finance. Benmelech and Frydman (2015) point out that CEOs with military experiences pursue lower corporate investment, are less likely to be involved in fraudulent corporate activity, and perform better during industry downturns. Moreover, Bernile et al. (2017) find that CEOs who experienced fatal disasters without extremely negative consequences lead firms to behave more aggressively, whereas those who witnessed the extreme downside of disasters behave more conservatively.

Thus, it is reasonable to embrace the notion that the early-life experiences of top executives will reverberate their CSR practice. The framework related to this association is presented in Figure 1:

[Inserts Figure 1 here]

As shown in Figure 1, we argue that the early hardship experiences of top executives will impose a significant stamp on their personality development; and this influence can be mediated by their demographical factors. Moreover, both personality traits and rational considerations will impact the CSR practice of top executives.

In this paper, we examine the impact of early-life hardship experiences on the CSR decisions of corporate leaders. Theoretically, whether early-life hardship experience positively or negatively affects corporate leaders' CSR practice is unclear. On the one hand, corporate leaders who have gone through extreme hardships might believe that they have already suffered enough in life and would not want to exert extra efforts in social contribution. Psychology literature has documented a wealth of evidence that trauma produced by hardship might decrease people's prosocial preferences. Frueh et al. (2001) find that the veterans who experienced traumas reported a significantly lower level of altruistic behavior than a normative comparative group. Many positive psychological traits related to prosocial preference might also be affected by hardship. Kashdan et al. (2006) find that veterans who have experienced significant hardship show a lower level of gratitude. Abramson et al. (1978) and Wortman and Brehm (1975) document that hardship experiences lead to negative personality-development and antisocial behaviors. Furthermore, many studies have shown that hardship will impart negative impacts on social relationships (Cook et al., 2004; Jordan et al., 1992; Riggs et al., 1998).

On the other hand, however, a strand of literature indicates that hardship can bring positive effects on people's prosocial preferences. Many psychologists argue that hardship breeds virtue and makes people who suffered more sympathetic and community-caring (Price, 2000; Schaefer and Moos, 1998; Tedeschi et al., 1998). These pieces of literature argue that people might gain deeper understandings to the common disaster and traumas imposed on others via their own experiences and hence be more sympathetic and other-caring. Hence, we would expect corporate leaders with previous hardship to appreciate the good life now and to feel the urge to contribute to society.

We address this question by exploiting a social experiment in China from 1956 to 1978, the Down-to-the-Countryside Movement (or the send-down Movement). This Movement provides a good setting to study this question because it is an early-life extreme hardship for the people involved, and it is mandatory, which eliminates the possibility that some people choose to experience hardships. The Down-to-the-Countryside Movement is a major component of the Cultural Revolution in Chinese history. During the movement, young graduates in middle schools and high schools were forcibly rusticated and separated from their original families to work in the countryside or the mountains to "help build and develop the country". The youngsters who were involved in the Down-to-the-Countryside Movement were between 16 and 19 years old, which is a crucial stage for personality development (Erikson and

Erikson, 1998). The movement lasted for nearly 20 years, during which 18 million people were involved (Bonnin and Horko, 2013), and it generated significant and profound impacts on the individuals involved. Those who experienced such movement perceived this experience as the most challenging experience in their lives. They suffered from an extreme lack of material supplies, harsh and tedious labor, among other challenges (Chen and Cheng, 1999). This movement also inflicted painfulness and heavy psychological burden on the parents who had no choice but to send their children to rural areas (Deng, 1993). As Xi (2003) pointed out: "To conclude, the impacts of the Down-to-the-Countryside Movement on my life is hugely profound." Similar narratives can be found in interviews and published memoirs of other people who have experienced the movement (Bonnin and Horko, 2013).

There is a growing body of quantitative research assessing the outcomes of the send-down movement. By exploiting the local files, Honig and Zhao (2015) conduct a qualitative study and found that the send-down movement entails positive impacts on knowledge accumulation in places where those young received an education. Further, by using a quantitative approach, Chen et al. (2018) find that greater exposure to the youth sent down increased the local's residents' education years. Their results indicated a significant knowledge spillover accompanied by this movement. Besides the macro outcomes, individual-level wellbeing is also illuminated. By using the China General Social Survey (CGSS) 2003 data, Wang and Zhou (2017) find that the send-down youths had lower-quality social networks and a lower level of happiness.

Moreover, when the future development tracks are completely beyond one's own efforts, this will change one's beliefs in risks and political stance. Fan (2017) finds that individuals being send-down spent less on housing, had a stronger preference for saving and insurance, and invested less in risky assets, compared with their non-rusticated peers. Shi and Zhang (2020) find that individuals' political participation (measured by their participation in community committee elections) significantly declined in groups who had been sent down.

However, there are no empirical studies assessing the impacts of the send-down movement on people's social preferences, especially corporate leaders' CSR practices. We identify the causal effects by exploiting the cut-offs of the occurring time of the Down to Countryside Movement. Individuals born between 1946 and 1961 were required to be part of the movement and to move to the countryside to do manual labor, whereas those born after 1961 were free from the movement, and thus most of these people born after 1961 dropped out. We construct a subsample of chairmen and CEOs who were born in 1960, 1961, 1962, and 1963. Those who were born in 1960 and 1961 would receive the treatment, while those born in 1962 and 1963 would not, to the most part. We first look at the sample of chairmen because, in China, chairmen make major corporate decisions, especially within state-owned companies. We also look at CEOs as robustness checks and find consistent results. Since these four years are very close to each other, it is reasonable to assume that unobserved factors of the chairmen are similar across these years.

We find that the chairmen who were sent to the countryside and mountains have significantly less CSR practice in their companies than their peers who were not sent. Such results are especially strong for state-owned companies and not significant for non-state-owned companies. We argue that one potential reason why the chairmen who had extreme hardships have less CSR practice is that they believe that they have already suffered enough in life and developed an in-depth aversion to social contribution (Bonnin and Horko, 2013). The reason why we do not observe such effects in these companies is that the risks of chairmen in non-state-owned companies are much less under-diversified than those in state-owned companies.

In China, chairmen do not own propriety rights of the companies; they play more of a role of a political officer than an entrepreneur. However, this is not the case in non-state-owned-companies, where chairman positions are usually held by the majority shareholders (Kang et al., 2008; Xu and Wang, 1999). In other words, chairmen in non-SOEs are more cautious about implementing any managerial decisions, which may potentially entail significant consequences. As a result, chairmen in non-SOEs are less prone to be driven by behavioral motivations.

The contribution of this study is threefold. First, our paper contributes to a growing body of research on motives of CSR. Recently, scholars have attempted to understand the driving force from a behavioral perspective. To the best of our knowledge, our paper is the first to highlight the effects of hardship experiences on CSR practice. Our findings provide a new perspective in understanding the motives for implementing CSR practice. Secondly, our findings also advance our understandings of the interplay between hardship experiences and altruistic behavior. The dispute concerning the nature of the motivation underlying helping behavior has been a topic of heated debate for many years (Piliavin and Charng, 1990). Our paper assessed the influences of early-life experiences on altruistic behavior, providing novel empirical evidence from the perspective of corporate finance. Finally, our research also adds to a growing strand of literature on quantitative history. We employ strict econometric analysis to follow through an important historical event. To the best of our knowledge, this paper is the first to empirically examine the influences of the send-down movement on the CSR practice.

3.2 Data and Methodology

3.2.1 Definitions of Variables

The outcome variable we study is CSR Practice. We have obtained the CSR scores for the listed companies in China from a unique dataset of the Hexun CSR Index. Based on the annual reports of listed enterprises in China and corporate social responsibility reports published by official websites, Hexun CSR Index employs a professional corporate social responsibility evaluation system which has been regarded as the latest and most authoritative database with an adequate rating data towards CSR (Pan et al., 2014). Hexun CSR Index is composed of an overall CSR score and 5 CSR sub-items (including Stakeholder Responsibility, Employee Responsibility, etc.). The specific contents of the Hexun CSR Index are presented in Table A1.

The main independent variable of interest is Zhiqing, which is a binominal variable indicating whether the chairman of the board has experienced the send-down movement. The exact information of the year when the chairman of the board was sent down is not collected by any database. And not all the information on sending down is disclosed publicly. The forceful send-down movement started in December

1968. For those who graduated in 1966, 1967, and 1968 (usually referred to as "Lao San Jie" in Chinese), the percentage of being sent down is close to 100%. In 1977, the Chinese National Higher Education Entrance Examination was re-established (Feng, 1999), and in 1978, the state council of China officially claimed that the send-down movement came to an end. Therefore, the probability of being sent down drastically decreased since 1977, and the event finally ended in 1978.

It was difficult to pinpoint the actual timeline when people were sent down. Due to political sensitivity, there are no complete official records. We employ an alternative method to infer whether the board directors had experienced the send-down movement. From 1950 to 1980, the entry age for preliminary school is 7-year-old.[2] According to the compulsory education years, it can be inferred that those born from 1947 to 1961 have a very strong probability to be rusticated (around 50%) and otherwise the probability would approach 0%.[3] This estimation fits in with the Chinese General Social Survey (CGSS). The probability of being sent down based on birth cohorts from CGSS is presented in Figure 2.

[Inserts Figure 2 here]

Figure 2 confirms our prediction that 1961 is a good cut-off to identify who was sent down and who was not. The construction of such a variable is also consistent

[2]In 1951, the Regulations for Primary Schools (a draft resolution) was established and set the entry age for primary school is 7-year-old.

[3]We confirm these details with Michel Bonnin, and we appreciate his generous help.

with previous literature. However, a crowd of unobserved factors (e.g., social atmosphere, governmental propaganda, etc.) other than send-down movement within this long period are very dispersed that might impose impacts on their decisions on CSR. To relieve the concerns, besides the regression using the full sample, we construct a subsample of board directors who were born in 1960, 1961, 1962, and 1963. As discussed, those who were born in 1960 and 1961 received the treatment, while those in 1962 and 1963 did not. Since this time-window is very narrow, we could rule out most of the confounding factors between treated and control groups. This also helps us rule out the possible effects of an important confound event — the Great Famine in China, which lasted from 1959 to 1961. Subjects who were born before 1961, without doubt, experienced this famine as opposed to those who were born after 1962. It is reasonable to raise concerns that memories of this distressful incident would have an impact on people's personalities. However, during the period from 1959 to 1962, those who experienced the incident were one to two years old. It is proved by both neuroscience and psychology that only those over the age of four can form and keep long-term memories (Akers et al., 2014; Josselyn and Frankland, 2012; Pillemer et al., 1994; Usher and Neisser, 1993). Hence, this confounding factor will not cast a huge shadow on the robustness of our estimations from the send-down movement on CSR. Thus, we could run regressions using this full sample and sub-samples. The descriptive statistics of the variables are presented in Table 1.

3.2.2 Specification

We construct a specification as to assess the impacts of the send-down movement on the CSR practice. Following Angrist and Pischke (2009), we conduct the reduced form of the Fuzzy Regression Discontinuity Design (RDD) analysis, since we do not observe the actual treatment variable. Since the assumed treated units could be in noncompliance for treatment, the actual treatment effects should be even stronger in magnitude than what we estimate in the reduced form.

Furthermore, CSR practice is considered to be both profit-oriented and altruism-incentivized. Personality traits are found to be associated with altruism (Oda et al., 2014). Certain personalities will drive the organization leaders to implement CSR. However, leaders might also take CSR practices to boost profits. In the latter case, CSR practice will no doubt be influenced by firm characteristics. Therefore, we have also included the relevant variables in our regression specification. We also make sure that there are no discontinuities at the cut-off for the firm characteristics. We document such facts in Figure 3.

The specification is as follows:

$$\begin{aligned} Model : CSR\ Activities_{i,t} =& \beta_0 + \beta_1 Chairman\_Born\_Before\_1962_{i,t} + \beta_2 YearBorn_{i,t} \\ & + X_{i,t}^T \beta_3 + \phi_j + \gamma_t + \epsilon_{i,t} \end{aligned}$$

where $Chairman\_Born\_Before\_1962_{i,t}$ is a dummy indicating whether a chairman was born before 1962. We also control year fixed effects and industry fixed effects.

3.3 Empirical Results

The summary statistics are presented in Table 1.

[Inserts Table 1 here]

As shown in Table 1, we can see that the chairmen in the sample were born between 1960 and 1987; approximately half of them were sent down.

We also run OLS regressions using the full sample and a host of subsamples. The results are shown in Table 2.

[Inserts Table 2 here]

Panel A in Table 2 shows that the variable Zhiqing is negatively associated with the CRS score, which indicates that board directors who experienced the send-down movement tend to perform worse in terms of CSR scores, i.e., the impacts from the send-down movement on CSR practice is significantly negative. Moreover, the negative impacts are not merely reflected in the overall score of CSR but also on employer responsibility, environmental responsibility, and supplier responsibility. Significant associations are also found between some control variables and the dependent variable. The age is positively associated with CSR scores, indicating that older board directors perform better in CSR practice. We propose the following explanations. This movement motivates youngsters to contribute, whereas their youth are sacrificed. The people who have experienced this movement might feel deceived and breed an in-depth aversion to the contribution (see Bonnin and Horko, 2013). To control the time-variant unobservable variables, we narrow our time-window and construct a subsample born within a 2-year-bandwidth. The results can be seen in Panel B in Table 2. Moreover, we also review the results using a 4-year bandwidth (see Panel C in Table 2). The results are consistent with what we found using full-sample data.

It is worth noting that we have used the chairmen's and CEOs' probability of being sent down to construct the treatment variable. An ideal scenario is that we could gain exact information on whether the chairmen and CEOs received the treatment or not. However, this drawback would not impinge on our estimation results. In our tests, we have documented that chairmen and CEOs who received the treatment would decrease their CSR investment. Since the assumed treated units in our dataset might not be 100-percent treated, the real effects of our treatment on CSR investment should be even more profound. In fact, the magnitude of the actual treatment effects should equal the magnitude of our results divided by the probability of being sent down.

As a further matter, we also run tests for the sample of CEOs. The results are presented in Table 3. From Table 3, we can find a negative association between CEOs' sending-down experiences and their CSR practice. This association is most salient between send-down experiences and Employee CSR practices.

[Inserts Table 3 here]

As showcased in our conceptual framework, CSR practice is driven by both behavioral traits and rational considerations of top executives. Hence, a variant of the corporate governance structure might also influence top executives' CSR practices. To test our hypothesis in this light, we first split our sample into State-owned companies and non-State-owned companies for chairmen. The results are shown in Table 4, Panel A and Panel B. The impacts on chairmen are significant in SOEs and insignificant in non-SOEs.

[Inserts Table 4 here]

We propose a tentative explanation that the risks of chairmen of SOEs are usually sufficiently diversified, for they are usually not the shareholders of the SOEs. Thus, they are more prone to be driven by behavioral traits. However, this is not the case in non-SOEs. Chairs are usually held by those who own the largest proportion of the shares of the companies. Hence, their risks are quite under-diversified. Any ruthless decisions might produce a severe outcome for the chairmen in non-SOEs. Thus, chairmen in non-SOEs are more prudential on managerial decisions, including CSR-related issues.

Similarly, we split our sample into State-owned companies and non-state-owned companies for CEOs. The results are reported in Panel C and Panel D in Table 4. In contrast, we find that the results for the non-SOEs are significant, and the results for SOEs are insignificant in the CEO sample. This might stem from the fact that CEOs mainly have decision power in non-SOEs in China, and hence their personal feelings are only revealed when they have decision powers. Furthermore, in Chinese SOEs, CEOs' managerial power is strictly constrained by the secretaries of the party committee (Kang et al., 2008).

Moreover, we also measure the mediating effects from CEOs' education level on the association between send-down experiences and CSR practice. The results can be seen in Table 5.

[Inserts Table 5 here]

As shown in Table 5, we can see that those featured with high education levels are inclined to give less social contribution (proxied by their CSR practice). As argued in the previous section, those people who were sent down might feel deceived and sacrificed by the government and thus developed a deep antagonism towards social contribution. For those who received higher education, they might be endowed with sharper capability in reflecting upon the history and politics at that instant. Consequently, their aversion towards social contribution might be more penetrating.

According to Figure 2, the first group of people who were sent down was born in 1946, which formulated another cut-off point to construct another regression discontinuity design. We construct a subsample, where people who were born in 1945 or 1946 were not likely to be treated, while those who were born in 1947 and 1948 were highly-probable to be sent down. The results are presented in Table 6.

[Inserts Table 6 here]

From Table 6, we can see that the results are analogous with our main findings. That is, CEOs who were sent down tended to invest significantly less in CSR practice.

Finally, as many psychologists suggested, experiencing hardship at different ages might exert assorted impacts (Franz and White, 1985; Hall, 1983; Mirowsky and Ross, 2001; Zeig, 2015). This study also assesses the possible mediating effects of the age at which the CEOs experienced the send-down treatment by constructing an interactive variable of age and treatment. The results after including the interactive variable can be seen in Table A2. The results are consistent with our main findings.

3.4 Discussion and Conclusion

We study how early-life hardships experienced by corporate leaders affect their CSR decisions. By exploiting the mandatory Sent Down to Countryside Movement in China, we empirically document that corporate leaders who went through such hardship in early life conduct significantly less CSR activities than corporate leaders who did not go through these challenges and hardships. We have also argued that one potential explanation is that these hardships have made them develop an aversion to social contribution.

We also propose a conceptual framework to explain our findings. Our empirical results support that hardship scales back CEOs' CSR investments, which can be recognized as a trait of prosocial preferences. To resolve the seemingly contradictory theories in psychology, we propose to include the mediating factor that affects the association between hardship and prosocial preferences. The most important mediating factor is how the persons who experienced the Send-down movement (SDM) reflect on the hardship imposed on them. According to a simplified cognitive model (Anderson, 2000), the interpretation of the situation determines the reaction rather than the situation itself. The model is presented in Figure 4.

[Inserts Figure 4 here]

In Figure 4, the first line illustrates the chains via which hardship experiences shape CEOs' prosocial preferences. We propose that how CEOs reflect and interpret their Send-down movement experiences mediates the association between hardship experiences and prosocial preferences. According to Bonnin and Horko (2013), the main purpose of initiating Send-down movement is not to help the poor but rather to abate the increasing employment pressure in the cities impelled by political radicalism. Thus, participants who experienced the SDM might feel deceived and focus on the dark sides when they interpret what they underwent. As a result, the positive impacts from SDM waned, while the negative ones bolstered.

Moreover, these associations between hardship in early-life and personality would be mediated by some demographical factors. Age is associated with this mechanism. The same hardship event might exert different impacts on individuals of various ages, for the cognitive stages vary according to a different age. Besides, we also consider that gender needs noteworthy attention. Empirical evidence shows that males and females might have different reflections on pressure events.

Future studies may take deeper steps into the following aspects. Firstly, due to the data attrition, we could not gain the exact information on whether board directors are sent down and the duration of their rustication. We can estimate their probability of being sent down by their year of birth. Although we believe that by employing a fuzzy RDD, the exogeneity is promised to some extent, future studies might seek to gain more accurate data. Due to information sensitivity and the social status of the subject, the support from the Chinese government might be crucial to realize this target. Moreover, we propose a conceptual framework to illustrate the mechanism via which hardship experiences in early life influence the CSR practice. We provide theoretical evidence to support our arguments, whereas we do not own the data on the mediating variable personality. Future studies might consider how to measure the personality of the board directors quantitatively.

Figure 3.1: Conceptual Framework of the Impacts of Hardship Experience on CSR Practice

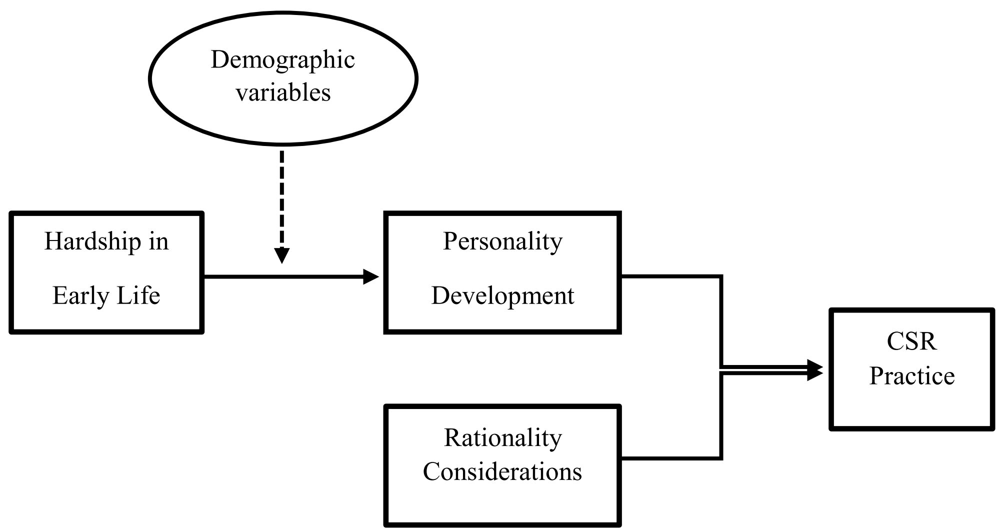

Figure 3.2: Probability of Sent-Down-to-Countryside Relative To Year Born

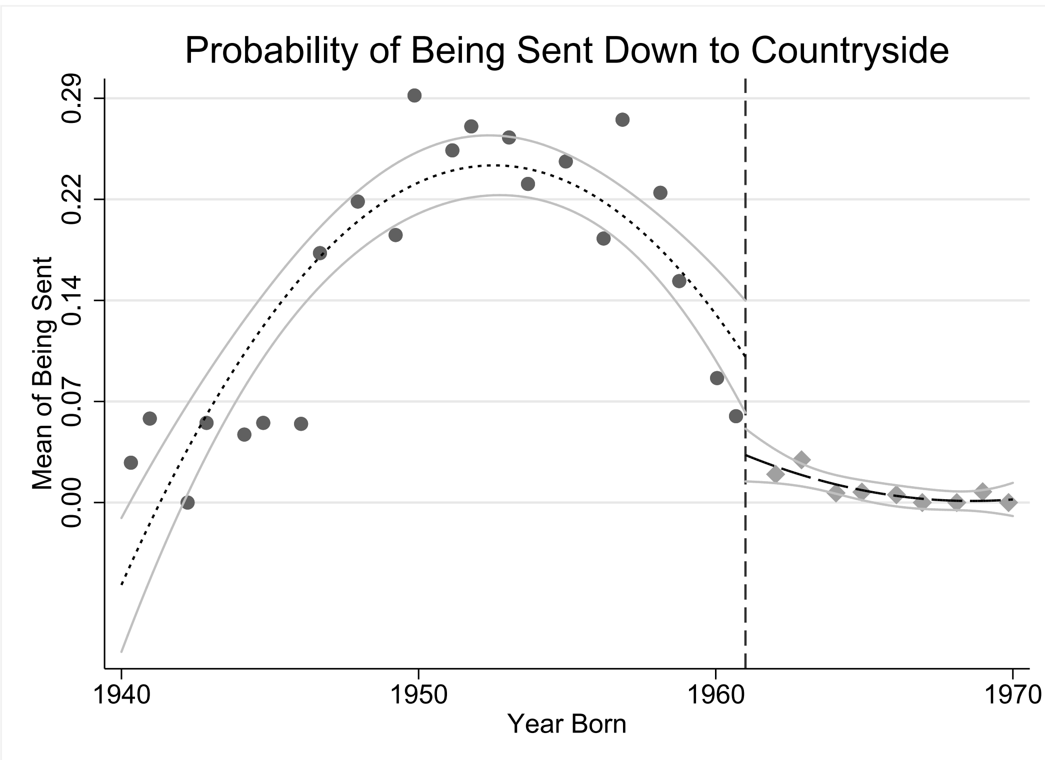

Figure 3.3: Continuity Tests for Firm Characteristics

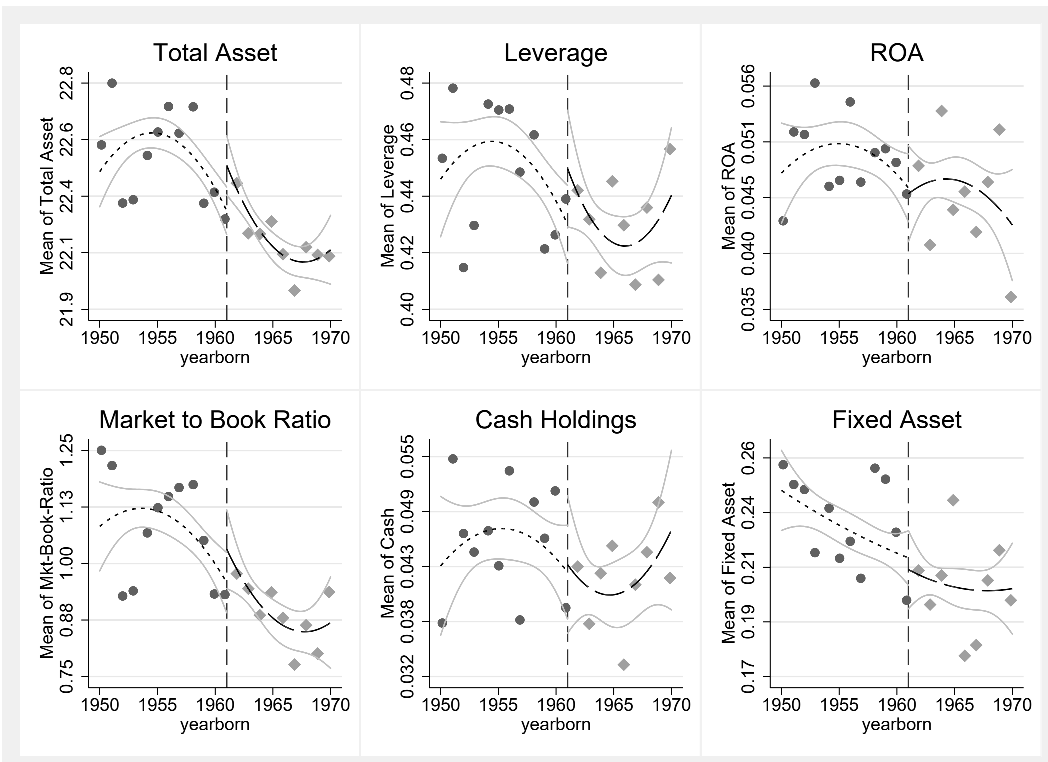

Figure 3.4: The Association Between Hardship and Prosocial Preferences Using a Simplified Cognitive Model

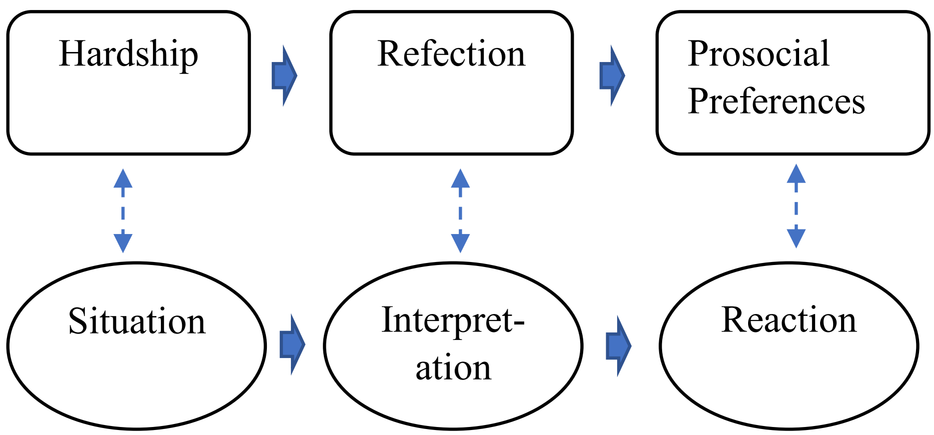

Table 3.1: Summary Statistics

	mean	p50	min	max	sd	count
BornBefore1962	0.51	1.00	0.00	1.00	0.50	9606
Gender	0.04	0.00	0.00	1.00	0.21	9606
Year Born	1960.23	1961.00	1940.00	1987.00	6.75	9606
CSR Score	3.33	3.23	0.83	4.36	0.59	9606
Emp	1.29	1.17	0.00	2.77	0.77	9606
Lnv	2.69	2.80	-1.97	3.26	0.58	9606
Env	0.68	0.00	0.00	3.18	1.17	9606
Sup	0.69	0.00	0.00	3.04	1.17	9606
Com	1.72	1.77	-3.91	2.98	0.67	9606
Total Asset	22.33	22.12	19.93	26.34	1.32	9606
Leverage	0.44	0.44	0.01	0.99	0.21	9606
Mkt-Book-Ratio	0.97	0.64	0.11	5.24	0.95	9606
ROA	0.05	0.04	-1.07	0.48	0.05	9606
Cash	0.04	0.04	-0.71	0.55	0.08	9606
Fixed Asset	0.22	0.18	0.00	0.95	0.17	9606
State-Owned Enterprises	0.44	0.00	0.00	1.00	0.50	9606
Firm Foundation Year	1996.75	1997.00	1950.00	2010.00	4.64	9606
College	0.88	1.00	0.00	1.00	0.32	9606

Table 3.2: The Effects of Chairmen's Down-to-the-Countryside Experience on Corporate Social Responsibility Activities: Regression Discontinuity Reduced Form

$Model : CSR\ Activities_{i,t} = \beta_0 + \beta_1 Chairman\_Born\_Before\_1962_{i,t} + \beta_2 YearBorn_{i,t} + X_{i,t}^T \beta_3 + \phi_j + \gamma_t + \epsilon_{i,t}$

This table reports the results of reduced-form regression discontinuity analysis of the effects of Chairmen's Down-to-the-Countryside experience on corporate social responsibility (CSR) activities over the sample period of 2010-2016. Chairmen born after 1961 have almost zero probability of being treated and chairmen born before 1961 have a high probability of being treated. $CSR\ Activities_{i,t}$ is the measure of CSR activities of firm i of year t. $Distance_{i,t}$ is the number of years from the born year to 1961. Standard errors are clustered at firm and year level and are reported in the parentheses. Statistical significance is represented according to $*p < 0.10, **p < 0.05, ***p < 0.01$.

Panel A: Full Sample

	(1) CSR_Score	(2) Emp	(3) Lnv	(4) Env	(5) Sup	(6) Com
BornBefore1962	-0.058***	-0.062**	0.007	-0.154***	-0.147***	-0.025
	(0.019)	(0.026)	(0.017)	(0.040)	(0.040)	(0.022)
Distance	-0.000	0.071**	-0.065***	0.091**	0.094**	-0.028
	(0.019)	(0.029)	(0.015)	(0.043)	(0.044)	(0.022)
Distance^2	0.000	0.000	-0.001***	0.001**	0.001**	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Distance^3	0.000**	0.000**	-0.000	0.000***	0.000***	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
ln(1+Chairman Age)	0.291	3.936***	-3.333***	5.496**	5.612**	-1.399
	(0.975)	(1.491)	(0.776)	(2.243)	(2.263)	(1.150)
Total Asset	0.206***	0.267***	0.083***	0.414***	0.414***	0.067***
	(0.006)	(0.009)	(0.007)	(0.013)	(0.013)	(0.007)
Leverage	-0.243***	-0.137***	-0.304***	-0.258***	-0.267***	0.176***
	(0.037)	(0.050)	(0.040)	(0.072)	(0.073)	(0.043)
ROA	4.294***	1.004***	7.900***	0.673**	0.681**	1.577***
	(0.260)	(0.242)	(0.410)	(0.281)	(0.281)	(0.230)
Mkt-Book-Ratio	-0.008	-0.056***	0.035***	-0.086***	-0.100***	0.025**
	(0.011)	(0.013)	(0.012)	(0.021)	(0.021)	(0.012)
Cash	0.050	0.457***	-0.260***	0.285*	0.311*	-0.027
	(0.078)	(0.108)	(0.085)	(0.157)	(0.160)	(0.095)
Fixed Asset	-0.105***	-0.079	-0.164***	0.219**	0.142*	-0.256***
	(0.041)	(0.055)	(0.038)	(0.085)	(0.084)	(0.048)
ln(1+Firm Age)	0.094***	0.164***	-0.054***	0.222***	0.275***	0.131***
	(0.017)	(0.025)	(0.014)	(0.040)	(0.039)	(0.020)
Observations	9606	9606	9606	9606	9606	9606
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted R-Square	0.344	0.224	0.473	0.205	0.197	0.290

Panel B: Two-Year Bandwidth (Year Born Between 1961 and 1962)

	(1) CSR_Score	(2) Emp	(3) Lnv	(4) Env	(5) Sup	(6) Com
Zhiqing	-0.063**	-0.067*	0.000	-0.141**	-0.135**	0.008
	(0.027)	(0.037)	(0.027)	(0.057)	(0.058)	(0.035)
Total Asset	0.180***	0.226***	0.069***	0.367***	0.356***	0.066***
	(0.016)	(0.023)	(0.015)	(0.036)	(0.036)	(0.020)
Leverage	-0.195**	-0.053	-0.258**	-0.311	-0.173	0.221*
	(0.096)	(0.134)	(0.109)	(0.197)	(0.204)	(0.115)
ROA	5.282***	2.041***	8.673***	1.214	1.550**	2.458***
	(0.486)	(0.527)	(0.750)	(0.767)	(0.763)	(0.626)
Mkt-Book-Ratio	0.050*	0.072**	0.070***	0.051	0.042	0.007
	(0.026)	(0.032)	(0.023)	(0.054)	(0.052)	(0.035)
Cash	-0.068	0.208	-0.023	-0.087	-0.191	-0.265
	(0.191)	(0.266)	(0.251)	(0.383)	(0.391)	(0.249)
Fixed Asset	-0.168	-0.274**	-0.338***	0.430**	0.260	-0.198
	(0.105)	(0.138)	(0.109)	(0.214)	(0.209)	(0.122)
ln(1+Firm Age)	0.002	-0.020	-0.058	0.002	0.019	0.157**
	(0.048)	(0.066)	(0.049)	(0.103)	(0.102)	(0.063)
Observations	1484	1484	1484	1484	1484	1484
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted R-Square	0.372	0.256	0.427	0.222	0.214	0.269

Panel C: Four-Year Bandwidth (Year Born Between 1960 and 1963)

	(1) CSR_Score	(2) Emp	(3) Lnv	(4) Env	(5) Sup	(6) Com
BornBefore1962	-0.133***	-0.122**	-0.029	-0.258***	-0.259***	-0.020
	(0.042)	(0.058)	(0.041)	(0.089)	(0.090)	(0.053)
Distance	-0.215	0.305	-0.377*	-0.249	-0.232	-0.102
	(0.229)	(0.327)	(0.203)	(0.487)	(0.496)	(0.269)
Distanceˆ2	-0.000	-0.002	0.010	-0.023	-0.024	-0.003
	(0.010)	(0.014)	(0.010)	(0.021)	(0.022)	(0.012)
ln(1+Chairman Age)	-7.978	19.050	-17.930*	-8.495	-7.377	-4.727
	(12.011)	(17.091)	(10.626)	(25.569)	(25.985)	(14.255)
Total Asset	0.198***	0.234***	0.080***	0.387***	0.377***	0.078***
	(0.012)	(0.017)	(0.011)	(0.027)	(0.027)	(0.015)
Leverage	-0.275***	-0.144	-0.312***	-0.404***	-0.339**	0.269***
	(0.066)	(0.093)	(0.071)	(0.135)	(0.140)	(0.082)
ROA	4.650***	1.061***	8.727***	0.333	0.538	2.294***
	(0.381)	(0.379)	(0.607)	(0.561)	(0.559)	(0.432)
Mkt-Book-Ratio	0.012	0.000	0.051**	-0.022	-0.020	0.000
	(0.021)	(0.025)	(0.021)	(0.041)	(0.040)	(0.025)
Cash	0.001	0.492**	-0.211	0.122	0.092	-0.340*
	(0.151)	(0.199)	(0.168)	(0.291)	(0.303)	(0.178)
Fixed Asset	-0.071	0.000	-0.285***	0.552***	0.433***	-0.214**
	(0.080)	(0.105)	(0.075)	(0.163)	(0.161)	(0.100)
ln(1+Firm Age)	0.067*	0.090*	-0.045	0.140*	0.161**	0.087*
	(0.038)	(0.051)	(0.031)	(0.084)	(0.080)	(0.048)
Observations	2710	2710	2710	2710	2710	2710
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted R-Square	0.354	0.229	0.468	0.203	0.193	0.286

Table 3.3: The Effects of CEOs' Down-to-the-Countryside Experience on Corporate Social Responsibility Activities: Regression Discontinuity Reduced Form

$Model : CSR\ Activities_{i,t} = \beta_0 + \beta_1 CEO\_Born\_Before\_1962_{i,t} + \beta_2 YearBorn_{i,t} + X_{i,t}^T \beta_3 + \phi_j + \gamma_t + \epsilon_{i,t}$

This table reports the results of reduced-form regression discontinuity analysis of the effects of CEOs' Down-to-the-Countryside experience on corporate social responsibility (CSR) activities over the sample period of 2010-2016. Chairmen born after 1961 have almost zero probability of being treated and chairmen born before 1961 have a high probability of being treated. $CSR\ Activities_{i,t}$ is the measure of CSR activities of firm i of year t. $Distance_{i,t}$ is the number of years from the born year to 1961. Standard errors are clustered at firm and year level and are reported in the parentheses. Statistical significance is represented according to $*p < 0.10, **p < 0.05, ***p < 0.01$.

Panel A: Full Sample

	(1) CSR_Score	(2) Emp	(3) Lnv	(4) Env	(5) Sup	(6) Com
BornBefore1962	-0.021	-0.062**	0.003	-0.029	-0.049	-0.004
	(0.019)	(0.027)	(0.017)	(0.042)	(0.041)	(0.022)
Distance	0.029*	0.067***	-0.014	0.097***	0.093**	-0.007
	(0.017)	(0.025)	(0.016)	(0.036)	(0.037)	(0.021)
Distanceˆ2	0.000	0.000	-0.000	0.001*	0.001	0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Distanceˆ3	0.000	0.000	0.000	-0.000	-0.000	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
ln(1+CEO Age)	1.527*	3.527***	-0.740	4.943***	4.790**	-0.337
	(0.867)	(1.314)	(0.846)	(1.900)	(1.948)	(1.092)
Total Asset	0.208***	0.266***	0.083***	0.420***	0.420***	0.068***
	(0.006)	(0.008)	(0.007)	(0.013)	(0.013)	(0.007)
Leverage	-0.247***	-0.130***	-0.307***	-0.266***	-0.279***	0.168***
	(0.037)	(0.050)	(0.041)	(0.072)	(0.072)	(0.043)
ROA	4.283***	1.031***	7.893***	0.677**	0.691**	1.589***
	(0.262)	(0.245)	(0.414)	(0.284)	(0.284)	(0.230)
Mkt-Book-Ratio	-0.012	-0.059***	0.035***	-0.096***	-0.109***	0.023*
	(0.011)	(0.013)	(0.011)	(0.021)	(0.021)	(0.012)
Cash	0.067	0.449***	-0.227***	0.299*	0.333**	-0.018
	(0.078)	(0.108)	(0.084)	(0.158)	(0.161)	(0.095)
Fixed Asset	-0.097**	-0.067	-0.171***	0.250***	0.180**	-0.234***
	(0.040)	(0.055)	(0.038)	(0.085)	(0.084)	(0.048)
ln(1+Firm Age)	0.093***	0.163***	-0.056***	0.218***	0.275***	0.134***
	(0.017)	(0.025)	(0.014)	(0.040)	(0.039)	(0.020)
Observations	9611	9611	9611	9611	9611	9611
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted R-Square	0.346	0.226	0.473	0.206	0.198	0.292

Panel B: Two-Year Bandwidth (Year Born Between 1961 and 1962)

	(1) CSR_Score	(2) Emp	(3) Lnv	(4) Env	(5) Sup	(6) Com
BornBefore1962	-0.021	-0.062**	0.003	-0.029	-0.049	-0.004
	(0.019)	(0.027)	(0.017)	(0.042)	(0.041)	(0.022)
Distance	0.029*	0.067***	-0.014	0.097***	0.093**	-0.007
	(0.017)	(0.025)	(0.016)	(0.036)	(0.037)	(0.021)
Distanceˆ2	0.000	0.000	-0.000	0.001*	0.001	0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Distanceˆ3	0.000	0.000	0.000	-0.000	-0.000	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
ln(1+CEO Age)	1.527*	3.527***	-0.740	4.943***	4.790**	-0.337
	(0.867)	(1.314)	(0.846)	(1.900)	(1.948)	(1.092)
Total Asset	0.208***	0.266***	0.083***	0.420***	0.420***	0.068***
	(0.006)	(0.008)	(0.007)	(0.013)	(0.013)	(0.007)
Leverage	-0.247***	-0.130***	-0.307***	-0.266***	-0.279***	0.168***
	(0.037)	(0.050)	(0.041)	(0.072)	(0.072)	(0.043)
ROA	4.283***	1.031***	7.893***	0.677**	0.691**	1.589***
	(0.262)	(0.245)	(0.414)	(0.284)	(0.284)	(0.230)
Mkt-Book-Ratio	-0.012	-0.059***	0.035***	-0.096***	-0.109***	0.023*
	(0.011)	(0.013)	(0.011)	(0.021)	(0.021)	(0.012)
Cash	0.067	0.449***	-0.227***	0.299*	0.333**	-0.018
	(0.078)	(0.108)	(0.084)	(0.158)	(0.161)	(0.095)
Fixed Asset	-0.097**	-0.067	-0.171***	0.250***	0.180**	-0.234***
	(0.040)	(0.055)	(0.038)	(0.085)	(0.084)	(0.048)
ln(1+Firm Age)	0.093***	0.163***	-0.056***	0.218***	0.275***	0.134***
	(0.017)	(0.025)	(0.014)	(0.040)	(0.039)	(0.020)
Observations	9611	9611	9611	9611	9611	9611
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted R-Square	0.346	0.226	0.473	0.206	0.198	0.292

Panel C: Four-Year Bandwidth (Year Born Between 1960 and 1963)

	(1) CSR_Score	(2) Emp	(3) Lnv	(4) Env	(5) Sup	(6) Com
BornBefore1962	-0.065	-0.069	-0.111***	0.045	0.040	-0.012
	(0.046)	(0.064)	(0.039)	(0.099)	(0.097)	(0.054)
Distance	-0.021	0.626*	-0.342*	0.220	0.296	-0.221
	(0.231)	(0.345)	(0.192)	(0.511)	(0.512)	(0.266)
Distanceˆ2	-0.002	-0.001	0.002	-0.011	-0.005	0.009
	(0.010)	(0.015)	(0.008)	(0.023)	(0.022)	(0.013)
ln(1+CEO Age)	-1.124	32.062*	-15.150	7.234	11.445	-11.789
	(12.071)	(17.991)	(10.081)	(26.745)	(26.745)	(14.056)
Total Asset	0.185***	0.234***	0.064***	0.374***	0.361***	0.066***
	(0.011)	(0.015)	(0.011)	(0.025)	(0.024)	(0.014)
Leverage	-0.261***	-0.280***	-0.295***	-0.371**	-0.408***	0.220**
	(0.072)	(0.097)	(0.076)	(0.146)	(0.145)	(0.090)
ROA	3.899***	0.181	7.860***	-0.065	0.040	1.974***
	(0.378)	(0.358)	(0.619)	(0.503)	(0.491)	(0.425)
Mkt-Book-Ratio	-0.007	-0.013	0.034*	-0.041	-0.038	0.008
	(0.018)	(0.023)	(0.018)	(0.038)	(0.037)	(0.022)
Cash	0.026	0.490**	-0.368**	0.540*	0.561*	-0.242
	(0.149)	(0.208)	(0.182)	(0.316)	(0.318)	(0.169)
Fixed Asset	-0.115	-0.110	-0.166**	0.161	0.059	-0.321***
	(0.075)	(0.106)	(0.065)	(0.163)	(0.158)	(0.097)
ln(1+Firm Age)	0.061*	0.110**	-0.052**	0.121	0.176**	0.170***
	(0.035)	(0.051)	(0.026)	(0.081)	(0.075)	(0.039)
Observations	2696	2696	2696	2696	2696	2696
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted R-Square	0.310	0.215	0.482	0.196	0.180	0.275

Table 3.4: The Effects of Down-to-the-Countryside Experience on Corporate Social Responsibility Activities: SOEs vs. Non-SOEs

$Model : CSR\ Activities_{i,t} = \beta_0 + \beta_1 Chairman\_Or\_CEO\_Born\_Before\_1962_{i,t} + \beta_2 YearBorn_{i,t} + X_{i,t}^T\beta_3 + \phi_j + \gamma_t + \epsilon_{i,t}$

This table reports the results of reduced-form regression discontinuity analysis of the effects of Chairmen's or CEOs' Down-to-the-Countryside experience on corporate social responsibility (CSR) activities of State-Owned-Enterprises and Non-State-Owned-Enterprises over the sample period of 2010-2016. Chairmen or CEO born after 1961 have almost zero probability of being treated and chairmen born before 1961 have a high probability of being treated. $CSR\ Activities_{i,t}$ is the measure of CSR activities of firm i of year t. $Distance_{i,t}$ is the number of years from the born year to 1961. Standard errors are clustered at firm and year level and are reported in the parentheses. Statistical significance is represented according to $*p < 0.10, **p < 0.05, ***p < 0.01$.

Panel A: Chairman and State-Owned Enterprise

	(1) CSR_Score	(2) Emp	(3) Lnv	(4) Env	(5) Sup	(6) Com
BornBefore1962	-0.190***	-0.254***	-0.022	-0.464***	-0.539***	0.043
	(0.068)	(0.093)	(0.050)	(0.147)	(0.146)	(0.070)
Distance	-0.200	0.418	-0.425	-0.181	-0.159	-0.436
	(0.339)	(0.459)	(0.279)	(0.738)	(0.742)	(0.370)
Distanceˆ2	0.005	0.004	-0.002	0.001	-0.009	-0.009
	(0.016)	(0.021)	(0.013)	(0.034)	(0.034)	(0.018)
ln(1+Chairman Age)	-6.194	26.412	-20.788	-0.379	1.991	-23.843
	(17.875)	(23.938)	(14.569)	(38.885)	(38.963)	(19.371)
Total Asset	0.212***	0.230***	0.102***	0.403***	0.392***	0.089***
	(0.017)	(0.024)	(0.014)	(0.039)	(0.038)	(0.019)
Leverage	-0.405***	-0.328**	-0.250**	-0.885***	-0.791***	0.266**
	(0.115)	(0.152)	(0.104)	(0.236)	(0.239)	(0.124)
ROA	4.535***	0.382	8.934***	-0.080	-0.180	2.793***
	(0.642)	(0.558)	(0.921)	(0.910)	(0.891)	(0.593)
Mkt-Book-Ratio	-0.016	-0.053*	0.011	-0.047	-0.035	-0.001
	(0.027)	(0.031)	(0.026)	(0.053)	(0.052)	(0.030)
Cash	-0.436*	-0.019	-0.441**	-0.412	-0.423	-0.829***
	(0.256)	(0.307)	(0.215)	(0.504)	(0.507)	(0.265)
Fixed Asset	-0.146	-0.471***	-0.176*	0.034	-0.070	-0.007
	(0.120)	(0.157)	(0.098)	(0.251)	(0.247)	(0.142)
ln(1+Firm Age)	0.053	0.024	-0.057	0.114	0.130	0.180***
	(0.057)	(0.075)	(0.039)	(0.133)	(0.126)	(0.067)
Observations	1330	1330	1330	1330	1330	1330
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted R-Square	0.353	0.234	0.524	0.212	0.204	0.361

Panel B: Chairman and Non-State-Owned Enterprise

	(1) CSR_Score	(2) Emp	(3) Lnv	(4) Env	(5) Sup	(6) Com
BornBefore1962	-0.045	0.048	-0.064	0.015	0.079	-0.040
	(0.055)	(0.076)	(0.069)	(0.105)	(0.111)	(0.083)
Distance	-0.199	0.274	-0.327	-0.388	-0.384	0.325
	(0.302)	(0.428)	(0.298)	(0.591)	(0.615)	(0.375)
Distanceˆ2	-0.008	-0.023	0.018	-0.061**	-0.059**	0.008
	(0.013)	(0.019)	(0.015)	(0.026)	(0.028)	(0.018)
ln(1+Chairman Age)	-9.048	14.222	-14.712	-22.568	-23.550	18.313
	(15.797)	(22.506)	(15.628)	(31.032)	(32.257)	(20.008)
Total Asset	0.175***	0.227***	0.052**	0.335***	0.342***	0.083***
	(0.020)	(0.028)	(0.022)	(0.043)	(0.044)	(0.026)
Leverage	-0.254***	-0.270**	-0.321***	-0.310**	-0.274*	0.310***
	(0.081)	(0.115)	(0.116)	(0.150)	(0.160)	(0.117)
ROA	4.603***	1.258**	8.585***	0.446	0.791	1.714***
	(0.471)	(0.500)	(0.820)	(0.680)	(0.711)	(0.641)
Mkt-Book-Ratio	0.041	0.050	0.117***	-0.001	-0.037	-0.019
	(0.033)	(0.051)	(0.040)	(0.083)	(0.077)	(0.055)
Cash	0.369**	0.912***	-0.035	0.612*	0.561	0.072
	(0.177)	(0.269)	(0.237)	(0.327)	(0.355)	(0.256)
Fixed Asset	-0.087	0.301*	-0.378**	0.905***	0.802***	-0.464**
	(0.136)	(0.167)	(0.161)	(0.240)	(0.245)	(0.181)
ln(1+Firm Age)	0.092*	0.119*	-0.025	0.142	0.161*	0.041
	(0.049)	(0.066)	(0.058)	(0.092)	(0.095)	(0.076)
Observations	1380	1380	1380	1380	1380	1380
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted R-Square	0.374	0.225	0.428	0.183	0.178	0.230

Panel C: CEO and State-Owned Enterprise

	(1) CSR_Score	(2) Emp	(3) Lnv	(4) Env	(5) Sup	(6) Com
BornBefore1962	-0.003	-0.022	-0.080	0.143	0.064	0.184**
	(0.074)	(0.097)	(0.052)	(0.158)	(0.152)	(0.072)
Distance	-0.085	0.555	-0.531*	0.032	0.058	-0.666
	(0.376)	(0.532)	(0.285)	(0.830)	(0.816)	(0.428)
Distanceˆ2	0.002	-0.027	0.013	-0.013	-0.005	0.010
	(0.017)	(0.023)	(0.012)	(0.037)	(0.036)	(0.018)
ln(1+CEO Age)	-5.243	25.437	-24.318*	-4.981	-1.476	-37.856*
	(19.581)	(27.717)	(14.640)	(43.489)	(42.608)	(22.693)
Total Asset	0.180***	0.229***	0.054***	0.374***	0.359***	0.086***
	(0.015)	(0.020)	(0.012)	(0.033)	(0.032)	(0.017)
Leverage	-0.275**	-0.305*	-0.045	-0.787***	-0.780***	0.263**
	(0.124)	(0.160)	(0.105)	(0.260)	(0.255)	(0.132)
ROA	4.112***	0.286	8.480***	0.053	-0.193	2.051***
	(0.673)	(0.598)	(0.931)	(0.997)	(0.978)	(0.664)
Mkt-Book-Ratio	-0.030	-0.057**	0.016	-0.070	-0.069	-0.027
	(0.024)	(0.029)	(0.021)	(0.049)	(0.049)	(0.029)
Cash	-0.427*	-0.423	-0.612***	-0.251	-0.127	-0.468*
	(0.244)	(0.316)	(0.193)	(0.528)	(0.525)	(0.251)
Fixed Asset	-0.399***	-0.423***	-0.342***	-0.527**	-0.575**	-0.408***
	(0.121)	(0.162)	(0.083)	(0.262)	(0.252)	(0.155)
ln(1+Firm Age)	-0.029	0.016	-0.077***	-0.074	-0.022	0.151***
	(0.051)	(0.074)	(0.028)	(0.121)	(0.111)	(0.056)
Observations	1322	1322	1322	1322	1322	1322
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted R-Square	0.289	0.221	0.505	0.216	0.194	0.314

Panel D: CEO and Non-State-Owned Enterprise

	(1) CSR_Score	(2) Emp	(3) Lnv	(4) Env	(5) Sup	(6) Com
BornBefore1962	-0.156***	-0.145*	-0.184***	-0.117	-0.040	-0.140
	(0.059)	(0.083)	(0.062)	(0.121)	(0.124)	(0.087)
Distance	-0.021	0.594	-0.155	0.112	0.280	0.235
	(0.281)	(0.428)	(0.265)	(0.592)	(0.624)	(0.344)
Distanceˆ2	-0.004	0.010	0.000	-0.010	-0.006	-0.002
	(0.013)	(0.019)	(0.013)	(0.026)	(0.027)	(0.017)
ln(1+CEO Age)	0.941	33.706	-4.235	6.562	13.280	12.846
	(14.572)	(22.259)	(14.139)	(30.783)	(32.396)	(17.926)
Total Asset	0.150***	0.177***	0.084***	0.234***	0.242***	0.043
	(0.020)	(0.028)	(0.021)	(0.043)	(0.043)	(0.028)
Leverage	-0.313***	-0.383***	-0.474***	-0.294*	-0.375**	0.276**
	(0.093)	(0.125)	(0.114)	(0.177)	(0.178)	(0.127)
ROA	3.740***	0.161	7.356***	0.107	0.336	1.592***
	(0.445)	(0.466)	(0.764)	(0.547)	(0.544)	(0.545)
Mkt-Book-Ratio	0.015	-0.012	0.059*	0.003	0.004	0.035
	(0.031)	(0.047)	(0.034)	(0.074)	(0.073)	(0.042)
Cash	0.240	0.977***	-0.264	0.834**	0.834**	-0.057
	(0.189)	(0.285)	(0.286)	(0.360)	(0.379)	(0.239)
Fixed Asset	0.041	0.061	0.017	0.521**	0.379*	-0.338**
	(0.099)	(0.142)	(0.099)	(0.208)	(0.206)	(0.137)
ln(1+Firm Age)	0.102**	0.160**	-0.027	0.192**	0.262***	0.168***
	(0.045)	(0.067)	(0.053)	(0.091)	(0.094)	(0.065)
Observations	1374	1374	1374	1374	1374	1374
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted R-Square	0.334	0.182	0.483	0.116	0.120	0.264

Table 3.5: The Heterogeneous Education Effects of Down-to-the-Countryside Experience on Corporate Social Responsibility Activities

$Model : CSR\ Activities_{i,t} = \beta_0 + \beta_1 Chairman\_Born\_Before\_1962_{i,t} + \beta_2 College_{i,t} + \beta_3 Chairman\_Born\_Before\_1962_{i,t} * College_{i,t} + \beta_4 Distance_{i,t} + \beta_5 Distance^2_{i,t} + \beta_6 Distance^3_{i,t} + X^T_{i,t}\beta_7 + \phi_j + \gamma_t + \epsilon_{i,t}$

This table reports the results of reduced-form regression discontinuity analysis of the heterogeneous gender effects of Chairmen's Down-to-the-Countryside experience on corporate social responsibility (CSR) activities of State-Owned-Enterprises and Non-State-Owned-Enterprises over the sample period of 2010-2016. Chairmen or CEO born after 1961 have almost zero probability of being treated and chairmen born before 1961 have a high probability of being treated. $CSR\ Activities_{i,t}$ is the measure of CSR activities of firm i of year t. $Distance_{i,t}$ is the number of years from the born year to 1961. $College_{i,t}$ is a dummy that equals to one if the chairman has at least a college degree. Standard errors are clustered at firm and year level and are reported in the parentheses. Statistical significance is represented according to $*p < 0.10, **p < 0.05, ***p < 0.01$.

	(1) CSR_Score	(2) Emp	(3) Lnv	(4) Env	(5) Sup	(6) Com
BornBefore1962	-0.136***	-0.127**	-0.031	-0.264***	-0.266***	-0.018
	(0.042)	(0.059)	(0.041)	(0.089)	(0.090)	(0.053)
College	0.022	0.114**	-0.040	0.094	0.130**	0.013
	(0.033)	(0.045)	(0.035)	(0.067)	(0.063)	(0.043)
BornBefore1962 * College	-0.138**	-0.257***	-0.073	-0.300***	-0.326***	0.095
	(0.055)	(0.099)	(0.057)	(0.106)	(0.114)	(0.062)
Distance	-0.230	0.283	-0.389*	-0.278	-0.261	-0.090
	(0.229)	(0.328)	(0.203)	(0.487)	(0.496)	(0.270)
Distanceˆ2	0.002	0.001	0.012	-0.019	-0.020	-0.005
	(0.010)	(0.014)	(0.010)	(0.022)	(0.022)	(0.013)
Observations	2710	2710	2710	2710	2710	2710
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted R-Square	0.354	0.231	0.468	0.204	0.194	0.286

Table 3.6: The Effects of Chairmen's Down-to-the-Countryside Experience on Corporate Social Responsibility Activities: 1961 as Prefix Discontinuity

$Model : CSR\ Activities_{i,t} = \beta_0 + \beta_1 Chairman\_Born\_After\_1946_{i,t} + \beta_2 YearBorn_{i,t} + X_{i,t}^T \beta_3 + \phi_j + \gamma_t + \epsilon_{i,t}$

This table reports the results of reduced-form regression discontinuity analysis of the effects of Chairmen's Down-to-the-Countryside experience on corporate social responsibility (CSR) activities over the sample period of 2010-2016. Chairmen born after 1961 have almost zero probability of being treated and chairmen born before 1961 have a high probability of being treated. $CSR\ Activities_{i,t}$ is the measure of CSR activities of firm i of year t. $Distance_{i,t}$ is the number of years from the born year to 1961. Standard errors are clustered at firm and year level and are reported in the parentheses. Statistical significance is represented according to $*p < 0.10, **p < 0.05, ***p < 0.01$.

	(1) CSR_Score	(2) Emp	(3) Lnv	(4) Env	(5) Sup	(6) Com
BornAfter1946	-0.154	-0.352**	-0.105*	-0.264	-0.294	-0.052
	(0.101)	(0.177)	(0.060)	(0.244)	(0.242)	(0.095)
Distance	1.996***	2.700**	0.052	5.236***	5.558***	2.208***
	(0.720)	(1.186)	(0.482)	(1.696)	(1.699)	(0.828)
Distanceˆ2	0.013	-0.042	0.008	0.070	0.074	0.033
	(0.027)	(0.043)	(0.018)	(0.061)	(0.060)	(0.027)
ln(1+Chairman Age)	124.831***	169.546**	0.702	327.962***	348.042***	140.511**
	(47.149)	(77.640)	(31.152)	(110.672)	(110.873)	(54.568)
Total Asset	0.305***	0.461***	0.096***	0.626***	0.674***	0.159***
	(0.036)	(0.052)	(0.021)	(0.078)	(0.073)	(0.044)
Leverage	-0.314	-0.486	-0.225*	-0.355	-0.366	0.139
	(0.200)	(0.302)	(0.130)	(0.433)	(0.451)	(0.223)
ROA	3.786***	1.776	5.076***	3.112**	3.202*	-0.579
	(0.771)	(1.117)	(0.787)	(1.530)	(1.689)	(0.809)
Mkt-Book-Ratio	-0.083*	-0.133*	-0.066*	-0.068	-0.110	-0.078**
	(0.043)	(0.080)	(0.036)	(0.105)	(0.099)	(0.037)
Cash	0.637	2.559***	-0.637**	1.871*	1.999**	0.525
	(0.429)	(0.706)	(0.298)	(0.966)	(0.940)	(0.382)
Fixed Asset	-0.198	-0.472	0.096	0.239	0.165	-0.362
	(0.210)	(0.334)	(0.150)	(0.481)	(0.475)	(0.247)
ln(1+Firm Age)	0.253***	0.403**	-0.084	0.527**	0.699***	0.354***
	(0.097)	(0.168)	(0.060)	(0.227)	(0.220)	(0.100)
Observations	323	323	323	323	323	323
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted R-Square	0.537	0.443	0.567	0.462	0.493	0.525

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— Appendix A —

Additional Materials for Chapter 1

Figure A1
Number of Fatal Car Accidents in a State Before Event Time

This figure plots the Number of Fatal Car Accidents in a State Around Event Time. Event year zero is the year when the treated states banned or restricted consumer litigation funding. Treated states are Ohio, Michigan, New York, Colorado, Arkansas, and Arizona. Control states are Kentucky, Georgia, New Jersey, California, Florida, and Utah, respectively, which did not ban or restrict consumer litigation funding during the respective periods. The sample period is 1998-2014.

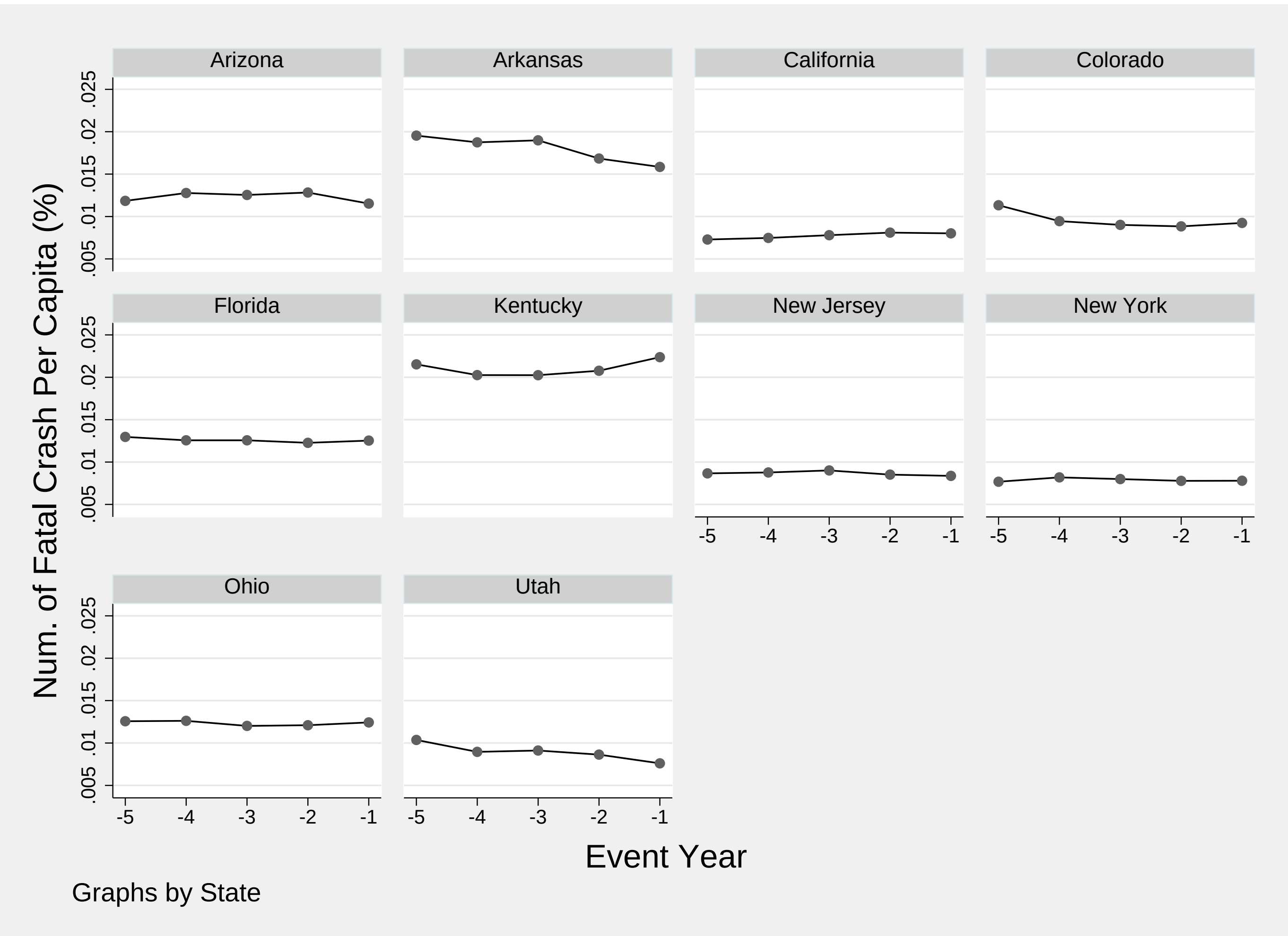

Table A1
Diff-in-Diff regressions between Treated and Non-Treated States: Other Specifications

$Model : Number\ of\ New\ Lawsuits\ Filing_{i,t}/Population_{i,t} = \beta_0 + \beta_1 Post_{i,t} \times Treatment_{i,t} + \beta_2 Post_{i,t} + \beta_3 Treatment_{i,t} + X^T\beta_4 + \phi_i + \gamma_t + \epsilon_{i,t}$

This table reports results of different specifications of difference-in-difference analysis of state-level regulation change regarding restrictions on litigation financing over the sample period of 2001-2010, using the states of Ohio, Kentucky, New York, and Michigan. The treated states are Ohio and New York, and the control states are Kentucky and Michigan. The models above test the difference between the treated states that passed laws restricting litigation finance and the states that did not pass laws in the sample periods, and estimate the effects of restrictions of litigation financing on the number of tort lawsuits filed with the courts. $Number\ of\ New\ Lawsuits\ Filed_{i,t}/Population_{i,t}$ is the ratio of the number of new lawsuits filed in county i of year t over the population in county i of year t. $Treatment_{i,t}$ is a dummy variable that equals to one if the state is a treated state, and equals to zero if the state is a non-treated state. $Post_{i,t}$ is a dummy variable equal to one in the years following the state's restriction of litigation financing and zero otherwise. Unemployment rate is the number of unemployed population scaled by the labor force in the same county. Economic growth is defined as $Y_{i,t}/Y_{i,t-1}$, where $Y_{i,t}$ is the per capita income in county i of year t, i.e. the annual personal income per county scaled by the annual county population, measured in 1983 U.S. dollars. Column (1) is a univariate regression analysis. Column (2) and (3) control for county demographic and economic characteristics. Column (2) controls for state fixed effects and column (3) controls for both state fixed effects and year fixed effects. All standard errors are clustered at county and year level and are reported in the parentheses. (Sample Period: 2001-2017)

Panel A: Event-window Diff-in-Diff : Raw Number of Lawsuits

	(1) New Filings	(2) New Filings	(3) New Filings
Treatment * Post	-138.300	-132.682***	-133.368***
	(94.184)	(33.533)	(33.514)
Pop - above 65 yrs old		0.024***	0.024***
		(0.008)	(0.008)
Pop - below 19 yrs old		0.020***	0.020***
		(0.003)	(0.003)
Pop - white race		-0.006***	-0.006***
		(0.001)	(0.001)
Pop - female		-0.003	-0.003
		(0.004)	(0.004)
Economic growth		120.527	64.803
		(146.096)	(163.704)
Unemployment population		-0.013**	-0.014**
		(0.006)	(0.006)
Labor force		0.001	0.001
		(0.003)	(0.003)
Year FE	No	Yes	Yes
State FE	No	Yes	No
County FE	No	No	Yes
Observations	2808	2808	2808
Adjusted R^2	0.048	0.883	0.883

Panel B: Staggerred Diff-in-Diff : Raw Number of Lawsuits

	(1) New Filings	(2) New Filings	(3) New Filings
Treatment * Post	-158.005**	-140.793***	-129.415***
	(77.479)	(24.749)	(28.626)
Pop - above 65 yrs old		0.018***	0.018***
		(0.005)	(0.005)
Pop - below 19 yrs old		0.020***	0.020***
		(0.002)	(0.002)
Pop - white race		-0.005***	-0.005***
		(0.001)	(0.001)
Pop - female		0.001	0.001
		(0.003)	(0.003)
Economic growth		32.896	70.141
		(121.372)	(120.135)
Unemployment population		-0.010*	-0.011*
		(0.006)	(0.006)
Labor force		-0.002	-0.002
		(0.002)	(0.002)
Year FE	No	Yes	Yes
State FE	No	Yes	No
County FE	No	No	Yes
Observations	5275	5275	5275
Adjusted R^2	0.048	0.887	0.887

Table A3
Income Heterogeneous Effects of Consumer Litigation Funding on the Number of Civil Lawsuits Filed

$Model : Number\ of\ Civil\ Lawsuits\ Filed_{i,t}/Population_{i,t} = \beta_0 + \beta_1 Treatment_{i,t} \times Post_{i,t} \times Income_{i,t} + \beta_2 Treatment_{i,t} \times Post_{i,t} + \beta_3 Treatment_{i,t} \times Income_{i,t} + \beta_4 Post_{i,t} \times Income_{i,t} + \beta_5 Income_{i,t} + \beta_6 Treatment_{i,t} + \beta_7 Post_{i,t} + X^T \beta_8 + \phi_i + \gamma_t + \epsilon_{i,t}$

This table reports the results of an event-window event-time difference-in-difference analysis of state actions regarding restrictions of consumer litigation funding over the sample period of 2001-2017, comparing the number of civil lawsuits filed between tort cases and non-tort cases within only treated states. Treated states are states that banned or restricted consumer litigation funding during Supreme Court rulings. Control states are states that did not ban or restrict consumer litigation funding during the respective periods. A tort case is defined as a civil wrong that causes a claimant to suffer loss or harm resulting in legal liability. $Number\ of\ Civil\ Lawsuits\ Filed_{i,j,t}/Population_{i,t}$ is the ratio of the number of civil lawsuits of type j filed in county i in year t over the population in county i of year t. $Post_{i,j,t}$ is a dummy variable equal to one in the years following the respective treated state's restriction of litigation financing and zero otherwise. $Tort_{i,j,t}$ is a dummy variable equal to one if the observation is a tort civil case type, and zero if the case is a non-tort civil case type. Unemployment rate is the number of unemployed population scaled by the labor force in the same county. Economic growth is defined as $Y_{i,t}/Y_{i,t-1}$, where $Y_{i,t}$ is the income per capita in county i of year t, measured in 1983 U.S. dollars. All standard errors are clustered at county and year level and are reported in the parentheses. Significance is represented according to $*p < 0.10, **p < 0.05, ***p < 0.01$.

	(1) New Filings/Popul.	(2) New Filings/Popul.
Treatment * Post * Income	-0.036	0.067
	(0.051)	(0.104)
Treatment * Post	-0.135***	-0.238**
	(0.027)	(0.115)
Treatment * Income	0.237***	0.271**
	(0.078)	(0.109)
Post * Income	-0.072**	-0.182*
	(0.033)	(0.099)
Income	-0.146	-0.029
	(0.094)	(0.100)
Observations	2930	2930
Control Variables	Yes	Yes
Year FE	No	Yes
State FE	No	Yes
Within R-Square	0.008	0.018

Table A4
The Effects of Consumer Litigation Funding on Non-tort Civil Lawsuit Plaintiff Winning Rates: Difference-in-Difference

$Model : Tort\ Lawsuit\ Plaintiff\ Wining\ Rate_{i,t} = \beta_0 + \beta_1 Treatment_{i,j,t} \times Post_{i,j,t} \times Tort_{i,j,t} + \beta_2 Treatment_{i,j,t} \times Post_{i,j,t} + \beta_3 Treatment_{i,j,t} \times Tort_{i,j,t} + \beta_4 Post_{i,j,t} \times Tort_{i,j,t} + \beta_5 Tort_{i,j,t} + \beta_6 Treatment_{i,j,t} + \beta_7 Post_{i,j,t} + X^T \beta_8 + \phi_i + \gamma_t + \epsilon_{i,j,t}$

This table reports the results of an event-window event-time difference-in-difference analysis of state actions regarding restrictions of consumer litigation funding over the sample period of 2001-2017, comparing the number of civil lawsuits filed between tort cases and non-tort cases within only treated states. Treated states are states that banned or restricted consumer litigation funding during Supreme Court rulings. Control states are states that did not ban or restrict consumer litigation funding during the respective periods. A tort case is defined as a civil wrong that causes a claimant to suffer loss or harm resulting in legal liability. $Number\ of\ Civil\ Lawsuits\ Filed_{i,j,t}/Population_{i,t}$ is the ratio of the number of civil lawsuits of type j filed in county i in year t over the population in county i of year t. $Post_{i,j,t}$ is a dummy variable equal to one in the years following the respective treated state's restriction of litigation financing and zero otherwise. $Tort_{i,j,t}$ is a dummy variable equal to one if the observation is a tort civil case type, and zero if the case is a non-tort civil case type. Unemployment rate is the number of unemployed population scaled by the labor force in the same county. Economic growth is defined as $Y_{i,t}/Y_{i,t-1}$, where $Y_{i,t}$ is the income per capita in county i of year t, measured in 1983 U.S. dollars. All standard errors are clustered at county and year level and are reported in the parentheses. Significance is represented according to $*p < 0.10, **p < 0.05, ***p < 0.01$.

	(1) Plaintiff Winning Rate (%)	(2) Plaintiff Winning Rate (%)
Treatment * Post	-0.004	0.004
	(0.039)	(0.022)
Pop ratio - above 65 yrs old	1.080***	1.010
	(0.346)	(0.783)
Pop ratio - below 19 yrs old	0.843**	1.979*
	(0.380)	(1.025)
Pop ratio - white race	-0.099	-1.135***
	(0.066)	(0.363)
Pop ratio - female	-2.544***	-5.454***
	(0.530)	(1.947)
percap_income_growth	0.075	-0.336**
	(0.228)	(0.160)
Unemployment rate	-0.002	-0.002
	(0.005)	(0.005)
Pop ratio - labor force	0.067	-0.939***
	(0.211)	(0.283)
Observations	2259	2259
Adjusted R^2	0.140	0.642
Year FE	Yes	Yes
State FE	Yes	No
County FE	No	Yes

Table A5
The Effects of Consumer Litigation Funding on Tort Lawsuit Plaintiff Winning Rates: Parallel Trends

$Model : Tort\ Lawsuit\ Plaintiff\ Wining\ Rate_{i,t} = \beta_0 + \beta_1 Treatment_{i,j,t} \times Post_{i,j,t} \times Tort_{i,j,t} + \beta_2 Treatment_{i,j,t} \times Post_{i,j,t} + \beta_3 Treatment_{i,j,t} \times Tort_{i,j,t} + \beta_4 Post_{i,j,t} \times Tort_{i,j,t} + \beta_5 Tort_{i,j,t} + \beta_6 Treatment_{i,j,t} + \beta_7 Post_{i,j,t} + X^T \beta_8 + \phi_i + \gamma_t + \epsilon_{i,j,t}$

This table reports the results of an event-window event-time difference-in-difference analysis of state actions regarding restrictions of consumer litigation funding over the sample period of 2001-2017, comparing the number of civil lawsuits filed between tort cases and non-tort cases within only treated states. Treated states are states that banned or restricted consumer litigation funding during Supreme Court rulings. Control states are states that did not ban or restrict consumer litigation funding during the respective periods. A tort case is defined as a civil wrong that causes a claimant to suffer loss or harm resulting in legal liability. $Number\ of\ Civil\ Lawsuits\ Filed_{i,j,t}/Population_{i,t}$ is the ratio of the number of civil lawsuits of type j filed in county i in year t over the population in county i of year t. $Post_{i,j,t}$ is a dummy variable equal to one in the years following the respective treated state's restriction of litigation financing and zero otherwise. $Tort_{i,j,t}$ is a dummy variable equal to one if the observation is a tort civil case type, and zero if the case is a non-tort civil case type. Unemployment rate is the number of unemployed population scaled by the labor force in the same county. Economic growth is defined as $Y_{i,t}/Y_{i,t-1}$, where $Y_{i,t}$ is the income per capita in county i of year t, measured in 1983 U.S. dollars. All standard errors are clustered at county and year level and are reported in the parentheses. Significance is represented according to $*p < 0.10, **p < 0.05, ***p < 0.01$.

	(1) Plaintiff Winning Rate	(2) Plaintiff Winning Rate
EventYear=-5	-0.017	0.007
	(0.190)	(0.090)
EventYear=-4	-0.060	0.031
	(0.334)	(0.093)
EventYear=-3	-0.123	0.027
	(0.480)	(0.113)
EventYear=-2	-0.222	0.008
	(0.598)	(0.120)
EventYear=-1	-0.250	0.058
	(0.725)	(0.154)
Observations	165	165
Year FE	Yes	Yes
State FE	Yes	No
County FE	No	Yes
Within R-Square	0.237	0.268

— Appendix B —

Additional Materials for Chapter 2

Table B1
Replication of Table 2 of Bali et al.(2014)
Contemporaneous returns for portfolios formed on LIQU

For month t , NYSE, AMEX, and NASDAQ stocks are sorted into 10 decile portfolios based on their liquidity shock (LIQU) using the NYSE breakpoints. LIQU is defined as the negative Amihud (2002) illiquidity measure (ILLIQ), demeaned using the past 12-month ILLIQ as the mean. This table reports the equal- and value-weighted monthly contemporaneous returns (month t) and the alpha with respect to the Fama-French (1993) factors for each LIQU portfolio. Columns "LIQU" and "ILLIQ" report the average LIQU and ILLIQ values for each decile portfolio. The last column shows the average market share of each portfolio. The last row shows the differences in monthly returns between high- and low-LIQU decile portfolios and the alphas.Average returns and alphas are defined in monthly percentage terms. Newey-West t -statistics are given in parentheses. The sample covers the period from January 2001 to December 2010.

	Equal-weighted		Value-weighted				
Decile	RET	Alpha	RET	Alpha	LIQU	ILLIQ	% Mkt. shr.
1(Low)	-0.28	-1.46***	-0.82***	-1.95***	-0.26***	0.51***	4.57
	(-1.16)	(-15.44)	(-3.39)	(-21.78)	(-10.75)	(14.26)	
2	-0.19	-1.34***	-0.43	-1.52***	-0.01***	0.03***	10.74
	(-0.74)	(-13.74)	(-1.64)	(-15.14)	(-4.31)	(6.11)	
3	0.23	-0.91***	0.05	-1.04***	-0.00***	0.02***	16.75
	(0.91)	(-10.57)	(0.19)	(-11.49)	(-3.60)	(5.22)	
4	0.61**	-0.51***	0.52*	-0.55***	-0.00***	0.01***	16.81
	(2.36)	(-6.18)	(1.92)	(-6.33)	(-2.66)	(5.31)	
5	0.97***	-0.12	0.95***	-0.05	-0.00	0.01***	15.22
	(3.97)	(-1.62)	(3.91)	(-0.73)	(-0.35)	(6.33)	
6	1.36***	0.29***	1.45***	0.46***	0.00***	0.01***	12.42
	(5.59)	(4.50)	(6.01)	(6.85)	(2.72)	(7.15)	
7	1.76***	0.69***	1.86***	0.88***	0.00***	0.01***	9.15
	(7.23)	(11.48)	(7.60)	(12.49)	(4.18)	(7.48)	
8	2.11***	1.03***	2.21***	1.22***	0.01***	0.02***	6.79
	(8.84)	(15.94)	(9.35)	(15.45)	(4.91)	(7.35)	
9	2.61***	1.48***	2.61***	1.58***	0.01***	0.03***	4.67
	(10.26)	(22.37)	(10.74)	(18.77)	(5.63)	(8.18)	
10(High)	4.29***	3.11***	3.45***	2.34***	0.28***	0.18***	2.87
	(14.90)	(35.97)	(12.91)	(30.99)	(15.30)	(18.39)	
High-Low	4.57***	4.56***	4.27***	4.28***			
	(27.42)	(36.58)	(25.11)	(29.27)			
Observations	558	558	558	558	558	558	558

Newey-West t-statistics in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table B2
Replication of Table 3 of Bali et al.(2014)
One-month-ahead returns for portfolios formed on LIQU

For month t , NYSE, AMEX, and NASDAQ stocks are sorted into 10 decile portfolios based on one of the three liquidity shock measures (LIQU, SPRDU, and LIQCU) using the NYSE breakpoints. LIQU denotes the liquidity shock, defined as the negative Amihud (2002) illiquidity measure (ILLIQ), demeaned using the past 12-month ILLIQ as the mean. This table reports the equal- and value-weighted one-month-ahead returns (month t+1) and the alpha with respect to the Fama-French (1993) factors for each liquidity shock decile portfolio. The last row shows the differences in monthly returns between high- and low-liquidity shock decile portfolios and the corresponding alphas. Average returns and alphas are defined in monthly percentage terms. Newey-West t-statistics are given in parentheses. The sample covers the period from January 2001 to December 2010.

	Equal-weighted		Value-weighted	
Decile	RET	Alpha	RET	Alpha
1(Low)	0.57**	-0.63***	0.43*	-0.70***
	(2.12)	(-7.65)	(1.65)	(-7.55)
2	0.71***	-0.44***	0.60**	-0.46***
	(2.69)	(-4.69)	(2.49)	(-4.82)
3	0.93***	-0.17**	0.84***	-0.18**
	(3.67)	(-2.01)	(3.61)	(-2.06)
4	1.01***	-0.06	0.98***	-0.02
	(4.20)	(-0.85)	(4.35)	(-0.24)
5	1.07***	-0.01	0.99***	-0.01
	(4.36)	(-0.11)	(4.31)	(-0.07)
6	1.09***	0.01	1.04***	0.05
	(4.47)	(0.11)	(4.69)	(0.85)
7	1.18***	0.11*	1.20***	0.23***
	(4.99)	(1.86)	(5.29)	(3.75)
8	1.24***	0.12**	1.17***	0.17**
	(4.97)	(2.03)	(4.93)	(2.20)
9	1.37***	0.23***	1.32***	0.28***
	(5.26)	(3.79)	(5.30)	(3.72)
10(High)	1.66***	0.50***	1.57***	0.47***
	(6.09)	(7.42)	(6.05)	(6.28)
High-Low	1.09***	1.13***	1.15***	1.18***
	(10.15)	(10.45)	(7.83)	(7.79)
Observations	557	557	557	557

Newey-West t-statistics in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table B3
Replication of Table 6 of Bali et al.(2014)
Stock-level cross-sectional regressions

Monthly excess stock returns are regressed on a set of lagged predictive variables using the Fama and MacBeth (1973) methodology. This table reports the average slope coefficients and Newey-West t -statistics in parentheses. LIQU denotes the liquidity shock, defined as the negativeAmihud (2002) illiquidity measure (ILLIQ), demeaned using the past 12-month ILLIQ as the mean. BETA, LNME, and LNBM denote the market beta, the natural logarithm of the market capitalization, and the natural logarithm of the book-to-market equity ratio, respectively. The sample covers the period from January 2001 to December 2010.

Variable	LIQU
LIQU	0.071***
	(4.69)
BETA	0.123
	(1.44)
LNME	-0.100***
	(-3.53)
LNBM	0.227***
	(4.22)
MOM	0.007***
	(5.38)
REV	-0.040***
	(-11.22)
IVOL	-0.245***
	(-7.65)
ILLIQ	0.016
	(-7.65)
Constant	1.532***
	(7.45)
Observations	1384971
R-squared	0.0692

Newey-West t-statistics in parentheses
*** p<0.01, ** p<0.05, * p<0.1

— Appendix C —

Additional Materials for Chapter 3

Table A1: Variables and Definitions

	Variable	Definition
Dependent Variable	CSR_Score	Natural logarithm of one plus the sum of strength scores for community, environment, employee relations, shareholder and supplier-customer components
	Com	Natural logarithm of one plus professional evaluation system of community by Hexun
	Env	Natural logarithm of one plus professional evaluation system of environment by Hexun
	Emp	Natural logarithm of one plus professional evaluation system of employee relations by Hexun
	Lnv	Natural logarithm of one plus professional evaluation system of shareholder by Hexun
	Sup	Natural logarithm of one plus professional evaluation system of supplier-customer by Hexun
Independent Variable	*Zhiqing*	Binominal variable, which equals 1 if the corporate leader was sent to the countryside or the mountain an zero otherwise.
	BornBefore1962	Binomial variable, which equals 1 if the corporate leader was born before 1962, and zero otherwise.
Controls	*Age*	The age of the board director
	ROA	Calculated by net income divided by total assets.
	Leverage	Debt-to-assets ratio of a company
	EstablishedTime	Natural logarithm of one plus number of years since the firm's IPO
	MB	The ratio of the market value of equity to the book value of equity at the end of the fiscal year
	Size	Natural logarithm of a company's equity market capitalization
	Cash	Cash and cash equivalents divided by total assets.
	Fixed_asset	which is firm property, plant, and equipment (PPE) scaled by total assets
	YEAR	8 dummy variables are adopted to proxy for the 8 years from 2010 to 2017
	SOE	Dummy variable for state-owned enterprises (1 for SOEs and 0 for Non-SOEs)

Table A2
The Heterogeneous Distance Effects of Down-to-the-Countryside Experience on Corporate Social Responsibility Activities

$Model : CSR\ Activities_{i,t} = \beta_0 + \beta_1 Chairman\_Born\_Before\_1962_{i,t} + \beta_2 Age_{i,t} + \beta_3 Chairman\_Born\_Before\_1962_{i,t} * Age_{i,t} + \beta_4 Distance_{i,t} + \beta_5 Distance^2_{i,t} + \beta_6 Distance^3_{i,t} + X^T_{i,t}\beta_7 + \phi_j + \gamma_t + \epsilon_{i,t}$

This table reports the results of reduced-form regression discontinuity analysis of the heterogeneous age effects of Chairmen's Down-to-the-Countryside experience on corporate social responsibility (CSR) activities of State-Owned-Enterprises and Non-State-Owned-Enterprises over the sample period of 2010-2016. Chairmen or CEO born after 1961 have almost zero probability of being treated and chairmen born before 1961 have a high probability of being treated. $CSR\ Activities_{i,t}$ is the measure of CSR activities of firm i of year t. $Distance_{i,t}$ is the number of years from the born year to 1961. Standard errors are clustered at firm and year level and are reported in the parentheses. Statistical significance is represented according to $*p < 0.10, **p < 0.05, ***p < 0.01$.

	(1) CSR_Score	(2) Emp	(3) Lnv	(4) Env	(5) Sup	(6) Com
BornBefore1962	-0.185**	-0.248**	-0.020	-0.471***	-0.567***	0.010
	(0.074)	(0.098)	(0.061)	(0.154)	(0.155)	(0.079)
BornBefore1962 * Distance	-0.011	-0.011	-0.002	0.014	0.056	0.065
	(0.062)	(0.083)	(0.052)	(0.134)	(0.134)	(0.069)
Distance	-0.080*	-0.095*	-0.020	-0.227***	-0.272***	-0.024
	(0.044)	(0.055)	(0.042)	(0.087)	(0.088)	(0.049)
ln(1+Chairman Age)	-6.025	26.489	-20.969	-0.016	2.406	-23.266
	(17.877)	(23.938)	(14.564)	(38.908)	(38.995)	(19.466)
Total Asset	0.209***	0.228***	0.105***	0.396***	0.384***	0.079***
	(0.017)	(0.023)	(0.014)	(0.038)	(0.038)	(0.018)
Leverage	-0.389***	-0.321**	-0.268***	-0.850***	-0.752***	0.321***
	(0.114)	(0.150)	(0.102)	(0.234)	(0.235)	(0.123)
ROA	4.565***	0.396	8.902***	-0.015	-0.106	2.896***
	(0.640)	(0.558)	(0.921)	(0.911)	(0.890)	(0.588)
Mkt-Book-Ratio	-0.013	-0.052*	0.008	-0.041	-0.029	0.008
	(0.026)	(0.031)	(0.026)	(0.052)	(0.052)	(0.030)
Cash	-0.430*	-0.016	-0.448**	-0.399	-0.408	-0.807***
	(0.257)	(0.306)	(0.215)	(0.505)	(0.508)	(0.265)
Fixed Asset	-0.146	-0.471***	-0.176*	0.034	-0.071	-0.008
	(0.120)	(0.157)	(0.099)	(0.251)	(0.247)	(0.142)
Age	0.105	-0.525	0.411	-0.050	-0.087	0.439
	(0.342)	(0.459)	(0.278)	(0.746)	(0.748)	(0.373)
Observations	1330	1330	1330	1330	1330	1330
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted R-Square	0.353	0.235	0.524	0.212	0.204	0.357