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Essays on Financial Economics

DISSERTATION

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DOCTOR OF PHILOSOPHY

in Management

by

Yue Li

Dissertation Committee:
Professor Yuhai Xuan, Chair
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2024

DEDICATION

To

my wife and parents

in recognition of their full support

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VITA

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ABSTRACT OF THE DISSERTATION

Essays on Financial Economics

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This dissertation consists of three essays on empirical financial economics. The first essay, "External Litigation Risk and Corporate ES Performance: Evidence from Federal Judge Ideology", explores how firms' environmental and social (ES) performance are affected by external ES litigation risk, proxied by judge political ideology. The second essay, "Local Environmental Beliefs and Corporate Environmental Performance", which was co-authored with Yuhai Xuan and Hong Wu, investigates the effect of local environmental beliefs on US establishments' environmental performance and on firms' pollution reallocation decisions. The third essay, "Information on Collateral Value and Bank Mortgage Lending", studies the US residential property market and the effect of collateral information advantages on banks' lending decisions.

Chapter 1

Introduction

This dissertation consists of three essays on empirical financial economics. The first two essays are related to a heated topic in finance literature over the past few years, namely corporate ESG performance. In the first essay, we examine how environmental and social (ES) litigation risk affects corporate ES performance. We measure ES-related litigation risk by using the political ideology of circuit court judges, based on the assumption that partisanship plays an important role in people's view towards environmental and social issues. We empirically verify the validity of this measure and find that higher ex-ante ES litigation risk is associated with fewer corporate ES violations. Moreover, such an effect becomes stronger for firms with higher institutional ownership or with at least one blockholder. In addition, we find that investors react negatively to liberal judge appointments, and such effect is stronger among firms with lower ES ratings. These firms also reduce capital investments and make fewer acquisitions. Overall, in this essay, we explore a novel and important determinant on corporate environmental performance.

The second essay, co-authored with Hong Wu and Yuhai Xuan, studies another important determinant on corporate environmental policy, i.e. local environmental beliefs. Using data on county-level environmental beliefs and firms' plant-level toxic emissions, we find that there is a negative relation between local environmental beliefs and the level of toxic emissions. In terms of the possible channels. We find that the negative relation is stronger if the firm has a greater local investor base and if the county has a greater number of stakeholders. Furthermore, we find that local environmental belief is associated with a higher severity of EPA enforcement activity, greater number of environmental lawsuits and judge support for these lawsuits. Finally, we discover a within-firm internal reallocation mechanism that toxic emissions of the plant in strong environmental beliefs counties are more likely to be shifted to other plants, after a formal enforcement action enforced by the EPA. Overall, our paper contributes to the literature by showing local environmental beliefs play an important role in corporate environment decisions.

The third essay speaks to an important topic about the information advantage concerning collateral value. Using property-level granular transaction and mortgage data in the US residential real estate market from 1995 to 2017, we investigate how banks exploit information on their collateral values when making lending decisions. The results show that the likelihood that the bank will continue to finance a property is positively associated with the value appreciation of the property between its current and previous sales. We attribute this finding to banks' proprietary information on their collaterals. Consistent with this argument, we find evidence that banks fail to exploit such information on properties they did not finance. Moreover, we test for empirical implications of such information advantage and find some supporting evidence. These results are consistent with the collateral lending channel.

Chapter 2

External Litigation Risk and Corporate ES Performance: Evidence from Federal Judge Ideology[1]

2.1 Introduction

US investors care more about sustainable investing now than ever before. By the start of 2020, one in three dollars invested in the United States is under management using sustainable investing strategies.[2] Owing to increased attention and pressure from investors, large companies start to revise their long-standing principle of shareholder primacy to incorporate stakeholder interests.[3] Therefore, how firms treat their stakeholders and what factors influence their relationships become timely questions, attracting a quickly expanding body of research in corporate finance (Heese, Pérez-Cavazos, and Peter (2022); Zaman, Atawnah, Baghdadi, and Liu (2021); Raghunandan and Rajgopal (2021)).

In this paper, we document that one crucial factor, external ES litigation risk, affects firms' performance in environmental and social areas. Motivated by legal research findings that political ideology in the courtroom plays an important role in judge decisions and lawsuit outcomes (Tate (1981); Segal and Cover (1989); Staudt, Epstein, and Wiedenbeck (2006)), we exploit a unique

[1] I am deeply grateful for the comments and suggestions on this paper provided by Yuhai Xuan, Hong Wu, Lu Zheng, Jinfei Sheng, Chong Huang, Jack Liebersohn and Wanyi Wang.

[2] Report on US Sustainable and Impact Investing Trends, US SIF. For more details, see https://www.ussif.org/files/Trends%20Report%202020%20Executive%20Summary.pdf.

[3] For example, in August 2019, 181 CEOs of large US companies signed a statement in Business Roundtable, committing to lead their companies for the benefit of all stakeholders, including customers, employees, suppliers, communities and shareholders. This new statement on the purpose of a corporation supersedes previous statements which endorsed principles of shareholder primacy.

feature of US federal court system to measure ES litigation risk: the ideology of judges in circuit courts. Inspired by previous works that judge ideology has important ramifications in corporate litigation (Huang, Hui, and Li (2019); Liu (2020)), we hypothesize that judges with liberal ideology are likely to be harsher on firms in ES lawsuits, compared to their counterparts.[4] The potential harsher punishment heightens corporate ES risk and anticipation of higher litigation risk has significant implications on corporate ES performance.

Our measure of ES litigation risk has a few advantages compared to existing measures. First and foremost, it circumvents the difficulty by measuring litigation risk from an exogenous perspective. In the literature of corporate litigation, it is notoriously difficult to reach causal interpretations as corporate litigation risk measured by firm or industry characteristics may be correlated to unobserved firm-level factors which potentially affect firm operations or managerial decisions.[5] Second, in addition to other measures on litigation risk that mostly capture the likelihood that firms will be sued, judge ideology also affects the cost of lawsuits to firms (Liu (2020)).

As laws set the minimum standard for ethical behavior, litigation risk may efficiently deter badwill but may not necessarily promote goodwill. In this regard, we hypothesize that ES litigation risk specifically affects the dark side of corporate ES behavior. To capture firms' ES bad behaviors,

[4] This hypothesis is motivated by an ever-widening divide between liberals and conservatives in many political topics. Anecdotal evidence shows that the US is becoming increasingly politically polarized. For example, in a NBC News poll, Some 80% of Democrats and Republicans believe that the other political party poses a threat that, if not stopped, "will destroy America as we know it". (Source: https://www. axios.com/2022/10/23/poll-midterm-election-democrats-republicans). Among all partisan divides, environmental and social related topics are among the most contentious ones. For example, Climate Insights 2020 documents significant partisan divide in terms of climate change and global warming. (Source: https://www.rff.org/publications/reports/climateinsights2020-partisan-divide/).

[5] In recent years, scholars in finance and accounting often utilize law shocks as quasi-natural experiments which presumably affect ex-ante corporate litigation risk from an exogenous perspective. For example, a number of papers utilize Universal Demand Law and Ninth Circuit ruling on July 2, 1999, as exogenous shocks to litigation risk and test implications on a variety of managerial decisions and corporate outcomes. An incomplete list of these studies includes Crane and Koch (2018); Freund, Nguyen, and Phan (2021); Chung, Kim, Rabarison, To, and Wu (2020); Hassan, Houston, and Karim (2021); Lin, Liu, and Manso (2021); Nguyen, Phan, and Lee (2020); Foroughi, Marcus, Nguyen, and Tehranian (2022). However, Donelson, Kettell, McInnis, and Toynbee (2022) caution against relying on the validity of using Universal Demand Law as an exogenous shock to litigation risk.

we obtain firms' violations against regulations enforced by federal and state agencies. Compared to other ES measures commonly used in literature such as third-party rating scores or media coverage, this outcome-based measure has the advantage of granularity, transparency and objectivity.

Using a sample covering 4,562 unique US public firms from 2000 to 2020, we find robust evidence that environmental litigation risk deters firms from committing environmental misconducts. The results hold for both extensive (likelihood of ES misconduct) and intensive (number and penalty amount of ES misconduct) margins and are valid in both environmental and social subgroups. To establish causality, we exploit exogenous variations of litigation risk driven by death of judges and find robust results.

Next, we investigate the role of institutional investors. We find the effect of litigation risk on corporate ES misconduct becomes stronger when a firm has higher institutional ownership or the presence of at least one blockholder. We then explore investor reactions and firm responses to ES litigation risk. We find significant negative stock price actions around days when judges nominated by democratic presidents are confirmed by the Senate. Such negative reactions are stronger for firms with worse ES profiles. These firms also reduce capital investment and make fewer mergers and acquisitions.

Finally, we conduct a battery of ancillary tests to verify the robustness of our results. First, we find our main result is robust to firm fixed effects and using a Poisson regression. Second, our main result is robust to alternative measures of corporate ES misconduct, such as the dummy indicator. Third, our main result is robust to two alternative measures of litigation risk, one based on the proportion of liberal judges, and the other based on the district court judges.

Our paper contributes to several strands of literature. First, to our knowledge, this is the first paper examining litigation risk in ES-related issues. Prior studies in finance and accounting mostly focus on either security litigation risk (Lowry and Shu (2002); Kim and Skinner (2012)) or risk from shareholder-initiated lawsuits (Crane and Koch (2018); Freund, Nguyen, and Phan (2021); Lin, Liu, and Manso (2021)), while some others look into all corporate lawsuits (Hutton, Jiang, and Kumar (2015); Arena and Julio (2021)). In this paper, we find that ES litigation risk functions as an important external corporate governance mechanism, which effectively curbs the dark side of firms' ES performance.

Second, this study adds to a rapidly expanding body of research examining stakeholder interests in corporate finance literature. Prior literature in corporate misconduct mostly concentrates on financial and accounting misconduct (Karpoff and Lou (2010); Karpoff, Koester, Lee, and Martin (2017); Parsons, Sulaeman, and Titman (2018)), while more recent studies begin to document a handful of determinants that affect firms' misconduct to their stakeholders, including board composition (Zaman, Atawnah, Baghdadi, and Liu (2021); Neukirchen, Posch, and Betzer (2022)), media monitoring (Heese, Pérez-Cavazos, and Peter (2022)) and asset liquidity (Zaman, Atawnah, Nadeem, Bahadar, and Shakri (2022)). This article provides corroborating evidence about the deterrent effect on ES misconduct from judicial perspective. This angle is new and noteworthy as courts function as the last resort when internal governance and personal negotiations fail.

Third, this paper contributes to the small but growing literature examining judges' roles in corporate finance (Iverson, Madsen, Wang, and Xu (2020); Liu (2020); Harit, Parupati, Pinto, and Sadka (2022)). The judicial system is one of the cornerstones of the US power structure and judges play extremely important roles in the modern finance industry. Huang, Hui, and Li (2019) first argue that judge ideology is a valid proxy for litigation risk and show that it captures ex ante risk in security class action litigation. In this paper, we further validate that judge ideology also works as a good proxy for ES litigation risk and has pronounced deterrent effect on corporate ES misbehavior.

Findings in this paper contribute to the literature of law and finance by demonstrating the significant effect of judicial system on managerial decisions and corporate performance.

The remainder of this paper is organized as follows. In section 2.2, we describe main data sources and sample construction. In section 2.3, we present the main empirical results, address endogeneity concerns, and validate the usage of judge ideology as a proxy for ES litigation risk. Section 2.4 investigates the role of institutional investors. Section 2.5 explores investor reaction and firms' response towards heightened ES litigation risk. Section 2.6 includes a set of robustness tests and section 2.7 concludes.

2.2 Data and Sample Selection

2.2.1 Federal Judicial Ideology

We use judge ideology at circuit courts to proxy for ES litigation risk[6]. We construct judge ideology at circuit-year-month level using judge biographical information provided by Federal Judicial Center (FJC)[7]. Established by Congress in 1967, the Federal Judicial Center is the research and education agency of the judicial branch of the U.S. government. The database contains service records for each federal judge, dating back to as early as the late 18th century. For each judge service record, it provides information on judge name, court name, appointing president with his/her party affiliation, confirmation date, termination date and termination reason[8]. Using judge service records,

[6] The rationale of using judge ideology at circuit courts is that, for the vast majority of cases, circuit courts are the final arbiters because the Supreme Court is not required to hear an appeal. For robustness, we also construct a sample using judge ideology at federal district courts. The results are basically unchanged and are provided in the robustness section.
[7] The website of federal judge biographical database: https://www.fjc.gov/history/judges.
[8] FJC classifies termination reason into 8 categories, namely Abolition of Court, Appointment to Another Judicial Position, Death, Impeachment& Conviction, Reassignment, Recess Appointment-Not Confirmed, Resignation and Retirement. As the U.S. Constitution gives federal judges life tenure, most terminations come with judge death. In our sample of judge service from 2000 to 2021, there are 783 cases of judge termination and 519 (66.3%) come from judge death. This unique institutional feature strengthens exogeneity of judge turnover on which the main independent variable is based, and thus guarantees exogenous variations of the variable.

we construct a panel dataset at circuit court-year-month level with information about all judges sitting in the courtroom at that time.

Inspired by prior studies in law and finance (Pinello, 1999; Chemerinsky, 2002; Huang, Hui, and Li, 2019; Liu, 2020), we measure judge ideology using the party affiliation of his/her appointing president[9]. As each case in a circuit court will be assigned to a panel of three randomly selected judges from the circuit, we follow Huang, Hui, and Li (2019) to measure the ex-ante probability that a lawsuit case will be handled by a panel with at least two judges appointed by democratic presidents. Specifically, the probability, denoted as *Liberal_prob*, can be calculated as:

$$Liberal\_Prob = \frac{\binom{x}{3} + \binom{x}{2} * \binom{y-x}{1}}{\binom{y}{3}}$$

where *x, y* indicate the number of democratic appointees and the total number of incumbent judges in the circuit, respectively. The binomial coefficient, $\binom{n}{k}$, is defined as:

$$\binom{n}{k} = \frac{n!}{k!\,(n-k)!}$$

Intuitively, a greater value of *Liberal_prob* implies a more liberal court. In robustness checks, we use an alternative measure, *Liberal_ratio*, as the main independent variable, defined as the number of democratic appointees divided by the total number of judges. The results are basically unchanged.

2.2.2 Corporate ES Misconduct

To measure corporate ES misconducts, we exploit a novel and comprehensive database named Violation Tracker Database, created by the non-profit organization Good Jobs First. In the updated

[9] It is extremely rare that one judge was appointed twice or more by presidents from different parties.

version of 2022, the database covers facility-level violations against federal and state laws enforced by over 300 agencies since the year 2000 and has been widely used to measure corporate misconduct in recent literature (Soltes (2019); Heese, Pérez-Cavazos, and Peter (2022); Zaman, Atawnah, Baghdadi, and Liu (2021); Heese and Pérez-Cavazos (2020); Raghunandan and Rajgopal (2021)). It tracks violations with penalty amounts over $5,000 and provides detailed information on company name, company location, agency name, offense type, penalty date and penalty amount. Violation Tracker classifies violations into nine categories based on offense type, namely competition, consumer protection, employment, environment, financial, government contracting, healthcare, safety, and miscellaneous.

As we specifically focus on ES violations, we follow prior studies (Raghunandan and Rajgopal, 2022; Raghunandan and Rajgopal, 2021) to exclude non-ES violations using offense classifications provided by Violation Tracker. We then aggregate the total number and penalty amounts of violations onto company-year level. Our main measure of corporate misconduct is *ES_Number*, the natural logarithm of one plus the number of ES violations.

2.2.3 Main Sample

We aggregate ES misconducts at firm-year level and then merge with Compustat/CRSP Merged Database. Next, we keep companies listed on NYSE, AMEX and Nasdaq and exclude firms if the headquarters of the companies are not located in the US, as well as companies in financial sectors (SIC 6000-6999), utility sectors (SIC 4900-4949). Next, we supplement the dataset with information on judge ideology at the circuit court where the company is headquartered. Finally, we obtain a sample with 4,562 unique public firms and 42,167 firm-year observations in the period of 2000 to 2020. The sample starts from 2000 because this is the first year Violation Tracker Database tracks corporate misconduct.

Inspired by prior studies on corporate misconduct (Johnson, Ryan, and Tian, 2009; Zaman, Atawnah, Bagh- dadi, and Liu, 2021; Zaman, Atawnah, Nadeem, Bahadar, and Shakri, 2022). we include the following control variables: size, return on asset (ROA), Tobin's Q, market-to-book ratio (MTB), capital expenditure (measured as capital expenditures divided by total assets), leverage, tangibility, firm age, retained earnings, cash holdings and stock volatility. We also control for local macroeconomic variables to alleviate the confounding effect of local political leaning and economic growth (Huang, Hui, and Li, 2019), including state-level GDP per capita, GDP growth rate and the fraction of voters who support democratic party in presidential elections. Detailed variable definitions can be found in Appendix 2.A.

Table 2.1 reports summary statistics of all variables used in the main regressions. All continuous variables are winsorized at the 1st and 99th percentiles to alleviate the effects of outliers. We observe that ES misconduct occurs in about 15% of all firm-year pairs. The average number of ES misconducts equals 0.38 in a given year and the average penalty amount is about $45,016.[10]

Figure 2.2 shows the trend of *Liberal_Prob* from 2000 to 2020. There are large differences in terms of judge ideology across circuit courts. The Eighth Circuit, for example, has a 20- year average of *Liberal_Prob* of 0.12 whereas the same number for the Ninth Circuit equals 0.63. Consistent with our expectation, *Liberal_Prob* generally falls when republican presidents are in the white house (2000-2008; 2016-2020) and rises vice versa. In Table 2.1, for all firm-year pairs, the average of *Liberal_Prob* and *Liberal_Ratio* equal 0.39 and 0.43, respectively.

2.3 Main Results

[10] For firm-year observations that have ES misconduct, the average penalty amount is slightly greater than $300,000.

2.3.1 Baseline Results

In the baseline regression, we estimate the effect of judge ideology on corporate misconduct using the following OLS model:

$$ES\_Number_{i,t+1} = \alpha + \beta Liberal\_Prob_{i,s,t} + \gamma X_{i,t} + \delta Y_{s,t} + Fixed\ Effects + \varepsilon_{i,t} \qquad (1)$$

where $ES\_Number_{i,t+1}$ measures corporate misconduct within one year after firm i's fiscal year-end. $Liberal\_Prob_{i,s,t}$ captures judge ideology in the circuit where firm i is headquartered in year t.[11] $X_{i,t}$ includes firm- level control variables, including size, ROA, Tobin's q, market to book, capital expenditure, leverage, tangibility, firm age, retained earnings, cash and stock volatility. $Y_{s,t}$ includes state-level control variables, namely logarithm of GDP, GDP growth rate and political leaning. In addition to control variables, we include a set of fixed effects to control for omitted or unobservable variables at levels. Following prior studies, all standard errors are clustered at firm level.

Table 2.2 presents the baseline results. We sequentially add control variables and fixed effects to ensure that the results survive different specifications. Following existing literature in corporate misconduct (Haß, Müller, and Vergauwe (2015); Zaman, Atawnah, Baghdadi, and Liu (2021)), in column (2) we control for industry and year fixed effects.[12] In column (3), we control for industry-by-year fixed effects; in column (4), and industry-by-year and state fixed effects, to alleviate the concern of time-varying industry level and time-invariant state level omitted variables. As jurisdictions of circuit courts include multiple states (See Figure 2.1 for details), adding state fixed effect mechanically absorbs all time-invariant state-level characteristics that might affect the judge ideology in the circuit.

[11] As we match monthly judge ideology with firms' fiscal year-end date in Compustat-CRSP Merged Database, *Liberal_prob* indicates judge ideology in the firm's fiscal month end in year t. The results are basically unchanged when we use a year-average measure of judge ideology, as judge turnover within a year is relatively infrequent.

[12] We group firms into 48 industries based on Fama-French 48 Industry Portfolios. See https://mba.tuck.dartmouth.edu/pages/faculty/ken.french/Data_Library/det_48_ind_port.html.

Across all columns, the coefficients of the main independent variable, *Liberal_Prob*, are largely stable and significantly negative across specifications. In column (4), when controlling for state fixed effect and industry-by-year fixed effect, the coefficient is -0.092 and is significant at 5%. Importantly, the magnitude is also economically significant: a one standard deviation increase in *Liberal_Prob* will lead to a 1.84% decrease in *ES_Number*, equivalent to 5.8% of the mean.

Taken together, these results show that firms react to a more liberal litigation environment by decreasing ES misconduct. As ES litigation risk gets intensified when circuit courts consist of more liberal judges, these findings support our hypothesis that firms reduce ES misconduct when faced with heightened litigation risk.

2.3.2 Subsample Analysis on Environmental and Social Violations

In this section, we disaggregate ES violations into environmental and social violations respectively to show our results hold for both these two types of violations. We classify social violations as violations related to consumer protection, competition and workplace safety violations.

We re-run the regressions in Equation (1) and the results are reported in Table 2.3. Both columns control for industry-by-year fixed effects and state fixed effects. We find the coefficients on *Liberal_Prob* are negative and significant across both columns, suggesting that both environmental and social misconducts are reduced when the court becomes more liberal-leaning. This finding is consistent based on the argument that liberal judges are more likely to support plaintiffs in both environment- and stakeholder-related lawsuits, compared to conservative counterparts, which we will verify in section 2.3.3.

2.3.3 IV Estimation

As mentioned earlier, measuring ex-ante litigation risk using judge ideology alleviates the confounding effect of any industry or firm level omitted variables because the measure is simply based on judge individual party affiliation and court composition. Furthermore, as federal judges are nominated by presidents and enjoy life tenure, turnovers are rare and less likely to correlate with factors that affect managerial decisions. However, one can still argue that some unobservable variables, say firm ideology, may correlate with both circuit's judge ideology and ES misconduct behaviors. Another concern might be that the number of corporate misconducts might be an equilibrium outcome determined by both firm misbehavior and agency enforcement. If agency enforcement actions are associated with circuit's judge ideology, our main results might be biased because of such omitted variables.

Given these concerns, we try to alleviate the potential endogeneity problem using an instrumental invariable (IV) by exploiting a unique feature of federal judge turnover. Specifically, our IV is a dummy variable at circuit-year level which equals one if at least one of the conservative judges passed away, and zero otherwise.

To establish the validity of the IV, one needs to verify both relevance and exclusion conditions. The relevance condition is satisfied mechanically, as death of conservative judges will lead to a higher ratio of liberal judges. Meanwhile, exclusion condition is likely to be met as deaths are arguably exogenous shocks. Compared to other termination reasons such as retirement and resignation, termination because of death is least likely to be anticipated and related with socio-economic factors (which could potentially affect agency enforcement).

Table 2.4 reports the results of the IV estimation. Column (1) reports the first-stage regression, where we regress the judge ideology *Liberal_Prob* on our instrumental variable *Conservative_Death*. We find that the coefficient on *Conservative_Death* is significant at the 1% level, suggesting that the relevance condition is satisfied. Column (2) reports the second-stage regression, where we regress our main dependent variable in Table 2.2, *ES_Number*, on the instrumented *Liberal_Prob*. We find that the coefficient on the (instrumented) *Liberal_Prob* is significantly negative at the 1% level, corroborating our previous findings and shows a clear and negative casual effect of court's liberal ideology and corporate ES misconduct.

2.3.4 Validation of the Measure

In this section, we provide evidence to support the validity of our measure of litigation risk, using data on ES lawsuits filed at federal courts. Intuitively, if liberal judges make harsher decisions in environmental and social lawsuits than conservative judges because of their stronger pro-environmental beliefs, firms will anticipate a higher litigation cost in the courtroom and reduce ES misconduct accordingly when the proportion of liberal judges in the local circuit court increases. Specifically, we conjecture that judge ideology is associated with higher number of ES lawsuits filed and higher likelihood of defendant loss in a lawsuit.

To empirically test our conjecture, we exploit a comprehensive legal database, Integrated Data Base (IDB) of the Federal Judicial Center. The IDB contains data on civil cases and criminal defendant filings and terminations in the federal district courts, along with bankruptcy court and appellate court case information.[13] We retrieve IDB data on civil cases filed in the US federal districts from 2000 to 2021 and only include environmental and social lawsuits.[14] We then end up with approximately 1.5 million ES cases in the US federal courts throughout the period.

We further aggregate the number of ES lawsuits filed on a circuit court-year basis and use the below regression model to conduct our analyses:

[13] The FJC receives regular updates of the case-related data that are routinely reported by the courts to the Administrative Office of the U.S. Courts (AOUSC). The FJC then post-processes the data, consistent with the policies of the Judicial Conference of the United States governing access to these data, into a unified longitudinal database, the IDB. For more information on IDB database, see https://www.fjc.gov/research/idb.

[14] The IDB database provides a case code describing the nature of the suit, which we rely on to identify ES lawsuits.

$$ES\_Lawsuits_{c,t+1} = \alpha + \beta Liberal\_Prob_{c,t} + \gamma X_{c,t} + \delta Y_{c,t} + Fixed\ Effects + \varepsilon_{c,t} \quad (2)$$

Column (1) of Table 2.5 reports the results. We find that the coefficient on *Liberal_Prob* is positive and significant, suggesting that a more liberal court is associated with greater volume of environmental and social lawsuits submitted, as defendants believe that these lawsuits will win greater support from judges. Economically, it shows that one standard deviation increase in *Liberal_Prob* leads to roughly 28.2% increase in the number of ES lawsuits, after accounting for court and year-month fixed effects.

We then examine the ex-post outcomes of ES lawsuits to see if they are indeed affected by judge ideology. In a recent article, Liu (2020) finds that lawsuits with Republican-appointed judges are approximately 12% less likely to succeed in reaching a settlement compared with those adjudicated by Democratic-appointed judges. Following Liu (2020), we use whether an outcome of settlement is reached in a lawsuit or not, to proxy for plaintiff's success. Specifically, we calculate the percentage of ES lawsuits that reach settlement out of the total number of ES lawsuits in each circuit court, and then estimate the following model:

$$Settlement_{c,t+1} = \alpha + \beta Liberal\_Prob_{c,t} + \gamma X_{c,t} + \delta Y_{c,t} + Fixed\ Effects + \varepsilon_{c,t}, \quad (3)$$

Column (2) of Table 2.5 reports the results. As we can see, after accounting for court and year fixed effects, plaintiffs are more likely to reach settlements in courts with more liberal judges. The coefficient on *Liberal_Prob* is significant under 1% level, suggesting that one standard deviation increase in *Liberal_Prob* leads to roughly 6.6% increase in the likelihood of settlement. This is consistent with what Liu (2020) finds and further lends support to the validity of using judge ideology to proxy for ES litigation risk.

2.4 The Role of Institutional Ownership

As ESG investing becomes more and more popular, there is plenty of anecdotal evidence that institutional investors push managers to pursue ESG goals. Recent literature finds that institutional investors may exert pressure on corporate ESG performance through engagement, activism and proxy voting (Hoepner, Oikonomou, Sautner, Starks, and Zhou (2018); DesJardine and Durand (2020); Dikolli, Frank, Guo, and Lynch (2022)). To the extent that ES litigation risk reduces corporate ES misconducts, we should expect the main result to be stronger when the institutional ownership is higher.

To test it empirically, we collect institutional holding data from Refinitiv's Institutional (13f) Holdings Database. According to Refinitiv, the primary source for the institutional holdings data is the 13F form that investment companies and professional money managers are required to file with the SEC on a quarterly basis. We calculate firms' institutional ownership immediately before each fiscal year-end and merge with our main sample. Based on Equation (1), we include the total institutional ownership (IO), and the interaction term, *IO ∗ Liberal_Prob*. We also include *Blockholder*, a dummy variable that equals one when there is at least one blockholder who takes at least 5% ownership of the firm, and zero otherwise, as well as the interaction term *Blockholder ∗ Liberal_Prob*.

Table 2.6 reports the results. We find that the coefficient on *IO ∗ Liberal_Prob* and the coefficient on *Blockholder ∗ Liberal_Prob* are both negative and significant, suggesting that the effect of judge ideology on the number of corporate misconducts is stronger when there is greater presence of institutional investors, who exert pressure on managers in face of heightened litigation risk.

2.5 Investor Reaction and Corporate Investment to ES Litigation Risk

So far, our results confirm that firms improve their ES performance and increase stakeholders' welfare by refraining from ES misconduct when circuit courts become more liberal. However, there ain't no such thing as a free lunch. A natural follow-up question is who pays when stakeholders gain. Corporate finance literature documents widespread evidence of conflict of interests between shareholders and other stakeholders (Barnea and Rubin (2010); Vinten (2001); Raghunandan and Rajgopal (2021)). In this section, we first explore whether shareholders are negatively affected when stakeholders become better off, and then investigate how firms respond to an increase in ES litigation risk.

2.5.1 Investor Reaction

Following Huang, Hui, and Li (2019), we first focus on stock price reactions to the official appointments of liberal judges. According to US law, a federal judge nominee must be officially confirmed by the Senate before he/she comes to power in the courtroom. Therefore, we study stock price reactions of firms around confirmation dates of circuit judges. Specifically, we calculate stock price reactions for confirmations of democratic/republican nominees, respectively. We employ market-adjusted models to estimate cumulative abnormal returns (CAR) within [-1,1] window and test the difference of CAR between firms with high/low ES ratings around the confirmation of liberal/conservative federal judges.

Table 2.7 reports the results. In Column (1), we find that firms experience significantly negative abnormal returns (from 0.2% to 0.43%) when the Senate confirms the nominations of liberal judges.[15] Consistent with heightened litigation risk posed by liberal judges, this finding reveals that investors

[15] In an untabulated table, we find that the three-day CAR is close to zero around confirmation of conservative judges.

react more negatively towards appointment of liberal judges, echoing the findings in Huang, Hui, and Li (2019).

An alternative explanation of the negative market reaction could be that a more liberal court is associated with greater overall litigation risk, including other types of litigation risks, such as those related to securities class action lawsuits. To exclude such alternative explanation, we obtain firms' ESG ratings from Refinitiv ESG Database and partition the main sample into two subsamples consisting of firms with high and low ES scores, respectively. We conjecture that if other types of litigation risks mainly drive the result, there will be no significant difference between the results for these two subsamples. However, if ES litigation risk mainly drives the result, then the result should be stronger when the firm's ES rating is lower, as these firms will be subject to higher ES litigation risk. Columns (2) and (3) in Table 2.7 report the results for the high and low ESG subsamples respectively. Indeed, we find that the market reaction is stronger in the low ES score subsample, and the difference remains significant. This finding implies that investors mainly react to the ES litigation risk, rather than other types of litigation risks.

2.5.2 Corporate Investment

Next, we explore firms' investment decisions in response to heightened ES litigation risk. As to the effect on capital investment, Arena and Julio (2015) find that firms cut capital expenditures in anticipation of higher litigation risk. Therefore, we expect firms to decrease capital investment. To exclude the alternative explanation that other types of litigation risk might drive the result, we also partition the sample into two subsamples based on high and low ES scores and run regressions on these two subsamples respectively.

Panel A of Table 2.8 reports the results on capital expenditure. Column (1) reports the result for the full sample, and columns (2) and (3) report the results for the high and low ES subsamples respectively. We find that the coefficients on *Liberal_Prob* are all negative across the three columns. However, it is not significant in column (1) and (2), but significantly negative in column (3). These results suggest that litigation risk does not significantly affect the overall capital investment, but significantly reduces the capital investment for the low ES subsample, consistent with the idea that the reduction of capital investment is mainly driven by ES litigation risk.

We then examine a specific and important type of corporate investment, i.e. merger & acquisitions (M&A). We collect data on M&As of US public companies from Refinitiv's SDC M&A Database[16] and create two variables to measure M&A activity: whether a firm has any M&A transactions in a given year (*MA_Dummy*) and the number of M&A transactions a firm has (*Number of M&A*). In the end, for 42,167 firm-year observations, 29.6% have at least one M&A. For those firm-years with at least one M&A, the average number of M&A equals 1.78.

Panel B of Table 2.8 reports the results on M&A activity. Columns (1) to (3) report results for *M&A Dummy* and columns (4) to (6) report results for *Number of M&A*. Columns (1) and (4) report results for the full sample, columns (2) and (5) report results for the high ES subsamples, and columns (3) and (6) report results for the low ES subsamples. We find that the coefficients on *Liberal_Prob* are significantly negative for columns (1), (3), (4) and (6), but insignificant for column (2) and (5). These results suggest that litigation risk is associated with lower chance of conducting M&A and lower number of acquisitions, and such reduction is mostly driven by firms in the low ES subsample, consistent with the idea that ES litigation risk, rather than other types of litigation risks, drives the deterrence effect on corporate investment.

[16] Following prior works on M&A, for a transaction to be included in our sample, it must satisfy following requirements: 1. Acquirers must be US public companies. 2. The status of transaction must be "completed". 3. Transaction form must be one of the followings: "Acquisition of major interest", "Merger", "Acquisition of assets". 4. Acquirer own less than 50% of target's shares before the transaction and acquire more than 50% of target's shares in the transaction.

Taken together, we find that firms respond to increased litigation risk by reducing capital investment and acquisitions. These reductions are more pronounced for firms with low ES profiles, which are subjective to greater ES-specific litigation risk.

2.6 Robustness Checks

In this section, we report four sets of robustness checks. First, we show that our main results are robust to alternative regression specifications, such as firm fixed effects and Poisson regression. Second, we show our results are robust to alternative measures of corporate misconducts. Finally, we use alternative measures for judge ideology and obtain robust results.

2.6.1 Alternative Regression Specifications

Recent studies have raised concerns about using log dependent variables. To alleviate such concern, we use Poisson regression to replicate our main results. Moreover, one may argue that some firm-level confounding issues may contaminate our main result. To further address such concern, we include firm and year fixed effects to control for all time-invariant firm characteristics.

Panel A of Table 2.9 reports the results. Column (1) reports the results for the Poisson regression and column (2) reports the results for firm fixed effects. We find that the coefficients on *Liberal_Prob* are significantly negative, suggesting that our main result is robust using the Poisson regression and using firm and year fixed effects.

2.6.2 Alternative Measures of Corporate Misconduct

In our main result, we use the number of corporate misconducts as the proxy for corporate ES performance. In robustness checks, we construct two alternative measures of corporate ES misconduct: *ES_Dummy*, a dummy variable that equals one when a firm commits at least one ES violation and zero otherwise; *ES_Penalty*, the natural logarithm of one plus total amount of penalty on ES violations that a firm commits. Panel B of Table 2.9 reports the results. We find the coefficients on *Liberal_Prob* are significantly negative at 5% level, suggesting that our main result is robust to alternative measures of corporate misconduct.

2.6.3 Alternative Measures of Judge Ideology

In previous analyses, we follow Huang, Hui, and Li (2019) and measure judge ideology using the probability of a liberal-dominated panel (consisting of three judges). However, it is likely that firm managers are not sophisticated enough to correctly interpret circuit judge ideology in the way defined above. Therefore, we use a more straightforward measure, *Liberal_Ratio*, calculated as the ratio of liberal judges within a court, to proxy for litigation risk alternatively, as firm managers may naively perceive ex-ante litigation risk by simply counting on the ratio of liberal judges.

Second, we carry out all previous tests at circuit courts mostly because rulings of district courts can be reversed by circuit courts, but the case will be barely likely for circuit courts as the Supreme Court rarely hears review request (Barnes Bowie and Songer (2009); Huang, Hui, and Li (2019)). Nevertheless, as lawsuits are first handled by one randomly assigned judge at district courts, it is plausible to expect that if our hypothesis is true, the same story should also hold at district court level. For robustness, we repeat the analysis but focus on district courts.

To start with, we construct a sample containing district judges' appointment information from FJC Judge Biographical Directory. We then calculate the proportion of democratic judges in a certain district court to proxy for litigation risk. However, as there are multiple district courts in some states (See Figure 2.1 for details), we match firms with district courts using county information of firms' headquarters. We then re-run the baseline regressions using judge ideology at district courts.

Panel C of Table 2.9 reports the results. Column (1) reports the results using *Liberal_Ratio*. The coefficient on *Liberal_Ratio* is significantly negative. Column (2) reports the results using *Liberal_Prob_District*, the judge ideology measured at the district court level. Again, we find that the coefficient on *Liberal_Prob_District* is significantly negative. These results suggest that our main result is robust to alternative measures of judge ideology.

2.7 Conclusion

When sustainable investing goes mainstream in the United States and the globe, understanding what deters firms from committing ES misconduct becomes more important now than ever. Speaking to what curbs corporate ES misconduct, this paper identifies and verifies an important deterrent factor, i.e. ES litigation risk. Motivated by findings from legal scholars that judge political ideology affects lawsuit outcomes, we apply judge ideology in circuit courts as a proxy for ES litigation risk.

We find robust evidence that the increase in litigation risk reduces ES misconduct. To address endogeneity concerns, we exploit exogenous shocks to litigation risk caused by judge death and find corroborating causal evidence. Importantly, an empirical test on ES lawsuit outcomes confirms the validity of judge ideology as a proxy for ES litigation risk.

We find that institutional investors seem to play an important role, as the main effect is stronger for firms with higher institutional ownership and at least one blockholder. Event studies around judge confirmation dates reveal that investors react much more negatively towards appointments of liberal judges. More importantly, such an effect is stronger for firms with worse ES profiles. These firms also reduce capital investment and conduct fewer acquisitions. Taken together, these results are consistent with the idea that firms that have been performing poorly in ES tend to make less investment in anticipation of the increasing liabilities arising from heightened ES litigation risk.

Our paper provides important implications to law enforcement agencies and the judicial system in the US. It also alerts ESG investors and asset managers of the potentially significant influence from the courtroom.

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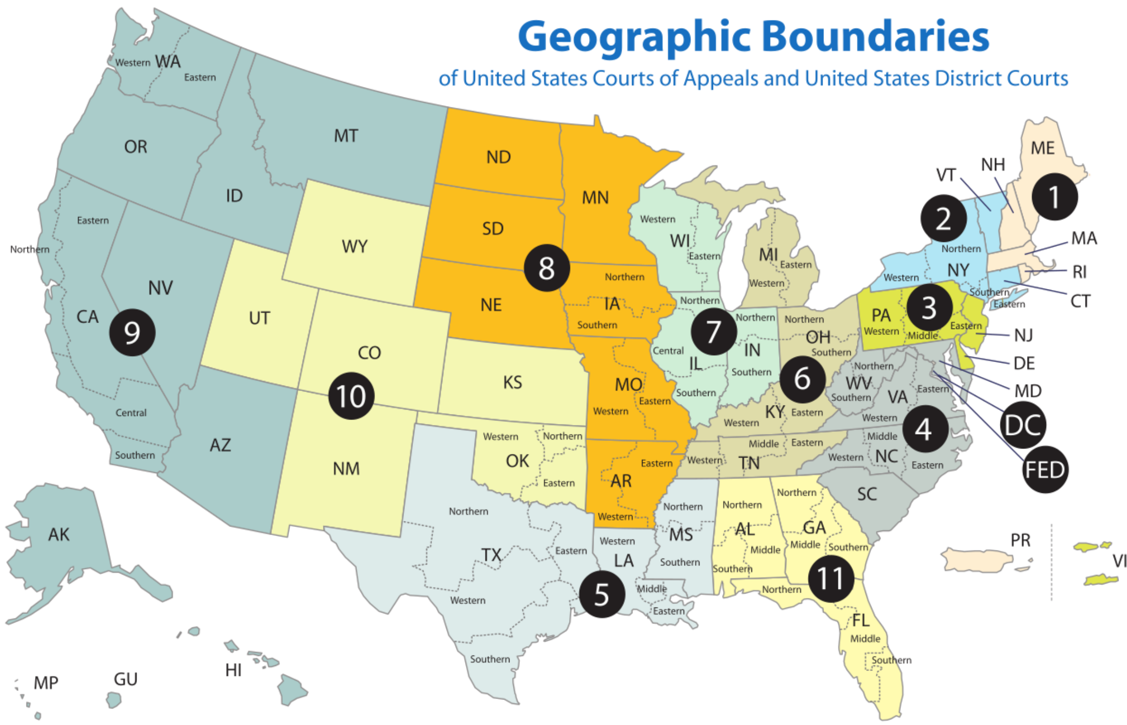
Figure 2.1: US Federal District and Circuit Court Boundaries



Note: Map of the geographic boundaries of the various United States courts of appeals (numbered and colored) and United States district courts (marked by state boundaries or dotted lines).

Source: https://en.wikipedia.org/wiki/United_States_courts_of_appeals

Figure 2.2: Ideology Trends of US Circuit Courts

Note: This figure shows the dynamic trend of our main variable of judge ideology, *Liberal_prob*, in each of the twelve circuit courts. The number on top of each graph indicates the number of circuit court and number "0" corresponds to D.C. circuit.

Table 2.1. Summary Statistics

This table reports summary statistics of the main variables in the baseline regression.

Panel A: ES Misconduct	Mean	Standard Deviation	Minimum	Median	Maximum	N
ES_Number	0.17	0.45	0	0	2.2	42617
ES_Number (non-log)	0.38	1.22	0	0	8	42617
ES_Penalty	1.64	3.98	0	0	14.8	42617
ES_Penalty(non-log)	45,016	264,428	0	0	2,250,000	42617
ES_Dummy	0.15	0.36	0	0	1	42617

Panel B: Judge Ideology	Mean	Standard Deviation	Minimum	Median	Maximum	N
Liberal_Prob	0.39	0.20	0	0.38	0.72	42617
Liberal_Ratio	0.43	0.14	0.056	0.43	0.65	42617

Panel C: Firm Characteristics	Mean	Standard Deviation	Minimum	Median	Maximum	N
Size (Log Asset)	6.30	2.00	2.04	6.24	11.1	42617
ROA	-0.035	0.25	-1.51	0.035	0.3	42617
Q	2.15	1.61	0.62	1.63	11.0	42617
MTB	3.82	5.21	0.33	2.32	38.9	42617
CAPEX	0.044	0.050	0	0.028	0.30	42617
Cash	0.16	0.18	0	0.1	0.92	42617
Leverage	0.19	0.19	0	0.16	0.94	42617
Tangibility	0.23	0.22	0.001	0.15	0.90	42617
Retain	-0.51	2.16	-14.6	0.12	1.02	42617
Stk_Volatility	0.47	0.30	0.12	0.39	1.94	42617
Firmage	19.6	15.3	1	16	71	42617

Panel D: State Characteristics	Mean	Standard Deviation	Minimum	Median	Maximum	N
GDP	10.9	0.15	10.5	10.9	11.2	42617
GDP Growth	0.018	0.023	-0.051	0.020	0.078	42617
Democratic	0.52	0.081	0.25	0.53	0.92	42617

Table 2.2: External Litigation Risk and Corporate ES Misconduct

This table reports results that examine the relation between external litigation risk and corporate ES misconduct. Our sample includes 42,167 firm-year observations in the period from 2001 to 2020. The dependent variable in all columns is *ES_Number*, calculated as the natural logarithm of one plus total number of violations on ES-related issues at firm level. The main independent variable is *Liberal_Prob*, calculated as the probability that at least two judges lean toward the Democratic Party in a random selection of a panel of three judges in a circuit court. The t-statistics are reported in parentheses and standard errors are clustered at firm level. ***, **, * correspond to statistical significance at the 1%, 5%, and 10% levels, respectively. Variable definitions are reported in Appendix 2.A.

	(1)	(2)	(3)	(4)
Liberal_Prob	-0.097***	-0.067**	-0.059**	-0.092**
	(-3.44)	(-2.30)	(-2.01)	(-2.50)
Size	0.090***	0.090***	0.090***	0.091***
	(18.07)	(18.75)	(18.47)	(18.65)
ROA	-0.070***	-0.075***	-0.076***	-0.075***
	(-6.04)	(-6.94)	(-6.82)	(-6.87)
Q	-0.003	-0.001	-0.001	-0.001
	(-1.04)	(-0.44)	(-0.25)	(-0.36)
MTB	0.002	0.002**	0.002**	0.002**
	(1.58)	(2.10)	(2.08)	(2.22)
CAPEX	-0.385***	-0.290**	-0.336**	-0.316**
	(-3.43)	(-2.30)	(-2.56)	(-2.43)
Cash	-0.003	0.032*	0.031*	0.033*
	(-0.17)	(1.91)	(1.83)	(1.93)
Leverage	-0.132***	-0.149***	-0.145***	-0.148***
	(-4.18)	(-4.88)	(-4.66)	(-4.69)
Tangibility	0.266***	0.176***	0.181***	0.170***
	(6.86)	(4.20)	(4.20)	(4.00)
Retain	-0.014***	-0.014***	-0.015***	-0.015***
	(-6.99)	(-7.68)	(-7.80)	(-7.68)
Stk_volatility	-0.000	0.021**	0.014	0.018*
	(-0.00)	(2.13)	(1.38)	(1.81)
Firmage	0.005***	0.004***	0.004***	0.004***
	(8.40)	(6.99)	(6.97)	(6.51)
GDP	-0.077*	-0.043	-0.042	-0.059
	(-1.74)	(-0.80)	(-0.78)	(-0.87)
GDP_Growth	0.023	-0.391**	-0.374**	0.148
	(0.21)	(-2.29)	(-2.11)	(1.26)
Democratic	-0.185**	-0.115	-0.110	-0.298**
	(-2.16)	(-1.18)	(-1.11)	(-2.57)
Constant	0.448	0.050	0.043	0.332
	(0.99)	(0.09)	(0.08)	(0.45)
Industry FE		Yes		
Year FE		Yes		
Industry × Year FE			Yes	Yes
State FE				Yes
Adjusted R^2	0.234	0.274	0.276	0.287
N	42167	42167	42167	42167

Table 2.3: Subsample Analysis on "E" and "S"

This table reports results that examine the relation between external litigation risk and corporate environmental and social misconduct, respectively. In columns (1), the dependent variable is *E_Number*, calculated as the natural logarithm of one plus total number of violations on environmental issues at firm level. In columns (2), the dependent variable is *S_Number*, calculated as the natural logarithm of one plus total number of violations on social issues at firm level. The main independent variable is Liberal_Prob, calculated as the probability that at least two judges lean toward the Democratic Party in a random selection of a panel of three judges in a circuit court. The t-statistics are reported in parentheses and standard errors are clustered at firm level. ***, **, * correspond to statistical significance at the 1%, 5%, and 10% levels, respectively. Variable definitions are reported in Appendix 2.A.

	E_Number	S_Number
	(1)	(4)
Liberal_Prob	-0.030**	-0.081***
	(-2.15)	(-2.59)
Size	0.036***	0.065***
	(14.05)	(16.56)
ROA	-0.029***	-0.055***
	(-5.33)	(-6.41)
Q	-0.000	-0.001
	(-0.15)	(-0.35)
MTB	0.001	0.002**
	(1.43)	(2.27)
CAPEX	-0.282***	-0.088
	(-3.33)	(-1.03)
Cash	0.033***	0.011
	(4.22)	(0.75)
Leverage	-0.062***	-0.111***
	(-3.90)	(-4.30)
Tangibility	0.133***	0.062*
	(5.45)	(1.80)
Retain	-0.007***	-0.010***
	(-8.21)	(-6.78)
Stk_volatility	0.007	0.016**
	(1.32)	(1.98)
Firmage	0.002***	0.003***
	(5.59)	(5.95)
GDP	-0.008	-0.051
	(-0.31)	(-0.92)
GDP_Growth	-0.249***	0.197*
	(-2.60)	(1.90)
Democratic	-0.015	-0.214**
	(-0.31)	(-2.16)
Constant	-0.111	0.344
	(-0.41)	(0.57)
Industry × Year FE	Yes	Yes
State FE	Yes	Yes
Adjusted R^2	0.191	0.233
N	42167	42167

Table 2.4: IV Estimation

This table shows the instrumental variable estimations for our baseline regression in Table 2.2. In the first-stage regressions, we regress *Liberal_Prob* on the instrument variable *Conservative_Death*, using court-year level regressions. *Liberal_Prob* is calculated as the probability that at least two judges lean toward the Democratic Party in a random selection of a panel of three judges in a circuit court. In the second-stage regressions, we regress the dependent variable *ES_Number* on the instrumented variable *Liberal_Prob*. *ES_Number* is calculated as the natural logarithm of one plus total number of violations on ES-related issues at firm level. The t-statistics are reported in parentheses and standard errors are clustered at firm level. ***, **, * correspond to statistical significance at the 1%, 5%, and 10% levels, respectively. Variable definitions are reported in Appendix 2.A.

	First Stage	Second Stage
	(1)	(2)
Liberal_Prob		-0.370***
		(-2.94)
Conservative_Death	0.033***	
	(35.14)	
Size	0.001***	0.091***
	(2.90)	(18.68)
ROA	-0.001	-0.076***
	(-0.62)	(-6.88)
Q	-0.001*	-0.001
	(-1.88)	(-0.45)
MTB	0.000	0.002**
	(0.67)	(2.24)
CAPEX	-0.003	-0.317**
	(-0.31)	(-2.44)
Cash	0.007**	0.035**
	(2.28)	(2.06)
Leverage	-0.003	-0.149***
	(-0.89)	(-4.71)
Tangibility	-0.002	0.170***
	(-0.52)	(3.99)
Retain	-0.000	-0.015***
	(-0.37)	(-7.67)
Stk_volatility	0.000	0.018*
	(0.30)	(1.82)
Firmage	-0.000**	0.004***
	(-2.35)	(6.48)
GDP	-0.161***	-0.109
	(-8.82)	(-1.52)
GDP_Growth	0.023	0.131
	(0.99)	(1.12)
Democratic	0.686***	-0.106
	(18.88)	(-0.75)
Industry × Year FE	Yes	Yes
State FE	Yes	Yes
Adjusted R^2	0.915	0.152
N	42167	42167

Table 2.5: Validation Tests

This table validates our proxy for external litigation risk. In column (1), the dependent variable is *ES Lawsuits*, the natural logarithm of one plus total number of ES-related lawsuits filed at circuit court-year level, based on the data from the Federal Judicial Center. In column (2), the dependent variable, *Settlement*, indicates the percentage of ES lawsuits where plaintiffs and defendants in the ES lawsuit reach a settlement before the trial out of the total number of ES lawsuits in each circuit court. The main independent variable is *Liberal_Prob*, calculated as the probability that at least two judges lean toward the Democratic Party in a random selection of a panel of three judges in a circuit court. The t-statistics are reported in parentheses and standard errors are clustered at the district court level. ***, **, * correspond to statistical significance at the 1%, 5%, and 10% levels, respectively. Variable definitions are reported in Appendix A.

	ES Lawsuits	Settlement
	(1)	(2)
Liberal_Prob	1.408**	0.330***
	(2.36)	(2.74)
Firm Controls	Yes	Yes
State Controls	Yes	Yes
Court FE	Yes	Yes
Year FE	Yes	Yes
Adjusted R^2	0.771	0.510
N	240	240

Table 2.6: Institutional Ownership

This table reports the results for the role of institutional investors. The dependent variable is *ES_Number*, calculated as the natural logarithm of one plus total number of violations on ES-related issues at firm level. For column (1), the main independent variable is the interaction term between *Liberal_Prob*, and *IO*, the total institutional ownership of the company. For column (2), the main independent variable is the interaction term between *Liberal_Prob*, and *Blockholder*, a dummy variable that equals one if a firm has at least one blockholder, who has ownership of more than 5% of the firm, and zero otherwise. The t-statistics are reported in parentheses and standard errors are clustered at the district court level. ***, **, * correspond to statistical significance at the 1%, 5%, and 10% levels, respectively. Variable definitions are reported in Appendix 2.A.

	(1)	(2)
Liberal_Prob	0.016	0.044
	(0.35)	(0.88)
IO	0.098***	
	(3.32)	
Liber_Prob ×IO	-0.202***	
	(-3.57)	
Blockholder		0.127***
		(5.49)
Liber_Prob ×Blockholder		-0.165**
		(-3.84)
Firm Controls	Yes	Yes
State Controls	Yes	Yes
Industry × Year FE	Yes	Yes
State FE	Yes	Yes
R^2	0.278	0.291
N	42167	42167

Table 2.7: Market Reaction

This table reports the results for market reactions to the appointment of liberal judges. We report the mean cumulative abnormal returns in the [-1, +1] window around the appointment dates of liberal judges, using the market-adjusted. Column (1) reports the result for the full sample; column (2) and (3) report the results for two subsamples based on high and low ES scores, respectively. ES scores are obtained from Refinitiv. High and low ES scores are defined based on the median of the ES score retrieved from Refinitiv Database. The t-statistics are reported in parentheses and standard errors are clustered at the district court level. ***, **, * correspond to statistical significance at the 1%, 5%, and 10% levels, respectively. Variable definitions are reported in Appendix 2.A.

	Market-Adjusted Model		
	Full Sample	High	Low
$[CAR]_{-1,1}$	-0.0043***	-.0023***	-0.0063***
	(-6.83)	(-3.29)	(-6.04)
Diff (Low-High)		-0.0039	
T-statistics		-3.14***	
N	3542	1776	1776

Table 2.8: External Litigation Risk and Corporate Investment

This table reports results that examine the relation between external litigation risk and corporate ES misconduct. Panel A reports the results for capital investment and Panel B reports the results for M&A activity. In Panel A, the dependent variable *CAPEX*, is capital investment divided by total assets. Column (1) reports the result for the full sample; column (2) and column (3) report the results for two subsamples based on high and low ES scores. In Panel B, the dependent variables in columns (1) to (3) are *M&A Dummy*, a dummy variable that equals one when a firm has made at least one acquisition, and zero otherwise. The dependent variables in columns (4) to (6) are *Number of M&A*, number of acquisitions conducted. Columns (1) and (4) report the results for the full sample; columns (2) and (5) report the results for the subsample with high ES score; columns (3) and (6) report the results for the subsample with low ES score. For both panels, the main independent variable is *Liberal_Prob*, calculated as the probability that at least two judges lean toward the Democratic Party in a random selection of a panel of three judges in a circuit court. The t-statistics are reported in parentheses and standard errors are clustered at firm level. ***, **, * correspond to statistical significance at the 1%, 5%, and 10% levels, respectively. Variable definitions are reported in Appendix 2.A.

Panel A: Capital Expenditure

	Full Sample	High	Low
	(1)	(2)	(3)
Liberal_Prob	-0.003	-0.001	-0.011**
	(-1.44)	(-0.35)	(-2.37)
Firm Controls	Yes	Yes	Yes
State Controls	Yes	Yes	Yes
Industry × Year FE	Yes	Yes	Yes
State FE	Yes	Yes	Yes
Adjusted R^2	0.667	0.803	0.773
N	42167	6873	6394

Panel B: Mergers & Acquisitions

	M&A Dummy			Number of M&A		
	Full Sample	High	Low	Full Sample	High	Low
	(1)	(2)	(3)	(4)	(5)	(6)
Liberal_Prob	-0.061***	-0.003	-0.156***	-0.124**	0.018	-0.359***
	(-2.90)	(-0.06)	(-3.18)	(-2.29)	(0.12)	(-2.87)
Firm Controls	Yes	Yes	Yes	Yes	Yes	Yes
State Controls	Yes	Yes	Yes	Yes	Yes	Yes
Industry × Year FE	Yes	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted R^2	0.133	0.149	0.142	0.149	0.207	0.157
N	42167	5754	5315	42617	5754	5315

Table 2.9: Robustness Checks

This table reports results that examine the relation between external litigation risk and corporate ES misconduct. Panel A reports the results for firm fixed effects and Poisson regression; Panel B reports results for alternative measures of corporate misconducts; Panel C reports the results for alternative measures of judge ideology. In Panel A, the dependent variable in column (1) is *ES_Number*; that in column (2) is *ES_Number* (unlogged). The main independent variable is *Liberal_Prob*. In Panel B, the dependent variable in column (1) is *ES_Dummy*, a dummy variable which takes the value of 1 if the firm is subject to ES violations in a given year.; that in column (2) is *ES_Penalty*, the natural logarithm of one plus the total penalty of ES-related violations that a firm is subject to. In Panel C, the dependent variable is *ES_Number*. In column (1), the independent variable is *Liberal_Ratio*, calculated as the number of judges appointed by democratic presidents divided by total number of judges; that in column (2) is *Liberal_Prob_District*, calculated as the number of judges appointed by democratic presidents divided by total number of judges measured at the district court level. The t-statistics are reported in parentheses and standard errors are clustered at firm level. ***, **, * correspond to statistical significance at the 1%, 5%, and 10% levels, respectively. Variable definitions are reported in Appendix A.

Panel A: Firm Fixed Effect and Poisson Regression

	ES_Number	ES_Number (unlogged)
	(1)	(2)
Liberal_Prob	-0.048*	-0.516**
	(-1.66)	(-2.10)
Firm Controls	Yes	Yes
State Controls	Yes	Yes
Year FE	Yes	Yes
Firm FE	Yes	Yes
Adjusted R^2	0.672	NA
N	42167	42167

Panel B: Alternative Measure of Corporate ES Misconduct

	ES_Dummy	ES_Penalty
	(1)	(2)
Liberal_Prob	-0.070**	-0.784**
	(-2.34)	(-2.36)
Firm Controls	Yes	Yes
State Controls	Yes	Yes
Industry × Year FE	Yes	Yes
State FE	Yes	Yes
Adjusted R^2	0.270	0.278
N	42167	42167

Panel C: Alternative Measure of Litigation Risk

	ES_Number	ES_Number
	(1)	(2)
Liberal_Ratio	-0.135**	
	(-2.57)	
Liberal_Prob_District		-0.144***
		(-2.88)
Firm Controls	Yes	Yes
State Controls	Yes	Yes
Industry × Year FE	Yes	Yes
State FE	Yes	Yes
Adjusted R^2	0.287	0.291
N	42167	42167

Appendix 2.A: Variable Definitions

Variable	Definition	Data Source
Liberal_Prob	Probability that a randomly selected three-judge panel in a circuit court is composed of at least two judges appointed by Democratic presidents.	Federal Judicial Center Judge Biographical Directory
Liberal_Ratio	Number of judges appointed by democratic presidents divided by total number of judges.	Federal Judicial Center Judge Biographical Directory
Liberal_Prob_District	Probability that a randomly selected three-judge panel in a district court is composed of at least two judges appointed by Democratic presidents.	Federal Judicial Center Judge Biographical Directory
ES_Number	Natural logarithm of one plus the number of ES-related violations that a firm is subject to.	Violation Tracker Database
ES_Penalty	Natural logarithm of one plus the total penalty amount of ES-related violations that a firm is subject to.	Violation Tracker Database
ES_Dummy	A dummy variable which takes the value of 1 if the firm is subject to ES violations in a given year.	Violation Tracker Database
E_Number	Natural logarithm of one plus the number of environmental violations that a firm is subject to.	Violation Tracker Database
S_Number	Natural logarithm of one plus the number of social violations that a firm is subject to.	Violation Tracker Database
Conservative_Death	A dummy variable which takes the value of 1 if at least one judge appointed by the Republican Party passed away at circuit court-year level.	Federal Judicial Center Judge Biographical Directory
ES_Lawsuit	Natural logarithm of one plus total number of ES-related lawsuits filed at circuit court-year level.	Federal Judicial Center
Settlement	The ratio of the ES-related lawsuits that are settled over the total number of ES-related lawsuits at circuit court-year level.	Federal Judicial Center
IO	Percentage of total ownership by institutional investors	Thomson Reuters 13F
Blockholder	Dummy variable that equals one when there is at least one blockholder, defined as an institution who has at least 5% of the ownership	Thomson Reuters 13F
Size	Natural logarithm of total assets.	Compustat/CRSP Merged Database
ROA	Return on asset.	Compustat/CRSP Merged Database
Q	Market value of the firm divided by the replacement cost of its assets.	Compustat/CRSP Merged Database
MTB	Market-to-book ratio.	Compustat/CRSP Merged Database
CAPEX	Capital expenditures divided by total assets.	Compustat/CRSP Merged Database
Leverage	The ratio of total liabilities over total assets.	Compustat/CRSP Merged Database
Tangibility	Net property, plant and equipment (PPE) divided by total assets.	Compustat/CRSP Merged Database
Firmage	Number of years since the firm first appears in Compustat/CRSP Database.	Compustat/CRSP Merged Database

Retain	Retained earnings divided by total assets.	Compustat/CRSP Merged Database
Cash	Cash holdings divided by total assets.	Compustat/CRSP Merged Database
Stk_Volatility	The annualized standard deviation of monthly stock returns.	Compustat/CRSP Merged Database
GDP	Natural logarithm of GDP per capita of a state.	U.S. Bureau of Economic Analysis
GDP Growth	Growth rate of GDP.	U.S. Bureau of Economic Analysis
Democratic	The ratio of votes that support democratic presidential candidate. For non-election years, applied to the value with the most recent presidential election.	MIT Election Lab
M&A Dummy	Dummy variable that equals one when a firm has made at least one acquisition, and zero otherwise.	Thomson Reuters SDC Platinum
Number of M&A	Number of acquisitions a firm has made in a year	Thomson Reuters SDC Platinum

Chapter 3

Local Environmental Beliefs and Corporate Environmental Performance[17]

3.1 Introduction

Researchers have examined various determinants of corporate environmental performance, including analyst coverage (Jing et al., 2023), local institutional ownership (Kim et al., 2019); CEO early life disaster experience (Liao, Nguyen and Truong, 2022), earnings pressure (Thomas et al., 2022) and financial constraints (Xu and Kim, 2022). The takeaway of these studies is that firms make environmental decisions in response to external pressure and internal constraints. While these are important drivers, little is known about how public belief and attitude, a crucial component of an effective response to environmental protection, contributes to corporate environmental decisions and performance.

In this paper, we investigate how local environmental beliefs, namely beliefs held by local residents about environmental protection, affect corporate environmental decisions and performance. Environmental beliefs can vary across locations, influenced by factors such as the extent of local media advocacy for environmental protection. Dolšak and Houston (2014) demonstrate this connection by illustrating how local news coverage on climate change affected legislative activity at the state and city levels. Additionally, regional differences in environmental beliefs may stem from varying experiences of natural disasters, which can shape residents' perceptions of environmental

[17] I am deeply grateful for the comments and suggestions on this paper provided by Anna Agapova and participants at 2024 ASFAAG conference.

protection. Numerous studies have highlighted the influence of personal experiences with natural disasters and extreme weather events on individuals' attitudes and actions concerning climate change (Lang and Ryder, 2016; Sloggy et al., 2021; Baccini and Leemann, 2021).

Local environmental beliefs could be shared by individuals occupying diverse roles in the corporate environmental decision-making process. Whether they are investors, members of local stakeholders, EPA inspectors, or court judges responsible for enforcing environmental regulations, the impact of these shared beliefs reverberates across various facets of the corporate landscape. Stronger local pro-environmental beliefs are anticipated to increase the costs associated with releasing toxic emissions at local plants. This increase in costs may be attributed to pressures from investors and stakeholders, regulatory compliance, and potential legal actions.

In this paper, we examine the relation between local environmental beliefs and corporate environmental performance, by employing county-level climate attitudes from Yale Climate Opinion Maps (Howe et al., 2015), and facility-level data from the Toxic Release Inventory (TRI) database of the U.S. Environmental Protection Agency (EPA). Howe et al. (2015) provide survey evidence on how respondents answer questions including (i) whether they believe that climate change is happening; (ii) whether they believe that climate change is human-caused; (iii) whether they believe that there is scientific consensus on whether climate change is happening; and (iv) whether they will be personally affected by climate change.

The TRI database covers both pollution prevention activities and the production of toxic chemicals at the facility-year level. Using a sample of industrial plants covered in the TRI database from 2015 to 2021, we find that plants located in counties with more pronounced environmental beliefs are more likely to reduce toxic emissions. A one standard deviation increase in local pro-environmental beliefs is associated with 38.6% lower local emission. We control for plant industry by year fixed effects, plant state by year fixed effects and firm by year fixed effects, in order to alleviate the concern that industry, state or firm level time-varying omitted variables might contaminate our results.

Next, we conduct two sets of cross-sectional analyses to delve into the mechanisms through which local environmental beliefs impact emissions at the plant level. A firm's emission decisions are integral to both its shareholders and stakeholders. Previous research has demonstrated that local investor preferences play a significant role in shaping corporate decisions. For instance, Becker, Ivković, and Weisbenner (2011) find that firms headquartered in areas with a higher proportion of seniors are more likely to pay dividends, while Adhikari, Cicero, and Sulaeman (2021) reveal that public firms in regions with higher proportions of senior citizens and women tend to use more debt financing. We posit and find that the negative relationship between local pro-environmental beliefs and plant-level emissions is more pronounced when there is greater local investor base.

Furthermore, toxic emissions generate negative externalities for local residents, and these external costs become more significant with a larger population. Consistent with this notion, our findings indicate that the negative relationship between local pro-environmental beliefs and plant-level emissions is more robust when the local county exhibits a higher number of plants, a larger workforce, and a greater overall population.

Another pivotal factor influencing environmental performance is the stringency of environmental regulations and their enforcement. Drawing from theories on social norms and identity (Sunstein, 1996; Cialdini and Goldstein, 2004; Abrams and Hogg, 1988; Tajfel, 1981), we posit that EPA inspectors and court judges, responsible for local environmental regulation enforcement, are influenced by local environmental beliefs. On the other hand, prior studies highlight regional variations in EPA inspections and enforcement activities, hence we propose a connection between local environmental beliefs and the enforcement landscape.

Utilizing the Enforcement and Compliance History Online (ECHO) Database, our study reveals a positive association between local pro-environmental beliefs and several key enforcement outcomes. Specifically, areas with stronger pro-environmental beliefs experience lower likelihoods of receiving formal enforcement actions (conditional on inspection) and higher penalties (conditional on formal enforcement actions). These findings collectively suggest that stronger local pro-environmental beliefs are linked to more robust enforcement of environmental regulations. Consequently, plants in these areas demonstrate higher compliance and a reduced likelihood of facing formal enforcement actions.

Furthermore, our analysis extends to the judicial realm, where we find that stronger local pro-environmental beliefs are associated with greater support from judges for plaintiffs in environmental lawsuits and a higher frequency of such lawsuits. In summary, our results indicate a comprehensive association between stronger local pro-environmental beliefs and the vigorous enforcement of environmental regulations, highlighting the multifaceted impact of local beliefs on regulatory compliance.

Finally, our study investigates the impact of local pro-environmental beliefs on internal emission redistributions within firms. Building on the insights from Bartram et al. (2022), which demonstrate policies addressing climate risk can lead financially constrained firms to shift emissions and output, we explore the role that environmental beliefs play in this process. Examining plant-level onsite and offsite emissions, our findings reveal that local pro-environmental beliefs are associated with a reduction in onsite emissions but not offsite emissions. This aligns with the notion that firms, influenced by pro-environmental beliefs, seek to minimize local emissions due to their perceived higher costs.

Turning our attention to the responses to EPA formal enforcement actions, our study uncovers non-uniform reactions. Specifically, after a plant receives formal enforcement actions, emissions are reduced only in areas where local pro-environmental beliefs are stronger. Additionally, when peer plants face formal enforcement actions, emissions are redistributed to the focal plant only when beliefs in the focal plant are weaker. These results suggest that firms strategically redistribute emissions to optimize compliance costs.

Our paper makes four contributions. First, we contribute to the literature on the drivers and constraints of corporate environmental performance. While prior research has predominantly focused on external monitors, CEO and firm characteristics as major drivers, our study introduces a novel perspective by highlighting the role of local environmental beliefs. Our study complements this literature by revealing that local environmental beliefs play a crucial role in influencing corporate environmental decisions and performance. By adding this dimension, our research enriches the understanding of the various forces at play in shaping the environmental landscape for corporations."

Secondly, our paper contributes to the broader literature on local culture and its impact on corporate decision-making. Hilary and Hui (2009) find associations between religiosity and risk exposure. Chen et al. (2014) explore the effects of local culture on innovative output. Alabbad et al. (2022) connect corporate environmental initiatives to religious diversity. Our research takes a step further by investigating the beliefs of external parties in specific decision-making processes. In this way, our study extends the current understanding of how local beliefs shapes corporate decisions across different locations. This distinctive focus underscores our contribution to understanding the external parties' role in the decision-making process.

Our paper is also related to a recent study by Giannetti and Wang (2023), who link heightened public attention to gender equality with an increase in board gender diversity. Similarly, we explore how local environmental beliefs, as a reflection of public sentiment, influence corporate decisions in a specific geographical context. We show firms can take advantage of the differences in local sentiment across different operations to minimize their compliance costs.

Thirdly, our paper contributes to the literature on firms' responses to EPA enforcement actions. Studies by Lim (2016) and Dasgupta, Huynh, and Xia (2022) have shed light on the specific and general deterrence impacts of EPA inspections and formal enforcement actions. Lim (2016) finds that penalties have a small but statistically significant specific deterrence impact, while inspections produce a general deterrence impact. Additionally, Dasgupta, Huynh, and Xia (2022) reveal that firms tend to reduce toxic emissions at their local plants following EPA enforcement actions against nearby plants operated by peer firms. Our paper adds a unique dimension to this literature by highlighting the heterogeneity in environmental compliance across geographic regions. This heterogeneity reflects the varying compliance costs in areas with different environmental beliefs. By exploring this regional variation, our research unveils a nuanced understanding of how local environmental beliefs influence firms' responses to EPA enforcement actions, contributing to the broader literature on environmental regulation and corporate behavior.

Finally, our paper speaks to the nascent literature on firms' internal emission redistributions. Ben-david et al. (2021) reveal that firms headquartered in countries with strict environmental policies perform their polluting activities abroad in countries with relatively weaker policies, indicating a strategic reallocation of pollution. Similarly, Dai et al. (2021) provide evidence that firms outsource part of their carbon emissions to foreign suppliers in response to pressures to reduce domestic emissions. We find that weaker pro-environmental beliefs are associated with weaker compliance costs, prompting firms to strategically redistribute emissions to plants with weaker pro-environmental beliefs. In a similar spirit, our paper extends the understanding of internal emission redistributions by highlighting the significance of local environmental beliefs as crucial frictions in this process.

The remainder of this chapter is organized as follows. Section 3.2 presents the data, sample selection and our empirical design. Section 3.3 discusses the main results. Section 3.4 presents our cross-sectional results. Section 3.5 discusses our results on environmental lawsuits. Section 3.6 discusses results on firms' internal emissions redistributions. Section 3.7 reports results of robustness checks and Section 3.8 concludes.

3.2 Sample Selection

3.2.1 Local Environmental Beliefs

To measure county-level pro-environmental beliefs, we exploit a novel dataset from the Yale Program on Climate Change Communication (the "YPCCC"[18]). The program conducts nationally representative surveys to American adults on climate change attitudes twice a year since 2008. Survey questions are shown in Appendix 3.B. The survey results, which show the diversity of Americans' views on climate change and how much Americans engage with the issue, are further processed by a proprietary statistic model to estimate public climate change understanding, risk perceptions, and policy support at US state and county levels[19] (Howe et al., 2015). For each question,

[18] For more information about the YPCCC, see https://climatecommunication.yale.edu/visualizations-data/ycom-us/.
[19] According to the YPCCC, the estimates are derived from a statistical model using multilevel regression with post-stratification (MRP) on a large national survey dataset (n>28,000), along with demographic and geographic population characteristics. The YPCCC validates the model estimates with a variety of techniques, including independent state and city-level surveys.

the model accurately predicts climate beliefs, risk perceptions and policy preferences at the state, congressional district, metropolitan and county levels, using a concise set of demographic and geographic predictors. In general, the analysis finds substantial variation in public opinions across the nation. For example, the estimated percentage of citizens who believe in global warming at the county-level ranges from 43% to 80%, suggesting that there is substantial variation in beliefs about climate policy.

We construct county-level pro-environmental beliefs by aggregating public views on each of the survey questions related to climate change issues. Specifically, our measure of pro-environmental beliefs is calculated as the average of the estimated percentage of people who explicitly express their attention or concerns to each survey question, ranging from 0 to 100. Our sample covers nearly all counties in the U.S. and spans from 2014 to 2020[20]. Figure 3.1 shows geographic distribution of county-level environmental beliefs, where a higher value indicates greater local attention and concerns on climate issues. The three counties with the strongest pro-environmental beliefs are Alameda County (California), District of Columbia and New York County (New York). Their average pro-environmental beliefs are 70, suggesting that about 70% of the population believes in climate change and strongly support environmental protection. Three counties with the weakest pro-environmental beliefs are Emery County (Utah), Millard County (Utah), Cameron Parish (Louisiana). Their average pro-environmental beliefs are 47, suggesting that about 47% of the population believes in climate change and have weaker support for environmental protection.

3.2.2 Plant Toxic Emissions

We collect data on plant-level toxic emissions from the Toxics Release Inventory (TRI) Program conducted by Environmental Protection Agency (EPA). Industrial facilities that meet TRI Program

[20] As the dataset does not provide climate attitude estimates for the year of 2015 and 2017, values for these two years are linearly interpolated.

reporting requirements[21] must submit their data to EPA on an annual basis. Though being self-reported, TRI data are periodically audited by the EPA which aims to verify and improve data accuracy (Akey and Appel, 2019). Our TRI data provides emission information by a variety of chemicals at plant level starting from 1987.

Because local environmental beliefs are available between 2014 and 2020, following Equation (1), we retrieve TRI data from 2015 to 2021 and measure plant-level toxic emissions by summing emissions across all chemicals that the plant releases. This sample has 149, 036 plant-year observations and 26, 660 unique plants. Our measure of plant-level emissions has been widely used in the finance literature to track firm- or plant-level environmental activities and performance (Kim, Wan, Wang and Yang, 2019; Xu and Kim, 2022; Jing, Keasey, Lim and Xu, 2023). In addition to chemical emissions, the TRI data provides rich plant-level information, such as location, industry, parent company name and its Dun & Bradstreet DUNS Number.

3.2.3 Plant Information

We further obtain plant-level information from the National Establishment Time-Series (NETS) Database. The NETS database provides detailed information on U.S. plants owned by both private and public firms. Following Equation (1), we retrieve 5,092,099 plant-year observations in the period of 2014 and 2020. This sample covers 890,557 unique plants and 79,502 firms. Following existing papers, we match TRI and NETS database using Dun & Bradstreet DUNS Number, as well as plant name and location information and conduct manual inspections to improve matching accuracy. We end up with 25,097 plant-year observations. Next, we restrict our sample to plants of public firms, and end up with 17,255 plant-year observations, which covers 3,604 unique plants.

[21] According to EPA, reporting requirements to TRI program include the number of full-time employees, primary NAICS code, and chemical activity threshold. For detailed information, see https://ordspub.epa.gov/ords/guideme_ext/f?p=guideme:rfi:::::rfi:2

Next, we obtain county-level demographic statistics from the American Community Survey database. Specifically, we consider a bunch of socioeconomic factors that are mostly related to local demographic and economic trends, including gender and racial makeup, unemployment rate, GDP per capita, and GDP growth rate. In addition, to account for county-level political leaning, we exploit data on presidential election voting from the MIT Election Lab.[22] We use logarithm of plant-level sales, reported in the NETS database, as the proxy for plant size.[23] Definitions of these control variables can be found in Appendix 3.A and their summary statistics are presented in Table 3.1. We then require all variables to be available for our regression analysis, and end up with 17, 255 plant-year observations.

Table 3.1 provides summary statistics of the main variables. To mitigate the influence of outliers, all continuous variables used in this study are winsorized at 1% and 99% percentiles. In our sample, the average of local environmental beliefs across all US counties equals 57.8, with a standard deviation of 4.82. The average of the plant-level toxic emissions is 202,287.9 pounds. The average plant size in terms of sales is $79,061,650.

3.2.4 Regression Specification

Our main independent variable is *Pro-environmental Beliefs*, proxied by the climate attitude at the county-year level; our main dependent variable is *Emissions*, the level of emissions at the plant level. In our main regression, we regress plant level emission on the climate attitude of the county where the plant is located, with various control variables and stringent fixed effects:

[22] MIT Election Lab reports data on presidential candidate votes at county-level since 2000. We calculate the support rate for Democratic (or Republican) parties by dividing the total votes cast for the Democratic (or Republican) candidate by the total votes. As presidential elections are held every four years, support rates for non-election years are linearly interpolated between two elections. We create a dummy variable to indicate the Democratic political leaning, with take the value of 1 if the support rate for the Democratic party exceeds 50%, and 0 otherwise.

[23] The NETS database reports plant sales and number of employees. As these two variables are highly correlated (with a correlation coefficient of over 0.9), we only add plant sales into the regression model as a control for plant size. Our empirical results are basically unchanged if we use the number of employees instead or use both as controls together.

Equation (1) shows our main regression model:

$$Emissions_{i,j,k,t+1} = \alpha_0 + \beta_1 Pro - environmental\ Belief_{k,t} + \beta_2 Sale_{i,j,k,t}$$

$$+County\ Controls_{k,t} + FEs + \epsilon_{i,t} \quad (1)$$

where $Emissions_{i,\mathrm{j,k},t+1}$ is logarithm of total emission of toxic pollutants from plant i located in county k owned by firm j in year t+1; $Pro - environmental\ Belief_{k,t}$ indicates the pro-environmental beliefs of county k in year t. The coefficient β_1 captures the changes in the plant emission driven by one unit change of the pro-environmental beliefs.

We include a set of lagged control variables that may affect plant emission decisions. Following Xu and Kim (2022), we include logarithm of plant-level sales to account for the effect of production scale on total toxic emissions. In addition, to account for local effects other than environmental belief, we control for a variety of county-level characteristics, including both demographic characteristics (i.e. gender and race) and economic characteristics (logarithm of GDP per capita, GDP growth rate and unemployment rate). In addition, we also control for *Democrat*, that captures the political orientation of the county, to disentangle the potential effect of local political leaning.

Importantly, the granularity of plant-level data further enables us to include a rich set of fixed effects to account for unobservable shocks at various levels. For example, environmental laws and enforcement activities vary from state to state, and there are large variations in terms of emissions across different industries. To address these concerns, we follow Kim et al. (2019) and sequentially add industry by year fixed effects, plant state by year fixed effects and firm by year fixed effects into our baseline model.[24] These fixed effects absorb all time-varying industry, state and firm trends which may affect plant emission decisions.

[24] We classify plant industry using the Fama-French 48 industry portfolios.

3.3 Main results

3.3.1 Local Environmental Beliefs and Toxic Emissions

We estimate our main result following Equation (1) and report our results in Table 3.2. Specifically, we regress plant-level toxic emissions onto lagged pro-environmental beliefs of the county in which the plant is located. We start from column (1) where no fixed effects are included, and then sequentially add fixed effects to account for unobservable factors such as industry, state, and firm level shocks. In column (6), we include all two-way fixed effects, namely plant industry by year, plant state by year and firm by year fixed effects. The granular inclusion of two-way fixed effects enables us to capture all time-varying trends at industry, state, and firm levels. Across all columns, the coefficients on *Pro-environmental Beliefs* are negative and significant at the 5% level. This finding suggests that plants located in counties with stronger pro-environmental beliefs (i.e., greater attention to and concerns about climate change) emit fewer toxic releases, consistent with our main conjecture. According to column (6), a one unit increase in *Pro-environmental Beliefs* leads to a roughly 8.3% decrease of toxic releases and a standard deviation increase in *Pro-environmental Beliefs* leads to a 40% decrease of toxic releases. [25]

3.3.2 IV Estimation

Although stringent fixed effects subsume all unobservable time-varying trends at industry, state and firm levels, one can still argue that our main results might be confounded by some omitted variables, such as unobservable time-variant shocks at county level. To further establish causality, we implement an instrumental variable (IV) estimation. Specifically, we exploit the occurrence of natural disasters in local counties as an IV for our main independent variable.

[25] This can be derived as 4.82*(-0.083) =-0.4. As our dependent variable is the logarithm of total toxic emissions, it implies a 40% decrease of total releases.

For an IV to be valid, it must satisfy both the relevance condition and the exclusion condition. As for the relevance condition, we hypothesize that local attention and concerns about climate will become stronger after catastrophic natural disasters. In fact, multiple studies in finance have documented that natural disasters and extreme weather events significantly affect people's view and beliefs on climate topics (Huang et al., 2022; Choi et al., 2023; Di Giuli et al., 2022). In addition, catastrophic natural disasters are relatively rare and random. Therefore, we believe they are not directly associated with unobserved factors which affect plant emission decisions.

We obtain county-level natural disaster information from the Spatial Hazard Events and Losses Database for the United States (SHELDUS) provided by the Arizona State University. This database, which has been used widely in recent articles in the finance literature (Bernile et al., 2017; Dessaint & Matray, 2017; Ivanov et al., 2022); records detailed information on various types of natural disaster in the US after 1960, such as date, affected counties and damages. We aggregate monetary losses resulting from natural disasters for each county-year pair and construct a dummy indicator *Damage* as our IV. *Damage* equals to 1 if the total damages exceed 20 million or 40 million dollars.[26]

Table 3.3 reports the results of IV estimation. Columns (1) and (2) report the first-stage results, using the 20 million and 40 million cutoffs respectively; columns (3) and (4) report the second-stage results. For our first-stage regressions, we regress *Pro-environmental Beliefs* on county-average control variables in our main specification. In the second stage, we use plant-level regressions and regress plant-level toxic emissions on the instrumented *Pro-environmental Beliefs* and other controls.

In columns (1) and (2), we find that the coefficients on *Damage* are positively associated with *Pro-environmental Beliefs*, consistent with our conjecture that catastrophic natural disasters heighten public concerns about climate issues. In columns (3) and (4), we replace *Pro-environmental Beliefs*

[26] Compared to existing studies (Huynh & Xia, 2021), we select a higher cutoff damage value to define affected counties because only catastrophic disasters can possibly reshape people's attention and beliefs towards climate issues.

with the instrumented values of *Pro-environmental Beliefs* estimated from the first stage regression. We find that the coefficients are negative and significant at 5% level. Overall, the IV estimation echoes our OLS results and helps alleviate the concern that plant-level time-varying omitted variables might contaminate the baseline results.

3.4 The Role of Local Investor Base and Local Stakeholders

In this section we report tests that examine the potential mechanisms through which local environmental beliefs affect plant-level toxic emissions. First of all, prior studies such as Becker, Ivković and Weisbenner (2011) and Adhikari, Cicero and Sulaeman (2021) find that local investor preferences and investor base affect corporate policy choices. Investors can thus voice their concerns about firms' harmful environmental policies and thus can therefore influence firms' emission decisions. When more investors are affected by firms' local toxic emissions, such concerns should be greater. Hence, we expect the negative effect would be stronger if a firm has a greater local investor base.

Second, local residents are the direct victims of toxic emissions. To the extent that local environmental beliefs negatively affect toxic emissions, such effect would be stronger when there are director victims of these emissions, including employees at other plants and local residents as a whole.

3.4.1 Local Investor Base

Following Hong et al. (2008) and Ucar (2016), we construct two proxies for local investor base, *Investor Base1* and *Investor Base2*. *Investor Base 1* is calculated as the total book value of equity of firms divided by total income in a county; *Investor Base 2* is calculated as is calculated as the total number of firms divided by total income in a county. We partition our main sample into two subsamples based on the median of these two proxies, and then estimate Equation (1) for the two subsamples respectively.

Table 3.4 reports the results. Columns (1) and (2) report results using *Investor Base1*; columns (3) and (4) report results using *Investor Base2*. The coefficient on *Pro-environmental Beliefs* in column (1), the low subsample, is 0.004 and insignificant and that in column (2), the high subsample is -0.162 and significant at 1% level. The difference is significant at the 5% level. The coefficient on *Pro-environmental Beliefs* in column (3), the low subsample, is 0.003 and insignificant and that in column (4), the high subsample, is -0.189 and significant at 1% level. The cross-equation difference (untabulated) is significant at the 5% level. Consistent with our conjecture, we find that for both proxies, the main effect is only significant in high investor base subgroup. Specifically, a one unit increase in local environmental beliefs leads to roughly 16% to 19% decrease in toxic emissions for plants in high investor base subgroup, while the effect is almost negligible for plants in the other subgroup.

3.4.2 Local Stakeholders

We use three different proxies to measure local stakeholders. From NETS database, we aggregate number of plants and their employees at county level, excluding the focal plant, and classify counties into high and low subgroups according to the number of plants and employees, respectively. In addition, we obtain population data from American Community Survey Dataset and classify counties into high and low subgroups according to the level of local population. Intuitively, the social costs of emissions in counties with more active plants, employees and total population are greater, therefore magnifying the curtailing effect of local environmental beliefs on emissions. For the analysis using each proxy, we partition our main sample into two subsamples based on the median of the proxy, and then estimate Equation (1) for the two subsamples respectively.

Table 3.5 reports the results. Columns (1) and (2) report results using the number of local plants excluding the focal plant; columns (3) and (4) report results using the number of local employees; columns (5) and (6) report results using the number of local populations. The coefficient on *Pro-environmental Beliefs* in column (1), the low subsample, is -0.004 and insignificant and that in column (2), the high subsample is -0.142 and significant at 1% level. The coefficient on *Pro-environmental Beliefs* in column (3), the low subsample, is -0.028 and insignificant and that in column (4), the high subsample, is -0.139 and significant at 1% level. Similarly, the coefficient on *Pro-environmental Beliefs* in column (5), the low subsample, is 0 and insignificant and that in column (6), the high subsample, is -0.1 and significant at 10% level. The cross-equation differences (untabulated) for these three sets of subsample analyses are significant at 5%, 5% and 10% level respectively.

Consistent with our conjecture, we find that the main effect is only significant in subsamples in which there are more stakeholders. Specifically, a one unit increase in local environmental beliefs leads to roughly 10% to 14% decrease in toxic emissions for plants in high stakeholder subgroup, while the magnitude reduces to no more than 3% for plants in the other subgroup.

3.5 Enforcement of Environmental Regulations

A negative association between local environmental beliefs and toxic emissions can also be due to more stringent enforcements of environmental regulations. As prior studies on social norms and social identity theory indicate, individuals are shaped and influenced by social norms (Sunstein, 1996; Cialdini and Goldstein, 2004) and individuals are influenced by their local environment through their social interactions. (Hogg and Abrams 1988; Tajfel, 1981). Because environmental regulations are enforced locally, individuals who enforce these regulations might share the same local environmental belief. Because the stringency of the enforcements is not uniform across different regions and depends on a variety of political and economic factors (Gray and Shimshack, 2011), we conjecture that stronger local pro-environmental beliefs are associated with more stringent enforcements of environmental regulations. To test our conjecture, we conduct two tests. First, we examine the relation between local pro-environmental beliefs and EPA enforcement actions. Second, we examine the relation between local pro-environmental beliefs and the judge's support for plaintiffs in environmental lawsuits.

3.5.1 EPA Enforcement

The EPA plays a pivotal role in overseeing and regulating plant emissions across the country. As the primary federal agency responsible for safeguarding the nation's environment and public health, the EPA monitors emissions from industrial facilities, power plants, and other sources and seeks to control harmful emissions of pollutants that can have adverse effects on air and water quality. Gray and Shimshack (2011) provide an overview of the EPA enforcement actions. The EPA establishes legal guidelines, on a statute-by-statute basis, for enforcement stringency. In general, the severity of enforcement should increase with the duration and extent of noncompliance.

Existing studies have consistently found a significant deterrent effect of EPA enforcement actions on local environmental misbehaviors[27]. For example, Lim (2016) finds that penalties produce a small but statistically significant deterrence impact. Similarly, Dasgupta, Huynh and Xia (2022) find that firms reduce toxic emissions at their local plants following EPA enforcement actions against nearby plants operated by peer firms. These results illustrate the strong deterrence effects of EPA inspections and enforcements.

[27] Gray & Shimshack (2011) provides a comprehensive summary of articles on the deterrence effect of EPA's enforcements.

To empirically test the association between local environmental beliefs and EPA's monitoring activities, we obtain data on EPA's inspection and enforcement actions from EPA's Enforcement and Compliance History Online (ECHO) Database. The ECHO system incorporates data from the Integrated Compliance Information System for Air (ICIS-Air), which contains emissions, compliance, and enforcement data on stationary sources of air pollution. We match plants in TRI dataset with their enforcement actions records in ECHO database and investigate how enforcement actions stringency are related to local environmental beliefs.[28] Out of our sample of 17,255 plant-year observations, we identify 527 plant-years have received formal enforcement actions. Among these 527 plant-years that have received formal enforcement actions, 483 plant-years have received fines.

Table 3.6 reports the impact of local environmental beliefs on enforcement penalties. The sample now consists of 537 plant-years that received formal enforcement actions. Column (1) reports the results without any fixed effects; column (2) reports the results with plant industry × year fixed effects. We find the coefficients on *Pro-environmental Beliefs* in both columns are positive and significant, suggesting that stronger local pro-environmental beliefs are associated with a higher amount of penalty in formal enforcement actions.

To summarize, these results suggest that in counties with stronger environmental beliefs, EPA enforcement tends to be more stringent: they conduct more inspections and impose greater fines when they take formal enforcement actions. As a result, we observe less formal enforcement actions.

3.5.2 Environmental Lawsuits

[28] After EPA discover or learn of violations, it will base its enforcement response on the following factors: type of violations, potential risk posed by the violations, and ability of the facility to address the violations.

To test the relation between local environmental beliefs and judge support for environmental lawsuits, we first retrieve firms' lawsuits from the Federal Judicial Center. Its Integrated Data Base (IDB) contains data on civil case and criminal defendant filings and terminations in the district courts, along with bankruptcy court and appellate court case information from 1970 to the present.[29] We restrict our search of lawsuits to the window of 2015 to 2021 and get 1,835,177 lawsuits. Then we restrict the nature of suit to be "environmental" and obtain 6,607 lawsuits. For each lawsuit, we define a case to be supported by judge when the settlement before trial or final judge is in favor of plaintiffs[30], and there are 4017 cases in total.

We then estimate the following court-year level equation:

$$Judge\ Support(Environmental\ Lawsuits)_{\ i,t} = \alpha_0 + \beta_1 Pro - environmental\ Beliefs_{i,t}$$

$$+County\ Controls_{k,t} + FEs + \epsilon_{i,t} \quad (2)$$

In this equation, we calculate the average pro-environmental beliefs across counties within the boundary of the federal district court i. We control state and year fixed effects to alleviate the concern of state-invariant and time-invariant omitted variables. *Judge Support* is calculated as the number of lawsuits in which decisions were favorable to plaintiffs divided by the total number of lawsuits, and *Environmental Lawsuits is* calculated as the number of environmental lawsuits filed in the local federal district court.

Table 3.7 reports the results. In Panel A, the dependent variable is *Judge Support*. In Panel B, the dependent variable is *Environmental Lawsuits*.[31] For both panels, we find that the coefficients on *Pro-environmental Beliefs* are positive and significant. These results confirm our results and seem to

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[29] More information about the database can be found at https://www.fjc.gov/research/idb.
[30] We follow related literature (Liu, C., 2020) and use settlement as a proxy for judge's support towards plaintiffs.
[31] If there is no environmental lawsuit filed for a certain court-year pair, we create a missing value for *"Judge Support"* as the denominator is zero. This is why the number of observations in Panel A of Table 3.7 is slightly smaller than that of Panel B.

suggest local environmental beliefs are associated with stronger judge support. A stronger judge support then facilitates the submission of environmental lawsuits by plaintiffs.

3.6 Local Environmental Beliefs and Internal Emission Redistributions

Most large public companies own plants in various locations across the US. Prior literature such as Dai et al. (2021) and Bartram et al. (2022) have shown that firms choose their emission locations to minimize the costs of complying with local environmental regulations. As we show earlier, stronger pro-environmental beliefs impose additional costs on emissions. Therefore, we conjecture that they create frictions in the process of internal emission redistributions. More specifically, a firm is more likely to redistribute emissions from plants located in counties with stronger pro-environmental beliefs to those with weaker beliefs.

To empirically test this hypothesis, we conduct two tests. First, we examine plant-level onsite versus offsite emissions. When emissions are generated at a plant, a firm can choose to release the emissions locally, defined as onsite emissions, or transport its emissions to other places, i.e. offsite emissions. To the extent pro-environmental beliefs curtail local toxic emissions, we conjecture that the deterrence effect of stronger pro-environmental beliefs should be mainly attributed to reduction of onsite emissions, as a plant's offsite emissions are released elsewhere and should not be affected by these local beliefs.

Second, we exploit EPA's formal enforcement actions as a unique setting to investigate emission redistributions. As prior studies show (Lim 2016; Shimshack & Ward, 2005), formal enforcement actions have disciplinary effects on plants' emissions. We conjecture that such reduction is mainly present in counties with stronger environmental beliefs, in which the stringency of enforcement actions is stronger. Next, when peer plants have received formal enforcement actions, a firm is more likely to redistribute emissions to plants with weaker pro-environmental beliefs, in which emissions are less costly.

3.6.1 Onsite and Offsite Emissions

Table 3.8 reports the results on onsite and offsite emissions. We use the specification as in column (6) of Table 3.2, to estimate the regressions. The dependent variables in columns (1) and (2) are *Onsite Emissions* and *Offsite Emissions* respectively. *Onsite (Offsite) Emissions* is defined as the natural logarithm of one plus the onsite (offsite) emissions at a plant. The coefficient on *Pro-environmental Beliefs* is negative and significant at 5% level in column (1) while it is much smaller and insignificant in column (2), suggesting that the negative effect of local pro-environmental beliefs on toxic emissions are mainly driven by the onsite emissions.

3.6.2 Emission Redistributions within Firms

To estimate firms' responses to plant-level enforcements, we first use the following equation with the difference-in-difference design:

$$\begin{aligned} Emissions_{i,j,k,t+1} &= \alpha_0 + \beta_1 Enforcement_{i,j,k,t} \\ &+ \beta_2 Peer\ Enforcement_{i,j,k,t} + \beta_3 Sale_{i,j,k,t} + County\ Controls_{k,t} + FEs + \epsilon_{i,t} \quad (3) \end{aligned}$$

where $Enforcement_{i,j,k,t}$ is a dummy variable that equals one if plant *i* has received an EPA formal enforcement action in any of the previous 2 years, and zero otherwise; $Peer\ Enforcement_{i,j,k,t}$ is a dummy variable that equals one if any *peer* plants of *i* received an EPA formal enforcement action in year t, and zero otherwise. The coefficient β_1 and β_2 capture the main effect of EPA enforcement actions and its spillover effect within firms, respectively. We partition the sample based on the median of local environmental beliefs and estimate the equation above for plants located in counties with strong and weak pro-environmental beliefs, respectively. Plant and year fixed effects are included to account for plant-level time-invariant characteristics and time trends. To alleviate the concern that a firm's own environmental performance is correlated with the likelihood of receiving enforcement actions, we start with our main sample and focus on plant-years that are inspected by EPA, and we have 10,582 observations.

Table 3.9 reports the results. Column (1) reports the results in the weak beliefs subsample; column (2) reports the results in the strong beliefs subsample. The coefficient on *Enforcement* in column (1) is -0.08 and insignificant; that in column (2) is -0.298 and significantly negative. The cross-equation difference between these two coefficients is significant at 5% level. The coefficients on *Enforcement* suggest that firms are more likely to reduce emissions when the local pro-environmental beliefs are stronger. The coefficient on *Peer Enforcement* in column (1) is 0.172 and significantly positive; that in column (2) is 0.056 and insignificant. The cross-equation difference between these two coefficients is significant at 5% level. The coefficients on *Peer Enforcement* suggest that firms are more likely to transfer emissions from other plants of the same firm to the focal plant if it is located in low beliefs areas, when other plants have experienced EPA formal enforcement actions.

Taken together, these results have two important implications. First, following formal enforcement actions, compliance with EPA regulations does not become stronger when local pro-environmental beliefs are weak. This is consistent with our finding that weaker pro-environmental beliefs are associated with smaller fines. In that case, firm's incentives to reduce emissions and better comply with EPA regulations are weaker. This result shows there exists substantial heterogeneity in the ways firms comply with EPA regulations, and weaker compliance exists for areas with weaker beliefs about environmental protection.

Second, plants with weaker pro-environmental beliefs facilitate internal emissions redistributions because they have lower compliance costs. Firms seem to redistribute emissions from plants with stronger pro-environmental beliefs, i.e., those with higher compliance costs, to plants with weaker pro-environmental beliefs, i.e., those with lower compliance costs. Overall, local environmental beliefs play a significant role in a firm's strategic redistribution of emissions to minimize overall compliance costs.

3.7 Robustness Check

In our main analysis in Table 3.2, we find that plants located in counties with strong pro-environmental beliefs emit less in terms of toxic releases quantity. In this section, we use the number of toxic chemicals in these emissions, as the alternative measure of toxic emission activity.

We obtain the information on the type of toxic chemicals from the TRI database. According to Table 3.1, a plant releases 4.78 different types of toxic chemicals on average and 3 for the median. We replace the dependent variable with the number of different types of toxic chemicals that a plant releases each year in Equation (1) and report results in Table 3.10. Consistent with our hypothesis, we also find a significant and negative association between local environmental beliefs and the toxic chemical numbers across all specifications. The coefficient in column (6) indicates that a one standard deviation increase in local pro- environmental beliefs leads to a 0.48 decrease of toxic chemical numbers, equivalent to 10% of the mean. Taken together, these results imply that in counties with stronger pro-environmental beliefs, plants have lower total toxic emissions and the number of toxic chemicals released. These findings confirm our hypothesis that plants located in areas with stronger pro-environmental beliefs are more environmentally friendly.

3.8 Conclusion

In this study, we contribute valuable insights to the understanding of corporate environmental decision-making processes, with significant implications for policy and practice. Our findings highlight the pivotal role of local environmental beliefs in shaping firms' environmental performance, as evidenced by overall reductions in toxic emissions in areas with stronger local environmental beliefs. We also find that the reduction in emissions is more pronounced when firms have a greater local investor base and more stakeholders. This suggests that the influence of local environmental beliefs on corporate emissions is partially driven by investors preferences and stakeholder pressures.

Drawing on prior studies on social norms and social identity, we extend our investigation to the stringency of environmental regulations. We conjecture that EPA inspectors and court judges might share the same environmental beliefs and enforce regulations consistent with beliefs. Consistent with our conjecture, we find stronger local pro-environmental beliefs are associated with more EPA inspections, higher penalties in formal enforcement actions, and lower chances of receiving formal enforcement actions. They are also associated with greater judge support, and a higher volume of environmental lawsuits. These results collectively suggest that stronger environmental beliefs impose additional compliance costs through more stringent enforcement of environmental regulations.

Furthermore, our study documents that local environmental beliefs serve as significant frictions when firms redistribute emissions internally. Firms respond to stronger environmental beliefs by reducing onsite emissions but not offsite emissions. Following EPA formal enforcement actions, emissions are reduced only in plants with stronger pro-environmental beliefs. Additionally, when peer plants receive formal enforcement actions, firms are more likely to shift emissions away to other plants with weaker pro-environmental beliefs. These results underscore the substantial heterogeneity in firms' responses to EPA enforcement actions based on local beliefs.

The policy implications of our findings are substantial. We demonstrate that a firm's environmental performance is not uniform across different plants, and discrepancies are likely due to local environmental beliefs. These beliefs are linked to different levels of enforcement stringency, enabling firms to arbitrage between plants with different beliefs and minimize compliance costs. Policymakers should consider these local variations and beliefs when designing and implementing environmental regulations to ensure effective and uniform compliance across diverse geographical contexts.

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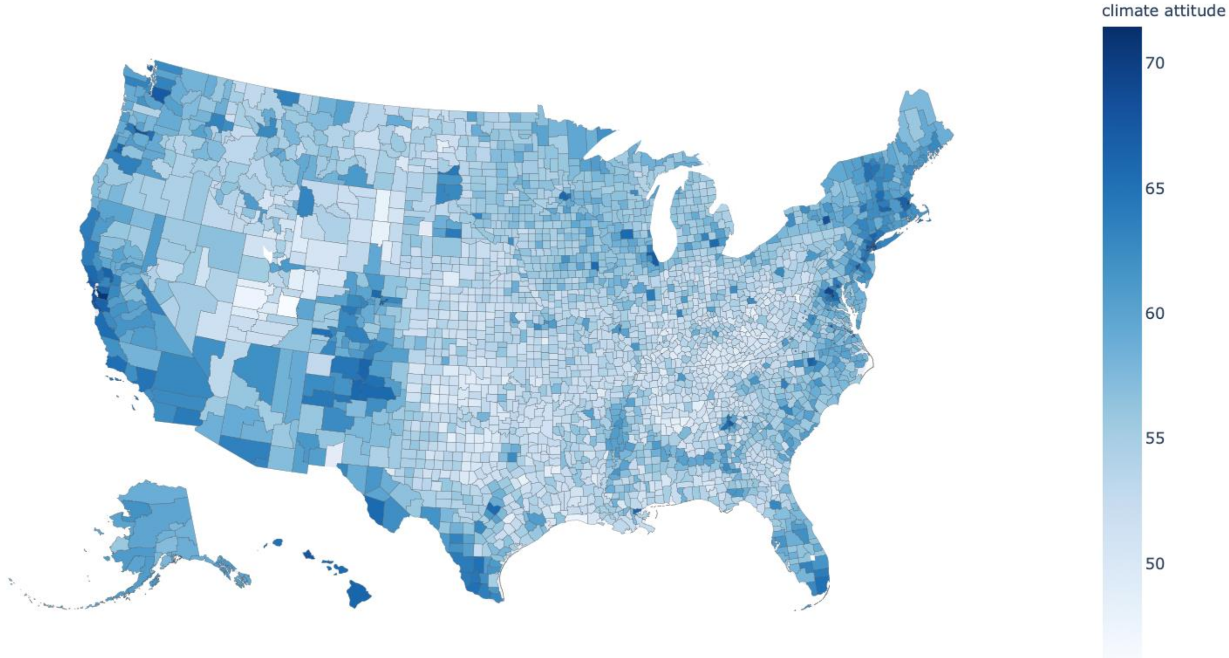
Figure 3.1: County-Level Environmental Beliefs in the US



Note: This figure shows county-level environmental beliefs in the US. Our measure of environmental beliefs is based on the Yale Program on Climate Change Communication (YPCCC) program and calculated as the average of the percentage of respondents who explicitly express their attention or concerns to each survey question, ranging from 0 to 100. Therefore, a higher value of this measure indicates stronger 'pro-environment' beliefs.

Table 3.1: Summary Statistics

This table reports summary statistics of the main variables in the baseline regression.

	Mean	Standard Deviation	Minimum	Median	Maximum	N
Emissions	6.81	4.50	0	7.55	15.1	17255
Emissions (non-log)	202287.9	1472843.7	0	1908.9	70879232	17255
Chemical Type	4.78	5.96	1	3	36	17255
Sales	16.9	1.77	12.3	17.1	20.9	17255
Sales (non-log)	79061650	159398100	207400	25187400	1106547000	17255
Pro-environmental Beliefs	57.8	4.82	47.6	57.3	69.2	17255
Male	0.49	0.011	0.47	0.49	0.53	17255
White	0.81	0.14	0.36	0.84	0.98	17255
Unemployment	0.051	0.018	0.024	0.047	0.11	17255
GDP Per Capita	10.8	0.38	9.94	10.8	11.8	17255
GDP Per Capita (non-log)	52137.2	20788.2	20668.8	48289.8	133185.2	17255
GDP Growth	0.01	0.042	-0.11	0.014	0.14	17255
Democrat	0.37	0.48	0	0	1	17255

Table 3.2: Local Environmental Beliefs and Toxic Emissions

This table reports results that examine the relation between local environmental beliefs and plant-level toxic emissions. Our sample includes 17,255 plant-year observations in the period from 2015 to 2021. The dependent variable in all columns is *Emissions*, calculated as the natural logarithm of one plus total emission generated at plant level. The main independent variable is *Pro-environmental Beliefs*, calculated as the estimated average percentage of people in a county who are pro-environment, based on the survey conducted by YPCCC. The list of questions is presented in Appendix 3.B. The t-statistics are reported in parentheses and standard errors are clustered at firm level. ***, **, * correspond to statistical significance at the 1%, 5%, and 10% levels, respectively. Variable definitions are reported in Appendix 3.A.

	(1)	(2)	(3)	(4)	(5)	(6)
Pro-environmental Beliefs	-0.114***	-0.148***	-0.149***	-0.114***	-0.101***	-0.083**
	(-4.21)	(-4.62)	(-4.60)	(-2.98)	(-3.13)	(-2.30)
Sales	0.378***	0.382***	0.380***	0.374***	0.325***	0.323***
	(5.46)	(6.17)	(6.07)	(6.05)	(5.87)	(5.71)
Male	17.283	14.243*	14.133*	17.015**	3.080	2.073
	(1.60)	(1.77)	(1.76)	(2.03)	(0.42)	(0.28)
White	-2.460**	-1.666*	-1.641*	-1.379	-0.840	-0.674
	(-2.29)	(-1.78)	(-1.74)	(-1.26)	(-1.07)	(-0.69)
Unemployment	5.419	9.205	9.320	23.631***	-2.317	3.981
	(1.41)	(1.46)	(1.46)	(2.73)	(-0.41)	(0.52)
GDP Per Capita	0.555**	0.503**	0.501**	0.562**	0.367	0.345
	(1.97)	(2.08)	(2.05)	(2.00)	(1.52)	(1.24)
GDP Growth	-3.783***	-3.261***	-3.291***	-2.551**	-1.736*	-1.126
	(-3.15)	(-2.83)	(-2.79)	(-2.28)	(-1.84)	(-1.26)
Democrat	-0.207	0.146	0.153	-0.028	-0.062	-0.243
	(-0.85)	(0.55)	(0.57)	(-0.10)	(-0.25)	(-0.99)
Constant	-5.699	-2.680	-2.580	-7.406	2.506	1.840
	(-0.86)	(-0.53)	(-0.51)	(-1.25)	(0.49)	(0.33)
Plant Industry FE		Yes				
Year FE		Yes				
Plant Industry × Year FE			Yes	Yes	Yes	Yes
Plant State × Year FE				Yes		Yes
Firm × Year FE					Yes	Yes
Adjusted R^2	0.039	0.163	0.156	0.162	0.360	0.364
N	17255	17255	17255	17255	17255	17255

Table 3.3: Instrumental Variable (IV) Estimation

This table shows the instrumental variable estimations for our baseline regression in Table 3.2. In the first-stage regressions, we regress *Pro-environmental Beliefs* on the instrument variable *Damage* using county-year level regressions. *Pro-environmental Beliefs* is calculated as the estimated average percentage of people in a county who are pro-environment, based on the survey conducted by YPCCC. The list of questions is presented in Appendix 3.B. *Damage* is a dummy variable that equals one when the annual monetary damage caused by natural disasters exceeds 20 million dollars in column (1), or 40 million dollars in column (3), and zero otherwise. In the second-stage regressions, we regress the dependent variable *Emissions* on the instrumented variable *Pro-environmental Beliefs*. *Emissions* is calculated as the natural logarithm of one plus total emission at the plant level. The t-statistics are reported in parentheses and standard errors are clustered at firm level. ***, **, * correspond to statistical significance at the 1%, 5%, and 10% levels, respectively. Variable definitions are reported in Appendix 3.A.

	First Stage		Second Stage	
	(1)	(2)	(3)	(4)
Pro-environmental Beliefs			-0.412**	-0.451**
			(-2.18)	(-2.24)
Damage	0.708***	0.755***		
	(3.44)	(3.02)		
Sales	0.006	0.006	0.324***	0.324***
	(0.19)	(0.19)	(5.78)	(5.79)
Male	-19.488***	-19.540***	-4.146	-4.948
	(-3.43)	(-3.43)	(-0.52)	(-0.59)
White	-10.724***	-10.735***	-4.186**	-4.606**
	(-12.55)	(-12.54)	(-1.97)	(-2.07)
Unemployment	-35.179***	-35.248***	-7.273	-8.564
	(-5.66)	(-5.66)	(-0.80)	(-0.89)
GDP Per Capita	0.432**	0.431**	0.463	0.479
	(2.05)	(2.04)	(1.53)	(1.59)
GDP Growth	3.282***	3.283***	-0.011	0.129
	(4.52)	(4.51)	(-0.01)	(0.12)
Democrat	3.780***	3.785***	1.041	1.183
	(14.93)	(14.84)	(1.42)	(1.52)
Constant	71.853***	71.897***	25.472*	28.249*
	(18.41)	(18.40)	(1.84)	(1.89)
State × Year FE	Yes	Yes		
Plant Industry × Year FE			Yes	Yes
Plant State × Year FE			Yes	Yes
Firm × Year FE			Yes	Yes
Adjusted R^2	0.792	0.791	0.362	0.362
N	7272	7272	17255	17255

Table 3.4: The Role of Local Investor Base

This table reports the results on the role of local investor base in the relation between local environmental beliefs and plant-level toxic emissions. The dependent variable across all columns is *Emissions*, calculated as the natural logarithm of one plus total emissions at plant level. The main independent variable is *Pro-environmental Beliefs*, calculated as the estimated average percentage of people in a county who are pro-environment, based on the survey conducted by YPCCC. The list of questions is presented in Appendix 3.B. In columns (1) and (2), we proxy for local investor base using *Investor Base1* and partition the sample based on the median; in columns (3) and (4), we proxy for local investor base using *Investor Base2* and partition the sample based on the median. Investor Base1 /Investor Base2 are calculated as the total book equity/total number of firms divided by total income in a county. The t-statistics are reported in parentheses and standard errors are clustered at firm level. ***, **, * correspond to statistical significance at the 1%, 5%, and 10% levels, respectively. Variable definitions are reported in Appendix 3.A.

	Investor Base 1		Investor Base 2	
	Low	High	Low	High
	(1)	(2)	(3)	(4)
Pro-environmental Beliefs	0.004	-0.162***	0.003	-0.189***
	(0.06)	(-2.67)	(0.05)	(-3.04)
Sales	0.394***	0.246***	0.401***	0.210***
	(4.91)	(3.22)	(5.53)	(2.60)
Male	-0.919	-15.030	5.316	-8.981
	(-0.11)	(-0.85)	(0.62)	(-0.56)
White	-0.429	-1.252	-0.023	-3.000
	(-0.34)	(-0.68)	(-0.02)	(-1.56)
Unemployment	3.654	14.542	2.708	13.643
	(0.35)	(1.01)	(0.25)	(0.92)
GDP Per Capita	0.129	0.591	0.372	0.752
	(0.30)	(0.87)	(1.00)	(1.18)
GDP Growth	0.524	-2.830	0.507	-3.421
	(0.40)	(-1.26)	(0.39)	(-1.40)
Democrat	-0.743*	-0.230	-0.566	-0.195
	(-1.80)	(-0.60)	(-1.47)	(-0.51)
Constant	-0.285	13.258	-6.321	12.119
	(-0.04)	(1.15)	(-0.95)	(1.09)
Plant Industry × Year FE	Yes	Yes	Yes	Yes
Plant State × Year FE	Yes	Yes	Yes	Yes
Firm × Year FE	Yes	Yes	Yes	Yes
Adjusted R^2	0.387	0.316	0.371	0.333
N	8577	8678	8602	8653

Table 3.5: The Role of Local Stakeholders

This table reports results on the role of local stakeholders in the relation between local environmental beliefs and plant-level toxic emissions. The dependent variable across all columns is *Emissions*, calculated as the natural logarithm of one plus total emission at plant level. The main independent variable is *Pro-environmental Beliefs*, calculated as the estimated average percentage of people in a county who are pro-environment, based on the survey conducted by YPCCC. The list of questions is presented in Appendix 3.B. In columns (1) and (2), we proxy for local stakeholders by the number of local plants, excluding the focal plant, and partition the sample based on the median; in columns (3) and (4), we proxy for local stakeholders by the number of local employees, excluding the employees in the focal plant, and partition the sample based on the median; in columns (5) and (6), we proxy for local stakeholders by the local population and partition the sample based on the median. The t-statistics are reported in parentheses and standard errors are clustered at firm level. ***, **, * correspond to statistical significance at the 1%, 5%, and 10% levels, respectively. Variable definitions are reported in Appendix 3.A.

	Local Plants		Local Employees		Local Population	
	Low	High	Low	High	Low	High
	(1)	(2)	(3)	(4)	(5)	(6)
Pro-environmental Beliefs	-0.004	-0.142***	-0.028	-0.139***	-0.000	-0.100*
	(-0.06)	(-2.69)	(-0.50)	(-2.66)	(-0.00)	(-1.90)
Sales	0.397***	0.244***	0.383***	0.264***	0.369***	0.253***
	(4.68)	(3.36)	(4.52)	(4.16)	(4.68)	(3.48)
Male	4.425	-27.410	-1.450	-24.389	-3.511	-19.324
	(0.49)	(-1.27)	(-0.17)	(-1.32)	(-0.37)	(-0.98)
White	-0.798	-1.040	-0.708	-1.299	-0.599	-0.836
	(-0.58)	(-0.60)	(-0.55)	(-0.80)	(-0.48)	(-0.51)
Unemployment	-8.217	27.322*	-6.077	21.979*	-12.876	22.590*
	(-0.69)	(1.96)	(-0.50)	(1.80)	(-1.16)	(1.77)
GDP Per Capita	0.278	0.718	0.271	0.758	0.238	0.665
	(0.69)	(1.18)	(0.70)	(1.33)	(0.64)	(1.10)
GDP Growth	0.278	-0.764	-0.018	-1.194	0.622	-0.219
	(0.24)	(-0.28)	(-0.02)	(-0.49)	(0.54)	(-0.11)
Democrat	-0.521	-0.005	-0.730	0.036	0.226	-0.219
	(-1.31)	(-0.01)	(-1.55)	(0.10)	(0.59)	(-0.71)
Constant	-3.331	15.853	1.076	13.855	1.375	9.934
	(-0.43)	(1.36)	(0.16)	(1.33)	(0.17)	(0.94)
Plant Industry × Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Plant State × Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Firm × Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted R^2	0.362	0.355	0.370	0.359	0.355	0.375
N	8493	8762	8542	8713	8540	8715

Table 3.6: Local Environmental Beliefs and EPA Enforcement

This table reports the results that examine the relation between local environmental beliefs and the amount of penalty for EPA formal enforcements, using the sample of plants that have received EPA formal enforcements. The dependent variable is *Penalty*, calculated as the natural logarithm of one plus the amount of penalty a plant receives. The main independent variable is *Pro-environmental Beliefs*, calculated as the estimated average percentage of people in a county who are pro-environment, based on the survey conducted by YPCCC. The list of questions is presented in Appendix 3.B. The t-statistics are reported in parentheses and standard errors are clustered at firm level. ***, **, * correspond to statistical significance at the 1%, 5%, and 10% levels, respectively. Variable definitions are reported in Appendix 3.A.

	(1)	(2)
Pro-environmental Beliefs	0.083*	0.155*
	(1.70)	(1.91)
Sales	0.087	0.062
	(0.91)	(0.50)
Male	-18.620	-2.150
	(-0.86)	(-0.10)
White	-0.978	-1.863
	(-0.75)	(-1.06)
Unemployment	-4.691	-19.923
	(-0.66)	(-1.15)
GDP Per Capita	-2.678	-2.662
	(-0.57)	(-0.45)
GDP Growth	-0.243	-0.243
	(-0.44)	(-0.35)
Democrat	-1.080*	-1.537**
	(-1.91)	(-2.00)
Constant	14.998	4.868
	(1.14)	(0.35)
Plant Industry × Year FE	No	Yes
Adjusted R^2	0.004	-0.018
N	537	537

Table 3.7: Local Environmental Beliefs and Environmental Lawsuits

This table reports results on the relation between local environmental beliefs and environmental lawsuits, using court-year level regressions. The dependent variable in Panel A is *Judge Support*, calculated as the number of environmental lawsuits in which decisions were favorable to plaintiffs divided by the total number of environmental lawsuits. The dependent variable in Panel B is *Pro-environmental Lawsuits*, defined the logarithm of the number of environmental lawsuits filed at court level; The key independent variable across two panels is *Pro-environmental Beliefs,* calculated as the estimated average percentage of people in a county who are pro-environment, based on the survey conducted by YPCCC. The list of questions is presented in Appendix 3.B. The t-statistics are reported in parentheses and standard errors are clustered at court level. ***, **, * correspond to statistical significance at the 1%, 5%, and 10% levels, respectively. Variable definitions are reported in Appendix 3.A.

Panel A: Local Environmental Beliefs and Court Support

	(1)	(2)	(3)
Pro-environmental Beliefs	0.063***	0.087***	0.053*
	(3.77)	(2.92)	(1.97)
Liberal	0.157	0.121	-0.159
	(0.51)	(0.39)	(-0.56)
Male	0.182	0.396	-37.886***
	(0.02)	(0.05)	(-3.04)
White	0.529	0.608	2.115*
	(0.95)	(1.07)	(1.90)
Unemployment	11.934***	12.635*	-10.614**
	(2.66)	(1.96)	(-2.48)
GDP Per Capita	0.624**	0.591*	0.302
	(2.23)	(1.94)	(0.65)
GDP Growth	6.100***	3.609*	-1.924
	(3.17)	(1.90)	(-1.56)
Democrat	0.180	-0.073	0.328
	(0.34)	(-0.12)	(0.66)
Constant	-10.103***	-10.956***	12.937**
	(-3.02)	(-3.29)	(2.46)
Year FE	No	Yes	Yes
State FE	No	No	Yes
Adjusted R^2	0.268	0.290	0.612
N	630	630	630

Panel B: Local Environmental Beliefs and the Number of Environmental Lawsuits

	(1)	(2)	(3)
Pro-environmental Beliefs	0.009*	0.021***	0.029**
	(1.76)	(2.83)	(2.04)
Liberal	0.155*	0.162**	-0.026
	(1.92)	(2.06)	(-0.29)
Male	-2.959	-3.137	-8.303**
	(-1.47)	(-1.51)	(-2.62)
White	0.113	0.143	0.313
	(0.86)	(1.07)	(1.00)
Unemployment	0.468	0.750	1.336
	(0.49)	(0.59)	(1.11)
GDP Per Capita	-0.061	-0.081	0.038
	(-0.75)	(-0.95)	(0.18)
GDP Growth	-0.117	-0.568	-0.181
	(-0.23)	(-0.97)	(-0.30)
Democrat	-0.058	-0.196	-0.186
	(-0.52)	(-1.50)	(-1.13)
Constant	1.798**	1.514**	2.216
	(2.24)	(2.01)	(0.67)
Year FE	No	Yes	Yes
State FE	No	No	Yes
Adjusted R^2	0.021	0.031	0.072
N	549	549	549

Table 3.8: Local Environmental Beliefs and Onsite & Offsite Emissions

This table reports the results that examine the relation between local environmental beliefs and plant-level onsite and offsite toxic emissions, respectively. Our sample includes 16,227 plant-year observations in the period from 2015 to 2021. The dependent variable in columns (1) is *Onsite Emissions*, calculated as the natural logarithm of one plus total onsite emission in year t+1 at plant level; the dependent variable in columns (2) is *Offsite Emissions*, calculated as the natural logarithm of one plus total offsite emission in year t+1 at plant level. Onsite emissions are defined as total emissions that were generated and released at the focal plant; offsite emissions are defined as total emissions that were generated at the focal plant but released at other plants. The main independent variable is *Pro-environmental Beliefs*, calculated as the estimated average percentage of people in a county who are pro-environment, based on the survey conducted by YPCCC. The list of questions is presented in Appendix 3.B. The t-statistics are reported in parentheses and standard errors are clustered at firm level. ***, **, * correspond to statistical significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)
Pro-environmental Beliefs	-0.086**	-0.016
	(-2.30)	(-0.50)
Sales	0.311***	0.211***
	(5.29)	(3.70)
Male	-1.704	2.771
	(-0.20)	(0.37)
White	-0.457	-0.435
	(-0.47)	(-0.44)
Unemployment	11.980	-2.401
	(1.46)	(-0.29)
GDP per Capita	0.329	0.409*
	(1.16)	(1.88)
GDP Growth	0.221	-2.141**
	(0.24)	(-2.31)
Democrat	-0.355	0.080
	(-1.45)	(0.33)
Constant	2.497	-4.215
	(0.38)	(-0.87)
Plant Industry × Year FE	Yes	Yes
Plant State × Year FE	Yes	Yes
Firm × Year FE	Yes	Yes
Adjusted R^2	0.386	0.264
N	17255	17255

Table 3.9: Local Environmental Beliefs and Emission Redistribution

This table reports results on the relation between local environmental beliefs and internal emission redistributions following plant-level EPA formal enforcement actions, using plant-year regressions. The dependent variable is *Emissions*, calculated as the natural logarithm of one plus total emission at plant level. *Enforcement* is a dummy variable that equals one when the observation is in the period of t to t+2 year, where year t is the year in which the focal plant receives the EPA formal enforcement action, and zero otherwise. *Peer Enforcement* is a dummy variable that equals one if observation is in the period of t to t+2 year, where year t is the year in which other plants of the same firm receive the EPA formal enforcement, and zero otherwise. *Pro-Environmental Beliefs* is calculated as the estimated average percentage of people in a county who are pro-environment, based on the survey conducted by YPCCC. The list of questions is presented in Appendix 3.B. The t-statistics are reported in parentheses and standard errors are clustered at firm level. ***, **, * correspond to statistical significance at the 1%, 5%, and 10% levels, respectively. Variable definitions are presented in Appendix 3.A.

	Low	High
	(1)	(2)
Enforcement	-0.080	-0.298**
	(-0.74)	(-2.57)
Peer Enforcement	0.172*	0.056
	(1.91)	(0.89)
Sale	0.022	0.010
	(0.74)	(0.48)
Male	-37.992**	24.463
	(-2.03)	(0.95)
White	-17.233*	1.264
	(-1.66)	(0.22)
Unemployment	-2.431	2.040
	(-0.85)	(0.66)
GDP Per Capita	0.401	0.421
	(1.35)	(0.42)
GDP Growth	0.152	1.000
	(0.31)	(1.29)
Democrat	0.164	0.179
	(0.44)	(1.59)
Constant	37.492***	-10.234
	(2.61)	(-0.54)
Year FE	Yes	Yes
Plant FE	Yes	Yes
Adjusted R^2	0.921	0.920
N	5420	5291

Table 3.10: Alternative Measure of Toxic Emissions

This table reports the relation between local environmental beliefs and plant-level toxic chemical emissions. Dependent variables are *Chemical Type*, defined as the total number of toxic chemical types in year t+1 at plant level, obtained from EPA's TRI database. The main independent variables, *Pro-environmental Beliefs*, is calculated as the estimated average percentage of people in a county who are pro-environment, based on the survey conducted by YPCCC. The sample includes TRI-NETS Merged Plants, owned by public companies. The t-statistics reported in the parentheses are clustered at firm level. ***, **, * correspond to statistical significance at the 1%, 5%, and 10% levels, respectively. Variable definitions are reported in Appendix 3.A.

	(1)	(2)	(3)	(4)	(5)	(6)
Pro-environmental Beliefs	-0.139***	-0.173***	-0.174***	-0.136***	-0.115***	-0.099*
	(-3.41)	(-3.80)	(-3.80)	(-2.68)	(-2.66)	(-1.81)
Sale	0.232*	0.355***	0.356***	0.345***	0.299***	0.281***
	(1.81)	(3.66)	(3.67)	(3.63)	(3.13)	(3.05)
Male	23.374	13.773	13.941	4.278	12.862	2.181
	(1.45)	(1.23)	(1.23)	(0.37)	(1.17)	(0.22)
White	-2.621	-1.310	-1.261	-1.466	-0.698	-0.767
	(-1.43)	(-0.91)	(-0.87)	(-0.90)	(-0.69)	(-0.60)
Unemployment	20.029***	26.026***	26.781***	47.710***	17.121**	32.553***
	(3.45)	(3.21)	(3.19)	(3.92)	(2.49)	(2.77)
GDP Per Capita	1.942***	1.502***	1.516***	1.285***	1.001***	0.747*
	(3.60)	(3.93)	(3.92)	(3.00)	(3.00)	(1.92)
GDP Growth	-1.474	-2.228	-2.315	-0.681	-1.920	-0.662
	(-0.87)	(-1.53)	(-1.55)	(-0.44)	(-1.29)	(-0.45)
Democrat	-0.316	0.125	0.138	0.178	-0.277	-0.275
	(-0.93)	(0.36)	(0.39)	(0.50)	(-0.87)	(-0.83)
Constant	-22.341**	-14.501**	-14.800**	-10.457	-10.972	-4.338
	(-2.00)	(-2.01)	(-2.03)	(-1.20)	(-1.64)	(-0.53)
Plant Industry FE		Yes				
Year FE		Yes				
Plant Industry × Year FE			Yes	Yes	Yes	Yes
Plant State × Year FE				Yes		Yes
Firm × Year FE					Yes	Yes
Adjusted R^2	0.026	0.236	0.229	0.231	0.426	0.430
N	17255	17255	17255	17255	17255	17255

Appendix 3.A: Variable Definitions

Variable	**Definition**	**Data Source**
Emission	Logarithm of total toxic releases at plant level.	TRI Database
Chemical Type	The number of toxic chemical types that a plant releases.	TRI Database
Sale	The natural logarithm of sales at plant level.	NETS Database
Pro-environmental Beliefs	The estimated average percentage of people in a county who are pro-environment, based on the survey conducted by YPCCC.	YPCCC Survey
Male	The proportion of male residents at county level.	American Community Survey
White	The proportion of white residents at county level.	American Community Survey
Unemployment	The rate of unemployment at county level.	American Community Survey
GDP per Capita	The logarithm of GDP per capital at county level.	American Community Survey
GDP Growth	GDP growth rate at county level, defined as GDP in year t divided by GDP in year t-1 and minus 1.	American Community Survey
Democrat	A dummy variable that takes 1 if the support rate for Democratic presidential candidates exceeds 50%. Support rates for non-election years are linearly interpolated between two elections.	MIT Election Lab
Damage	A dummy indicator at county level which takes the value of 1 if the total monetary loss from natural disasters exceeds a certain threshold (20 million or 50 million), and zero otherwise.	Spatial Hazard Events and Losses Database (SHELDUS)
Environmental Lawsuits	The logarithm of the number of environmental lawsuits filed at each federal circuit court.	Federal Judicial Center
Judge Support	The number of environmental lawsuits in which decisions were favorable to plaintiffs divided by the total number of environmental lawsuits at each federal circuit court.	Federal Judicial Center

Liberal	The number of federal judges who are nominated by democratic presidents divided by the total number of federal judges at each federal circuit court.	Federal Judicial Center
Onsite Inspections	The number of total onsite EPA inspections that a plant receives.	EPA ECHO Database
Offsite Inspections	The number of total offsite EPA inspections that a plant receives.	EPA ECHO Database
Fraction of Formal Enforcement	The number of formal EPA enforcement actions received by a plant, divided by the total number of inspections received by the plant.	EPA ECHO Database
Fraction of Informal Enforcement	The number of informal EPA enforcement actions received by a plant, divided by the total number of inspections received by the plant.	EPA ECHO Database
Penalty	The natural logarithm of one plus the amount of penalty resulting from a plant receives.	EPA ECHO Database
Enforcement	A dummy variable that equals one when the observation is in the period of t to t+2 year, where year t is the year in which the focal plant receives the EPA formal enforcement action, and zero otherwise.	EPA ECHO Database
Peer Enforcement	A dummy variable that equals one if observation is in the period of t to t+2 year, where year t is the year in which other plants of the same firm receive the EPA formal enforcement action, and zero otherwise.	EPA ECHO Database
Onsite Emissions	The natural logarithm of one plus total onsite emission (generated and released at the focal plant) in year t+1 at plant level;	TRI Database
Offsite Emissions	The natural logarithm of one plus total offsite emission (generated at the focal plant but transported to other locations.)in year t+1 at plant level;	TRI Database

Appendix 3.B: Questions in the YPCCC Estimation Model

YPCCC utilized a proprietary statistical model to estimate the percentage of the adult population who agree/support a particular topic related to climate change and environmental issues. The statistical model used to estimate opinions incorporates original survey data and combines it with additional information such as the percentage of people in each geographic area who voted for a particular presidential candidate, along with economic and demographic variables that predict Americans' climate attitudes. The following table summarizes all the relevant climate questions that serve as the basis for opinions.

Variable Name	Variable Description
affectweather	Estimated percentage who somewhat/strongly agree that global warming is affecting the weather in the United States
citizens	Estimated percentage who think citizens themselves should be doing more/much more to address global warming
CO2limits	Estimated percentage who somewhat/strongly support setting strict limits on existing coal-fired power plants
congress	Estimated percentage who think Congress should be doing more/much more to address global warming
consensus	Estimated percentage who believe that most scientists think global warming is happening
corporations	Estimated percentage who think corporations and industry should be doing more/much more to address global warming
devharm	Estimated percentage who think global warming will harm people in developing countries a moderate amount/a great deal
discuss	Estimated percentage who discuss global warming occasionally or often with friends and family
drillANWR	Estimated percentage who somewhat/strongly support drilling for oil in the Arctic National Wildlife Refuge
drilloffshore	Estimated percentage who somewhat/strongly support expanding offshore drilling for oil and natural gas off the U.S. coast
exp	Estimated percentage who somewhat/strongly agree that they have personally experienced the effects of global warming
fundrenewables	Estimated percentage who somewhat/strongly support funding research into renewable energy sources
futuregen	Estimated percentage who think global warming will harm future generations a moderate amount/a great deal

governor	Estimated percentage who think their governor should be doing more/much more to address global warming
gwvoteimp	Estimated percentage who say global warming should be a high priority for the next president and Congress
happening	Estimated percentage who think that global warming is happening
harmplants	Estimated percentage who think global warming will harm plants and animal species a moderate amount/a great deal
harmUS	Estimated percentage who think global warming will harm people in the US a moderate amount/a great deal
human	Estimated percentage who think that global warming is caused mostly by human activities
localofficials	Estimated percentage who think their local officials should be doing more/much more to address global warming
mediaweekly	Estimated percentage who hear about global warming in the media at least weekly
personal	Estimated percentage who think global warming will harm them personally a moderate amount/a great deal
president	Estimated percentage who think the President themselves should be doing more/much more to address global warming
prienv	Estimated percentage who think protecting the environment is more important than economic growth, even if it reduces economic growth
priority	Estimated percentage who say a candidate's views on GW are important to their vote
rebates	Estimated percentage who somewhat/strongly support providing tax rebates for people who purchase energy-efficient vehicles or solar panels
reducetax	Estimated percentage who somewhat/strongly support requiring fossil fuel companies to pay a carbon tax and use the money to reduce other taxes (such as income tax) by an equal amount
regulate	Estimated percentage who somewhat/strongly support regulating CO_2 as a pollutant
supportRPS	Estimated percentage who somewhat/strongly support requiring utilities to produce 20% electricity from renewable sources
taxdividend	Estimated percentage of population who somewhat/strongly support a carbon tax if refunded to every American household
teachGW	Estimated percentage who somewhat/strongly agree that schools should teach about the causes, consequences, and potential solutions to global warming

timing	Estimated percentage who think global warming will start to harm people in the United now/within 10 years
trustclimsciSST	Estimated percentage who somewhat or strongly trust climate scientists as a source of information about global warming
worried	Estimated percentage who are somewhat/very worried about global warming

Chapter 4

Information on Collateral Value and Bank Mortgage Lending[32]

4.1 Introduction

Collateral value plays a critical role in bank lending decisions. Seminal papers by Bernanke and Gertler (1986) and Kiyotaki and Moore (1997) argue that improvements in collateral values mitigate credit constraints for borrowers. This idea, formally theorized as the collateral channel, posits that information asymmetry between banks and borrowers will be alleviated by a high collateral value. Empirical studies later investigate the implications of collateral value on loan contracting and find supporting evidence for the collateral lending channel. Cerqueiro, Ongena, and Roszbach (2016) document that banks respond to reduced collateral values by increasing interest rates, tightening credit limits, and reducing the intensity of its monitoring of borrowers and collaterals. Adelino, Schoar, and Severino (2015) show that areas with rising house prices experiences a significantly bigger increase in small business starts compared with areas that did not see an increase in house prices. Schmalz, Sraer, and Thesmar (2017) find that an increase in collateral value leads to a higher probability for homeowners to become an entrepreneur. Fougère, Lecat, and Ray (2019) argue that real estate prices affect firms' borrowing capacities through collateral lending channel.

Unlike corporate lending where collaterals may not be required, their role in securing banks in case of defaults is of primary importance in residential real estate. However, in contrast to fruitful

[32] I am deeply grateful for the comments and suggestions on this paper provided by Yuhai Xuan, Mingzhu Tai, Jinfei and Wanyi Wang.

research findings on collateral channel in corporate and entrepreneurial finance, it is surprising that very few existent papers focus on the residential real estate market. This article aims to fill this gap. Using nationwide data of residential real estate transactions and mortgage originations, we empirically test whether change of collateral value plays a role in bank mortgage lending decisions.

This paper is closely related to Stroebel (2016) which, to our knowledge, is the only paper that investigates collateral value in residential real estate. Using deed records in Arizona, Stroebel (2016) studied newly developed properties and found that vertically integrated lenders have superior information about the construction quality of individual homes and exploit this information to lend against higher quality collateral. An important distinction between our paper and Stroebel (2016) is that our paper quantitatively measures change of collateral value and focuses on whether banks take this information into consideration when making lending decisions, whereas Stroebel (2016) emphasizes more the implications of information asymmetry on ex-post mortgage contracting.

We focus on the likelihood of bank retention in home transactions, i.e. whether the bank continues to finance the same property after a sale. In the baseline results, we find a significantly positive association between the likelihood of bank retention and value appreciation of the property. In particular, we attribute this finding to banks' information on value change of properties that they finance. Based on collateral lending channel, greater appreciation in property value mitigates information friction between banks and borrowers.

If banks exploit information on collateral value in lending decisions, a natural question would be whether such information at property level has spillover effect within the neighborhood. We empirically investigate whether value appreciation of properties that a bank finances within a neighborhood affects its lending decision to a property located in the same neighborhood. We find significantly positive results, implying that banks also exploit information on neighboring properties when making lending decisions.

Next, we examine whether information on the collateral value is proprietary to the financing bank. In corporate lending, the lending bank acquires a firm-specific information advantage over other non-lenders.[33] A parallel argument in residential mortgage lending would then posit that banks own private information on borrowers and collaterals. However, owing to increasing transparency and accessibility of real estate transaction data in the past decade,[34] banks may gain access to historical transaction details of properties which they do not finance. To empirically test if banks are able to exploit information on value change of properties that they do not finance, we investigate the association between lending decisions and value change of all properties within the neighborhood, including properties not financed by the bank in question. We find evidence that banks do not base their lending decisions on the value of properties that they do not finance. We propose two different reasons for this result. First, banks may simply not have access to transaction data. This might be true for the early years of our data coverage (e.g. late 1990s). An alternative explanation could be that transaction data is public but banks grudge paying the search cost. Using data in recent years, subsample analysis lends support to the latter explanation.

We further analyze implications of asymmetric information. Stroebel (2016) argues that information asymmetry on collateral value could possibly lead to adverse selection in mortgage lending. Since we find that banks are more likely to continue financing properties whose value appreciate more in the past, adverse selection implies that change of mortgage lender in a transaction signals poorer collateral quality. To compensate for higher risk of default due to poorer collateral

[33] The idea that lending banks learn more information about borrowers has been widely discussed in existing literature. See Fama (1985), Sharpe (1990), Rajan (1992), Schenone (2010).

[34] An anecdotal example is the advent of online real estate marketplace companies, such as Redfin(2004), Trulia(2005), Zillow(2006) etc..

quality, banks will exert more stringent mortgage policies on properties they did not finance before, ceteris paribus. We empirically test for these implications and find that in the circumstance of a change of lender, banks charge higher interest rates and originate smaller mortgage amounts.

Finally, we investigate whether these properties and mortgages indeed underperform in the future. The granular nature of the data enables us to examine future value change by focusing on repeat sales of the same properties. We find that there is no ex-post difference in performance between properties with and without lender change. In terms of mortgage performance, we find that the likelihood of default within five years after a transaction is negatively correlated with lender change. These findings are consistent with tighter lending criteria (i.e. higher interest rate and lower loan amount) of uninformed banks to properties that they did not finance before.

A widely discussed challenge in cleanly identifying the causal effect of collateral stems from the endogeneity of collateral value change. Specifically, increased collateral value facilitates lending but higher collateral value can be the result of improvements in local economic conditions (Iacoviello (2005)). Using big data at property-level across the US, we circumvent this empirical difficulty by adding a large set of restrictive fixed effects at bank, quarter and census tract level.

To our knowledge, this paper is the first to link collateral value with information advantage and mortgage lending decisions at property level. Traditional views on residential mortgage lending typically assume that banks rely on hard information to gauge borrower's repayment capacity when making lending decisions (Munnell, Tootell, Browne, and McEneaney (1996)). Such information includes borrower's current and future expected income, credit score, marital status, asset liquidity, etc. Early studies in this area also found that borrower's demographic characteristics, such as race and gender, play an important role in mortgage lending and that banks discriminate against female and minorities in mortgage lending (Black, Schweitzer, and Mandell (1978); Black, Robinson, Schlottmann, and Schweitzer (2003); Robinson (2002);Dymski, Hernandez, and Mohanty (2013)).

This article contributes to a few strands of literature. First, it contributes to the literature on the effect of collateral value on bank lending (Jimenez, Salas, and Saurina (2006); Adelino, Schoar, and Severino (2015); Stroebel (2016); Chaney, Sraer, and Thesmar (2012); Luck and San- tos (2019)). In addition to evidence under corporate and entrepreneurial context, we document that collateral value change has a causal effect on banks' mortgage lending decision in residential real estate. The unique setting and granularity of data enables us to overcome traditional difficulties in identifying the collateral lending channel.

Second, this paper sheds light on a new lending relationship that was surprisingly obscure in existing literature, i.e. bank-collateral relationship. The theory of financial intermediation suggests that relationship lending can mitigate information asymmetry between lenders and borrowers (Fama (1985)). Empirical literature focuses predominantly on the determinants and outcomes of bank-firm relationships (Bharath, Dahiya, Saunders, and Srinivasan (2007); En- gelberg, Gao, and Parsons (2012); Bharath, Dahiya, Saunders, and Srinivasan (2011); Schwert (2018)), while research on relationship lending between banks and other entities is still at its infancy. Karolyi (2018) disentangles bank-CEO relationships from general bank-firm relationships and finds that the former affects loan contracting. Brown and Hoffmann (2013) explore bank-household relationships in mortgage lending. Our paper complements these studies by identifying a key determinant that form bank-collateral relationship and exploring its implications on mortgage contracting.

In addition, it builds on literature which studies the neighborhood effect in bank lending decisions. There was a heated debate on redlining in residential mortgage lending in terms of neighborhood characteristics (Lang and Nakamura (1993); Holmes and Horvitz (1994); Schill and

Wachter (1994); Berkovec, Canner, Gabriel, and Hannan (1994); Cho and Megbolugbe (1996); Avery, Beeson, and Sniderman (1999)). In this paper, we also find evidence that banks base their lending decisions on information of properties in the neighborhood.

Finally, this article is related to a rich research field that focuses on the use of collateral as a consequence of information asymmetry. Traditional theory motivates collateral as a mechanism that mitigates information asymmetry between lenders and borrowers (Bester (1985); Chan and Kanatas (1985); Igawa and Kanatas (1990)). Empirical research later finds supporting evidence for such a channel (Sufi (2007); Berger, Espinosa-Vega, Frame, and Miller (2011)). In contrast to existing literature which mainly focuses on information asymmetry between lenders and borrowers, we look into adverse selection within lenders induced by heterogenous information on collateral value. Our findings are consistent with the hypothesis that lenders are informed differently of the collateral value and information asymmetry results in adverse selection in mortgage lending decisions.

The rest of the paper proceeds as follows. We develop empirical hypotheses in section 4.2 and describe data and sample filtering in section 4.3. Section 4.4 presents empirical methodology and baseline results. Section 4.5 discusses potential extensions. Section 4.6 implements robustness checks and section 4.7 concludes.

4.2 Hypothesis Development

Consider a house financed by bank A is now on sale. As the incumbent mortgage lender, bank A owns some proprietary information on the property. One of the most important pieces of information is the sale price of the property in the previous transaction. Bank A can observe the previous transaction price without any search cost. If bank A finds that the property appreciates more than its peers within the same neighborhood, will bank A be more likely to continue financing the property?

Some homes may have construction defects (such as poorly laid foundation) which are unobservable to banks at the beginning, but which will be gradually revealed and affect house prices. For banks, greater appreciation of collateral value signals a better collateral quality and thus indicates lower risk of future default, ceteris paribus. According to collateral theory, a higher collateral quality mitigates information asymmetry between lenders and borrowers. Therefore, we hypothesize that bank A will be more likely to continue financing the property.

Hypothesis 1: *Banks are more likely to continue financing properties which experience greater value appreciation between two sales.*

If banks exploit their information on value change of properties they finance, one natural question to ask is whether such information has spillover effect in the neighborhood. Specifically, greater value appreciation within a neighborhood acts as a good indicator for property (collateral) value in the neighborhood, probably driven by some unobservable neighborhood-level factors. This intuition has been adopted in papers that investigate redlining in mortgage origination (Lang and Nakamura (1993)), which argue that number of house transactions in a neighborhood is associated with potential collateral risk. Based on this idea, when properties within a neighborhood have a higher return, we hypothesize that banks will be more likely to finance a property in the same neighborhood.

Hypothesis 2: *Banks are more likely to continue financing properties located in the neighborhood where the properties they finance experience greater value appreciation between two sales.*

If our hypothesis 2 is true, a deeper question would then be whether banks can exploit price information on properties they do not finance. As sale price in housing market becomes increasingly transparent and accessible over the past decade, banks may have access to historical sale price data on properties they do not finance. However, considering search cost of information, it is unclear ex-ante whether banks will search and exploit it. Since we do not have an unambiguous anticipation for this hypothesis, we separate it into two mutually exclusive ones.

Hypothesis 3A: *Information on historical prices is accessible and banks exploit such information on properties they do not finance when making mortgage lending decisions.*

Hypothesis 3B: *Banks fail to exploit price information on properties they do not finance when making mortgage lending decisions.*

It should be noted that if hypothesis 3B holds, it might be that banks fail to exploit price information because this information is not public (especially for early years, such as 1990s) or because they do not incur the search cost to collect the information. Due to data limitation, we will provide suggestive but not conclusive evidence to show which channel dominates. In section 4.4, we will sequentially test each of these hypotheses under a unique setting of mortgage lending. In section 4.5, we will discuss some important implications of these hypotheses.

4.3 Sample Selection

4.3.1 Data Source

We obtain data on real estate transactions from Zillow's Assessor and Real Estate Database (ZTRAX).[35] The ZTRAX database contains information on the near universe of housing transactions drawn from county records across the country over the past three decades. According to Zillow, this

[35] Data provided by Zillow through the Zillow Transaction and Assessment Dataset (ZTRAX). More information on accessing the data can be found at http://www.zillow.com/ztrax. The results and opinions are those of the author(s) and do not reflect the position of Zillow Group.

database is the largest real estate database in the US as it covered 1) more than 400 million detailed public records across 2,750 U.S. counties; 2) more than 20 years of deed transfers, mortgages, foreclosures, auctions, property tax delinquencies; 3) Property characteristics, geographic information and prior valuations for approximately 150 million parcels in 3,100 counties nationwide.

The ZTRAX database is comprised of two main datasets, namely ZAsmt (Assessment) dataset and ZTrans (Transaction) dataset. ZAsmt dataset contains property assessment records, which include information on property specific characteristics, such as address, zipcode, building area, lot size etc. ZTrans dataset contains event records at property level. There are four classes of events, i.e. deed, mortgage, deed concurrent with mortgage and foreclosure. We collect sale price information from transaction events, loan and lender information from mortgage events and default information from foreclosure events.

The second database we use is the Home Mortgage Disclosure Act's (HMDA) Loan Application Registry. HMDA is a United States federal law that requires certain financial institutions to provide mortgage data to the public. This dataset includes information on every mortgage application in Metropolitan Statistical Areas at calendar year level. Specifically, it contains information on bank ID, application outcome, loan amount, bank as well as a few applicants' demographic characteristics.

4.3.2 Sample Construction

We focus on all arms-length transactions[36] in the United States from January 1995 to December 2017. We cleanse the raw ZTRAX data carefully and the details can be found in the appendix.[37] To begin with, we drop mortgages made by all non-bank institutions, such as mortgage

[36] To identify arm's-length transactions, we delete all intra-family transactions and partial-interest transfers. Intra-family transactions are traded by family members and in a partial-interest property, ownership is broken down into fractional interests from a single, unified ownership. Therefore, in these two circumstances, the transaction price may not reflect the arm's-length market price.
[37] Appendix 4.A includes details of the sample selection process and criteria.

companies, lending companies, credit unions etc. For each mortgage, we match bank names with their corresponding RSSD IDs.[38] We drop mortgage lenders whose RSSD numbers cannot be found, and further drop mortgages with loan amount less than $5,000 because these are unlikely to be mortgages originated for house purchase.

Following the sampling procedure mentioned above, we are able to identify 2,661,982 transaction events from 2,459,309 unique properties across the US from 1995 to 2017. By comparing the RSSD number between seller's lender and buyer's lender, we find that 2,320,202 observations (87.16%) come with a lender change, while 341,780 observations (12.84%) do not. For each property in the sample, we import its static property characteristics from ZAsmt dataset. Since ZAsmt dataset does not report census tract information for properties, we manually geocode all unique properties in the dataset using API by Census Bureau.[39] The geocoding process is based on property address and returns property geocoding information, including census tract, census block and coordinates. For all 2,459,309 unique properties, we successfully geocode 2,276,313.

4.3.3 Variables and Summary statistics

We carefully scrutinized missing data and extreme values. In order to eliminate the effect of possibly spurious outliers, all of the continuous variables are winsorised at the 1% and 99% levels. Table 4.1 presents the definitions of all variables that we include in the regressions. To quantitatively measure value change of collateral, we focus on repeat sales where we can observe the previous or subsequent arm's-length transaction price and then calculate the annualized returns within each holding period based on sale price information and time interval between two sales. We restrict the

[38] RSSD ID is a unique identifier assigned to financial institutions by the Federal Reserve.
[39] The website is https://geocoding.geo.census.gov/geocoder/geographies/addressbatch?form.

time interval between two sales to at least six months in order to avoid possible measurement errors from frequent trading.

Property characteristics include building area (square feet), lot size (square feet), whether the property has a pool/garage, and the life of the property. We take logarithm of building area and lot size and calculate the year interval between transaction year and construction year to denote property life. We use the year when the property was remodeled if this information is available to replace the construction year. Mortgage characteristics include loan-to-value ratio (LTV), loan-to-income ratio (LTI), initial interest rate and mortgage maturity. Since absolute mortgage amount is highly correlated with the property size, we calculate mortgage per unit area[40] to indicate relative mortgage amount across properties. Borrower characteristics come from HMDA dataset and include borrower income, race, and gender information, as well as whether there is a co-applicant.

Table 4.2 presents summary statistics of all these variables, which are classified into four groups, i.e. transaction, property, mortgage, and borrower information. In panel A of Table 4.2, we include the full sample and in panel B, the sample is merged with the HMDA dataset. The first three columns show the mean, standard deviation and median for transactions without lender change, and the next three columns show these statistics for transactions with lender change. The second from the right column presents the difference between these two groups and the last column shows the t-statistics of the difference.

A quick look at the summary statistics reveals a few interesting patterns. For properties without lender change, their annualized returns before transaction (*Last_ret*) are 0.8 percentage point (hereinafter pp) higher than properties to which the lender changes, and the difference is highly significant. This implies that past returns are negatively correlated with the likelihood of lender

[40] This is calculated as mortgage amount divided by building area (square foot).

change. In addition, we find that subsequent annualized returns (*Sub_ret*) are also higher for properties without lender change, although the magnitude becomes much smaller (only 0.2 pp). These two patterns are valid for both panels and they provide suggestive evidence that banks are more likely to continue lending to properties with a higher past return.

4.4 Empirical Analysis

In this section, we apply OLS models to investigate the effect of collateral value change on lending decisions. Thanks to the granular nature of the data, we can include a bunch of restrictive fixed effects to control unobservable heterogenous trends across time and locations. Specifically, we add location fixed effect at census tract level, the smallest geographic unit in most studies in real estate finance, and time fixed effect at quarter level. Since different regions may experience highly heterogenous trends across time, we include census tract*quarter fixed effect in the most robust specification. In addition, we also add bank fixed effect to control for bank level idiosyncratic characteristics.

4.4.1 Return on Specific Property

We first test the relation between the annualized return of a property in the previous holding period and the likelihood that the incumbent bank will continue to finance the property. If hypothesis 1 holds, we should be able to find a significant positive effect of property return on bank lending decisions. The model we estimate takes the following form:

$$Len\_change_{i,l,t} = \alpha_0 + \alpha_1 Last\_ret_{i,t} + Property\ Controls_i + Y_{c,t} + Z_l + \epsilon_{i,t} \quad (1)$$

where $Len\_change_{i,l,t}$ is a dummy indicator which takes the value of 1 if the incumbent bank l terminates lending to property *i* in a transaction at time *t*; $Last\_ret_{i,t}$ is the annualized return between preceding and current sale price for property *i* sold at time *t*; $Property\ Controls_i$ denotes the hedonic characteristics of property i, including property life, building area, garage, pool and lot size; $Y_{c,t}$ is a fixed effect of the interaction between census tract and time, which captures unobservable and heterogenous time-varying movements at census tract level, such as housing martket, socio-economic as well as demographic trends; Z_l is a bank fixed effect that captures all time-invariant characteristics of the incumbent bank, such as lending standards and specialization.

Table 4.3 presents the baseline regression results. We sequentially add controls for bank fixed effect, property characteristics and census tract-quarter fixed effect into the model. The coefficients of value change (*Last_ret*) are all significantly negative across specifications. In column (1) & (2), we add census tract and quarter fixed effects seperately. In column (3) & (4), we add census tract by quarter fixed effect. In column (4), the most robust specification where property fixed effect, bank fixed effect and census tract by quarter fixed effect are all included, the coefficient is significantly negative. Its magnitude implies that within census tract, when annualized returns increase by one standard deviation (0.12), the likelihood that incumbent banks continue lending to the property rises by 0.12 pp. This finding is consistent with our hypothesis 1.

4.4.2 Neighborhood Effect

In the interpretation of the findings in the baseline regression, one may argue that since we cannot observe and measure borrower's willingness to borrow from a bank, the ex-post lending outcome may be driven by demand-side (borrower) instead of supply-side (bank) effect. For example, buyers may have communications with sellers and thus get more familiar with incumbent financing banks. Therefore, the demand for credit would be tilted towards incumbent banks. If buyers' mortgage preference is also positively correlated with the past returns on property value,[41] the empirical results in Tabel 4.3 will be contaminated by omitted variable bias.

We provide two tests to rule out this confounding channel. First, in the robustness check section, we obtain borrower characteristics from HMDA dataset and use the HMDA-merged sample to re-estimate the model. The results are basically unchanged. Second, we propose a new perspective to verify the collateral channel by testing whether such effect exists in the neighborhood. Specifically, we test whether average returns of properties financed by a certain bank at census tract level affects the bank's subsequent lending decisions. If the effect in Table 4.3 is mainly driven by demand side factors from borrowers, the same effect will not be expected to exist in the neighborhood-level test. The model we estimate takes the following form:

$$Len\_change_{i,l,t} = \alpha_0 + \alpha_1 Last\_ret_{i,c,t-1} + Property\ Controls_i + Y_{z,t} + Z_l + \epsilon_{i,t} \quad (2)$$

The definitions of the variables are the same as before, but we replace the return of the specific property with $Last\_ret_{i,c,t-1}$, the average annualized return of all properties financed by the bank l at the census tract c in the previous quarter $t-1$. In addition, since the independent variable varies at census tract level, we control location fixed effect at a marginally larger geographic unit, i.e. zipcode level.

Table 4.4 presents the regression results of the model above. Across specifications, the coefficients are all significantly negative, consistent with the baseline results. In column (4), the most robust specification, the magnitude is larger than what we find in the baseline regression. This is probably because in this regression, we control laxer fixed effect at geographic unit, which leads to

[41] This assumption is unlikely because buyers cannot easily know the historical sale price of the house. Even if they can, it is hard for buyers to compare the return of the property with the overall return within a local region.

larger variations. The results in this model are consistent with the collateral channel we propose but inconsistent with the demand-side channel.

4.4.3 Private or Public Information

In the previous tests, we show that incumbent banks base their lending decisions on collateral value change of properties they finance. A related question to ask would be, do banks exploit similar information on properties that they do not finance? To test this hypothesis, we implement a placebo test in which we examine whether returns of properties financed by other banks in the neighborhood affect the lending decision. Specifically, we estimate the following model:

$$Len\_change_{i,l,t} = \alpha_0 + \alpha_1 Last\_ret_{i,c,t-1} + Property\ Controls_i + Y_{z,t} + Z_l + \epsilon_{i,t} \quad (3)$$

In this model, the independent variable, $Last\_ret_{i,c,t-1}$, is the average annualized return of all properties sold at census tract c in the previous quarter $t-1$. Since we focus on all properties in the neighborhood, there is no subscript *l* to denote specific bank.

Table 4.5 presents the regression results of the model above. Coefficients in column (1) is negative and marginally significant. However, when we further control property characteristics in column (2), the coefficient decreases and becomes insignificant. In contrast to the findings from Table 4.4, the results in Table 4.5 imply that banks fail to exploit information on properties they do not finance when making mortgage lending decisions. We propose two explanations. First, banks may not have access to historical sale price information of properties they do not finance. This might be the case for early years (such as 1990s) when property mortgage data is by and large private. Second, although information is public, banks have to take up the search cost. It could be rational for banks to ignore such information if the search cost outweighs the benefit. Unfortunately, given data limitation, we cannot tell safely which explanation dominates.

4.5 Discussions

4.5.1 Adverse Selection

This section discusses some important extensions of the basic idea about information exploitation in mortgage lending. If incumbent financing banks make lending decisions based on their superior information about the property, information asymmetry could possibly lead to adverse selection in mortgage lending. Theoretically, in a strategic game, if banks take it as common knowledge that properties with higher returns are more likely to be financed by incumbent banks, then financing termination of incumbent banks signals a lower return, possibly due to some property imperfections unobservable to uninformed banks. Therefore, to be compensated from a higher default risk, uninformed banks will likely exert stricter rules on mortgage. For instance, they might charge higher interest rates and grant lower mortgage amount scaled by property area.

To empirically test these hypotheses, one crucial set of control is borrower's characteristics, as estimates will be biased if buyer's characteristics are related to both the likelihood of a lender change and mortgage outcomes. For example, buyers with lower income may be rejected by incumbent banks, and uninformed banks will also charge higher interest rates and grant less mortgage amount towards such buyers.

To better control buyer characteristics, we merge the main dataset with HMDA. Adverse selection theory implies that if the incumbent bank stops financing the property, uninformed banks will charge higher interest rates to compensate themselves for lower collateral quality, due to potential unobservable property imperfections. In the meantime, they are likely to grant smaller mortgage amounts to decrease the likelihood of future default. We use the initial interest rate to measure mortgage interest rate and use mortgage amount per square foot to measure mortgage size.

HMDA dataset reports applicant's number, sex, race and annual income, which are included as buyer characteristics (Bit) into the following model:

$$DV_{i,t} = \alpha_0 + \alpha_1 Len\_change_{i,t} + Property\ Controls_i + Buyer\ Characteristics_{i,t} + T_c + Y_t + Z_l + \epsilon_{i,t} \quad (4)$$

In this model, the dependent variables are the initial interest rate and loan-to-area ratio of the mortgage. If adverse selection holds, α_1 should be positive for initial interest rate and negative for loan-to-area ratio.

Table 4.6 presents the results of the initial interest rate. We find that across specifications, the coefficients on $Len\_change_{i,t}$ are all significantly positive, implying that uninformed banks charge higher interest rate, consistent with the adverse selection hypothesis. In column (4), the most robust specification, the coefficient 0.032 indicates that compared to banks that continue financing the property ("incumbent banks"), uninformed banks charge 0.032 pp higher initial interest rate. Since the average initial interest rate is about 4.7 pp, this means 0.7% higher in terms of the magnitude.

Table 4.7 shows the results of mortgage amount. We find that the coefficients on $Len\_change_{i,t}$ are all negative, implying that uninformed banks grant smaller mortgage amount, consistent with the adverse selection hypothesis. In column (4), the most robust specification, the coefficient -0.814 indicates that uninformed banks originate mortgage $0.814 less per square foot. This corresponds to 0.5% lower compared to the mean.

4.5.2 Subsequent Performance

In the previous sub-section, we show evidence of adverse selection between incumbent (thus informed) and uninformed banks. In this sub-section, we empirically test whether properties and mortgages financed by uninformed banks indeed underperform ex-post. To measure property performance, we filter out a subset of the sample for which we can observe a subsequent arm's-length repeat sale and calculate the annualized return between the current and subsequent sale price. For mortgage performance, we create a binary variable, *Default*, to indicate if the mortgage will be in default within 5 years. As the likelihood of default monotonically increases with time, I restrict the time interval to five years following Stroebel (2016).[42] We test the following model:

$$Sub\_ret_{i,t} = \alpha_0 + \alpha_1 Len\_change_{i,t} + Property\ Controls_i + Y_{c,t} + Z_l + \epsilon_{i,t} \quad (5)$$

$Sub\_ret_{i,t}$ represents the annualized return be- tween current and subsequent sale price of property i sold again at quarter t. Table 4.8 presents the regression results of the model above.[43] Across specifications, the coefficients on $Len\_change_{i,t}$ are all close to 0. This finding suggests that there is no difference in future performance between properties financed by incumbent and uninformed lenders.

Next, we examine the probability that a buyer will go into default and use it as the dependent variable:

$$Sub\_default_{i,t} = \alpha_0 + \alpha_1 Len\_change_{i,t} + Property\ Controls_i + Buyer\ Characteristics_{i,t} + T_c + Y_t + Z_l + \epsilon_{i,t} \quad (6)$$

Table 4.9 presents the regression results. The coefficients across specifications are very stable and significantly negative. It implies that compared to properties financed by incumbent banks, properties financed by uninformed banks face a lower likelihood of going into default within five years. Since there is no major difference on housing valuation between the two groups implied by Table 4.8, the outperformance of mortgages from uninformed banks can be traced back to the

[42] Stroebel (2016) restricts the time interval within three years. Since our sample covers a longer horizon, we choose a relatively longer window.

[43] We use the full sample (without HMDA information) in this regression because borrower and mortgage characteristics are unlikely affect property value. The results from HMDA-merged dataset remains the same.

findings in Table 4.7, where uninformed banks originate relatively smaller mortgage amounts, making them less exposed to default risk.

4.6 Robustness Check

4.6.1 Borrower and Mortgage Characteristics

Although we show evidence that the baseline regression is unlikely to be biased by demand side factors, one might still be concerned that baseline results in Table 4.3 may be biased due to omission of borrower or mortgage characteristics. To thoroughly rule out this concern, we re-estimate the baseline model using ZTRAX-HMDA merged dataset where we can observe borrower and mortgage information. The results are reported in Table 4.A2. The coefficients are still significantly negative across specifications. This evidence further verifies the finding that incumbent banks are more likely to continue financing properties with higher past returns.

4.6.2 Census Block Group

In Table 4.5, we show that the lending decision is unrelated to average returns at census tract level, which implies that banks cannot exploit information from properties they do not finance within census tract. However, it is still possible that banks exploit information within a smaller geographic region than census tract, such as census block group.[44] To test this hypothesis, we re-estimate Table 4.5 by grouping properties and calculating average returns at census block level.

[44] Census block group is the smallest geographical unit for which the bureau publishes sample data, i.e. data which is only collected from a fraction of all households. Typically, a census block groups has a population of 600 to 3,000 people.

Tabel 4.A3 presents the results. In the most robust specification, the coefficient becomes insignificant, consistent with the finding in Table 4.5. This result implies that even in a smaller geographical unit, banks fail to exploit price information on properties they do not finance.

4.7 Conclusion

Financial economists have been working to uncover the role that collateral plays in determining bank lending. Using a granular dataset at property level across the US over 22 years, we show that banks base their mortgage lending decision on collateral value change that they observe. Specifically, the likelihood that the incumbent bank continues to finance the property is positively associated with the return of the property between two sales. In addition, we find a spillover effect of property-level sale price information on lending decisions in the neighbourhood. However, there is evidence that banks fail to exploit similar information on properties they do not finance.

Following the theory of adverse selection induced by information asymmetry, we find supporting evidence that uninformed banks apply more stringent mortgage lending policies. However, we do not find ex-post evidence that they finance lower quality properties compared to their incumbent counterparts. As consequence of stringent mortgage lending, mortgages financed by uninformed banks have even lower likelihood of default.

This article documents the important role of collateral value on mortgage lending and sheds light on the determinants that foster bank-collateral relationships. It paves the way for future research in this field. Do uninformed banks learn over time and adjust their lending decisions?

What are other factors that determine bank-collateral relationships? We believe that these questions deserve more attention in the future.

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Table 4.1: Description of Variables

Variable	Definition
Panel A: Transaction Variables	
SalesPriceAmount	Sale price of a property.
Len_change	A dummy variable that indicates if the mortgage lender changes in a transaction. 0=No; 1=Yes.
Last_ret	Annualized return between preceding and current transaction prices.
Sub_ret	Annualized return between current and subsequent transaction prices.
Sub_default	A dummy variable that indicates whether the property will be in default within five years after the transaction. 0=No; 1=Yes.
Panel B: Property Level Controls	
BuildingAreaSqft	Logarithm of building area (square feet)
LotSize	Logarithm of lot size (square feet).
Pool	A dummy variable that indicates whether the property has a pool. 0=No; 1=Yes.
Garage	A dummy variable that indicates whether the property has a garage. 0=No; 1=Yes.
PtyLife	Time interval between transaction year and built (remodeled, if available) year.
Panel C: Borrower Level Controls	
Single	A dummy variable that indicates whether there is only one mortgage applicant (borrower). 0=No; 1=Yes.
Income	Logarithm of borrower's annual income.
Race	Borrower's race. 1=American Indian or Alaska Native; 2=Asian; 3=African American; 4=Native Hawaiian or Other Pacific Islander; 5=White.
Gender	Borrower's gender. 0=Male; 1=Female.
Panel D: Mortgage Level Controls	
LoanArea	Loan amount per building area (square foot). LoanArea=Loan Amount / Building Area.
LTV	Loan-to-Value ratio. LTV=Loan Amount / Sale Price.
LTI	Loan-to-Income ratio. LTI=Annual Loan Repayment / Annual Income.
LoanMaturity	Maturity of the loan (month).
InterestRate	The initial interest rate of a mortgage (percentage point).

Table 4.2: Summary Statistics

This table reports summary statistics of the main variables in the baseline regression. ***, **, * correspond to statistical significance at the 1%, 5%, and 10% levels, respectively. Variable definitions are reported in Table 4.1.

	No Lender Change			**Lender Change**			**Difference**	**t-statistics**
	Mean	Median	S.D.	Mean	Median	S.D.		
Transaction Information								
SalesPriceAmount	427519	294000	411776	419967	291000	396270	7553***	8.3
Last_ret	0.077	0.0452	0.121	0.0689	.0393	0.114	0.00809***	28.47
Sub_ret	0.0263	0.0162	0.0841	0.0242	0.0148	0.0801	0.00215***	7.25
Sub_default	0.103	0	0.304	0.0861	0	0.28	0.0169***	26.04
Property Information								
BuildingAreaSqFt	7.5	7.46	0.472	7.52	7.49	0.455	-0.0180***	-17.27
LotSize	9.31	9.14	1.09	9.3	9.16	1.06	0.00158	0.55
Pool	0.0367	0	0.188	0.0432	0	.229	-0.00647***	-14.06
Garage	0.31	0	0.463	0.324	0	0.468	-0.0142***	-13.28
PtyLife	27.8	20	25.3	27.9	20	25	-0.175**	-3.07
Mortgage Information								
LoanAmount	329461	230400	569021	323972	234000	438176	5489.1***	5.26
LoanArea	161	124	115	158	124	108	3.097***	12.46
LTV	0.795	0.8	0.172	0.803	0.8	0.168	-0.00840***	-21.85
LoanMaturity	28.7	30.1	4.81	28.7	30.1	4.71	-0.0376**	-3.11
InterestRate	5.22	5.37	1.64	5.10	5.12	1.59	0.12***	8.8

Table 4.3: Property Returns and Lender Change

This table reports results that examine the relation between returns of property and the likelihood of a lender change. The dependent variable across all columns is *Len_change*, a binary variable which equals 1 if lender changes and 0 otherwise. The main independent variable is $Last\_ret_{i,t}$, calculated as the the annualized return between preceding and current sale price for property i sold at time (quarter) t. Property Controls include the hedonic characteristics of the property, such as property life, building area, garage, pool, lot size. The standard errors are reported in parentheses. ***, **, * correspond to statistical significance at the 1%, 5%, and 10% levels, respectively. Variable definitions are reported in Table 4.1.

	(1)	(2)	(3)	(4)
$Last\_ret_{i,t}$	-0.020***	-0.015***	-0.013***	-0.01*
	(0.003)	0.003)	(0.005)	(0.006)
Constant	0.880***	0.961***	0.882***	0.960***
	(0.001)	(0.007)	(0.000)	(0.011)
Property Controls	No	Yes	No	Yes
Bank FE	Yes	Yes	Yes	Yes
Census Tract FE	Yes	Yes		
Quarter FE	Yes	Yes		
Census Tract × Quarter FE			Yes	Yes
R^2	0.110	0.115	0.412	0.422
N	1519781	1128927	960857	678968

Table 4.4: Neighborhood Returns and Lender Change

This table reports results that examine the relation between returns of properties financed by a certain bank within a census tract and the likelihood of a lender change. The dependent variable across all columns is *Len_change*, a binary variable which equals 1 if lender changes and 0 otherwise. The main independent variable is $Last\_ret_{l,c,t-1}$, calculated as the average annualized for all properties financed by bank *l* at census tract *c* sold at time (quarter) t-1. Property Controls include the hedonic characteristics of the property, such as property life, building area, garage, pool, lot size. The standard errors are reported in parentheses. ***, **, * correspond to statistical significance at the 1%, 5%, and 10% levels, respectively. Variable definitions are reported in Table 4.1.

	(1)	(2)	(3)	(4)
$Last\_ret_{l,c,t-1}$	-0.035***	-0.027**	-0.043**	-0.045**
	(0.010)	0.011)	(0.017)	(0.020)
Constant	0.812***	0.860***	0.810***	0.875***
	(0.001)	(0.023)	(0.002)	(0.034)
Property Controls	No	Yes	No	Yes
Bank FE	Yes	Yes	Yes	Yes
Zipcode FE	Yes	Yes		
Quarter FE	Yes	Yes		
Zipcode × Quarter FE			Yes	Yes
R^2	0.100	0.113	0.342	0.366
N	212510	145276	157612	101937

Table 4.5: Outside Lender, Property Return and Lender Change

This table reports results that examine the relation between returns of all properties a census tract and the likelihood of a lender change. The dependent variable across all columns is *Len_change*, a binary variable which equals 1 if lender changes and 0 otherwise. The main independent variable is $Last\_ret_{c,t-1}$, calculated as the average annualized for all properties at census tract *c* sold at time (quarter) t-1. Property Controls include the hedonic characteristics of the property, such as property life, building area, garage, pool, lot size. The standard errors are reported in parentheses. ***, **, * correspond to statistical significance at the 1%, 5%, and 10% levels, respectively. Variable definitions are reported in Table 4.1.

	(1)	(2)
$Last\_ret_{c,t-1}$	-0.010^{*}	-0.002
	(0.006)	(0.007)
Constant	0.881^{***}	0.955^{***}
	(0.001)	(0.010)
Property Controls	No	Yes
Bank FE	Yes	Yes
Zipcode × Quarter FE	Yes	Yes
R^2	0.246	0.260
N	956647	679841

Table 4.6: Lender Change and Initial Interest Rate

This table reports results that examine the relation between a change of lender and the initial interest rate of the mortgage. The dependent variable across all columns is *InterestRate*, the initial interest rate of the mortgage. The main independent variable is *Len_change*, a binary variable which equals 1 if lender changes and 0 otherwise. Property Controls include the hedonic characteristics of the property, such as property life, building area, garage, pool, lot size. Borrower Controls include borrower personal characteristics, such as borrower's income, race, gender and whether there is only one borrower. Mortgage controls include loan-to-value ratio (LTV), loan-to-income ratio (LTI) and loan maturity. The standard errors are reported in parentheses. ***, **, * correspond to statistical significance at the 1%, 5%, and 10% levels, respectively. Variable definitions are reported in Table 4.1.

	(1)	(2)	(3)	(4)
Len_change	0.019*	0.020*	0.029**	0.032**
	(0.011)	0.012)	(0.013)	(0.013)
Constant	4.640***	5.460***	5.445***	5.662***
	(0.010)	(0.087)	(0.128)	(0.169)
Property Controls		Yes	Yes	Yes
Borrower Controls			Yes	Yes
Mortgage Controls				Yes
Bank FE	Yes	Yes	Yes	Yes
Census Tract FE	Yes	Yes	Yes	Yes
Quarter FE	Yes	Yes	Yes	Yes
R^2	0.807	0.812	0.816	0.820
N	56214	47348	41091	39385

Table 4.7: Lender Change and Mortgage per Building Area

This table reports results that examine the relation between a change of lender and the initial mortgage amount per square foot. The dependent variable across all columns is *LoanArea*, the ratio between loan amount and building area (square foot). The main independent variable is *Len_change*, a binary variable which equals 1 if lender changes and 0 otherwise. Property Controls include the hedonic characteristics of the property, such as property life, building area, garage, pool, lot size. Borrower Controls include borrower personal characteristics, such as borrower's income, race, gender and whether there is only one borrower. Mortgage controls include loan-to-value ratio (LTV), loan-to-income ratio (LTI) and loan maturity. The standard errors are reported in parentheses. ***, **, * correspond to statistical significance at the 1%, 5%, and 10% levels, respectively. Variable definitions are reported in Table 4.1.

	(1)	(2)	(3)	(4)
Len_change	-0.206	-0.294	-0.414*	-0.814***
	(0.211)	(0.224)	(0.229)	(0.222)
Constant	168.661***	125.678***	-60.738***	-567.257***
	(0.194)	(0.087)	(2.313)	(3.281)
Property Controls		Yes	Yes	Yes
Borrower Controls			Yes	Yes
Mortgage Controls				Yes
Bank FE	Yes	Yes	Yes	Yes
Census Tract FE	Yes	Yes	Yes	Yes
Quarter FE	Yes	Yes	Yes	Yes
R^2	0.744	0.720	0.729	0.800
N	850207	629814	557495	503669

Table 4.8: Lender Change and Property Future Returns

This table reports results that examine the relation between a change of lender and the property returns between current transaction at time (quarter) *t* and the next repeat sale. The dependent variable across all columns is *Sub_ret*, the annualized return between current and subsequent sale price for property *i* sold again at time (quarter) *t* in the future. The main independent variable is *Len_change*, a binary variable which equals 1 if lender changes and 0 otherwise. Property Controls include the hedonic characteristics of the property, such as property life, building area, garage, pool, lot size. The standard errors are reported in parentheses. ***, **, * correspond to statistical significance at the 1%, 5%, and 10% levels, respectively. Variable definitions are reported in Table 4.1.

	(1)	(2)	(3)	(4)
Len_change	0.000	0.000	0.000	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)
Constant	0.024***	0.018***	0.023***	0.019***
	(0.000)	(0.002)	(0.000)	(0.004)
Property Controls		Yes		Yes
Bank FE	Yes	Yes	Yes	Yes
Census Tract FE	Yes	Yes		
Quarter FE	Yes	Yes		
Census Tract × Quarter FE			Yes	Yes
R^2	0.441	0.427	0.719	0.707
N	639270	460437	299068	201434

Table 4.9: Lender Change and Mortgage Default

This table reports results that examine the relation between a change of lender and the likelihood that the mortgage goes into default within the subsequent five years. The dependent variable across all columns is *Sub_default*, a binary variable that take the value of 1 if the mortgage goes into default within five years after the transaction. The main independent variable is *Len_change*, a binary variable which equals 1 if lender changes and 0 otherwise. Property Controls include the hedonic characteristics of the property, such as property life, building area, garage, pool, lot size. Borrower Controls include borrower personal characteristics, such as borrower's income, race, gender and whether there is only one borrower. Mortgage controls include loan-to-value ratio (LTV), loan-to-income ratio (LTI) and loan maturity. The standard errors are reported in parentheses. ***, **, * correspond to statistical significance at the 1%, 5%, and 10% levels, respectively. Variable definitions are reported in Table 4.1.

	(1)	(2)	(3)	(4)
Len_change	-0.023***	-0.021***	-0.022***	-0.020**
	(0.006)	(0.008)	(0.008)	(0.009)
Constant	0.505***	0.245***	-0.604***	-2.298***
	(0.005)	(0.078)	(0.101)	(0.133)
Property Controls		Yes	Yes	Yes
Borrower Controls			Yes	Yes
Mortgage Controls				Yes
Bank FE	Yes	Yes	Yes	Yes
Census Tract FE	Yes	Yes	Yes	Yes
Quarter FE	Yes	Yes	Yes	Yes
R^2	0.533	0.562	0.595	0.614
N	40022	25374	21999	20849

Appendix 4.A1: Details in Cleaning the Main Dataset

In this section we introduce data cleaning steps. To obtain the main dataset that we use for the main regressions, we cleaned the ZTrans dataset in the following steps.

- Step 1. Clean Text Format of Lender Name in ZTrans Dataset

  First, in ZTrans dataset, we keep mortgage events where the lender type corresponds to banks and drop other lender types, such as mortgage companies, credit unions etc. There are 188,168 unique lender name-year pairs across the nation in the dataset. We remove all leading trailing blanks and special symbols and correct some noticeable misspellings. We then drop lenders with fewer than 100 observations. There remain 82,920 unique lender-year pairs in the dataset.

- Step 2. Match Lender Name with RSSD Link Table.

  We link lender names in ZTrans dataset with their RSSD IDs by using the matching file from Federal Reserve Bank of Chicago. We successfully matched 60,419 out of 82,920 unique lender-year pairs in ZTrans dataset.

- Step 3. Link RSSD to Lender Name in Mortgage Origination

  We further link RSSD to each mortgage transaction using Lender Name-RSSD linking file. We drop properties which are not matched with any lender. These include properties without mortgage or, with mortgages events but without lender's RSSD information.

- Step 4. Create Lender Change Indicator

  We require that in a transaction, the RSSD number of both seller's and buyer's lendersare available and create a dummy variable toindicate if there is a change of lender in a transaction.

- Step 5. Finalize the Sample

We further select a subset of transactions in the dataset which satisfy the following three requirements: (1) Sale price must be greater than \$5,000; (2) The seller of the property has an outstanding mortgage from a bank whose RSSD number is identifiable; (3) The buyer of the property is originated a mortgage from a bank whose RSSD number is identifiable. Following the sampling procedure mentioned above, it turns out that we are able to include 2,661,982 residential property transactions from 2,459,309 unique properties across the US from 1995 to 2017.

Table 4.A2: Property Returns and Lender Change

This table reports results that examine the relation between returns of property and the likelihood of a lender change, using the ZTRAX-HMDA merged sample. The dependent variable across all columns is *Len_change*, a binary variable which equals 1 if lender changes and 0 otherwise. The main independent variable is $Last\_ret_{i,t}$, calculated as the annualized return between preceding and current sale price for property *i* sold at time (quarter) *t*. Property Controls include the hedonic characteristics of the property, such as property life, building area, garage, pool, lot size. Borrower Controls include borrower personal characteristics, such as borrower's income, race, gender and whether there is only one borrower. Mortgage controls include loan-to-value ratio (LTV), loan-to-income ratio (LTI) and loan maturity. The standard errors are reported in parentheses. ***, **, * correspond to statistical significance at the 1%, 5%, and 10% levels, respectively. Variable definitions are reported in Table 4.1.

	(1)	(2)	(3)	(4)
Len_change	-0.039***	-0.028***	-0.0032***	-0.029***
	(0.006)	(0.006)	(0.007)	(0.007)
Constant	0.871***	0.797***	0.790***	0.815***
	(0.000)	(0.011)	(0.016)	(0.019)
Property Controls		Yes	Yes	Yes
Borrower Controls			Yes	Yes
Mortgage Controls				Yes
Bank FE	Yes	Yes	Yes	Yes
Census Tract FE	Yes	Yes	Yes	Yes
Quarter FE	Yes	Yes	Yes	Yes
R^2	0.113	0.121	0.126	0.125
N	764092	566503	500591	452434

Table 4.A3: Outside Lender, Property Return and Lender Change

This table reports results that examine the relation between returns of all properties a census block group and the likelihood of a lender change. The dependent variable across all columns is *Len_change*, a binary variable which equals 1 if lender changes and 0 otherwise. The main independent variable is $Last\_ret_{b,t-1}$, calculated as the average annualized for all properties at census block group *b* sold at time (quarter) t-1. Property Controls include the hedonic characteristics of the property, such as property life, building area, garage, pool, lot size. The standard errors are reported in parentheses. ***, **, * correspond to statistical significance at the 1%, 5%, and 10% levels, respectively. Variable definitions are reported in Table 4.1.

	(1)	(2)
$Last\_ret_{b,t-1}$	0.003	0.010
	(0.013)	(0.015)
Constant	0.879^{***}	0.953^{***}
	(0.001)	(0.018)
Property Controls	No	Yes
Bank FE	Yes	Yes
Census Tract × Quarter FE	Yes	Yes
R^2	0.388	0.399
N	440969	300849

Chapter 5

Conclusion

This dissertation consists of three essays on empirical financial economics. The first two essays are both related to climate corporate finance, a heated and controversial topic in recent years, but they try to investigate climate finance questions from two opposite angles. In the first essay, we explore the deterrence effects of litigation risk on firms' environmental and social misbehavior, while in the second essay, we document the positive effect of local environmental beliefs on corporate environmental decisions.

In the first essay, we document that firms respond to higher ES litigation risk by reducing ES violations. Moreover, we find evidence consistent with the argument that institutional investors are pushing for ES friendly policies. We also find that litigation risk reduces shareholder wealth and changes corporate investment behaviors, and these effects are stronger for firms with lower ES ratings. These results consistently imply that courtroom has a significant impact on corporate ES performance.

The second essay, co-authored with Hong Wu and Yuhai Xuan, investigates a social determinant of corporate environmental policy, i.e. local environmental beliefs. We find that plants release fewer toxic emissions when they are located in areas with stronger pro-environmental beliefs. More interesting, we discover a dynamic within-firm internal reallocation mechanism where toxic emissions of the plant in strong environmental beliefs counties are more likely to be shifted to other plants, after a formal enforcement action enforced by the EPA. In terms of policy implications, our paper suggests that raising public awareness about environmental issues helps reduce environmental pollution.

The third essay contributes to the literature on collateral lending by demonstrating the significant role of collateral value changes in mortgage origination decisions within the US residential real estate market. We find that banks possess information advantage on their collaterals, and we present empirical evidence for some implications of such information advantage in lending decisions.