The London School of Economics and Political Science

ESSAYS ON DIGITAL AND SUSTAINABLE FINANCE

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Declaration

I certify that the thesis I have presented for examination for the PhD degree of the London School of Economics and Political Science is solely my own work other than where I have clearly indicated that it is the work of others (in which case the extent of any work carried out jointly by me and any other person is clearly identified in it).

I warrant that this authorisation does not, to the best of my belief, infringe the rights of any third party.

I declare that my thesis consists of 29,526 words excluding appendices.

Statement of co-authored work

I confirm that Chapter 3 was jointly co-authored with Huiyun Li and Qianying Liu, and I contributed 33% of this work.

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Abstract

The first chapter sheds light on the impact of data risks on the increasingly digitalized financial system by examining the direct and spillover effects of bank data breaches on deposits. Leveraging a hand-collected novel dataset that identifies breaches at the bank-state level in the U.S., I find that data breaches reduce deposits at breached banks. Moreover, within the local deposit market, data breaches lead to not only a reallocation but also a net drop in deposits. Beyond the local market, I document negative within-bank, cross-state spillovers, with smaller banks being more vulnerable than larger ones. Further analysis reveals that depositor reactions are primarily driven by the demand for privacy and intensify as the scale of the breach increases.

The second chapter examines the impact of digital reporting on the sustainability information environment. Exploiting the staggered implementation of the SEC's iXBRL mandate as a quasi-experiment, I find that digital reporting induces firms to expand sustainability disclosure, reduces ESG rating disagreement, but also incentivizes cheap talk. These results suggest that digital reporting improves the accessibility and comparability of sustainability information but may undermine its quality. This chapter highlights both the benefits and unintended consequences of emerging technologies in shaping the non-financial information environment.

The third chapter, co-authored with Huiyun Li and Qianying Liu, investigates the association between common ownership and corporate sustainability performance, as well as the moderating role of public attention to environmental issues. Using data on Chinese A-share listed firms, we show that common ownership is positively associated with firms' sustainability performance, and that this relationship is positively moderated by public attention to environmental issues. The underlying channel varies with the level of public attention: the information transmission channel dominates in regions with high public attention, whereas the governance channel becomes more pronounced in regions with low public attention.

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Chapter 1

Lost Deposits: When Bank Data Security Fails

Abstract

This paper examines the economic consequences of data security failures in the banking sector, focusing on the direct and spillover effects of bank data breaches. Leveraging a hand-collected novel dataset that identifies breaches at the bank-state level in the U.S., I find that data breaches not only reduce deposits at breached banks but also lead to a reallocation and a net drop in deposits within the local market. There are contrasting spillover effects on small and large non-breached banks. Specifically, following a data breach in a county, small non-breached banks in the same county experience deposit losses, while larger non-breached banks see an increase in deposits. Beyond the local market, I document negative within-bank, cross-state spillovers, with smaller banks being more vulnerable than larger ones. Further analysis reveals that depositor reactions are primarily driven by the demand for privacy and intensify as the scale of the breach increases.

1.1 Introduction

In the digital age, data security has become a pivotal concern for consumers, financial institutions, and regulators. With financial systems increasingly digitalized and interconnected, regulatory bodies, such as the Bank of England and the SEC, view data security risks as significant threats to financial stability, prompting new policies that mandate disclosure and strengthen governance standards around customer data management.[1] However, despite this intensified regulatory focus, we still lack empirical evidence on how data breaches in financial institutions, which are often isolated operational failures at individual entities, might impact the broader financial system.

This paper sheds light on how data security failures at individual banks affect the banking system by examining the direct and spillover effects of bank data breaches. Leveraging a novel, hand-collected dataset that identifies data breaches at the bank-state level in the U.S., I find that data breaches not only reduce deposits at breached banks but also lead to a reallocation and a net drop in deposits within the local market due to contrasting spillover effects on small and large non-breached banks. Specifically, following a data breach in a county, small non-breached banks in the same county experience deposit losses, while larger non-breached banks see an increase in deposits. These findings suggest that the impact of data breaches extends beyond a simple reallocation of deposits, imposing a net loss on local banking systems.

My bank-state-level breach dataset also allows me to investigate within-bank, cross-state spillovers, offering insights into how bank data breaches affect the banking system beyond the local market. I find significant declines in deposits at breached banks in both breached and non-breached states, with smaller effects observed in non-breached states. Larger banks demonstrate greater resilience to these cross-state spillovers compared to small-to-medium sized banks. Furthermore, heterogeneity analysis reveals that depositor reactions to data breaches are

[1]See, e.g., Bank of England, *Financial Stability in Focus: The FPC's macroprudential approach to operational resilience*, March 2024; Erik Gerding, *Cybersecurity Disclosure*, Speech by Director, Division of Corporation Finance, SEC, December, 2023

primarily driven by their demand for privacy rather than concerns over direct monetary loss at breached banks. Breaches involving privacy information exhibit significantly negative direct and within-bank spillover effects, while breaches involving only financial information show no evidence of such effects. Depositors' reactions become more pronounced as the scale of the breach increases.

I show in this paper that, within an average local deposit market, a \$1 decrease in deposits at breached banks is associated with a \$0.15 decrease in deposits at small non-breached banks and a \$1 increase in deposits at larger non-breached banks. This indicates that a \$1 decrease in deposits at breached banks is associated with a net loss of \$0.15 in the local deposit market. As for within-bank, cross-state spillovers, a \$1 decrease in deposits at a large breached bank in a breached state is associated with a \$0.3 decrease in deposits in the same bank in a non-breached state. In contrast, a \$1 decrease in deposits at a small-to-medium breached bank in a breached state is associated with a \$0.7 decrease in deposits in the same bank in a non-breached state.

Bank customers might respond to data breaches in a variety of ways. One mechanism is a loss of confidence in banks' data security. In a world with asymmetric information, depositors have limited knowledge of a bank's ability to protect their information and funds (Chen et al., 2022; Dang et al., 2017). As a result, a bank's reputation is closely tied to its perceived reliability and security, which are crucial for attracting and retaining depositors. A data breach can damage a bank's reputation by signaling vulnerabilities in its information security. This perceived weakness erodes depositors' trust in a bank's data security, leading to reduced deposit demand. Alternatively, depositors may interpret a data breach as a negative signal of the bank's overall viability rather than its data security, thereby increasing their perceived risk of the bank's default and reducing deposit demand.

These interpretations are consistent with real-world evidence showing that data breaches cause tangible harm to consumers. I examine consumer complaints submitted to the Consumer Financial Protection Bureau (CFPB) from 2015 to 2024. Among complaints that explicitly describe data breaches, two forms of harm emerge: (i) *identity misuse*, where stolen personal information is used to open fraudulent accounts, loans, or credit cards; and (ii) *direct monetary loss* resulting from unauthorized transactions or withdrawals on existing accounts. I identify 24,039 complaints reporting identity misuse and 1,510 complaints involving direct monetary loss. This asymmetry is consistent with the fact that unauthorized transactions are often reimbursed by banks, whereas identity misuse can generate long-lasting and difficult-to-resolve consequences, such as damaged credit records, fraudulent debt collections, and disrupted access to financial services. These consequences are further illustrated by the following complaint narratives.

First, many consumers report that fraudulent accounts opened due to identity misuse have impaired their credit, restricted their access to financial services, and caused financial or emotional distress. One consumer wrote, *"I am in the process of purchasing a home and this has caused my loan to be halted until this is remedied. This has caused extreme financial distress..."*. Second, several consumers attribute ongoing harms to data breaches that occurred years earlier. For instance, one wrote, *"I am a victim of identity theft. Several years ago my bank, XXXX, had a data breach... my sensitive information including name, social security number, date of birth and address were sent to over XXXX people."* Such complaints reveal that consumers' concerns extend beyond the immediate breach, because compromised personal information cannot be retracted and exposes consumers to recurring identity-misuse risks. Third, some consumers describe losing confidence simply after becoming aware of a breach, even without confirmed harm. One noted, *"I have become aware of an ongoing data breach... I have reason to believe that my account records may have been compromised..."*. These cases highlight that awareness of a breach alone can update consumers' beliefs about a bank's reliability and prompt precautionary behavior even among those not directly affected.

Overall, these complaints indicate that data breaches cause tangible harm and can trigger concern even among consumers who do not experience direct monetary loss or are not the immediate victims. They provide anecdotal evidence consistent with the "loss of confidence in data security" channel.

Empirical results of this paper also align with the "loss of confidence in data security" channel. Data breaches erode trust in the data security of directly affected bank-state units, leading to reduced deposit demand at these units. For the negative local spillovers to non-breached small banks, I provide suggestive evidence that when small banks are breached in a county, depositors update their beliefs about the vulnerabilities of non-breached small banks in the same county, resulting in lower confidence and reduced deposits. Additionally, my findings on large banks being more resilient to within-bank spillovers than small-to-medium banks suggest that depositors are more likely to perceive breaches at smaller banks as signals of broader institutional vulnerabilities. In contrast, breaches at large banks tend to be seen as isolated operational issues contained within the affected state.[2] Moreover, I show that depositors react to breaches involving privacy information but not to those involving only financial information. This supports the "loss of confidence in data security" channel rather than the "loss of confidence in viability" channel. If depositor reactions were driven by concerns over viability, we would observe negative reactions to breaches involving only financial information.

This study's findings are enabled by the unique granularity of my novel breach dataset, which captures data breaches at the bank-state level using breach notices reported to state governments. Other studies have relied on commercial databases that identify breaches at the bank level. The bank-level approach implicitly assumes that depositors' responses to breaches are uniform across all states in which a bank operates. However, my findings on within-bank, cross-state spillovers indicate that this assumption does not hold in practice. Therefore, ignoring the heterogeneity in depositor reactions across branches in breached and non-breached states can lead to substantially biased estimates. I find that the bank-level treatment approach severely underestimates the negative impact

[2] I also show in this paper that the observed heterogeneity in spillovers between large and small banks is not driven by geographic proximity. Geographic proximity: compared to large banks, small-to-medium banks tend to operate within a more concentrated geographic area, which may expose depositors in nearby non-breached states to greater local media coverage and stronger regional connections to the breach.

of data breaches on the local deposit market. Additionally, I employ a stacked difference-in-differences method, which mitigates biases arising from time-varying treatment effects in a staggered treatment setting.

This paper contributes to the growing literature on the real effects of data security failures. Existing literature mostly studies the effects on firms other than banks (see, e.g., Akey et al. (2023); Kamiya et al. (2021); Rosati et al. (2019)). These studies show that data breaches of firms lead to declines in shareholder value, stock price, risk appetite and profitability, while increasing audit fees. Other studies focus on consumer reactions to personal information leaks. For example, Agarwal et al. (2024) find that consumers reduce their use of digital payments following a large-scale data breach at a food delivery platform. The effects are transient, indicating that consumers prioritize convenience over security concerns. Empirical studies on the impacts of data breaches on banks and the broader financial systems, however, remain scarce. I add to this literature by documenting and quantifying that data breaches trigger a reallocation and a net loss in deposits in the local banking system, as well as negative within-bank, cross-state spillovers.

This paper is also related to the literature that examines institutions' operational resilience and financial stability. An operational failure may transmit across the financial system through three channels: operational contagion, financial contagion, and loss of confidence.[3]. Existing studies have predominantly focused on the financial contagion channel (see, e.g., Eisenbach et al. (2022); Kotidis and Schreft (2022); Duffie and Younger (2019)), by showing that operational disruptions of payment networks, particularly from cyberattacks, can impair liquidity flows across financial institutions. My research adds new evidence in support of the loss of confidence channel.

This paper is among the first to examine the real effects of bank data breaches. Two closely related papers are Engels et al. (2022) and Gogolin et al. (2021), both of which rely on a commercial database that identifies data breaches

[3] Bank of England, *Financial Stability in Focus: The FPC's macroprudential approach to operational resilience*, March 2024

at the bank level and use propensity score matching. My work differs in several aspects. First, my study places an emphasis on understanding the broader implications of bank data breaches for the banking system, quantifying the net loss of deposits within affected local markets, and examining within-bank, cross-state spillovers. These analyses are enabled by my hand-collected bank-state-level breach dataset. Second, as discussed above, the bank-state-level granularity in my dataset allows for a more precise estimation of data breach effects. Third, when estimating the direct effects of data breaches, I explicitly account for potential spillovers. Accounting for spillovers is essential, as they can introduce complex biases in estimating treatment effects (Berg et al., 2021). Indeed, I find that ignoring spillovers overestimates the direct effect by about 14% while underestimates the persistence of the negative impact.

The rest of this paper is organized as follows. Section 1.2 describes data sources and summary statistics. Section 1.3 outlines the empirical design. Section 1.4 estimates the direct effects of data breaches on deposits. Section 1.5 examines local spillover effects. Section 1.6 presents findings on within-bank, cross-state spillovers and heterogeneity analyses. Section 1.7 assesses the alternative approach of identifying breaches at the bank rather than bank-state level. Section 1.8 concludes.

1.2 Data and Sample

1.2.1 Bank Data Breaches

I hand-collect data on bank data breaches from breach notices published by state governments. Since 2003, states across the U.S. have steadily enacted state-level data breach notification laws. A state notification law requires that, if an entity experiences a data breach that affects residents of that state, the entity must promptly notify the affected individuals and the state government. The notice must include the entity's name, circumstances of the breach, types of information compromised, and date of the breach (if known). Appendix Figure A1.1 shows the timeline of notification law adoption by state. By the end of 2023, all 50 states and the District of Columbia have enacted data breach notification laws, and governments of 20 states are publishing notices they receive on their websites. Appendix Figure A1.2 presents examples of data breach notices. From a breach notice, I am able to extract the name of the breached bank, type of breach, information compromised, number of affected state residents, and date reported to the state government.

My sample covers bank data breaches reported to state governments between July 2009 and June 2019. By June 2019, 14 states[4] had begun publishing each breach notice on their websites upon receiving it. Appendix Table A1.1 provides details on breach notification laws in these 14 states. I include only these 14 states in my sample.[5] I start with all the notices published by these states' governments and match the breached entities to banks insured by the Federal Deposit Insurance Corporation (FDIC). I identify 109 distinct banks that reported a data breach at least once between July 2009 and June 2019. A bank in a given state can be affected by multiple data breaches over time. I include in my sample only the first reported breach for each bank within each state during the period July 2009–June 2019. That is, once a bank in a state experiences a data breach between July 2009 and June 2019, I treat that bank in that state as breached for the remainder of the sample period. Finally, I exclude breaches of banks that ceased operations in the affected states in the year of the breach, as well as banks that became inactive at the institutional level within the event window around their respective breaches. My final sample consists of 99 distinct banks affected by 144 data breaches.

Figure 1.1 shows the distribution of data breaches in my sample across bank size groups, where bank size is measured by a bank's total assets in the year prior

[4]The 14 states are California, Hawaii, Indiana, Iowa, Maine, Massachusetts, Montana, New Hampshire, North Carolina, Oregon, South Carolina, Virginia, Washington, and Wisconsin

[5]For other states, not publishing breach notices does not imply that their residents are unaware of data breaches occurring in those states. For example, residents may learn about breaches through local news or by subscribing to state data security alerts. Including those states as non-breached controls would therefore bias the estimates, so I exclude them from my analysis.

to the breach. The group of the smallest banks (with assets below \$500 million) and the group of the largest banks (with assets of \$100 billion or more) each account for 25% of the sampled breaches. Groups of mid-sized banks represent a slightly lower share of breaches, ranging from 14% to 19%. This distribution suggests that data breaches target banks across a range of sizes but tend to be more frequent among both the smallest and largest banks.

Figure 1.2 displays the distribution of data breaches across five mutually exclusive breach types: external hack/phishing, inadvertent disclosure, employee misconduct, lost/stolen document/device, and uncategorized paper-based compromise. Definitions and examples of each breach type are provided in Appendix Table A1.2. While external hacks and phishing constitute the largest category, representing 30% of the 74 breaches with known breach types, bank data breaches are not limited to cyberattacks. Inadvertent disclosures follow closely at 27%, and breaches due to employee misconduct and lost/stolen documents or devices together account for 18%.

A data breach can compromise two types of information: privacy and financial. Privacy information includes Social Security numbers, driver's license or other government-issued ID numbers, and addresses. Financial information includes bank account information and credit/debit card details. As shown in Figure 1.3 and Panel A of Table 1.1, out of the 120 breaches with known types of compromised information, 90% involve financial information, and 47% involve privacy information.

Panel A of Table 1.1 also presents summary statistics for the number of affected state residents ("victims") based on the 131 breaches with a known number of affected residents. On average, a breach affects 568 state residents. The number of affected residents shows substantial variation, with a median of 18 and a standard deviation of 2009.

1.2.2 Deposits and Bank Assets

I collect branch-level deposits from FDIC's Summary of Deposits (SOD), which is a database of annual surveys of branch office deposits as of June 30 for all FDIC-insured institutions. In addition to deposits, I extract branch office locations and bank-level assets from the SOD database as well. All these data are annual. For my analysis, I use SOD data as of June 30 for each year from 2006 to 2022, as my empirical strategy, which is described in Section 1.3.3, requires a [-4 years, +3 years] event window around data breaches. Branch-level deposits for each bank are aggregated at the county level in the analysis. Panel B of Table 1.1 reports summary statistics for bank-county-year-level deposits and bank-year-level assets for the full sample.

1.3 Empirical Design

1.3.1 Stacked Difference-in-Differences

Data breaches are staggered treatments, as they attack different units in different states at various points in time. Recent research has highlighted significant challenges with traditional two-way fixed effects (TWFE) estimators when applied to staggered treatment settings. The traditional TWFE estimators can lead to substantially biased estimates of the treatment effect's direction or magnitude due to "forbidden comparisons" between already-treated units and newly treated units (e.g., see Baker et al. (2022); Goodman-Bacon (2021); Callaway and Sant'Anna (2021); De Chaisemartin and d'Haultfoeuille (2020)). This bias is particularly pronounced when treatment effects vary over time.

To address this issue, I apply the stacked difference-in-differences (DiD) (Roth et al., 2023; Wing et al., 2024b; Cengiz et al., 2019; Deshpande and Li, 2019) method in this paper. The stacked DiD model partitions the sample into separate cohorts based on treatment timing, viewing each cohort as a sub-experiment.

Within each cohort, the units in the treatment group are treated in the same period, and the control group consists only of units that get treated late enough or are never treated. This setup provides estimators robust to time-varying treatment effects as well as shifts in the composition of treated and control groups at each event time.

1.3.2 Bank-State-Level Data Breaches

Given the granularity of my data breach dataset, I identify data breaches at the bank-state level. If the government of state s receives a data breach notice from bank i , then bank i in state s is considered breached by that incident. Bank i in other states, as well as all other banks, are considered non-breached for that particular incident.

The treatment date for a data breach can be defined as either the date reported to the state government or the actual date of the breach. I use the former in this paper because the exact date of a breach is missing for about 30% of the breaches in my sample. Additionally, depositor reactions and public responses are more likely to be triggered by the public disclosure of the breach, which aligns with the reporting date, rather than the actual breach date. The states in my sample require banks to notify both affected residents and the state government without delay upon detecting a data breach, and the states promptly publish the notices received. Hence, the date reported to the state is a close proxy for the earliest public disclosure, making it an appropriate choice for the treatment date in my analysis.

Although data breaches are identified at the bank-state level, I conduct regressions at the bank-county-year level, as this approach allows me to control for confounders at the county level as well as to estimate spillover effects within a county. I aggregate annual branch-level deposits for each bank at the county level. When bank i in state s is breached, bank i is considered breached in each county where it has branches within state s.

1.3.3 Baseline Specification

I start with a baseline model that does not account for potential spillover effects of data breaches. My baseline specification is:

$$\begin{aligned}\log(Deposits_{i,c,t,h}) = \beta_0 + \beta_1 Breached_{i,s,h} \times Post_{t,h} \\ + \gamma_1 Post_{t,h} + \gamma_2 BrNum_{i,c,t,h} + \mathbf{FE} + \epsilon_{i,c,s,t,h}.\end{aligned} \tag{1.1}$$

I use a [-4 years, +3 years] event window around data breaches. Year t refers to deposits as of June 30 in calendar year t. Data breaches for year t are those reported to state governments between July 1 of calendar year $t-1$ and June 30 of calendar year t. Stack cohorts are denoted by h, which are defined by treatment years of data breaches. In stack cohort h, the treatment group consists of bank-state units that were breached in year $t = h$, and the control group includes bank-state units that were never breached as well as those breached in year $t > h + 3$.

The dependent variable $\log(Deposits_{i,c,t,h})$ is the natural logarithm of deposits (in \$millions) of bank i in county c in year t. $Breached_{i,s,h}$ is a dummy equal to one if bank i in state s is a breached unit in stack cohort h. $Post_{t,h}$ is a post-event dummy equal to one for year $t \geq h$ in stack cohort h. The interaction term $Breached_{i,s,h} \times Post_{t,h}$ is the variable of interest.

FE is a set of fixed effects. I include county-year fixed effects to control for time-varying county-level and state-level factors that might influence both the revelation of data breaches and the local deposit demand. These confounders include variables such as regional population, income levels, unemployment rates, and state regulations on data security. Additionally, I incorporate bank-county-stack fixed effects to account for time-invariant characteristics at the bank-county level, capturing factors like established customer relationships, local market influence, and local banking strategies that may affect deposit demand. To further account for the influence of the bank's local presence, I also include the number of branches of bank i in county c in year t, denoted by $BrNum_{i,c,t,h}$, as a control variable.

1.3.3.1 Endogeneity Issues and Parallel Pre-Trends

To recover the effects of data breaches on deposits using the stacked DiD design, a key underlying assumption is no anticipation. This assumption would be violated if depositors were able to foresee a data breach and therefore reduce deposits before the breach occurs. In practice, however, the no anticipation assumption is likely to hold because the timing of a breach tends to be unpredictable: intentional data breaches are mainly driven by arbitrary personal motivations, such as financial need or revenge (Kaffenberger and Kopp, 2019), while unintentional breaches (e.g., inadvertent disclosure of depositors' information) are the result of random human error.

Another concern is that breached banks may differ systematically from non-breached banks in terms of operational and governance weaknesses, which may increase their likelihood of experiencing a breach while also reducing deposits. To address this concern, I exclude from my sample any breaches occurring in banks that ceased operations in the affected states during the year of the breach. Additionally, I exclude banks that became inactive at the institutional level within the event window. Moreover, Table 1.2 compares the pre-breach characteristics of breached and non-breached banks. Prior to a breach, the two groups are comparable in terms of capital adequacy, asset quality, capital structure, and profitability: differences in Tier 1 capital ratio, non-performing loan ratio, equity-to-asset ratio, and return on assets are small in economic magnitude. The main difference is size, with breached banks holding more assets on average. To address any remaining concern that breached and non-breached banks differ in systematic ways that could confound the analysis, all regression specifications include bank-county-stack fixed effects, which control for *baseline* bank characteristics, including observable factors as shown in Table 1.2 as well as unobservables such as managerial quality and long-run governance.[6]

To further validate that the effects of data breaches cannot be attributed to endogeneity issues and confounders discussed above, I test for parallel pre-trends

[6]Baseline characteristics are measured at the start of each stack's event window, so they are time-invariant within each stack and are absorbed by the bank–county–stack fixed effects.

using the dynamic DiD model of Equation 1.1 and plot the coefficients in Figure 1.4. The year prior to a breach serve as the benchmark and is omitted. As shown in the figure, there is no evidence of differential trends in deposits between breached and non-breached units before a data breach, while the coefficients in post-breach periods are significantly negative. This suggests that the relative drop in deposits following data breaches cannot be explained by anticipation or impaired bank operations and governance.

1.3.4 Spillover Specification

The point estimates in the baseline model may be biased when potential spillover effects of data breaches are ignored. For example, suppose bank i in county c experiences reduced deposit demand after a breach, and depositors reallocate their funds to non-breached banks within the same county. In this case, the non-breached banks in county c are subject to positive local spillovers. Including them in the control group would overestimate the negative direct effects of data breaches. Similarly, when bank i is breached in state s but remains unaffected in another state s', residents in state s', upon becoming aware of the breach, might lose confidence in bank i' and reduce their deposits there. In this case, bank i in state s' is subject to negative within-bank, cross-state spillovers, and including it in the control group would underestimate the negative direct effects of data breaches. In addition, examining spillovers is essential for understanding the economic implications of bank data breaches on the banking system. Therefore, I extend the baseline to the following spillover specification:

$$\begin{aligned}\log(Deposits_{i,c,t,h}) = \beta_0 &+ \beta_1 Breached_{i,s,h} \times Post_{t,h} \\ &+ \beta_2 CountySpill_{i,c,h} \times Post_{t,h} \\ &+ \beta_3 BankSpill_{i,s,h} \times Post_{t,h} \\ &+ \gamma_1 Post_{t,h} + \gamma_2 BrNum_{i,c,t,h} + \gamma_3 Cont_{i,c,h} \times Post_{t,h} \\ &+ \mathbf{FE} + \epsilon_{i,c,s,t,h},\end{aligned} \tag{1.2}$$

$CountySpill_{i,c,h}$ is a dummy equal to one if bank i in county c is subject to within-county spillovers of data breaches in stack cohort h. Bank i in county c is considered subject to within-county spillovers in stack cohort h if there exists another bank j ($j \neq i$) in county c that is a breached unit in stack cohort h. $BankSpill_{i,s,h}$ is a dummy equal to one if bank i in state s is subject to within-bank spillovers in stack cohort h. Bank i in state s is considered subject to within-bank spillovers in stack cohort h if the same bank i is breached in another state s' ($s' \neq s$) in that stack cohort. I assume the direct effects of data breaches dominate the spillover effects. That is, $CountySpill_{i,c,h} = 0$ and $BankSpill_{i,s,h} = 0$ when $Breached_{i,s,h} = 1$. A graphical illustration of these treatment dummies is provided in Panel (b) of Figure 1.5. The baseline specification is illustrated in Panel (a) of Figure 1.5 for comparison.

As illustrated in Panel (b) of Figure 1.5, when bank i in county c is subject to within-bank spillovers, the non-breached units in county c might be contaminated. Simply dropping these potentially contaminated observations can lead to bias in estimating treatment effects (Berg et al., 2021). Hence, I retain these observations in the sample and introduce the variable $Cont_{i,c,h}$, a dummy equal to one if bank i in county c is subject to such contamination in stack cohort h. All other variables and fixed effects are as in Equation (1.1).

In the spillover specification, units subject to within-county and within-bank spillovers are also considered treated. To mitigate bias from time-varying treatment effects, as discussed in Section 1.3.1, units subject to earlier spillovers and those subject to spillovers within the three-year post-breach window, i.e., units subject to spillovers in year $t < h$ or in the window $h < t < h + 3$, are excluded from the control group in stack cohort h.

1.4 Direct Effects of Data Breaches on Deposits

1.4.1 Baseline Results

To estimate the direct effects of data breaches on deposits, I begin with the baseline model, which does not account for potential spillover effects. Panel A of Table 1.3 reports the estimated coefficients of Equation (1.1). The event window is [-4 years, +1 year] around each treatment year for columns (1) and (2), and is extended to three years after the breach for columns (3) and (4). Following Wing et al. (2024a), I weight each stack cohort in my regressions by its share of the treated sample. I focus on the coefficient estimates from the weighted stacked DiD regressions (columns (1) and (2)) for the economic interpretation throughout the paper. Nevertheless, I also show that the results are robust to unweighted stacked DiD regressions (columns (3) and (4)).

As shown in column (1) of Table 1.3, Panel A, within one year following a breach, the deposit level in an average breached bank-state unit drops by 7.4% relative to its non-breached counterparts. The effects are statistically significant at the 1% level. The effects remain significantly negative when the event window is extended to three years post-breach, with a 6.8% relative drop in deposits. This suggests that the effects of data breaches is persistently negative. Figure 1.4 shows dynamic effects, which further confirms the persistence of the impact: the significant relative drop in deposits remains in each post-breach year up to three years, with no sign of recovery. The results are similar in the unweighted stacked DiD regressions.

1.4.2 Accounting For Spillovers

As discussed in Section 1.3.4, point estimates of the baseline model can be biased because potential spillover effects of data breaches are ignored. To address this issue, I extend the baseline model to include within-county and within-bank spillover terms, as specified in Equation (1.2), and report the estimated coefficients in Panel B of Table 1.3. Column (1) shows that, in the short term, the deposit level in an average breached bank-state unit drops by 6.5% relative to its non-breached counterparts. In the medium term, as shown in column (3), the deposit level in an average breached bank-state unit experiences a relative drop of 6.8%. Both the short- and medium-term effects are statistically significant at the 1% level. Thus, after taking into account potential spillover effects, the impact of data breaches on deposits remains persistently negative.

The significant negative impact of bank data breaches on deposits can be interpreted through the lens of banks' reputational damage and the loss of depositor confidence. In a world with asymmetric information, depositors have limited knowledge of a bank's ability to protect their information and funds (Chen et al., 2022; Dang et al., 2017). A bank's reputation is closely tied to its perceived reliability and security, which are crucial for attracting and retaining depositors. As a result, a data breach damages a bank's reputation by signaling vulnerabilities in its information security. This perceived weakness erodes depositors' trust in the breached bank, leading to reduced deposit demand.

The results in Panel B of Table 1.3 also provide preliminary evidence of spillover effects. In an average bank-state unit that is not breached but is subject to within-bank spillovers, the deposit level drops by about 6% relative to units that are neither breached nor affected by spillovers. I further show in Section 1.6 that the within-bank spillovers exhibit significant variation across bank sizes, types of information compromised, and scale of breaches. Regarding within-county spillovers, the results in the table are inconclusive. However, in Section 1.5, I find evidence of significantly negative within-county spillovers on small non-breached banks, alongside positive spillovers on larger non-breached banks. This suggests that the inconclusive aggregate results may stem from offsetting spillover effects across non-breached banks of different sizes.

1.4.3 Estimation Bias When Ignoring Spillovers

In column (1) of Table 1.3, using an event window up to one year after the data breach, the estimated coefficient on the interaction term $Breached_{i,s,h} \times Post_{t,h}$ in the baseline model is 14% (= $\frac{0.074-0.065}{0.065}$) more negative than that in the spillover model. This difference suggests that the baseline model, by not accounting for spillover effects, overestimates the negative direct impact of data breaches by 14% in the short term.

When shifting to the three-year post-breach window, the discrepancy between the baseline and spillover models narrows as the baseline estimate becomes less negative (from -0.074 to -0.068) while the spillover model's estimate becomes slightly more negative (from -0.065 to -0.068). This indicates that, over the medium term, the baseline model may underestimate the persistence of data breaches' impact, misleadingly generating a slight reversal pattern.

These findings underscore the importance of accounting for spillovers to avoid biased estimates of both the magnitude and duration of treatment effects in empirical research.

1.5 Local Spillover Effects of Data Breaches

To understand the implications of data security failures for the stability of the local banking system, it is essential to examine whether a data breach of some banks in a local market leads to an overall loss of deposits in the market or merely a reallocation of deposits from breached to non-breached banks. Therefore, in this section, I investigate the local spillover effects of bank data breaches in greater detail. Consistent with the literature, I define the local market as a county.

1.5.1 Regression Specification

In examining within-county spillover effects, I differentiate small non-breached banks from large non-breached banks, as depositor responses to breaches might vary by bank size. Large non-breached banks may be perceived as more trustworthy in terms of data security, leading depositors to reallocate funds toward these banks when a breach occurs in another bank. In contrast, small non-breached banks may not experience the same reallocation of funds. Accordingly, I run the following extended spillover regression:

$$\begin{aligned}\log(Deposits_{i,c,t,h}) = {} & \beta_0 + \beta_1 Breached_{i,s,h} \times Post_{t,h} \\ & + \beta_2 CountySpill_{i,c,h} \times Post_{t,h} \times Small_{i,h} \\ & + \beta_3 CountySpill_{i,c,h} \times Post_{t,h} \times MedLarg_{i,h} \\ & + \beta_4 BankSpill_{i,s,h} \times Post_{t,h} + \gamma_1 Post_{t,h} \\ & + \gamma_2 BrNum_{i,c,t,h} + \gamma_3 Cont_{i,c,h} \times Post_{t,h} + \mathbf{FE} + \epsilon_{i,c,s,t,h}.\end{aligned} \tag{1.3}$$

The dummy variable $Small_{i,h}$ is equal to one if bank i's asset value is below \$500 million in the year prior to the breach of stack cohort h. $MedLarg_{i,h}$ is a dummy equal to one if bank i's asset value is \$500 million or more in the year prior to the breach in stack cohort h. I use \$500 million as the threshold because banks with assets below this level have traditionally been classified as community or regional banks and generally operate under lighter regulatory oversight. Survey evidence indicates that customers tend to express lower trust in data security at community or regional banks compared to larger institutions.[7] All other variables and fixed effects are as in Equation (1.2). The coefficients β_2 and β_3 capture the within-county spillover effects to small and larger non-breached banks, respectively.

[7] McKinsey & Company, Consumer Digital Payments: Already Mainstream, Increasingly Embedded, Still Evolving, 2022. Available at: https://www.mckinsey.com/industries/financial-services/our-insights/banking-matters/consumer-digital-payments-already-mainstream-increasingly-embedded-still-evolving.

1.5.2 Findings and Interpretation

Table 1.4 presents the estimated within-county spillover effects of bank data breaches. The results indicate that when a data breach occurs in a county, it has significantly negative spillover effects on small non-breached banks (with assets below \$500 million) in the same county, while larger non-breached banks (with assets of \$500 million or more) experience significantly positive spillovers. These spillover effects persist for at least three years post-breach. Specifically, as shown in columns (1) and (3), deposits at an average small non-breached bank in a breached county decrease by 5% within one year of the breach and by 6.8% over three years, relative to banks that are neither breached nor affected by spillovers. In contrast, deposits at an average larger non-breached bank in a breached county increase by 3.6% in the short term and by 2.9% in the medium term. These estimates are statistically significant at the 1% level. Results in columns (1) and (3) are from weighted stacked DiD regressions. Columns (2) and (4) show that the results are robust to unweighted stacked DiD specifications.

Economic magnitudes. In an average breached county, over three years following a breach, a \$1 decrease in deposits at breached banks is associated with an approximately \$0.15 decrease in deposits at small non-breached banks[8] and a \$1 increase in deposits at larger non-breached banks[9]. This suggests that, although there is evidence of reallocation of deposits from breached banks to larger non-breached banks, small non-breached banks experience negative spillover effects, resulting in an overall net loss of about \$0.15 in deposits for each dollar decreased at the breached banks in the local banking system.

[8]Back-of-the-envelope calculation: In specifications with a three-year post-breach window, in an average breached county, an average breached bank has deposits of \$1,085.6 million and an average small non-breached bank has deposits of \$103.9 million. There are 1.9 distinct breached banks and 3.0 distinct small non-breached banks in an average breached county. Given the estimated coefficients in column (3) of Table 1.4, $(103.9 \times 0.068 \times 3.0)/(1085.6 \times 0.067 \times 1.9) =$ \$0.15

[9]Back-of-the-envelope calculation: In specifications with a three-year post-breach window, in an average breached county, an average breached bank has deposits of \$1,085.6 million and an average non-breached larger bank has deposits of \$730.0 million. There are 1.9 distinct breached banks and 6.5 distinct larger non-breached banks in an average breached county. Given the estimated coefficients in column (3) of Table 1.4, $(730.0 \times 0.029 \times 6.5)/(1085.6 \times 0.067 \times 1.9) =$ \$1.0

Interpretation. The positive within county spillovers to larger non-breached banks are consistent with the explanation that depositors have more confidence in the data security of larger institutions. Larger banks tend to invest more in IT infrastructure(Modi et al., 2022; He et al., 2021), including data security technologies, and hence might be perceived as having more advanced data protection measures. Additionally, larger banks are subject to stricter regulatory oversight, which might reinforce public confidence in their compliance with data security standards.

A potential mechanism for the negative spillovers to small banks is that, when a small bank in a county experiences a data breach, local depositors may update their beliefs about the data security risks of other small banks in the same area, leading to reduced confidence in small non-breached banks. I test this mechanism by adding the interaction terms $CountySpill_{i,c,h} \times Post_{t,h} \times Small_{i,h} \times NumSmallBrea_{c,h}$ and $CountySpill_{i,c,h} \times Post_{t,h} \times MedLarg_{i,h} \times NumSmallBrea_{c,h}$ to Equation (1.3), where $NumSmallBrea_{c,h}$ is the number of distinct small breached banks in county c in stack cohort h. A breached bank is small if its asset value in the year prior to the breach is below \$500 million. Results are presented in Table 1.5.

The coefficient estimate on the interaction term $CountySpill_{i,c,h} \times Post_{t,h} \times Small_{i,h} \times NumSmallBrea_{c,h}$ in Table 1.5 is significantly negative, indicating that the negative spillover effects on small non-breached banks in a county tend to be stronger when more small banks are breached in that county. This provides suggestive evidence that local depositors may update their beliefs about the data security of small banks in general following breaches of other small banks in the same area.

An alternative explanation for the negative spillover effects on small non-breached banks is that certain breaches of small banks may not be captured in my dataset. In some states within my sample, data breach notification laws require banks to report breaches to the state when a minimum threshold of residents is affected. As a result, breaches of small local banks may go unreported to the state if they do not meet these thresholds. However, these local banks may still hold significant importance in their communities, leading local news to report on the breach, making depositors aware of it and prompting a reaction. In this case, some of the small banks classified as non-breached might actually be affected by breaches, meaning that the observed "negative spillover" effects could instead represent the negative direct effects of unobserved breaches.

To rule out this alternative explanation, I utilize the commercial data breach database from the Privacy Rights Clearinghouse (PRC), a non-profit organization that tracks data breaches in the U.S. primarily through media coverage.[10] Given that some breaches of small banks might still attract media attention even if they go unreported to the state, a database that tracks breaches primarily through media coverage can help identify unobserved breaches relevant to my sample. The PRC database identifies breaches at the institutional level, which means I can see which bank was affected but not the specific state. To take a conservative approach, if a breach recorded by the PRC is not in my dataset and the breached bank operates in one or more of the states in my sample, I consider this as a potential unobserved breach. Over my sample period, the PRC recorded five breaches affecting small banks operating within my sampled states. Of these, only one breach is not captured in my dataset. Therefore, it is unlikely that my results are driven by unobserved breaches in small banks.

1.6 Within-Bank Spillovers and Heterogeneity Analyses

In this section, I investigate within-bank, cross-state spillover effects of bank data breaches to understand how these events impact the banking system beyond the local market.

[10]Existing literature that has used the PRC database includes, e.g., Kamiya et al. (2021), Akey et al. (2023).

1.6.1 Heterogeneity Across Breached Banks of Different Sizes

Depositors might view a data breach of a large bank as an isolated operational lapse due to the bank's perceived ability to manage and contain such incidents effectively, whereas interpret a breach of a small bank as a signal of broader institutional weaknesses. Hence, I expect that large breached banks are more resilient to within-bank, cross-state spillovers compared to small breached banks. To test this, I extend Equation (1.2) as follows:

$$\begin{aligned}\log(Deposits_{i,c,t,h}) = \beta_0 &+ \beta_1 Breached_{i,s,h} \times Post_{t,h} \times LargBrea_{i,h} \\ &+ \beta_2 Breached_{i,s,h} \times Post_{t,h} \times SmallMedBrea_{i,h} \\ &+ \beta_3 BankSpill_{i,s,h} \times Post_{t,h} \times LargBrea_{i,h} \\ &+ \beta_4 BankSpill_{i,s,h} \times Post_{t,h} \times SmallMedBrea_{i,h} \\ &+ \beta_5 CountySpill_{i,c,h} \times Post_{t,h} + \gamma_1 Post_{t,h} \\ &+ \gamma_2 BrNum_{i,c,t,h} + \gamma_3 Cont_{i,c,h} \times Post_{t,h} + \mathbf{FE} + \epsilon_{i,c,s,t,h}.\end{aligned} \tag{1.4}$$

$LargBrea_{i,h}$ is a dummy equal to one if bank i is breached in stack cohort h and with assets of \$100 billion or more ("Too Big To Fail" bank) in the year prior to the breach. $SmallMedBrea_{i,h}$ is a dummy equal to one if bank i is breached in stack cohort h and with assets below \$100 billion in the year prior to the breach. All other variables and fixed effects are as in Equation (1.2). The coefficients β_1 (β_3) and β_2 (β_4) capture the direct (within-bank, cross-state spillover) effects of data breaches involving large and small-to-medium banks, respectively. In this paper, a "breached state" refers to any state where the breached bank has branches and where the breach was reported to the state government. A "non-breached state" refers to other states where the same bank operates branches no breach was reported to the state government.

Results are reported in Panel A of Table 1.6. The results show that both large and small-to-medium banks experience statistically significant and persistent negative direct effects on deposits following a breach, as well as significant and persistent negative within-bank spillovers across states. Specifically, columns (1) and (3) show that deposits at an average large breached bank in a breached state decrease by about 8% within one year of the breach and by 7% over three years, relative to bank-state units that are neither breached nor affected by spillovers. Meanwhile, deposits at an average large breached bank in a non-breached state decrease by 5.6% in the short term and by 6.3% over the medium term.

When the size of a breached bank is smaller, deposits at an average small-to-medium breached bank in a breached state decrease by 5.4% within one year of the breach and by 6.5% over three years, relative to bank-state units that are neither breached nor affected by spillovers. Deposits at an average small-to-medium breached bank in a non-breached state decrease by 5.9% in the short term and by about 6% over the medium term.

The specifications in columns (1) and (3) are estimated by weighted stacked DiD regressions. I shown in columns (2) and (4) show that the results are robust to unweighted stacked DiD specifications.

Economic magnitudes. Over three years following a breach, a \$1 decrease in deposits at an average large breached bank in a breached state is associated with an approximately \$0.3 decrease in deposits at the same bank in a non-breached state.[11], indicating that within-bank spillover effects for large breached banks are about 70% smaller than the direct effect. In contrast, over three years following a breach, a \$1 decrease in deposits at an average small-to-medium breached bank in a breached state is associated with an approximately \$0.7 decrease in deposits in at the same bank in a non-breached state.[12], indicating that within-bank spillover effects for small-to-medium breached banks are only about 30% smaller than the direct effect. These results suggest that compared to small-to-medium banks, large banks tend to be more resilient to negative

[11] Back-of-the-envelope calculation: In specifications with a three-year post-breach window, an average large breached bank in a breached state has deposits of \$29,258 million and an average large breached bank in a non-breached state has deposits of \$10,569 million. Given the estimated coefficients in column (3) of Table 1.6, Panel A, $(10569 \times 0.063)/(29258 \times 0.072) =$ \$0.32

[12] Back-of-the-envelope calculation: In specifications with a three-year post-breach window, an average small breached bank in a breached state has deposits of \$531.63 million and an average small breached bank in a non-breached state has deposits of \$413.28 million. Given the estimated coefficients in column (3) of Table 1.6, Panel A, $(413.28 \times 0.059)/(531.63 \times 0.065) =$ \$0.71

within-bank spillovers of data breaches, with the impact of data breaches more contained in the breached states.

Interpretation. These findings align with the confidence-based interpretation. The extent of within-bank spillovers should depend on whether depositors in non-breached states view the breach as an isolated operational lapse or as an indicator of broader institutional weaknesses. As shown in Figure 1.2, a substantial portion of breaches are caused by inadvertent disclosures, employee misconduct, and lost/stolen documents or devices. Such incidents may be perceived as operational failures specific to the breached bank within the breached state, suggesting that the impact of these incidents should be contained within the affected state.

For larger banks, depositors tend to have greater confidence in their ability to contain and manage incidents due to their perceived resources and resilience. Consequently, depositors in non-breached states are more likely to interpret a breach of a large bank as a contained, localized issue rather than a reflection of systemic risk across the institution. In contrast, when a data breach occurs at a small-to-medium bank, depositors might perceive it as a signal of underlying vulnerabilities within the whole institution. As a result, upon a data breach of a small-to-medium bank, depositors are more likely to update their beliefs about the bank's overall data security strength and governance, and renew their view on the probability of future data breaches of that bank.

An alternative explanation is geographic proximity. Small-to-medium banks often operate within a more concentrated geographic area, meaning that depositors in non-breached states are likely to be geographically closer to the breached state. Depositors in nearby non-breached states may have more exposure to local media coverage of the breach and potentially stronger connections to the affected region. In contrast, large banks typically operate across a wider geographic range, so their breached and non-breached states are often farther apart, which could limit the reach of media coverage and weaken the perceived relevance of the breach for depositors in non-breached states.

To ensure that my findings are not driven by geographic proximity, I rerun regression (1.4) on a subsample: I exclude observations that are subject to within-bank spillovers and are located in non-breached states that are geographically adjacent to the breached state. If the observed heterogeneity in within-bank spillovers across bank sizes is due to geographic proximity rather than the loss of confidence channel, I would expect this heterogeneity to disappear in this subsample. Panel B of Table 1.6 reports the coefficient estimates. Even after excluding these geographically proximate observations, the heterogeneity remains. For example, in the short term[13], each \$1 decrease in deposits at an average large breached bank in a breached state is associated with an approximately \$0.3 decrease in deposits in a non-breached state.[14], indicating that within-bank spillover effects for large breached banks are about 70% smaller than the direct effect in the breached state. In contrast, each \$1 decrease in deposits at an average small-to-medium breached bank in a breached state is associated with an approximately \$0.9 decrease in deposits in a non-breached state.[15], indicating that within-bank spillover effects for small-to-medium breached banks are only about 10% smaller than the direct effect in the breached state in the short term.

1.6.2 Heterogeneity Across Types of Information Compromised

The negative effects of data breaches on deposits, as documented in the previous sections, suggest that depositors do value data security in the banking sector. In this section, I investigate the possible drivers behind this reaction - particularly,

[13] I use the short-term window here because the estimated coefficient that captures small-to-medium bank spillovers loses statistical power in the weighted stacked DiD specification over the three-year post-breach window while remaining statistically significant across all other specifications. The loss of statistical power is likely due to the loss of observations in the subsample.

[14] Back-of-the-envelope calculation: In specifications with a one-year post-breach window, an average large breached bank in a breached state has deposits of \$27,283 million in the subsample, and an average large breached bank in a non-breached state has deposits of \$11,616 million. Given the estimated coefficients in column (1) of Table 1.6, Panel B, $(11616 \times 0.059)/(27283 \times 0.081) = \$0.31$

[15] Back-of-the-envelope calculation: In specifications with a one-year post-breach window, an average small-to-medium breached bank in a breached state has deposits of \$460.38 million in the subsample, and an average small breached bank in a non-breached state has deposits of \$360.58 million. Given the estimated coefficients in column (1) of Table 1.6, Panel A, $(360.58 \times 0.061)/(460.38 \times 0.053) = \$0.90$

whether it is driven by depositors' demand for privacy or concerns over direct monetary loss at the breached banks . This analysis also sheds light on whether depositors lose confidence in bank data security or bank viability following data breaches. I extend Equation (1.2) as follows:

$$\begin{aligned}\log(Deposits_{i,c,t,h}) = \beta_0 &+ \beta_1 Breached_{i,s,h} \times Post_{t,h} \times Privacy_{i,h} \\ &+ \beta_2 Breached_{i,s,h} \times Post_{t,h} \times FinOnly_{i,h} \\ &+ \beta_3 BankSpill_{i,s,h} \times Post_{t,h} \times Privacy_{i,h} \\ &+ \beta_4 BankSpill_{i,s,h} \times Post_{t,h} \times FinOnly_{i,h} \\ &+ \beta_5 CountySpill_{i,c,h} \times Post_{t,h} + \gamma_1 Post_{t,h} \\ &+ \gamma_2 BrNum_{i,c,t,h} + \gamma_3 Cont_{i,c,h} \times Post_{t,h} + \mathbf{FE} + \epsilon_{i,c,s,t,h}.\end{aligned} \tag{1.5}$$

$Privacy_{i,h}$ is a dummy equal to one if *any* personal privacy information (Social Security number, driver's license or other government-issued ID number, address) is compromised in the breach of bank i in stack cohort h. $FinOnly_{i,h}$ is a dummy equal to one if *only* financial information (bank account information, debit/credit card details) is compromised in the breach of bank i in stack cohort h. All other variables and fixed effects are as in Equation (1.4). The coefficient β_1 (β_3) captures the direct (within-bank, cross-state spillover) effects of data breaches involving any privacy information, and β_2 (β_4) captures the direct (within-bank, cross-state spillover) effects of data breaches involving financial information only. This analysis is based on the subsample with known types of information compromised. As reported in Table 1.1, this subsample includes 120 data breaches, with 56 breaches involving privacy information and 64 involving financial information only.

Coefficient estimates are reported in Table 1.7. According to columns (1) and (3), for breaches involving privacy information, deposits at breached bank-state units decline by 7.4% within one year of the breach and by 6.7% over three years, relative to bank-state units that are neither breached nor subject to spillovers. These effects are statistically significant at the 1% to 5% level. Additionally, breaches involving privacy information are associated with significantly negative within-bank spillover effects: deposits drop by 8% in the short term and by 9.4%

over the medium term, relative to bank-state units that are neither breached nor subject to spillovers. In contrast, breaches involving only financial information show no evidence of significant direct effects or within-bank spillovers. These results suggest that depositors' responses to bank data breaches are primarily driven by their demand for privacy. These results also support a "loss of confidence in bank data security" channel rather than a "loss of confidence in bank viability" channel. If depositor reactions were driven by concerns over viability, we would observe negative reactions to breaches involving only financial information.

My findings are consistent with existing evidence on consumer preferences, which shows that consumers value privacy and react to privacy leakage due to concerns about potential identity theft (see, e.g., Acquisti et al. (2020); Armantier et al. (2021); Bian et al. (2023)). Consumers tend to believe that privacy leakage poses threats that are far-reaching and difficult to mitigate. In contrast, breaches that compromise only financial information tend to pose more limited risks, as consumers can contain the impact by freezing accounts or changing passwords.

1.6.3 Heterogeneity Across Scale of Breaches

To gain deeper insight into depositor reactions to bank data breaches, I examine how responses vary with the scale of breaches. The scale of a breach is measured by the number of affected residents in the breached state. I extend Equation (1.2) as follows:

$$\begin{aligned}\log(Deposits_{i,c,t,h}) = \beta_0 &+ \beta_1 Breached_{i,s,h} \times Post_{t,h} \mathbb{1}[Victims > q]_{i,h} \\ &+ \beta_2 Breached_{i,s,h} \times Post_{t,h} \mathbb{1}[Victims \leq q]_{i,h} \\ &+ \beta_3 BankSpill_{i,s,h} \times Post_{t,h} \times \mathbb{1}[Victims > q]_{i,h} \\ &+ \beta_4 BankSpill_{i,s,h} \times Post_{t,h} \times \mathbb{1}[Victims \leq q]_{i,h} \\ &+ \beta_5 CountySpill_{i,c,h} \times Post_{t,h} + \gamma_1 Post_{t,h} \\ &+ \gamma_2 BrNum_{i,c,t,h} + \gamma_3 Cont_{i,c,h} \times Post_{t,h} + \mathbf{FE} + \epsilon_{i,c,s,t,h}.\end{aligned} \tag{1.6}$$

The dummy variable $\mathbb{1}[Victims > q]_{i,h}$ ($\mathbb{1}[Victims \leq q]_{i,h}$) is equal to one if the data breach of bank i in stack cohort h affects more than (less than or equal to) q residents in the breached state. All other variables and fixed effects are as in Equation (1.4). This analysis is based on the subsample with known number of affected state residents (131 data breaches).[16]

Table 1.8 presents the estimated effects over the three-year period following a data breach. In columns (1) and (2), the threshold q is set to 18 affected state residents, representing the median of the sample and a relatively small-scale breach. For breaches affecting up to 18 residents, there is no evidence of significant direct effects or within-bank, cross-state spillovers. However, when the threshold is raised to 294 affected residents, which is the 75^{th} percentile of the sample, both direct effects and within-bank spillovers become significantly negative. This suggests that depositor responses to data breaches intensify as the scale of the breach increases.

1.6.4 Banks' Pricing Responses to Breaches

Having established the within-bank spillover effects, I next examine whether banks raise deposit rates to retain and attract deposits following a breach. I construct bank-level deposit spreads for savings deposits, insured time deposits, and uninsured time deposits using Call Report data. For each product category, the deposit spread is defined as the difference between the federal funds rate and the bank's interest rate on the corresponding deposit product.

I run the following stacked DiD regression over an event window of [-4 years, +1 year]:

$$Deposit\ Spread_{i,t,h} = \beta_0 + \beta_1 Breached_{i,h} \times Post_{t,h} + \gamma_1 Post_{t,h} + \mathbf{FE} + \epsilon_{i,t,h}. \quad (1.7)$$

[16] For California and Iowa, of which the state governments do not publish the exact number of affected residents, a value of 500 is imputed, as these states require reporting if at least 500 state residents are impacted. This should not affect the interpretation of my results, as the thresholds I use are below 500.

The outcome variable is deposit spread of bank i in year t. $Breached_{i,h}$ is a dummy equal to one if bank i experienced a data breach in any of its operating states (within the sample) in stack cohort h. **FE** includes bank-stack and year fixed effects.

Table 1.9 reports the results. Following a data breach, breached banks increase interest rates (i.e., lower deposit spreads) on savings deposits and uninsured time deposits relative to non-breached banks. Column (1) shows that breached banks reduce the spread on savings deposits by 7.6 basis points, and column (3) shows a reduction of 9.8 basis points in the spread on uninsured time deposits. In contrast, column (5) shows no statistically significant change in the spread on insured time deposits. These findings are robust to unweighted stacked DiD specifications (columns (2), (4), and (6)).

1.7 Assessment of an Alternative Empirical Approach: Bank-Level Data Breaches

Prior studies have relied on commercial databases of data breaches, which identify breaches at the institutional level rather than at the more granular bank-state level provided by my hand-collected dataset. The bank-level approach implicitly assumes that depositors' responses to data breaches are uniform across all states in which a bank operates. However, my findings in Section 1.6 show that this assumption does not hold in practice. Depositors in breached and non-breached states react heterogeneously to the same breach incident. Particularly, for breaches of large banks, the relative drop in deposits in the non-breached state is 70% smaller that in the breached state, while it is only 30% smaller for breaches in small-to-medium sized banks. Therefore, aggregating breaches at the bank level is likely to lead to biased estimates of data breaches' impact on the banking system.

In this section, I estimate the bias that arises from using the bank-level breach treatment. I repeat the analysis from Section 1.3 with the same data breach incidents but with bank-level breach treatment. Equation (1.3) is reduced to the following specification:

$$\begin{aligned}\log(Deposits_{i,c,t,h}) = \beta_0 &+ \beta_1 Breached_{i,s,h} \times Post_{t,h} \\ &+ \beta_2 CountySpill_{i,c,h} \times Post_{t,h} \times Small_{i,h} \\ &+ \beta_3 CountySpill_{i,c,h} \times Post_{t,h} \times MedLarg_{i,h} \\ &+ \gamma_1 Post_{t,h} + \gamma_2 BrNum_{i,c,t,h} + \gamma_3 Cont_{i,c,h} \times Post_{t,h} \\ &+ \mathbf{FE} + \epsilon_{i,c,s,t,h}.\end{aligned} \tag{1.8}$$

Appendix Table A1.3 reports the estimated coefficients. Consistent with my findings using the bank-state-level treatment, the bank-level approach also shows significantly negative and persistent direct effects, negative spillovers to small non-breached banks, and positive spillovers to larger non-breached banks within a county. However, the economic magnitudes vary notably. Under the bank-level approach, a \$1 relative drop in deposits in a breached bank in a county is associated with a \$0.175 decrease[17] in deposits at small non-breached banks and a \$1.15 increase[18] at larger non-breached banks within the same county. This results in a net loss of only \$0.025 for the local banking system, which is more than five times smaller relative to the bank-state-level approach (a net loss of about \$0.15 , as shown in Section 1.5). This finding suggests that, when the heterogeneity in depositor reactions across breached and non-breached states is not accounted for, the negative impact of data breaches on local banking system is severely underestimated.

[17] Back-of-the-envelope calculation: In specifications with a three-year post-breach window, in an average breached county, an average breached bank has deposits of \$855.2 million and an average small non-breached bank has deposits of \$97.3 million. There are 1.9 distinct breached banks and 2.9 distinct small non-breached banks in an average breached county. Given the estimated coefficients in column (3) of Table 1.4, $(97.3 \times 0.065 \times 2.9)/(855.2 \times 0.064 \times 1.9) =$ \$0.175

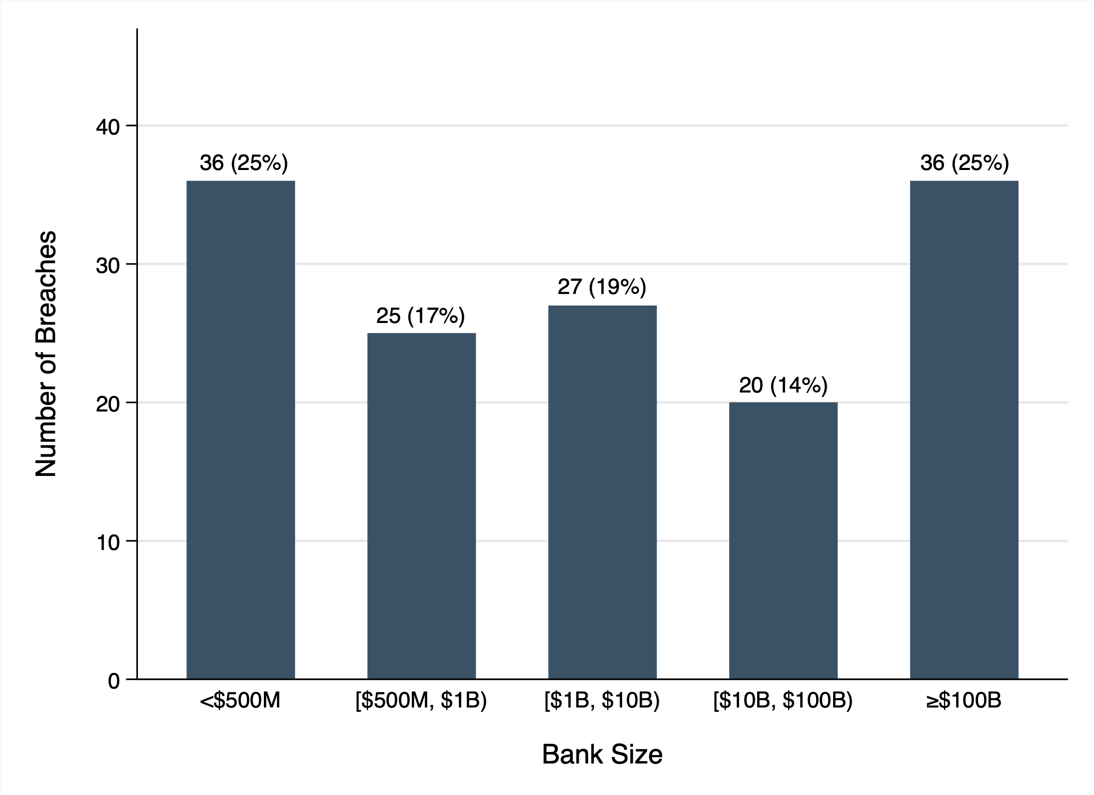
[18] Back-of-the-envelope calculation: In specifications with a three-year post-breach window, in an average breached county, an average breached bank has deposits of \$855.2 million and an average large non-breached bank has deposits of \$602.8 million. There are 1.9 distinct breached banks and 6.2 distinct large non-breached banks in an average breached county. Given the estimated coefficients in column (3) of Table 1.4, $(602.8 \times 0.032 \times 6.2)/(855.2 \times 0.064 \times 1.9) =$ \$1.15

1.8 Conclusion

In this paper, I examine the effects of bank data breaches on deposits, leveraging a novel dataset that identifies data breaches at the bank-state level in the U.S. I find that data breaches not only reduce deposits at breached banks but also lead to a net drop in deposits within the local market due to contrasting spillover effects on small and large non-breached banks. Specifically, over three years after the breach, the deposit level in an average breached bank-state unit drops by about 6.8% relative to its non-breached counterparts. Within an average local deposit market, a breach that triggers a \$1 decrease in deposits at breached banks is associated with a \$0.15 decrease in deposits at small non-breached banks and a \$1 increase in deposits at larger non-breached banks, indicating that a \$1 decrease in deposits at breached banks is associated with a net loss of \$0.15 in the local deposit market. I show suggestive evidence that when small banks are breached in a county, depositors update their beliefs about the vulnerabilities of non-breached small banks in the same county, resulting in lower confidence and reduced deposits.

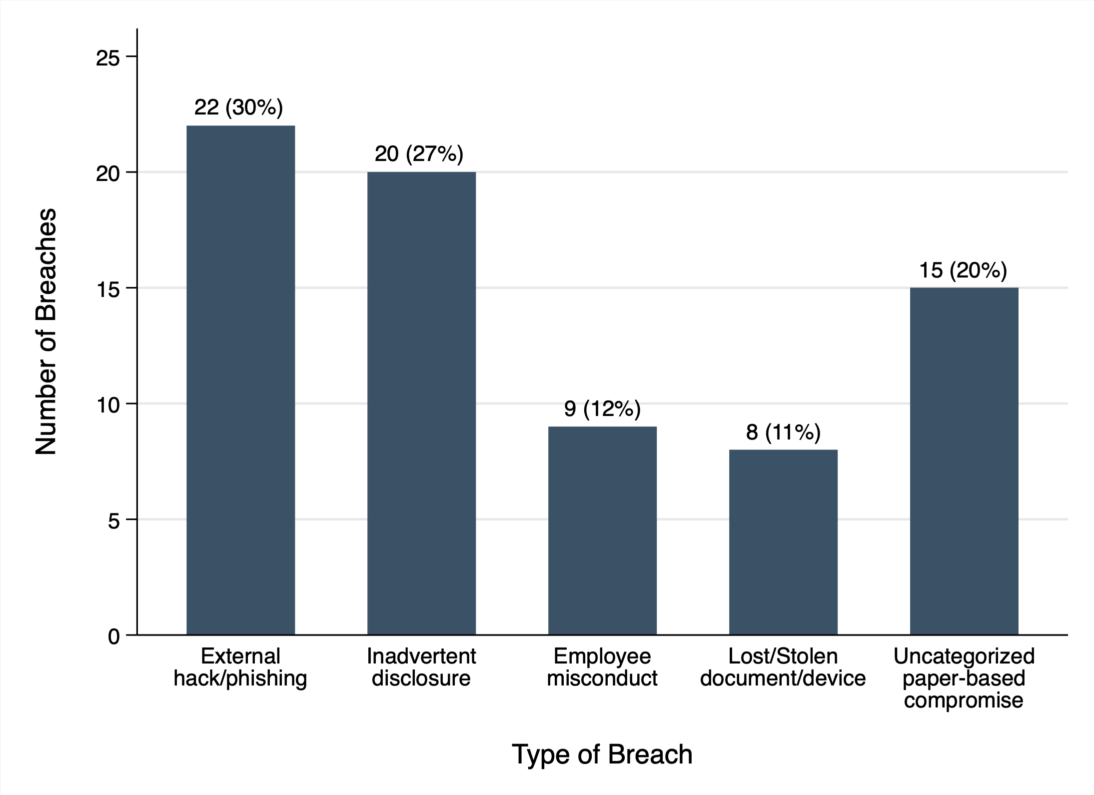
Beyond the local market, I document negative within-bank, cross-state spillovers, which are larger for smaller banks than larger ones. Over three years following a breach, a \$1 decrease in deposits at an average small-to-medium breached bank in a breached state is associated with an approximately \$0.7 decrease in deposits at the same bank in a non-breached state, whereas the same deposit loss at an average large breached bank in a breached state is associated with only a \$0.3 drop in deposits at the same bank in a non-breached state. Further analysis reveals that depositor reactions are primarily driven by privacy concerns and intensify as the scale of the breach increases. These findings are consistent with the “loss of confidence in bank data security” channel.

Figure 1.1: Data Breaches by Bank Size



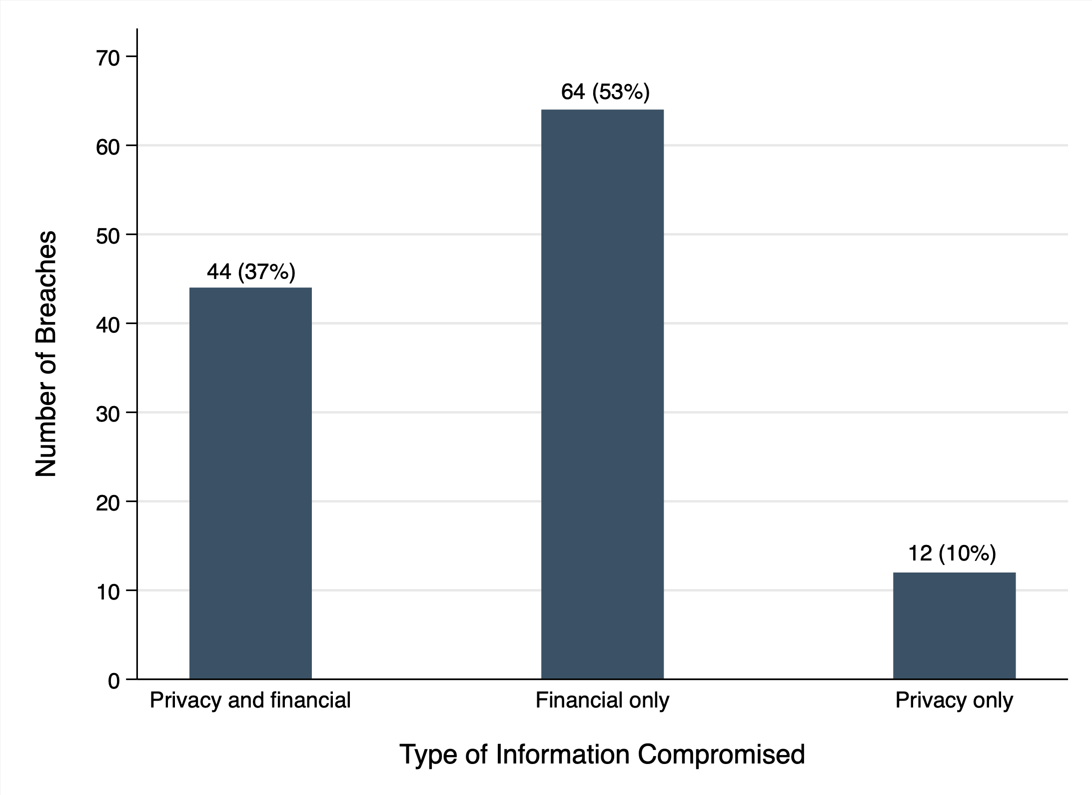
Note. This figure shows the distribution of data breaches in my sample across bank size groups, where bank size is measured by a bank's total assets in the year prior to the breach. Each bar represents the number of breaches within each asset size group. The exact number of breaches is labeled at the top of each bar, with percentages in parentheses indicating each group's share of the total sample (144 breaches).

Figure 1.2: Data Breaches by Breach Type



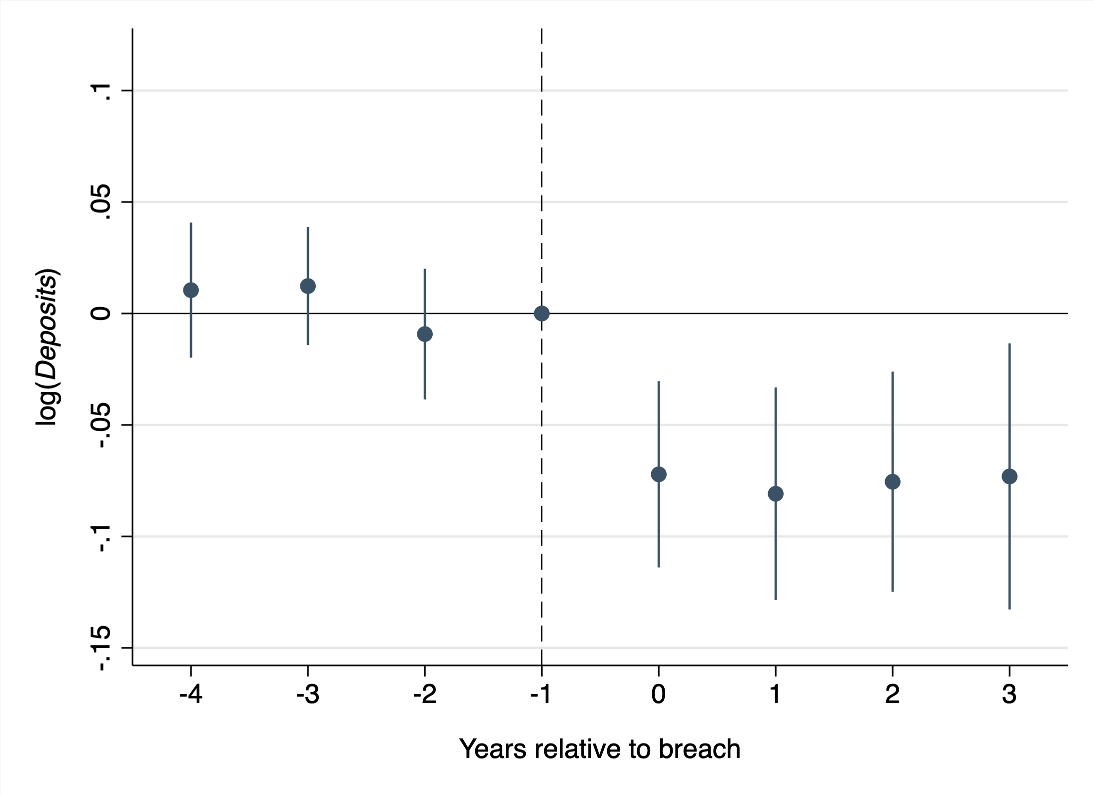
Note. This figure shows the distribution of data breaches across five mutually exclusive breach types. "External hack/phishing": unauthorized access to depositor information through hacking or phishing methods by an outside party. "Inadvertent disclosure": unintentional exposure of depositor information by bank employees or contractors. "Employee misconduct": intentional, unauthorized access to depositor information by bank employees or contractors. "Lost/Stolen document/device": loss or theft of physical documents or devices containing depositor data. "Uncategorized paper-based compromise": compromise of depositor information in physical (non-electronic) form, where detailed nature of the breach is unavailable. This figure is based on breaches with known breach types (74 breaches). Each bar represents the number of breaches within each type. The exact number of breaches is labeled at the top of each bar, with percentages in parentheses indicating each type's share of breaches in the sample with known breach types.

Figure 1.3: Types of Information Compromised in Data Breaches



Note. This figure shows the types of information compromised in data breaches, categorized as breaches involving both privacy and financial information, only financial information, and only privacy information. Privacy information includes Social Security numbers, driver's license or other government-issued ID numbers, and addresses. Financial information includes bank account information and credit/debit card details. This figure is based on breaches with known types of compromised information (120 breaches). Each bar represents the number of breaches within each category, with the exact number labeled at the top of each bar and percentages in parentheses indicating each category's share of breaches in the sample with known types of compromised information.

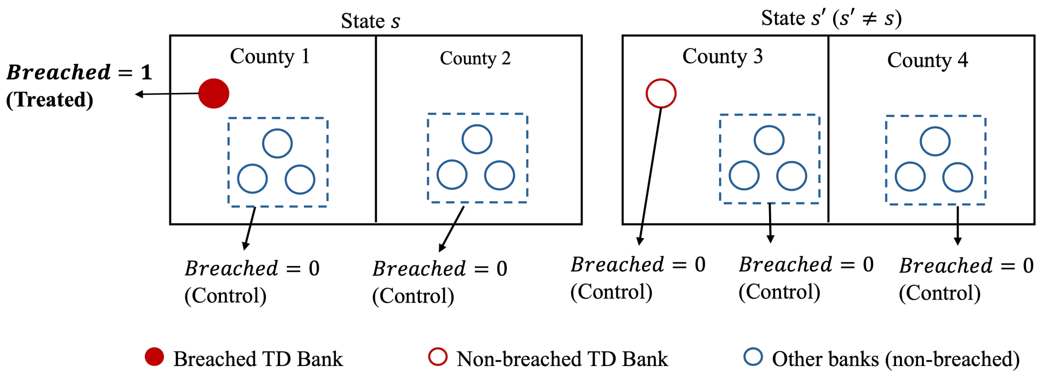
Figure 1.4: Dynamic Effects of Data Breaches on Deposits



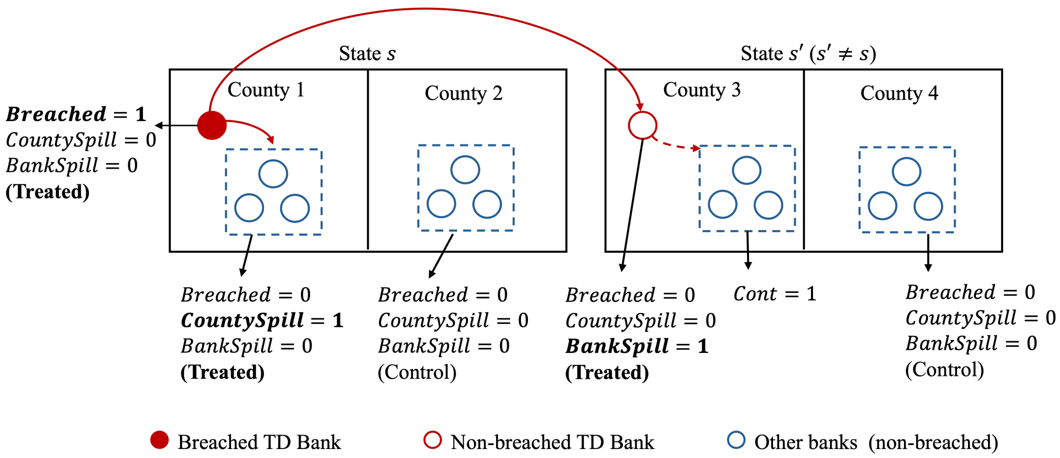
Note. This figure plots the coefficient estimates of $\{\beta_1^k\}$ from the dynamic DiD model of Equation (1.1), where k denotes the number of years relative to the data breach treatment. Year -1 serves as the benchmark and is omitted. The bands around the coefficient estimates represent the 95% confidence intervals. Each stack is weighted by its share of the treated sample. Standard errors are double-clustered at bank and county levels.

Figure 1.5: Graphical Illustration of Baseline and Spillover Specifications

(a) Baseline Specification



(b) Spillover Specification



Note. Panel (a) illustrates the baseline specification (Equation (1.1)). Panel (b) illustrates the spillover specification (Equation (1.2)). In this simplified example, TD Bank operates branches in County 1 of State s and County 3 of State s', but has no branches in County 2 of State s or County 4 of State s'. A data breach occurs at TD Bank in State s (solid red circle), whereas TD Bank in State s' remains unaffected (hollow red circle). Other banks in the counties (hollow blue circles) are not breached. The solid red arrows represent potential within-county spillovers and within-bank, cross-state spillovers. The dashed red arrow indicates potential contamination.

Table 1.1: Summary Statistics

	Mean	P25	Median	P75	SD	N
Panel A: Data Breach Characteristics						
Privacy Breached (1/0)	0.46	0	0	1	0.50	120
Financial Only (1/0)	0.53	0	1	1	0.50	120
Victims (num.)	568.08	2	18	294	2009.10	131
Panel B: Bank Characteristics						
Assets ($M, bank×year)	10,653	67.01	342.03	11,098	108,259	16,175
Deposits ($M, bank×county×year)	410.05	8.17	82.94	1,086	2,998	78,194

Note. This table presents the summary statistics for the main variables used in this study. Panel A summarizes the characteristics of data breach incidents in my sample. "Privacy Breached" is a dummy equal to one if *any* personal privacy information (Social Security number, driver's license or other government-issued ID number, address) was compromised in a breach. "Financial Only" is a dummy equal to one if *only* financial information (bank account information, debit/credit card details) was compromised. "Victims" is the number of affected state residents in a breach. Statistics for "Privacy Breached" and "Financial Only" are based on the subsample with known types of information compromised, and statistics for "Victims" are based on the subsample with a known number of affected residents. (For California and Iowa, of which the state governments do not publish the exact number of affected residents, a value of 500 is imputed, as these states require reporting if at least 500 state residents are impacted.) Panel B characterizes deposits and bank assets for the full sample. "Assets" are measured in millions of dollars at the bank-year level. "Deposits" are measured in millions of U.S. dollars at the bank-county-year level.

Table 1.2: Pre-Breach Bank Characteristics

	Breached banks	Non-breached banks	Difference in means (1)-(2)	t-test
	(1)	(2)	(3)	(4)
Tier 1 Capital Ratio	0.150	0.163	-0.013	-0.620
Non-Performing Loan Ratio	0.011	0.016	-0.005*	-1.852
Equity/Assets	0.106	0.114	-0.008	-1.393
ROA	0.003	0.004	-0.001**	-2.222
log(Assets $M)	8.157	5.986	2.170***	14.163

Note. This table compares pre-breach bank characteristics for breached and non-breached banks. The bank characteristics are Tier 1 capital to risk-weighted assets ratio, non-performing loan ratio, equity-to-asset ratio, and return on assets (ROA). Pre-breach characteristics are measured as the four-year average prior to the breach treatment year of each stack cohort. Columns (1) and (2) report the pre-breach mean values for breached and non-breached banks, respectively. Column (3) presents the difference in means, and column (4) reports the corresponding t-tests. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

Table 1.3: Direct and Spillover Effects of Data Breaches on Deposits

	Dep. var = log(*Deposits*)			
	Up to 1 yr after breach		Up to 3 yrs after breach	
	(1)	(2)	(3)	(4)
Panel A: Baseline Model				
Breached* × *Post	**-0.074*****	**-0.074*****	**-0.068*****	**-0.068*****
	(0.020)	**(0.020)**	**(0.021)**	**(0.021)**
Post	0.003*	0.004**	0.005***	0.004***
	(0.002)	(0.002)	(0.002)	(0.002)
BrNum	0.100***	0.101***	0.102***	0.104***
	(0.024)	(0.024)	(0.019)	(0.020)
Obs.	191,757	191,757	259,269	259,269
R-squared	0.981	0.980	0.977	0.976
Panel B: Spillover Model				
Breached* × *Post	**-0.065*****	**-0.067*****	**-0.068*****	**-0.070*****
	(0.018)	**(0.018)**	**(0.020)**	**(0.020)**
CountySpill × *Post*	0.012**	0.010	0.002	0.000
	(0.006)	(0.006)	(0.007)	(0.008)
BankSpill × *Post*	-0.057**	-0.058**	-0.063**	-0.065**
	(0.023)	(0.023)	(0.028)	(0.029)
Post	0.003	0.004	0.007**	0.008**
	(0.003)	(0.003)	(0.003)	(0.003)
BrNum	0.094***	0.095***	0.098***	0.100***
	(0.026)	(0.026)	(0.021)	(0.021)
Obs.	137,235	137,235	185,996	185,996
R-squared	0.980	0.979	0.975	0.974
Bank × County × Stack FE	Yes	Yes	Yes	Yes
County × Year FE	Yes	Yes	Yes	Yes
Stack Weights	Yes	No	Yes	No

Note. Panel A of this table presents the estimated coefficients of Equation (1.1). Panel B shows the estimated coefficients on main variables in Equation (1.2). The dependent variable across all specifications is $\log(Deposits_{i,c,t,h})$, the natural logarithm of deposits in \$000s of bank i in county c in year t. $Breached_{i,s,h}$ is a dummy equal to one if bank i in state s is breached in stack h. $CountySpill_{i,c,h}$ is a dummy equal to one if bank i in county c is subject to within-county spillovers of data breaches in stack h. $BankSpill_{i,s,h}$ is a dummy equal to one if bank i in state s is subject to within-bank spillovers in stack h. $Post_{t,h}$ is a post-event dummy equal to one for year $t \geq h$ in stack h. The control variable is $BrNum_{i,c,t,h}$, the number of branches of bank i in county c in year t. Specifications (1) and (2) apply an event window of [-4 years, +1 year] around each treatment year, and specifications (3) and (4) apply an event window of [-4 years, +3 years]. Specifications (1) and (3) weight each stack cohort by its share of the treated sample. Specifications (2) and (4) assign no stack cohort weights. All specifications include bank-county-stack fixed effects and county-year fixed effects. Standard errors are double-clustered at bank and county levels and are reported in parentheses. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

Table 1.4: Within-County Spillover Effects by Bank Size

	Dep. var = log(*Deposits*)			
	Up to 1 yr after breach		Up to 3 yrs after breach	
	(1)	(2)	(3)	(4)
Breached × *Post*	-0.064***	-0.066***	-0.067***	-0.070***
	(0.018)	(0.018)	(0.020)	(0.020)
CountySpill* × *Post				
×***MedLarg***	**0.036*** **	**0.034*** **	**0.029*** **	**0.027** **
	(0.009)	**(0.009)**	**(0.010)**	**(0.011)**
×***Small***	**-0.050*** **	**-0.052*** **	**-0.068*** **	**-0.071*** **
	(0.014)	**(0.014)**	**(0.017)**	**(0.018)**
BankSpill × *Post*	-0.057**	-0.058**	-0.063**	-0.065**
	(0.023)	(0.023)	(0.028)	(0.029)
Obs.	137,235	137,235	185,996	185,996
R-squared	0.980	0.979	0.975	0.974
Bank × County × Stack FE	Yes	Yes	Yes	Yes
County × Year FE	Yes	Yes	Yes	Yes
Stack Weights	Yes	No	Yes	No

Note. This table reports the estimated coefficients of Equation (1.3). *Small* is a dummy equal to one if the bank's asset value is below $500 million in the year prior to the breach. *MedLarg* is a dummy equal to one if the bank's asset value is $500 million or more in the year prior to the breach. Specifications (1) and (2) apply an event window of [-4 years, +1 year] around each treatment year, and specifications (3) and (4) apply an event window of [-4 years, +3 years]. Specifications (1) and (3) weight each stack cohort by its share of the treated sample. Specifications (2) and (4) assign no stack cohort weights. All specifications include bank-county-stack fixed effects and county-year fixed effects. Standard errors are double-clustered at bank and county levels and are reported in parentheses. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

Table 1.5: Within-County Spillover Effects: Mechanism

	Dep. var = log(*Deposits*)			
	Up to 1 yr after breach		Up to 3 yrs after breach	
	(1)	(2)	(3)	(4)
$Breached \times Post$	-0.064***	-0.066***	-0.067***	-0.069***
	(0.018)	(0.018)	(0.020)	(0.020)
$CountySpill \times Post$				
$\times MedLarg$	0.033***	0.031***	0.025**	0.023**
	(0.009)	(0.009)	(0.011)	(0.011)
$\times Small$	-0.044***	-0.046***	-0.059***	-0.063***
	(0.015)	(0.015)	(0.018)	(0.019)
$\boldsymbol{CountySpill \times Post}$				
$\times MedLarg \times NumSmallBrea$	0.028	0.029	0.041*	0.042*
	(0.019)	(0.019)	(0.022)	(0.022)
$\boldsymbol{\times Small \times NumSmallBrea}$	**-0.026***	**-0.025**	**-0.034****	**-0.033****
	(0.016)	**(0.016)**	**(0.016)**	**(0.017)**
$BankSpill \times Post$	-0.057**	-0.059**	-0.063**	-0.065**
	(0.023)	(0.023)	(0.028)	(0.029)
Obs.	137,235	137,235	185,996	185,996
R-squared	0.980	0.979	0.975	0.974
Bank × County × Stack FE	Yes	Yes	Yes	Yes
County × Year FE	Yes	Yes	Yes	Yes
Stack Weights	Yes	No	Yes	No

Note. This table reports the estimated coefficients of Equation (1.3) with two additional interaction terms: $CountySpill_{i,c,h} \times Post_{t,h} \times Small_{i,h} \times NumSmallBrea_{c,h}$ and $CountySpill_{i,c,h} \times Post_{t,h} \times MedLarg_{i,h} \times NumSmallBrea_{c,h}$, where $NumSmallBrea_{c,h}$ is the number of distinct small breached banks in county c in stack h. A breached bank is small if its asset value in the year prior to the breach is below $500 million. Specifications (1) and (2) apply an event window of [-4 years, +1 year] around each treatment year, and specifications (3) and (4) apply an event window of [-4 years, +3 years]. Specifications (1) and (3) weight each stack cohort by its share of the treated sample. Specifications (2) and (4) assign no stack cohort weights. All specifications include bank-county-stack fixed effects and county-year fixed effects. Standard errors are double-clustered at bank and county levels and are reported in parentheses. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

Table 1.6: Heterogeneous Effects Across Breached Bank Size

	Dep. var = log(*Deposits*)			
	Up to 1 yr after breach		Up to 3 yrs after breach	
	(1)	(2)	(3)	(4)
Panel A: Full Sample				
Breached* × *Post				
×***LargBrea***	**-0.081*****	**-0.083*****	**-0.072*****	**-0.073*****
	(0.026)	**(0.027)**	**(0.027)**	**(0.027)**
×***SmallMedBrea***	**-0.054****	**-0.055****	**-0.065****	**-0.067****
	(0.023)	**(0.023)**	**(0.027)**	**(0.027)**
BankSpill* × *Post				
×***LargBrea***	**-0.056****	**-0.058****	**-0.063****	**-0.065****
	(0.024)	**(0.024)**	**(0.029)**	**(0.030)**
×***SmallMedBrea***	**-0.065****	**-0.067*****	**-0.059***	**-0.061****
	(0.026)	**(0.026)**	**(0.031)**	**(0.030)**
CountySpill × *Post*	0.012**	0.010	0.002	0.000
	(0.006)	(0.006)	(0.007)	(0.008)
Obs.	137,235	137,235	185,996	185,996
R-squared	0.980	0.979	0.975	0.974
Panel B: Subsample				
Breached* × *Post				
×***LargBrea***	**-0.081*****	**-0.083*****	**-0.072*****	**-0.073*****
	(0.026)	**(0.027)**	**(0.027)**	**(0.027)**
×***SmallMedBrea***	**-0.053****	**-0.055****	**-0.065****	**-0.067****
	(0.023)	**(0.023)**	**(0.027)**	**(0.027)**
BankSpill* × *Post				
×***LargBrea***	**-0.059*****	**-0.060*****	**-0.067*****	**-0.070*****
	(0.021)	**(0.020)**	**(0.026)**	**(0.026)**
×***SmallMedBrea***	**-0.061***	**-0.065***	**-0.069**	**-0.071***
	(0.034)	**(0.034)**	**(0.042)**	**(0.041)**
CountySpill × Post	0.012**	0.010	0.002	0.000
	(0.006)	(0.006)	(0.007)	(0.008)
Obs.	136,709	136,709	185,290	185,290
R-squared	0.980	0.979	0.975	0.974
Bank × County × Stack FE	Yes	Yes	Yes	Yes
County × Year FE	Yes	Yes	Yes	Yes
Stack Weights	Yes	No	Yes	No

Note. This table presents the estimated coefficients of Equation (1.4). Panel A is on the full sample. Panel B is on the subsample: I exclude observations that are subject to within-bank spillovers and are located in non-breached states that are geographically adjacent to the breached state. *LargBrea* (*SmallMedBrea*) is a dummy equal to one if the breached bank has assets≥\$100 billion (assets<\$100 billion) in the year prior to the breach. Specifications (1) and (2) apply an event window of [-4 years, +1 year] around each treatment year, and specifications (3) and (4) apply an event window of [-4 years, +3 years]. Specifications (1) and (3) weight each stack cohort by its share of the treated sample. Specifications (2) and (4) assign no stack cohort weights. All specifications include bank-county-stack fixed effects and county-year fixed effects. Standard errors are double-clustered at bank and county levels and are reported in parentheses. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

Table 1.7: Heterogeneous Effects Across Types of Information Compromised

	Dep. var = log(*Deposits*)			
	Up to 1 yr after breach		Up to 3 yrs after breach	
	(1)	(2)	(3)	(4)
Breached* × *Post				
×***Privacy***	**-0.074*****	**-0.076*****	**-0.067****	**-0.069****
	(0.027)	**(0.028)**	**(0.027)**	**(0.028)**
×***FinOnly***	**-0.046**	**-0.048**	**-0.053**	**-0.055**
	(0.034)	**(0.034)**	**(0.039)**	**(0.039)**
BankSpill* × *Post				
×***Privacy***	**-0.080*****	**-0.082*****	**-0.094*****	**-0.096*****
	(0.019)	**(0.019)**	**(0.020)**	**(0.021)**
×***FinOnly***	**-0.013**	**-0.014**	**0.000**	**-0.001**
	(0.033)	**(0.034)**	**(0.039)**	**(0.039)**
CountySpill × *Post*	0.012**	0.010	0.002	-0.000
	(0.006)	(0.006)	(0.007)	(0.008)
Obs.	135,061	135,061	183,225	183,225
R-squared	0.980	0.979	0.975	0.974
Bank × County × Stack FE	Yes	Yes	Yes	Yes
County × Year FE	Yes	Yes	Yes	Yes
Stack Weights	Yes	No	Yes	No

Note. This table reports the estimated coefficients of Equation (1.5). *Privacy* is a dummy equal to one if *any* personal privacy information (Social Security number, driver's license or other government-issued ID number, address) is compromised in the data breach. *FinOnly* is a dummy equal to one if *only* financial information (bank account information, debit/credit card details) is compromised in the breach. Specifications (1) and (2) apply an event window of [-4 years, +1 year] around each treatment year, and specifications (3) and (4) apply an event window of [-4 years, +3 years]. Specifications (1) and (3) weight each stack cohort by its share of the treated sample. Specifications (2) and (4) assign no stack cohort weights. All specifications include bank-county-stack fixed effects and county-year fixed effects. This analysis is based on the subsample with known types of information compromised. Standard errors are double-clustered at bank and county levels and are reported in parentheses. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

Table 1.8: Heterogeneous Effects Across Scale of Breaches

	Dep. var = log(*Deposits*)			
	$q = 18$		$q = 294$	
	(1)	(2)	(3)	(4)
$\boldsymbol{Breached \times Post}$				
$\times \mathbb{1}[\boldsymbol{Victims > q}]$	**-0.081*** **	**-0.083*** **	**-0.083*** **	**-0.085*** **
	(0.025)	**(0.025)**	**(0.028)**	**(0.029)**
$\times \mathbb{1}[\boldsymbol{Victims \leq q}]$	**-0.035**	**-0.036**	**-0.050***	**-0.052***
	(0.035)	**(0.035)**	**(0.030)**	**(0.030)**
$\boldsymbol{BankSpill \times Post}$				
$\times \mathbb{1}[\boldsymbol{Victims > q}]$	**-0.121*** **	**-0.123*** **	**-0.124*** **	**-0.126*** **
	(0.023)	**(0.023)**	**(0.026)**	**(0.026)**
$\times \mathbb{1}[\boldsymbol{Victims \leq q}]$	**-0.038**	**-0.040**	**-0.050***	**-0.052***
	(0.033)	**(0.034)**	**(0.027)**	**(0.028)**
$CountySpill \times Post$	0.003	0.001	0.003	0.001
	(0.007)	(0.008)	(0.007)	(0.008)
Obs.	184,710	184,710	184,710	184,710
R-squared	0.975	0.974	0.975	0.974
Bank × County × Stack FE	Yes	Yes	Yes	Yes
County × Year FE	Yes	Yes	Yes	Yes
Stack Weights	Yes	No	Yes	No

Note. This table presents the estimated coefficients of Equation (1.6) with an event window of [-4 years, +3 years]. The dummy variable $\mathbb{1}[Victims > q]$ ($\mathbb{1}[Victims \leq q]$) is equal to one if the data breach affects > 18 (≤ 18) residents in the breached state in specifications (1) and (2), and > 294 (≤ 294) residents in specifications (3) and (4). Specifications (1) and (3) weight each stack cohort by its share of the treated sample. Specifications (2) and (4) assign no stack cohort weights. All specifications include bank-county-stack fixed effects and county-year fixed effects. This analysis is based on the subsample with a known number of affected residents. For California and Iowa, of which the state governments do not publish the exact number of affected residents, a value of 500 is imputed, as these states require reporting if at least 500 state residents are impacted. Standard errors are double-clustered at bank and county levels and are reported in parentheses. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

Table 1.9: Effects of Data Breaches on Bank Deposit Spreads

	Dep. var = *Deposit Spread*					
	Savings		Uninsured time		Insured time	
	(1)	(2)	(3)	(4)	(5)	(6)
Breached × *Post*	-0.076***	-0.078***	-0.098**	-0.095**	0.022	0.013
	(0.025)	(0.024)	(0.041)	(0.041)	(0.085)	(0.071)
Post	-0.006	-0.002	0.009	0.000	-0.009	-0.082
	(0.006)	(0.002)	(0.010)	(0.005)	(0.021)	(0.080)
Obs.	36,384	36,384	36,372	36,372	36,374	36,374
R-squared	0.955	0.954	0.860	0.840	0.647	0.630
Bank × Stack FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Stack Weights	Yes	No	Yes	No	Yes	No

Note. This table presents the estimated coefficients of Equation (1.7) with an event window of [-4 years, +1 year]. The dependent variable is the bank-level deposit spread, defined as the federal funds rate minus the bank's interest rate on the corresponding deposit product (savings deposits, uninsured time deposits, and insured time deposits. Specifications (1), (3), and (5) weight each stack cohort by its share of the treated sample. Specifications (2), (4), and (6) assign no stack cohort weights. All specifications include bank-stack fixed effects and year fixed effects. Standard errors are clustered at the bank level and are reported in parentheses. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

1.9 Appendix

A. Bank Data Breaches

This section provides supplementary information on bank data breaches. Figure A1.1 shows the timeline of state-level adoption of data breach notification laws in the U.S. Figure A1.2 presents examples of breach notices published by state governments. Upon detecting a breach, the breached entity sends a notification letter to the governments of the affected states. The letter is issued by either the breached entity itself or a solicitor representing the entity. An example of such a letter is provides in Panel (a) of Figure A1.2, with the name and address of the breached entity, number of breached state residents, and types of information compromised highlighted. Among the states that publish breach notices, some retain all original letters on their websites, while others only retain recent letters and provide summary reports for historical breaches. Those reports present information on each historical breach incident. Panel (b) of Figure A1.2 shows an example of such a report on historical breach notices. Table A1.1 provides additional details on data breach notification laws in the 14 states in my sample. Table A1.2 provides definitions and examples of each breach type in my sample.

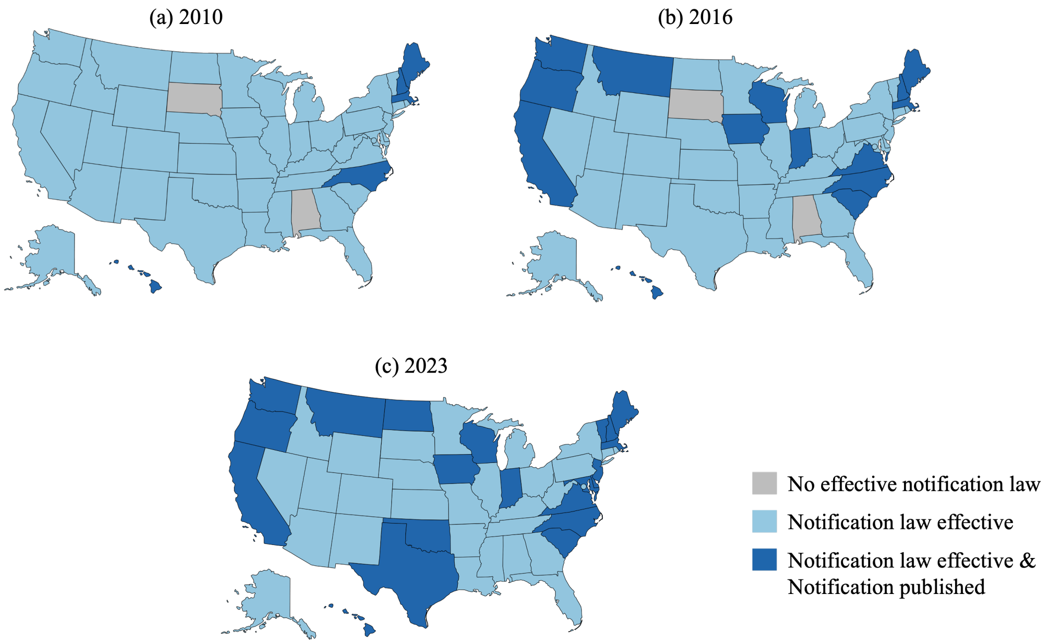
B. Additional Tests

Alternative empirical approach. In Section 1.7, I assess an alternative empirical approach that identifies data breaches at the bank level rather than the bank-state level. Table A1.3 presents the results of this alternative approach.

Robustness checks. In my main analysis, standard errors are double-clustered at the bank and county levels to account for potential correlations in errors within banks across regions as well as potential correlations in errors within counties across banks. I show in Table A1.4, Table A1.5, and Table A1.6 that my main results are robust to a more conservative approach, where standard errors are double-clustered at the bank and state levels.

Appendix Figures and Tables

Figure A1.1: Timeline of Notification Law Adoption



Note. This figure shows the timeline of state-level adoption of data breach notification laws. Panels (a), (b), and (c) display the adoption status by the end of the calendar years 2010, 2016, and 2023, respectively.

Figure A1.2: Examples of State Notices

(a) Letter

Re: **Notice of Data Event**

Dear Sir or Madam:

We represent Timberland Bank, ("Timberland"), 624 Simpson Ave, Hoquiam, WA 98550, and are writing to notify your office of an incident that may affect the security of personal information relating to five thousand, six hundred and eighty-eight (5,688) Washington residents. The investigation into this event is ongoing, and this notice will be supplemented with any new significant facts learned subsequent to its submission. By providing this notice, Timberland does not waive any rights or defenses regarding the applicability of Washington law or personal jurisdiction.

Nature of the Data Event

On July 18, 2017, Timberland learned that courier bags containing loan files and paper checks processed by Timberland had been stolen from the company that provides Timberland's courier services during transit between Timberland branches. Immediately following the incident, the theft was reported to the Thurston County Sheriff's Office.

The following types of personal information may have been contained in the loan files and on the checks: name, address, date of birth, Social Security number, driver's license information, telephone number, bank account information, routing number, and other documentation typically collected during the loan application process.

(b) Report

CHARLES D. BAKER
GOVERNOR

KARYN E. POLITO
LIEUTENANT GOVERNOR

COMMONWEALTH OF MASSACHUSETTS
Office of Consumer Affairs and Business Regulation
501 Boylston Street, Suite 5100, Boston, MA 02116
(617) 973-8700 FAX (617) 973-8799
www.mass.gov/consumer

MIKE KENNEALY
SECRETARY OF HOUSING AND ECONOMIC DEVELOPMENT

JOHN C. CHAPMAN
UNDERSECRETARY

Data Breach Notification Report

Assigned Breach Number	Date Reported To OCA	Organization Name	Breach Type Description	Breach Occur at the Reporting Entity?	MA Residents Affected	SSNBreached	Account Number Breached	Drivers Licenses Breached	Credit Debit Numbers Breached	Provided Credit Monitoring	Data Encrypted	Mobile Device Lost Stolen
12310	1/3/2018	LiveGlam Inc	Electronic		393				Yes	Yes		
12303	1/3/2018	Watertown Savings Bank	Electronic		13				Yes			
12304	1/3/2018	Webster Five Cents Savings Bank	Electronic		56				Yes			
12305	1/3/2018	DJI Technology, Inc.	Electronic		20			Yes				
12306	1/3/2018	Multnomah Athletic Club	Paper	Yes	18	Yes	Yes			Yes		
12326	1/3/2018	Bank of America	Electronic	Yes	2	Yes	Yes			Yes		
12327	1/3/2018	Mutual One Bank	Electronic		1				Yes			
12328	1/3/2018	Savers Bank	Electronic		2				Yes			
12308	1/4/2018	American Express Travel Related Services Company Inc	Electronic		12				Yes	Yes		

Note. This figure presents examples of breach notices published by state governments. Panel (a) shows an example of a notification letter for a specific breach incident. Panel (b) shows an example of a report summarizing historical breach notices.

Table A1.1: Data Breach Notification Laws in Sample States

State	**Initial Implementation Year**	**Notify State Government**	**Publish Notices Since**
CA	2003	Yes if $\geq$500 California residents affected	2012
HI	2007	Yes if $\geq$1000 Hawaii residents affected	2007
IN	2006	Yes if any Indiana resident affected	2014
IA	2008	Yes if $\geq$500 Iowa residents affected	2011
ME	2006	Yes if any Maine resident affected	2010
MA	2007	Yes if any Massachusetts resident affected	2007
MT	2006	Yes if any Montana resident affected	2015
NH	2007	Yes if any New Hampshire resident affected	2007
NC	2005	Yes if any North Carolina resident affected	2005
OR	2007	Yes if $\geq$250 Oregon residents affected	2015
SC	2009	Yes if $\geq$1000 South Carolina residents affected	2015
VA	2008	Yes if any Virginia resident affected	2012
WA	2005	Yes if $\geq$500 Washington residents affected	2015
WI	2006	Yes if $\geq$1000 Wisconsin resident affected	2012

Note. This table provides details on data breach notification laws in the sampled states. It includes the initial implementation year of each state's notification law, the threshold for notifying the state government based on the number of affected residents, and the year when each state government began publishing breach notices. A data breach is defined as the unauthorized access or acquisition of records or data containing personal information. Personal information includes an individual's name, Social Security number, driver's license or other government-issued ID number, address, bank account information, and credit/debit card details.

Table A1.2: Definitions and Examples of Breach Types

Breach Type	Definition	Example
External hack/phishing	Unauthorized access to depositor information through hacking or phishing methods by an outside party	"A single bank employee email account was compromised on April 5, 2018, by an unknown and unauthorized person spoofing the identity of a different banker at a different bank."
Inadvertent disclosure	Unintentional exposure of depositor information by bank employees or contractors	"As a service provider for the University of New Hampshire, we recently learned that a file containing student names and bank account numbers was inadvertently emailed on January 16th by TD to the University in a way that was inconsistent with our standard protocol."
Employee misconduct	Intentional, unauthorized access to depositor information by bank employees or contractors	"...one of our employees may have improperly obtained customer information and provided it to an unauthorized party."
Lost/Stolen document/device	Loss or theft of physical documents or devices containing depositor data	"... a back-up tape containing certain of your personal information including account number(s), account balances, taxpayer identification number, and social security number was stolen on February 1, 2013. This theft did not involve any of our employees."
Uncategorized paper-based compromise	Compromise of depositor information in physical (non-electronic) form, where detailed nature of the breach is unavailable	N/A

Note. This table provides definitions and examples of each breach type in my sample.

Table A1.3: Bank-Level Treatment: Within-County Spillover Effects

	Dep. var = log(*Deposits*)			
	Up to 1 yr after breach		Up to 3 yrs after breach	
	(1)	(2)	(3)	(4)
Breached × *Post*	-0.061***	-0.062***	-0.064***	-0.064***
	(0.018)	(0.018)	(0.020)	(0.020)
CountySpill × *Post*				
×*MedLarg*	0.039***	0.038***	0.032***	0.032***
	(0.009)	(0.009)	(0.009)	(0.009)
×*Small*	-0.047***	-0.048***	-0.065***	-0.066***
	(0.013)	(0.013)	(0.017)	(0.017)
Obs.	137,235	137,235	185,996	185,996
R-squared	0.980	0.979	0.975	0.974
Bank × County × Stack FE	Yes	Yes	Yes	Yes
County × Year FE	Yes	Yes	Yes	Yes
Stack Weights	Yes	No	Yes	No

Note. This table reports the estimated coefficients of Equation (1.8). *Small* is a dummy equal to one if the bank's asset value is below $500 million in the year of breach. *MedLarg* is a dummy equal to one if the bank's asset value is $500 million or more in the year of breach. Specifications (1) and (2) apply an event window of [-4 years, +1 year] around each treatment year, and specifications (3) and (4) apply an event window of [-4 years, +3 years]. Specifications (1) and (3) weight each stack cohort by its share of the treated sample. Specifications (2) and (4) assume no stack cohort weights. All specifications include bank-county-stack fixed effects and county-year fixed effects. Standard errors are double-clustered at bank and county levels and are reported in parentheses. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

Table A1.4: Robustness: Within-County Spillover Effects

	Dep. var = log(*Deposits*)			
	Up to 1 yr after breach		Up to 3 yrs after breach	
	(1)	(2)	(3)	(4)
Breached × *Post*	-0.064***	-0.066***	-0.067***	-0.070***
	(0.018)	(0.019)	(0.020)	(0.021)
CountySpill* × *Post				
×***MedLarg***	**0.036*** **	**0.034*** **	**0.029** **	**0.027** **
	(0.007)	**(0.007)**	**(0.010)**	**(0.010)**
×***Small***	**-0.050*** **	**-0.052*** **	**-0.068*** **	**-0.071*** **
	(0.011)	**(0.011)**	**(0.015)**	**(0.015)**
BankSpill × *Post*	-0.057**	-0.058**	-0.063**	-0.065**
	(0.019)	(0.020)	(0.024)	(0.024)
Obs.	137,235	137,235	185,996	185,996
R-squared	0.980	0.979	0.975	0.974
Bank × County × Stack FE	Yes	Yes	Yes	Yes
County × Year FE	Yes	Yes	Yes	Yes
Stack Weights	Yes	No	Yes	No

Note. This table reports the estimated coefficients of Equation (1.3). *Small* is a dummy equal to one if the bank's asset value is below $500 million in the year of breach. *MedLarg* is a dummy equal to one if the bank's asset value is $500 million or more in the year of breach. Specifications (1) and (2) apply an event window of [-4 years, +1 year] around each treatment year, and specifications (3) and (4) apply an event window of [-4 years, +3 years]. Specifications (1) and (3) weight each stack cohort by its share of the treated sample. Specifications (2) and (4) assume no stack cohort weights. All specifications include bank-county-stack fixed effects and county-year fixed effects. Standard errors are double-clustered at bank and state levels and are reported in parentheses. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

Table A1.5: Robustness: Heterogeneous Effects Across Breached Bank Size

	Dep. var = log(*Deposits*)			
	Up to 1 yr after breach		Up to 3 yrs after breach	
	(1)	(2)	(3)	(4)
Breached $\times$ ***Post***				
$\times$***LargBrea***	**-0.081****	**-0.083****	**-0.072****	**-0.073****
	(0.035)	**(0.036)**	**(0.029)**	**(0.031)**
$\times$***SmallMedBrea***	**-0.054*****	**-0.055*****	**-0.065*****	**-0.067*****
	(0.016)	**(0.016)**	**(0.021)**	**(0.021)**
BankSpill $\times$ ***Post***				
$\times$***LargBrea***	**-0.056****	**-0.058****	**-0.063****	**-0.065****
	(0.021)	**(0.021)**	**(0.025)**	**(0.026)**
$\times$***SmallMedBrea***	**-0.065****	**-0.067*****	**-0.059****	**-0.061****
	(0.027)	**(0.021)**	**(0.025)**	**(0.023)**
CountySpill $\times$ *Post*	0.012**	0.010**	0.002	0.000
	(0.005)	(0.004)	(0.006)	(0.005)
Obs.	137,235	137,235	185,996	185,996
R-squared	0.980	0.979	0.975	0.974

Note. This table presents the estimated coefficients of Equation (1.4). This analysis is based on the full sample. *LargBrea* (*SmallMedBrea*) is a dummy equal to one if the breached bank has assets≥\$100 billion (assets<\$100 billion) in the year prior to the breach. Specifications (1) and (2) apply an event window of [-4 years, +1 year] around each treatment year, and specifications (3) and (4) apply an event window of [-4 years, +3 years]. Specifications (1) and (3) weight each stack cohort by its share of the treated sample. Specifications (2) and (4) assume no stack cohort weights. All specifications include bank-county-stack fixed effects and county-year fixed effects. Standard errors are double-clustered at bank and state levels and are reported in parentheses. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

Table A1.6: Robustness: Heterogeneous Effects Across Types of Information Compromised

	Dep. var = log(*Deposits*)			
	Up to 1 yr after breach		Up to 3 yrs after breach	
	(1)	(2)	(3)	(4)
Breached $\times$ ***Post***				
$\times$***Privacy***	**-0.074****	**-0.076****	**-0.067****	**-0.069****
	(0.026)	**(0.027)**	**(0.026)**	**(0.027)**
$\times$***FinOnly***	**-0.046**	**-0.048**	**-0.053**	**-0.055**
	(0.029)	**(0.029)**	**(0.039)**	**(0.040)**
BankSpill $\times$ ***Post***				
$\times$***Privacy***	**-0.080*****	**-0.082*****	**-0.094*****	**-0.096*****
	(0.017)	**(0.017)**	**(0.018)**	**(0.018)**
$\times$***FinOnly***	**-0.013**	**-0.014**	**0.000**	**-0.001**
	(0.037)	**(0.038)**	**(0.042)**	**(0.042)**
CountySpill $\times$ *Post*	0.012**	0.010**	0.002	-0.000
	(0.004)	(0.004)	(0.005)	(0.005)
Obs.	135,061	135,061	183,225	183,225
R-squared	0.980	0.979	0.975	0.974
Bank $\times$ County $\times$ Stack FE	Yes	Yes	Yes	Yes
County $\times$ Year FE	Yes	Yes	Yes	Yes
Stack Weights	Yes	No	Yes	No

Note. This table reports the estimated coefficients of Equation (1.5). *Privacy* is a dummy equal to one if *any* personal privacy information (Social Security number, driver's license or other government-issued ID number, address) is compromised in the data breach. *FinOnly* is a dummy equal to one if *only* financial information (bank account information, debit/credit card details) is compromised in the breach. Specifications (1) and (2) apply an event window of [-4 years, +1 year] around each treatment year, and specifications (3) and (4) apply an event window of [-4 years, +3 years]. Specifications (1) and (3) weight each stack cohort by its share of the treated sample. Specifications (2) and (4) assume no stack cohort weights. All specifications include bank-county-stack fixed effects and county-year fixed effects. This analysis is based on the subsample with known types of information compromised. Standard errors are double-clustered at bank and state levels and are reported in parentheses.***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

Chapter 2

Digital Reporting and Corporate Sustainability Information

Abstract

This paper examines the impact of digital reporting on the sustainability information environment. Exploiting the staggered implementation of the SEC's iXBRL mandate as a quasi-experiment, I find that digital reporting induces firms to expand sustainability disclosure, reduces ESG rating disagreement, but also incentivizes firms to engage in cheap talk. These findings suggest that digital reporting enhances the accessibility and comparability of sustainability information but may undermine its quality. This paper highlights both the promise and unintended consequences of emerging technologies in shaping the non-financial information environment and informs policy debates on how digital tools can advance, but also complicate, the sustainable finance agenda.

2.1 Introduction

Digital technologies are transforming the global financial systems and hold significant potential to accelerate the sustainable finance agenda. A persistent challenge in sustainability transitions is the limited accessibility, comparability, and reliability of corporate sustainability information, which constrains capital markets from effectively assessing sustainability risks and allocating capital toward sustainable assets. To overcome these information frictions, regulators increasingly view digital reporting as a powerful solution. However, concerns remain that digital reporting may amplify non-substantive disclosure, making firms louder about sustainability without being more truthful. This paper investigates whether the digitalization of sustainability reporting genuinely improves the sustainability information environment in capital markets.

To examine the effects of digital reporting, I exploit the U.S. Securities and Exchange Commission (SEC)'s mandate on inline eXtensible Business Reporting Language (iXBRL), which provides an exogenous shock to the digitalization of sustainability reporting. I find that, following the adoption of iXBRL, firms disclose significantly more sustainability information and that disagreement among environmental, social, and governance (ESG) rating agencies declines. However, I find no improvement in the ability of ESG ratings to predict firms' future sustainability outcomes, and textual analysis of company annual reports reveals an increase in non-substantive disclosure. Taken together, the evidence suggests that while digital reporting enhances the accessibility and comparability of sustainability information, it can undermine the quality of that information.

On June 28, 2018, the SEC adopted the iXBRL mandate that requires all U.S. registrants to file annual and quarterly reports in the iXBRL format. In contrast to the well-documented 2009 XBRL mandate, which primarily transformed the reporting of financial information, the iXBRL mandate extends digitalization to non-financial information, such as sustainability disclosure. By allowing firms to apply machine-readable tags to sustainability metrics and to structure narrative disclosures in extractable formats, iXBRL is expected to reduce the costs of extracting and analyzing corporate sustainability information for market participants.

The SEC's iXBRL mandate provides a useful setting to examine the causal effects of digital reporting on the sustainability information environment. The mandate was imposed exogenously by the regulator, targeting disclosure format rather than content. Moreover, the mandate was implemented in three phases based on firms' filer status. Filer status is the SEC's classification of firms as large accelerated, accelerated, or non-accelerated filers, based primarily on firms' public float. This staggered implementation allows for a staggered difference-in-differences research design. I find no evidence that firms manipulated their filer status to influence their treatment timing, and show that the parallel pre-trends assumption is supported. To address potential biases associated with traditional two-way fixed effects estimators under staggered treatments (e.g., De Chaisemartin and d'Haultfoeuille (2020), Callaway and Sant'Anna (2021), Goodman-Bacon (2021), Baker et al. (2022), and Borusyak et al. (2024)), I implement the doubly robust estimator introduced by Sant'Anna and Zhao (2020) and Callaway and Sant'Anna (2021).

This paper first examines the impact of digital reporting on the extent of corporate sustainability disclosure. I hypothesize that the adoption of iXBRL increases the extent of sustainability disclosure by firms. This hypothesis follows from the role of information processing costs in shaping disclosure incentives (see Blankespoor et al. (2020) for a review). The adoption of iXBRL is expected to reduce information processing costs for market participants, thereby increasing both the benefits of disclosure and the costs of withholding it.

When investors face lower information processing costs, they incorporate disclosures into decisions more promptly, which reduces information asymmetry, improves stock liquidity, and lowers firms' cost of capital. These capital market outcomes represent greater benefits firms gain from disclosure, strengthening firms' incentives to provide more information. Meanwhile, lower information processing costs strengthen monitoring from regulators over company disclosure.[1] Machine-readable and structured reporting formats enable the analysis of larger quantities of information and peer benchmarking, making omissions or insufficient disclosure more visible. As a result, firms may expect to face higher regulatory costs of withholding information.

[1]For example, machine-readable filings have facilitated SEC's scrutiny process over corporate reports(SEC, 2024).

Consistent with this hypothesis, I find that, following the adoption of iXBRL, the Bloomberg ESG disclosure score of an average firm increases by 3.15 points, which is 8% of the sample mean. To verify that this increase is not merely driven by potential adjustments in Bloomberg's methodology, I conduct a textual analysis of corporate annual reports (i.e., Form 10-Ks), where I identify ESG-related sentences in firms' 10-K filings using a fine-tuned ESG-BERT model (Schimanski et al., 2024). The textual analysis shows that the fraction of ESG-related sentences in an average annual report rises by 0.9 percentage points following the adoption of iXBRL, which is equivalent to 23% of the sample mean. This result reinforces the conclusion that digital reporting induces firms to expand their sustainability disclosure.

Next, this paper studies whether digital reporting improves the comparability of sustainability information. A persistent challenge in sustainable finance is that sustainability information is mainly qualitative, narrative, non-standardized, and prone to subjective interpretations, resulting in low comparability across firms and over time. Such low comparability is reflected in the substantial ESG rating disagreement, which can arise from divergence in information accessed, scope of evaluation, measurement, weighting, and subjective judgment (Cookson and Niessner, 2020; Berg et al., 2022).

Digital reporting may help mitigate ESG rating disagreement. First, machine-readable reporting improves the discoverability of sustainability content. This reduces the likelihood that analysts overlook relevant information and, in turn, enables more uniform access to information across rating agencies. Second, the structured format and standardized tags reduce ambiguity in sustainability disclosure, improves cross-firm comparability, and promotes more consistent evaluation criteria among analysts, thereby limiting the scope for subjective judgment.

The impact of digital reporting on ESG rating disagreement is likely to be heterogeneous across firms' baseline disclosure levels. Christensen et al. (2022) shows that greater sustainability disclosure is associated with ESG rating disagreement, as richer information creates more dimensions over which analysts may disagree. This implies that firms with higher levels of sustainability disclosure prior to the iXBRL mandate faced greater potential for disagreement reduction, whereas low-disclosure firms provided limited scope for divergence to begin with. If digital reporting improves comparability through the channels discussed above, its effect should therefore be most pronounced among firms that were already disclosing more. Accordingly, I hypothesize that the adoption of iXBRL reduces ESG rating disagreement more strongly for firms with higher levels of pre-mandate sustainability disclosure.

I test this hypothesis using data from three of the largest ESG rating agencies: MSCI, Refinitiv ASSET4, and S&P Global. I find that, among firms with higher levels of pre-mandate sustainability disclosure, an average firm experiences a 14.8% drop in social score disagreement and a 19.0% drop in governance score disagreement following the adoption of iXBRL. By contrast, no significant changes are observed for low-disclosure firms. I also find no corresponding effect on environmental score disagreement, which is consistent with the notion that environmental metrics are already more standardized and less subject to interpretation than social and governance metrics.

Finally, this paper examines the impact of digital reporting on the quality of corporate sustainability information. The rational inattention model predicts that market participants pay more attention when information processing costs are lower (Blankespoor et al., 2020). Firms, in turn, may strategically adjust their disclosure in response to the higher attention. For instance, Bertomeu et al. (2023) studies corporate financial information and show that firms increase the disclosure of favorable information while withholding unfavorable content when investor attention intensifies. Whether and how firms strategically adjust non-financial disclosure, however, remains an open question. This paper investigates this issue in the context of sustainability disclosure and digital reporting, where the outcome may depend on how firms anticipate their sustainability information will be interpreted as market participants increasingly rely on algorithmic tools to parse and analyze corporate reports.[2]

[2] A Reuters Events survey (2024) reports that sustainability professionals are increasingly shifting investments toward AI-powered analytics, particularly AI for use in materiality assessments. (Source: https://www.reuters.com/sustainability/sustainable-finance-

On one hand, digital reporting may improve the quality of sustainability disclosure. When market participants pay more attention to corporate reports and increasingly rely on algorithmic tools, they are more capable of distinguishing substantive disclosure from cheap talk statements, that is, non-specific or empty sustainability commitments that lack measurable targets or credible follow-through. Under this heightened scrutiny, firms may expect higher financial, reputational, and regulatory costs of cheap talk, thereby shifting toward more credible and verifiable sustainability disclosure.

On the other hand, digital reporting may undermine the quality of sustainability disclosure. Machine-readable and structured formats make it easier for market participants to benchmark a firm's sustainability reporting against its peers, making omissions or insufficient disclosure more visible. In this context, silence relative to peers can signal lagging sustainability commitment, pressuring firms to fill disclosure gaps.[3] Instead of releasing more substantive information, which may be either unfavorable or simply exhausted, firms may expand disclosure with cheap talk to improve their perceived sustainability image. This tendency is reinforced when investors and rating agencies increasingly rely on algorithmic tools that reward the volume of sustainability-related content, keywords, or superficial signals without fully assessing their substance.

This paper finds evidence consistent with the cheap talk prediction. First, I find that, following the adoption of iXBRL, none of the MSCI, Refinitiv ASSET4, and S&P Global ESG scores becomes more predictive of firms' future real sustainability outcomes, such as greenhouse gas emissions, the probability of experiencing an ESG risk incident, or the number of ESG risk incidents. These results suggest that digital reporting, while expanding disclosure, does not make ESG ratings more informative about firms' actual sustainability performance.

To validate that these results reflect firms' disclosure strategies rather than being fully driven by methodological changes within ESG rating agencies, I con-

reporting/sustainability-professionals-turning-ai-help-with-materiality-assessments-2024-05-10)

[3] Existing evidence suggests that firms do respond to such peer pressure. For example, Bolton and Kacperczyk (2025) find that a firm is more likely to make decarbonization commitments when its industry peers have committed.

duct a textual analysis of 10-K reports, where I measure sustainability cheap talk as the fraction of non-specific ESG commitments. I identify ESG-related commitments and their specificity using fine-tuned BERT models based on the ESG-BERT (Schimanski et al., 2024) and ClimateBERT (Bingler et al., 2024) models. The textual analysis shows that, out of ESG-related content in an average 10-K report, the fraction of ESG commitment sentences increases by 3.7 percentage points after iXBRL adoption. Importantly, this increase in commitments is driven by *non-specific* statements: the fraction of *non-specific* ESG commitment sentences rises by 3.1 percentage points, while no significant change in *specific* ESG commitments.

Taken together, these findings suggest that digital reporting induces firms to expand sustainability disclosure primarily through cheap talk, thereby undermining the informativeness and quality of corporate sustainability reporting. This pattern is consistent with recent evidence showing that disclosure has become longer, more boilerplate, and less specific (Lin et al., 2024), and that firms' decarbonization pledges are often overly optimistic or made without credible evaluation (Bolton and Kacperczyk, 2025).

This paper relates to the literature that examines how information technologies reshape the information environment in capital markets. From the perspective of investors, electronic dissemination of corporate reports (e.g., the SEC's EDGAR system) and machine-readable reporting formats (e.g., XBRL and iXBRL) reduce investors' information processing costs, improve price responsiveness and informativeness, increase stock liquidity and trading volume, increase analyst forecast accuracy, and reduce cost of capital for firms, with heterogeneous effects on retail and institutional investors (e.g., Asthana et al. (2004), Blankespoor et al. (2014), Bhattacharya et al. (2018), Gao and Huang (2020), Luo et al. (2023), and Call et al. (2023)). An emerging literature studies the underexplored information supply side. Blankespoor (2019) finds that firms increase quantitative footnote disclosure following the adoption of XBRL. Cao et al. (2023) documents that firms strategically adapt disclosure tone to machine readers.

This paper contributes to this supply-side perspective by shifting the focus from financial to sustainability information, which is typically qualitative, hard to verify, and subject to greater scope for managerial discretion. I show that digital reporting not only expands the volume of sustainability disclosure but also tilts its composition toward cheap talk. These findings highlight that the impact of information technologies depends on the nature of the underlying information, offering new insights for both theory and policy on the unintended consequences of new technologies.

Second, this paper contributes to the literature on the determinants and economic consequences of firms' sustainability reporting strategies. Prior research shows that firm size, ownership structure, managerial characteristics, firms' economic activities, and external stakeholder pressure are important drivers of the extent of voluntary sustainability disclosure, and that the capital-market effects of such disclosure are mixed (see Christensen et al. (2021) for a detailed review). Meanwhile, a growing literature examines the quality of sustainability reporting. Lin et al. (2024) document that environmental and social disclosure has become increasingly boilerplate and less specific over time, with reporting quality deteriorating following disclosure mandates. Müller et al. (2024) show that firms providing more climate-related disclosure in financial statements tend to make fewer non-specific climate commitments in their standalone sustainability reports. This paper adds a new dimension to this literature by identifying digital reporting technology as a previously underexplored determinant of corporate sustainability disclosure strategy.

Finally, this paper adds to the literature on ESG rating disagreement. While existing studies primarily investigate the drivers and market consequences of divergence in ESG ratings (e.g., Chatterji et al. (2016), Gibson Brandon et al. (2021), Avramov et al. (2022), Berg et al. (2022), Christensen et al. (2022), and Serafeim and Yoon (2023)), this paper explores a potential solution to the divergence. I provide novel evidence that digital reporting can mitigate ESG rating disagreement. These findings suggest that reporting technologies play a crucial role in addressing ESG rating uncertainty, shedding light on the ongoing discussion of the role of digitalization in shaping sustainable finance.

The remainder of this paper proceeds as follows. Section 2.2 provides institutional background on the SEC's iXBRL mandate. Section 2.3 outlines the empirical design. Section 2.4 describes the data, main variables, and sample. Section 2.5 presents the results and Section 2.6 concludes.

2.2 Institutional Background: iXBRL and Sustainability Reporting

iXBRL is a digital reporting format that embeds machine-readable tags directly into the human-readable text of corporate reports. This integration allows a single document to simultaneously provide both human-readable information and structured, machine-readable data.

iXBRL builds on the earlier XBRL format. Under the SEC's 2009 XBRL mandate, firms were required to submit their reports in HyperText Markup Language (HTML), along with separate XBRL exhibits containing tagged financial statement numbers and footnotes. While this approach improved the machine readability of financial data, it had a limited impact on non-financial information, such as sustainability disclosure.

In contrast, iXBRL significantly expands the scope of machine-readability to the entire report. Firms adopting iXBRL are required to submit reports in Extensible HyperText Markup Language (XHTML), a stricter and more standardized version of HTML. The XHTML format enhances structural consistency and enables automated data extraction from both numerical and narrative content across the entire filing. As shown by Call et al. (2023), iXBRL improves the machine readability of both textual and numerical content throughout company quarterly and annual reports.

The mandatory adoption of iXBRL required by the SEC is particularly relevant for the reporting and analysis of sustainability information, which has historically relied on unstructured, narrative disclosure.

The adoption of iXBRL enables firms to apply machine-readable tags to sustainability metrics in their corporate reports. This allows firms to improve the visibility and comparability of their sustainability metrics to ESG rating agencies and investors. Appendix Figure A2.1 illustrates an example from Etsy's 10-K filings. Panel (a) shows that prior to iXBRL adoption, Etsy disclosed its workforce diversity only through narrative text. Such content, while understandable by human readers, could not be automatically identified or retrieved by algorithms. By contrast, Panel (b) shows that following iXBRL adoption, Etsy reported the racial and ethnic composition of its workforce in a structured table with iXBRL tags. These tags specify data elements such as the reporting concept (e.g., "Percentage of Management Who Are Black or African American"), relevant dimensions (e.g., fiscal period, race/ethnicity category, and employee group), and associated percentage changes. Using this structured tagging, Etsy's diversity disclosure become not only human-readable but also machine-accessible, facilitating more efficient, consistent, and objective evaluation by both automated systems and human analysts.

In addition, the mandate encourages firms to present narrative sustainability content in more structured and extractable formats. Strategically, anticipating that ESG rating agencies and institutional investors increasingly rely on automated tools to process and analyze filings, firms may have stronger incentives to format their sustainability disclosure in ways that are more structured, comparable, and machine-readable. Supporting this view, Cao et al. (2023) document that firms do respond to the rising machine readership by making their filings more algorithm-friendly. Meanwhile, the iXBRL mandate reduces the marginal cost of structuring sustainability disclosure in a machine-readable way. Since firms are already required to prepare their entire filings in XHTML, often using tagging software or third-party service providers, extending these formatting practices to narrative sustainability sections becomes easier and less costly.

Appendix Figure A2.2 presents an example of this. Panel (a) shows that prior to iXBRL adoption, Etsy disclosed its "Impact Strategy" using a static image embedded in its 10-K report. This format was readable by humans but invisible to machines. In contrast, Panel (b) shows that after iXBRL adoption, the same content was reformatted into structured HTML tables with labeled textual entries, making it easily extractable by algorithms.

In summary, the SEC's iXBRL mandate introduces a digital disclosure format that standardizes sustainability reporting and enhances visibility to machine readers. These changes are intended to reduce the costs of extracting and analyzing corporate sustainability information for investors, ESG rating agencies, and other market participants.

2.3 Empirical Design

2.3.1 Staggered Implementation of SEC's iXBRL Mandate

To examine the effects of digital reporting on the transparency, comparability, and credibility of sustainability information in capital markets, I exploit the staggered implementation of the SEC's iXBRL mandate as a quasi-experiment. The mandate was adopted on June 28, 2018, and became effective on September 17, 2018.[4] The mandate requires all U.S. registrants to prepare and submit annual and quarterly reports in the iXBRL format.

The mandate is implemented in three phases based on filer status. Large accelerated filers are required to adopt iXBRL for reports covering fiscal periods ending on or after June 15, 2019. Accelerated filers follow for fiscal periods ending on or after June 15, 2020. All remaining filers are required to comply for reports covering fiscal periods ending on or after June 15, 2021.

This paper defines treatment timing based the adoption of iXBRL in firms' annual reports (i.e., 10-K filings). While the SEC's mandate applies to both quarterly and annual reports, I define treatment timing based on the adoption of iXBRL in annual reports since the Bloomberg ESG disclosure score and most ESG ratings used in this study are updated annually to assess firms' sustainabil-

[4] SEC final rule: Release No.33-10514

ity performance for a given fiscal year.

The staggered implementation allows for a difference-in-differences (DiD) design. A growing literature has highlighted that in staggered DiD settings, the traditional two-way fixed effects estimator can lead to biased estimates when treatment effects vary over time or across adoption cohorts (e.g., see De Chaisemartin and d'Haultfoeuille (2020), Callaway and Sant'Anna (2021), Goodman-Bacon (2021), Baker et al. (2022), and Borusyak et al. (2024)). In the context of mandatory iXBRL adoption, treatment effects are likely to vary over time because both firms and information users may need time to adjust to the new reporting and processing formats, leading to smaller effects in the first year of iXBRL adoption. In addition, treatment effects may also differ across adoption cohorts due to learning effects, where later adopters adjust their reporting strategies based on the behavior of early adopters. Furthermore, adoption cohorts are defined by filer status, and the treatment effects may vary across filer types—that is, large accelerated, accelerated, and non-accelerated filers.

To address the potential bias inherent in the traditional two-way fixed effects estimator, I employ the doubly robust estimator introduced by Sant'Anna and Zhao (2020) and Callaway and Sant'Anna (2021) (hereafter "CS estimator"). Among alternative estimators developed in recent econometric literature (e.g., De Chaisemartin and d'Haultfoeuille (2020), Sun and Abraham (2021), and Wing et al. (2024a)), the CS estimator offers the flexibility that is well suited for the setting in this study. First, the mandatory iXBRL adoption is irreversible, which satisfies the assumption of irreversible treatment in the CS estimator. Second, as the SEC mandate requires all firms to eventually adopt iXBRL, there are no never-treated firms but only not-yet-treated ones. The CS estimator accommodates this feature by allowing not-yet-treated units as valid comparison groups. Third, the CS estimator allows the parallel trends assumption to hold conditional on covariates.

2.3.2 Exogenous Assignment to Treatment Cohorts

The identification strategy in this paper relies on the assumption that the assignment to treatment cohorts is exogenous to firms' disclosure strategy. Under the SEC's iXBRL mandate, the timing of iXBRL adoption is determined by a firm's filer status, which is primarily defined by the firm's public float. Public float refers to the market value of a company's shares held by public shareholders. Typically, a large accelerated filer, whose required compliance date is June 15, 2019, is a public firm with a public float of $700 million or more. An accelerated filer, which is required to comply by June 15, 2020, is a public firm with a public float of $75 million or more but less than $700 million. A non-accelerated filer, whose compliance date is June 15, 2021, is a public firm with a public float of less than $75 million. According to the SEC's final rule, the phase-in schedule was designed to lower the initial iXBRL transition costs for smaller filers, as these costs tend to have a greater impact on them. This phase-in approach is expected to ease the transition burden on smaller filers by allowing filing agents and software vendors to accumulate iXBRL expertise and offer more competitive pricing over time.

Although the accessibility and quality of firm sustainability information is not directly endogenous to the timing of iXBRL adoption, the public float itself may be correlated with firms' sustainability disclosure practices. For example, larger firms, which tend to have higher public float, are generally subject to greater investor scrutiny and regulatory oversight. Consequently, they often exhibit more transparent and standardized disclosure practices. This implies that the accessibility and comparability of sustainability information may be systematically higher for firms assigned to earlier adoption cohorts.

Nevertheless, the identifying assumption in a DiD framework does not require the same baseline levels, but rather that the outcomes of treated and control groups would have followed similar trends in the absence of treatment. To further mitigate concerns about potential confounders, I control for firm fixed effects and a set of firm characteristics, including firm size, financial performance, and capital structure. Consistent with the assumption of parallel trends, the analyses of dynamic effects, which are presented in Section 2.5, show no evidence of differential trends in the years before the iXBRL adoption.

Another threat to the exogeneity assumption is that if firms manipulated their public float to alter their filer status and delay compliance with the mandatory iXBRL adoption, for example to align compliance timing with their disclosure strategies, then the estimates will be biased. To address this concern, I examine the distribution of public float around the $75 million and $700 million thresholds. If large accelerated filers attempted to delay the iXBRL adoption by lowering their public float, we would expect an excess number of firms just below the $700 million threshold in fiscal year 2019 (i.e., fiscal year ending between June 15, 2019, and June 14, 2020). However, as shown in Panel (a) of Figure 2.1, the distribution appears smooth around the threshold. Similarly, if accelerated filers manipulated their public float to delay compliance, we would expect to observe a spike in the number of firms just below the $75 million threshold in fiscal year 2020 (i.e., fiscal year ending between June 15, 2020, and June 14, 2021). Panel (b) of Figure 2.1 reveals no noticeable discontinuity at this cutoff. Overall, these patterns suggest no evidence of manipulation.

2.3.3 Regression Specification

The following difference-in-differences specification is estimated using the CS estimator in the main analysis:

$$y_{i,t} = \beta_0 + \beta_1 Post\text{-}iXRBL_{i,t} + \boldsymbol{\gamma}'\boldsymbol{X} + \boldsymbol{FE} + \epsilon_{i,t}, \tag{2.1}$$

where $y_{i,t}$ denotes the outcome variable of firm i in fiscal year t. The variable of interest is $Post\text{-}iXBRL_{i,t}$, which is a dummy equal to one if firm i files its 10-K report for fiscal year t in iXBRL, and zero otherwise. The coefficient β_1 captures the average treatment effect of iXBRL adoption. The vector $\boldsymbol{X}$ controls for pre-treatment covariates,[5] which include firm size (= natural logarithm of book value of total assets), return on assets (= operating income before depreciation divided by book value of total assets), and leverage (= book value of total debt divided by book value of total assets). $\boldsymbol{FE}$ includes firm and year fixed effects. Standard errors are clustered at the firm level.

2.4 Data and Sample

2.4.1 Data and Main Variables

2.4.1.1 Filer Status and iXBRL Adoption

I collect data on firms' filer status, iXBRL adoption status, fiscal year-end dates, and 10-K filing dates from the SEC's EDGAR system. These information are used to determine the timing of each firm's compliance with the iXBRL mandate.

2.4.1.2 The Extent of Sustainability Disclosure

I measure the extent of firms' sustainability disclosure using the Bloomberg ESG disclosure score, which measures the volume and transparency of a company's publicly reported ESG data. The score ranges from 0 to 100, with higher values indicating more comprehensive disclosure. The score is solely based on the extent of ESG disclosure, not the firm's actual ESG performance. Thus, a higher score does not necessarily imply stronger sustainability outcomes. Bloomberg's methodology for calculating this score remained consistent throughout the sample period used in this study.

[5]For time-varying covariates, the CS estimator uses values lagged by one period for the estimation of pre-treatment periods, and values from the last period before treatment begins for the estimation of post-treatment periods.

2.4.1.3 ESG Rating Disagreement

I obtain ESG ratings from three major ESG rating providers, namely, MSCI, Refinitiv ASSET4, and S&P Global. These rating agencies did not make significant methodological changes during the sample period of this study. Drawing on the approach in Avramov et al. (2022), I measure firm-level ESG rating disagreement as the standard deviation of the firm's percentile ranks across all available raters in a given year. Specifically, for each rater-year, I sort all firms covered by the rater within the 6-digit Global Industry Classification Standard (GICS) code according to the original rating scores and compute the percentile rank (scaled between 0 and 1) for each firm-rater-year observation. The firm-level ESG rating disagreement in each year is measured as the standard deviation of the firm's percentile ranks across all raters. This measure leverages all available ratings without requiring common rater coverage.

2.4.1.4 Other Data

I obtain data on company ESG risk incidents from RepRisk and firm-level greenhouse gas (GHG) emissions from the Trucost dataset. Firm characteristics, including value of total assets, return on assets, and leverage, are extracted from the Compustat database.

2.4.2 Sample Construction and Summary Statistics

The sample construction begins with all firms covered by Compustat that have valid 10-K filings submitted to the SEC. The sample period spans fiscal years 2016 to 2020, corresponding to fiscal years ending between June 15, 2016, and June 14, 2020. I exclude fiscal year 2021 and beyond because valid control units are no longer available after 2020. The SEC's iXBRL mandate requires all firms to comply by fiscal year 2021. As a result, starting in 2021, there are no never-treated or not-yet-treated firms that can serve as the clean controls required by the robust CS estimator, as discussed in Section 2.3.1. Next, I retain firm-year observations that are covered by Bloomberg's ESG disclosure score and have ESG ratings from at least two of the three ESG rating agencies: MSCI, Refinitiv ASSET4, and S&P Global. Finally, I drop voluntary iXBRL adopters, restricting the sample to mandated adopters only.[6] The resulting sample consists of 6,562 firm-year observations covering 1,491 unique firms.

Table 2.1 presents summary statistics for the sample. Panel A shows that 73.6% of the 1,491 firms adopted iXBRL in 2019, 23.5% in 2020, and 2.9% in 2021. Panel B reports that the average firm has an ESG disclosure score of 39.5. Firms in the sample exhibit considerable disagreement across ESG rating agencies, with greater divergence observed in the social and governance performance scores compared to the environmental performance score. In terms of firm characteristics, the average firm has total assets of $2.95 billion, a return on assets of 0.09, and a leverage ratio of 0.29.

2.5 Empirical Results

2.5.1 Effects of Digital Reporting on the Extent of Sustainability Disclosure

I begin by examining whether digital reporting improves the extent of firms' sustainability disclosure. Table 2.2 reports the estimates of Equation (2.1), where the dependent variable is the raw Bloomberg disclosure score. Panel A presents the average treatment effects over the first two fiscal years following iXBRL adoption. Column (1) shows that the overall ESG disclosure score increases by 3.15

[6] Firms may voluntarily adopt iXBRL before their assigned compliance date under the SEC's exemptive order or by the SEC's discretion. Since a firm's decision to accelerate adoption may be endogenous to its disclosure supply or strategies, I identify and exclude voluntary adopters in the main analysis. However, excluding them may itself introduce endogenous sample selection, as firms effectively self-select into or out of the sample. To address this concern, I repeat the main analysis without excluding voluntary adopters for robustness. As shown in Appendix Tables A2.1–A2.3, the results remain qualitatively unchanged, and the estimated coefficient of interest is quantitatively close to that in the main analysis.

points, which is 8% of the sample mean. Columns (2)–(4) show that the adoption of iXBRL also results in statistically significant increases in each of the environmental, social, and governance disclosure scores.

Figure 2.2 plots the dynamic effects, and Panel B of 2.2 reports the estimated coefficients for the post-iXBRL periods. The dynamic analysis shows that the effect of iXBRL adoption materializes gradually over time. This pattern is consistent with the expectation that firms and information users face an initial learning curve and implementation costs in adapting to the new reporting format, which can delay the full impact of digital reporting. As shown in panels (a)–(d) of Figure 2.2, the coefficients prior to the iXBRL adoption oscillate around zero and show no evidence of pre-trends.

The observed increase in Bloomberg disclosure scores following iXBRL adoption reflects two potential channels. First, as described in Section 2.2, digital reporting enhances the machine readability of sustainability disclosure by tagging data and making narrative information more structured. This allows Bloomberg to detect and extract ESG-relevant content more efficiently, even if the actual volume of sustainability disclosure remains unchanged. Second, firms may respond to the digital reporting format by expanding the actual scope and detail of their sustainability disclosure.

To verify that firms indeed disclose more sustainability information in response to the digital reporting mandate, I conduct a textual analysis of 10-K reports as a robustness check. Using a fine-tuned ESG-BERT model (Schimanski et al., 2024), I identify ESG-related sentences in 10-K reports.[7] I then re-estimate Equation (2.1) with the fraction of ESG-related sentences in each 10-K report as the dependent variable. The result, shown in column (5) of Table 2.2, indicates that the fraction of ESG-related sentences increases by 0.9 percentage points following iXBRL adoption, which corresponds to 23% of the sample mean.

Overall, the empirical evidence in this section suggests that digital reporting

[7]I focus on Item 1 “Business,” Item 1A “Risk Factors,” and Item 7 “Management’s Discussion and Analysis of Financial Condition and Results of Operations” (MD&A), as sustainability-related information is most frequently disclosed in these sections.The textual analysis is conducted on 10-K reports whose text was correctly extracted and parsed, resulting in fewer observations than in the main sample.

substantially enhances the accessibility and breadth of corporate sustainability information.

2.5.2 Effects of Digital Reporting on ESG Rating Disagreement

This section examines whether digital reporting improves the comparability of corporate sustainability information and helps address the long-standing issue of ESG rating disagreement.

In Section 2.1, I hypothesize that the adoption of iXBRL reduces ESG rating disagreement more strongly for firms that disclosed more sustainability information prior to the mandate. To test this hypothesis, I split the sample based on firms' Bloomberg ESG disclosure scores during fiscal years 2016–2018. The high-disclosure (low-disclosure) group consists of firms whose median ESG disclosure score over 2016–2018 is above (below) the sample median calculated over the same period. I then estimate Equation (2.1) separately for each subsample, using disagreement in the environmental, social, and governance performance scores as the dependent variables.[8] Particularly, I include rater-set fixed effects to account for systematic disagreement arising from divergence in scope, measurement, and weight across rating agencies. This ensures that treated firms are compared with control firms covered by the same set of rating agencies.

Panel A of Table 2.3 presents the static treatment effects. The dynamic effects are plotted in Figure 2.3, with the corresponding estimates for post-iXBRL periods reported in Panel B of Table 2.3. The results for the disagreement in social and governance performance scores are consistent with the hypothesis. Among high-disclosure firms, the dynamics show that disagreement in the social and governance performance scores declines significantly in the second year of iXBRL adoption. As for the static treatment effects, an average high-disclosure

[8] I focus on pillar-level disagreement rather than overall ESG score disagreement to isolate iXBRL's effects from agency-specific weighting schemes used to aggregate pillar scores.

firm experiences a 14.8%[9] drop in social score disagreement and a 19.0%[10] drop in governance score disagreement in the first two fiscal years following the adoption of iXBRL. By contrast, no significant changes are observed for low-disclosure firms. Moreover, Figure 2.3 shows no evidence of pre-trends.

Nevertheless, I find no similar pattern for environmental score disagreement. This makes sense, as environmental metrics tend to be more standardized and less subjective than social and governance dimensions,[11] thereby leaving less room for digital reporting to improve comparability or reduce analyst disagreement.

2.5.3 Effects of Digital Reporting on the Quality of Sustainability Reporting

2.5.3.1 Predictive Power of ESG Ratings

The hypothesis that digital reporting increases cheap talk in sustainability reporting can lead to the following testable prediction. If the additional sustainability information disclosed under digital reporting is substantive, ESG ratings should become more informative and exhibit stronger predictive power for firms' real sustainability outcomes. In contrast, if firms mainly engage in cheap talk, we expect to observe no significant improvement in the predictive power of ESG ratings following the adoption of digital reporting.

To test this prediction, I estimate the following regression separately for the periods before and after iXBRL adoption:

$$y_{i,t+1} = \beta_0 + \beta_1 \log(ESG\ Score_{i,t}) + \boldsymbol{\gamma' X_{i,t}} + \boldsymbol{FE} + \epsilon_{i,t}, \tag{2.2}$$

where $y_{i,t+1}$ represents one of several negative sustainability outcomes of firm i in fiscal year $t+1$. I examine whether, relative to pre-iXBRL periods, the coefficient

[9] $\frac{\text{DiD estimate}}{\text{High-disclosure subsample mean}} = \frac{-0.036}{0.244} = 14.8\%$

[10] $\frac{\text{DiD estimate}}{\text{High-disclosure subsample mean}} = \frac{-0.047}{0.248} = 19.0\%$

[11] Consistent with this notion, summary statistics in Table 2.1 show that both the mean and standard deviation of environmental score disagreement are lower than those for the social and governance scores.

β_1 becomes significantly more negative (or less positive) after iXBRL adoption. The vector $\boldsymbol{X}$ stacks control variables, including firm size, return on assets, and leverage.

The first set of sustainability outcomes I examine are firms' greenhouse gas (GHG) emissions, including total, direct (i.e., Scope 1 emissions), and indirect (i.e., Scope 2 and Scope 3 emissions) emissions.[12] Table 2.4 presents the results. Across all three ESG rating agencies, MSCI, Refinitiv ASSET4, and S&P Global, there is no statistically significant change in the association between ESG scores and future GHG emissions following iXBRL adoption. The results are consistent across total, direct, and indirect emissions.

Next, I analyze the predictive power of ESG scores for future ESG risk incidents and report the results in Table 2.5. Columns (1) and (2) show that, across all three ESG rating agencies, there is no significant change in the association between ESG scores and the probability of future ESG risk incidents after iXBRL adoption. Columns (3) and (4) examine the number of future incidents, restricting the sample to firms that experience at least one incident in the following fiscal year. For ASSET4 and S&P Global, the post-iXBRL association between the number of future incidents and ESG scores does not significantly differ from the pre-iXBRL periods. For MSCI, while the association between ESG scores and the number of future incidents becomes more negative after iXBRL adoption, it is only weakly significant at the 10% level.

Also, as shown in Appendix Table A2.4, the probability of having an ESG risk incident for an average firm remains between 0.3 and 0.4 throughout 2016–2021. Therefore, the absence of improvement in predictive power is unlikely to be driven by a decline in the overall incidence of ESG issues over time.

Overall, these results provide no consistent evidence that the predictive power of ESG ratings for firms' future sustainability outcomes improves after the adoption of digital reporting. Together with the earlier findings that iXBRL increases in the extent of sustainability disclosure, these results suggest that much

[12]Scope 1 emissions are direct GHG emissions from sources owned or controlled by the firm. Scope 2 emissions are from the firm's consumption of purchased energy. Scope 3 emissions arise from upstream and downstream activities in the firm's value chain.

of the additional information disclosed under digital reporting may be superficial rather than substantive. To validate that these results are driven by firms' disclosure strategies rather than methodological changes by ESG rating agencies, I next conduct a textual analysis of firms' 10-K reports.

2.5.3.2 Evidence of Cheap Talk from Textual Analysis

To examine the textual content of sustainability disclosure in 10-K reports, I proceed as follows. First, I identify ESG-related sentences in Item 1 "Business," Item 1A "Risk Factors," and Item 7 "Management's Discussion and Analysis of Financial Condition and Results of Operations" using a fine-tuned ESG-BERT model (Schimanski et al., 2024). Second, I classify each ESG-related sentence into commitment, *specific* commitment,[13] *non-specific* commitment,[14] and non-commitment categories using a fine-tuned BERT model based on the Climate-BERT model (Bingler et al., 2024). Third, for each of the commitment, *specific* commitment, and *non-specific* commitment categories, I calculate the fraction of sentences in that category relative to the total number of ESG-related sentences in each 10-K report. Finally, I re-estimate Equation (2.1) using these fractions as the dependent variables. The regressions include rater-coverage fixed effects to address the concern that changes in a firm's ESG disclosure strategy may partly reflect adjustments to specific raters' methodologies rather than the adoption of iXBRL. This textual analysis is conducted on 10-K reports whose text was correctly extracted and parsed, resulting in fewer observations than in the main sample.

Panel A of Table 2.6 reports the estimated static treatment effects, and Panel B presents the dynamic effects. Column (1) of Panel A shows that, out of ESG-related content in an average 10-K report, the fraction of ESG commit-

[13]Example of a specific ESG commitment: "In February 2019, we began offsetting 100% of carbon emissions generated by shipping on Etsy.com, which represent 97% of our total measured emissions."

[14]Example of a non-specific ESG commitment: "Our mission is to 'Keep Commerce Human', and we're committed to using the power of business and technology to strengthen communities and empower people around the world."

ment sentences increases by 3.7 percentage points in the first two years following iXBRL adoption, which corresponds to 28% of the sample mean. This increase in commitments is primarily driven by *non-specific* statements. Specifically, column (2) of Panel A shows that the fraction of *non-specific* ESG commitment sentences rises by 3.1 percentage points, while column (3) indicates no significant change in *specific* ESG commitments. Figure 2.4 plots the dynamic effects and shows no evidence of pre-trends.

The textual analysis reveals that the increase in sustainability disclosure following iXBRL adoption is primarily driven by non-specific commitments rather than concrete, verifiable actions. This pattern is consistent with the notion of cheap talk, where firms expand the volume of sustainability-related statements without providing substantive information about their actual practices.

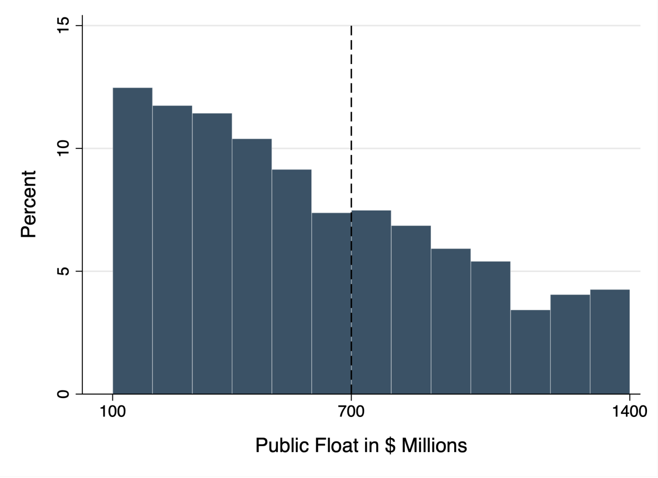
2.6 Conclusion

This paper examines the impact of digital reporting on the sustainability information environment. Exploiting the SEC's iXBRL mandate as an exogenous shock, I provide novel evidence on how digital reporting formats affect the extent, comparability, and quality of corporate sustainability disclosure. The main findings are threefold. First, digital reporting induces firms to expand the volume of sustainability disclosure. Second, it mitigates ESG rating disagreement. Third, while sustainability disclosure becomes more extensive and comparable, firms engage more in cheap talk. These results suggest that digital reporting improves the accessibility and comparability of corporate sustainability information but may undermine its quality.

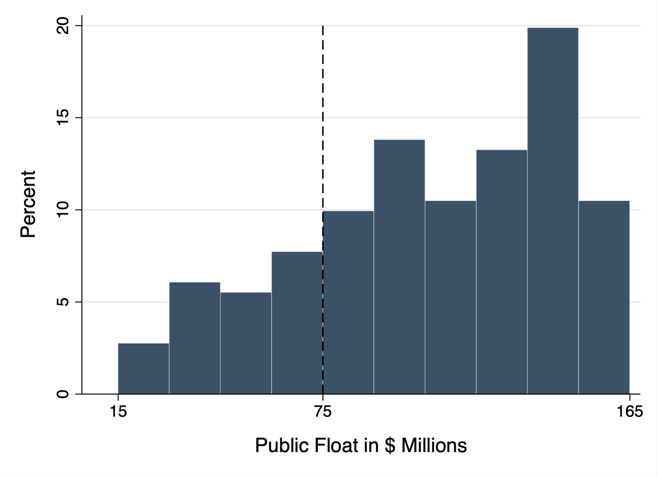
This paper highlights both the promise and unintended consequences of emerging technologies in shaping the information environment and informs policy debates on how digital tools can advance, but also complicate, the sustainable finance agenda.

Figure 2.1: Assessment of Manipulation: Distribution of Public Float Around Mandate Thresholds

(a) Around $700 million Threshold, Fiscal Year 2019



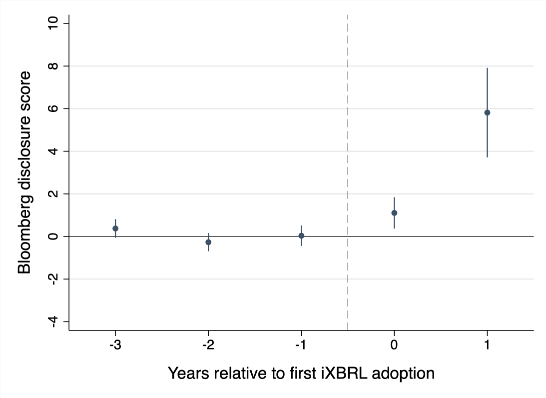
(b) Around $75 million Threshold, Fiscal Year 2020



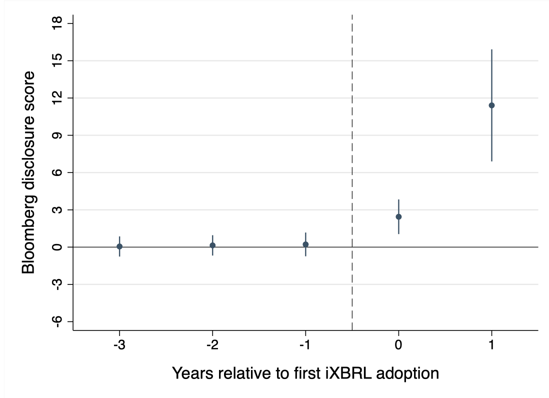
Note. This figure plots the distribution of firm public float around the SEC thresholds that determine filer status. Panel (a) displays the distribution around the $700 million threshold for fiscal year 2019 (i.e., fiscal year ending between June 15, 2019, and June 14, 2020). Panel (b) displays the distribution around the $75 million threshold for fiscal year 2020 (i.e., fiscal year ending between June 15, 2020, and June 14, 2021).

Figure 2.2: Dynamic Effect of iXBRL on the Extent of Sustainability Disclosure

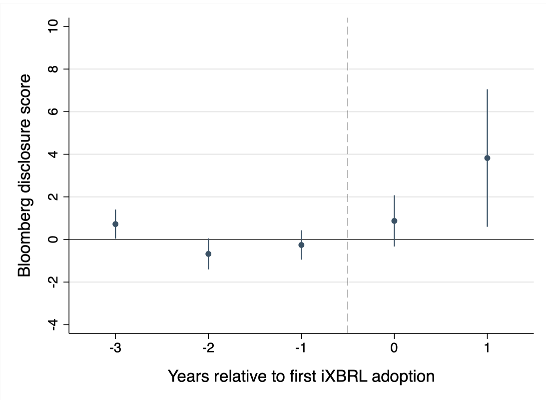
(a) ESG



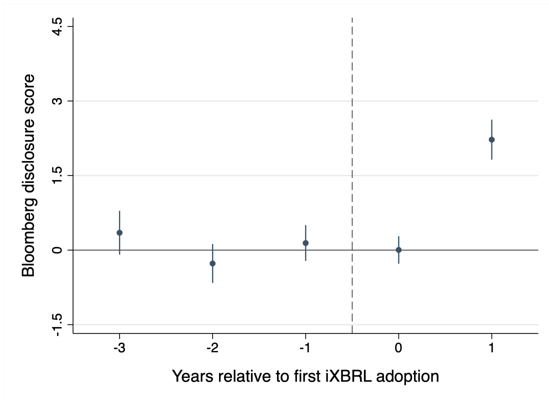
(b) Environmental



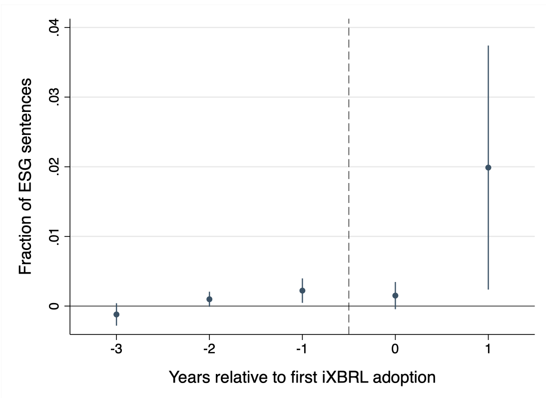
(c) Social



(d) Governance



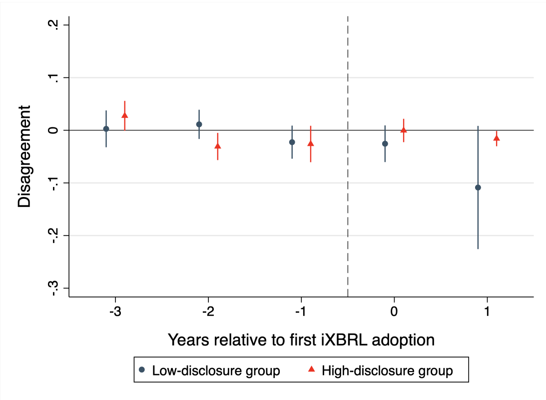
(e) Textual Measure



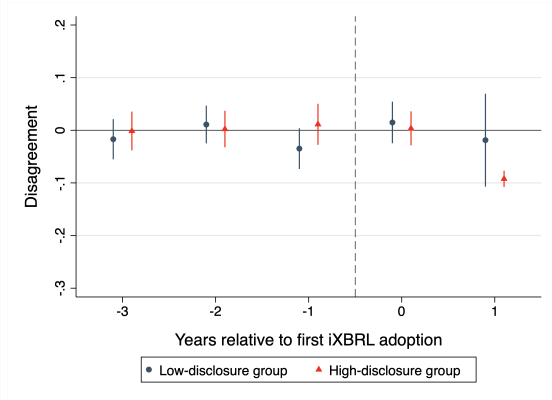
Note. This figure plots the Callaway & Sant'Anna estimates of dynamic treatment effects. In Panels (a)–(d), the dependent variable is the Bloomberg ESG disclosure score, environmental disclosure score, social disclosure score, and governance disclosure score, respectively. All disclosure scores are on a scale from 0 to 100. In panel (e), the dependent variable is the fraction of ESG-related sentences to the total number of sentences in Item1, Item 1A and Item 7 of a given firm's 10-K report. All specifications include firm and year fixed effects and control for pre-treatment firm characteristics (*Firm Size*, *ROA*, and *Leverage*), as defined in Table 2.1. The bands around the coefficient estimates represent the 95% confidence intervals. Standard errors are clustered at the firm level.

Figure 2.3: Dynamic Effect of iXBRL on ESG Rating Disagreement

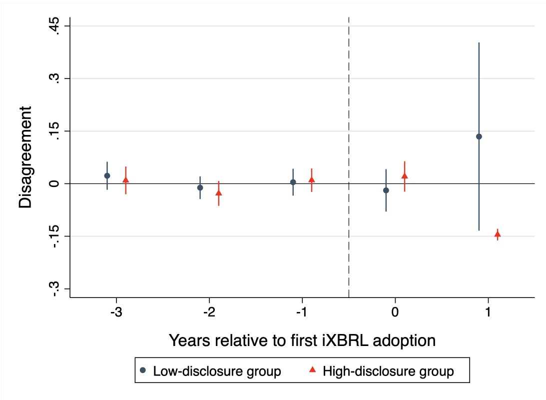
(a) Environmental



(b) Social



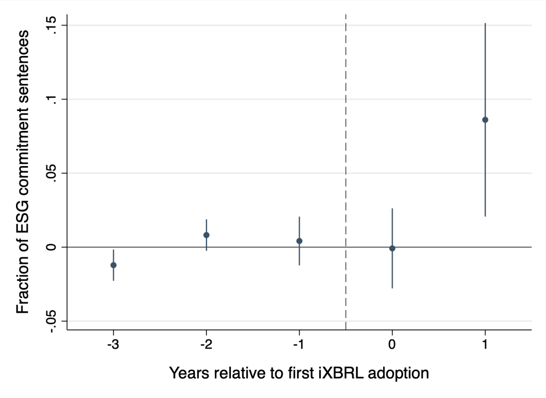
(c) Governance



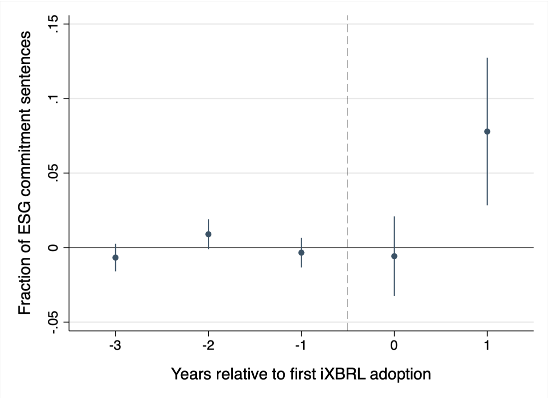
Note. This figure plots the Callaway & Sant'Anna estimates of dynamic treatment effects. The dependent variable is the level of disagreement in the environmental, social, and governance performance scores for a given firm's performance in a given fiscal year. The construction of the disagreement measure is described in Section 2.4.1.3. The high-disclosure (low-disclosure) group consists of firms whose median Bloomberg ESG disclosure score over 2016–2018 is above (below) the sample median calculated over the same period. All specifications include firm, year, and rater-set fixed effects and control for pre-treatment firm characteristics (*Firm Size*, *ROA*, and *Leverage*), as defined in Table 2.1. The bands around the coefficient estimates represent the 95% confidence intervals. Standard errors are clustered at the firm level.

Figure 2.4: Dynamic Effect of iXBRL on ESG Commitments

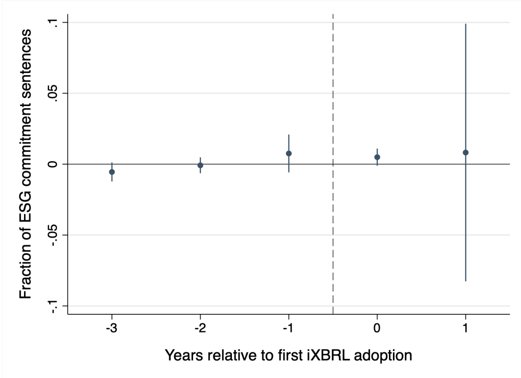
(a) Total Commitments



(b) Non-specific Commitments



(c) Specific Commitments



Note. This figure plots the Callaway & Sant'Anna estimates of dynamic treatment effects. The dependent variable is the fraction of ESG commitment sentences to the total number of ESG-related sentences in Item1, Item 1A and Item 7 of a given firm's 10-K report for a given fiscal year. Panels (a), (b), and (c) report results for total, non-specific, and specific ESG commitments, respectively. All specifications include firm, year, and rater-set fixed effects and control for pre-treatment firm characteristics (*Firm Size*, *ROA*, and *Leverage*), as defined in Table 2.1. The bands around the coefficient estimates represent the 95% confidence intervals. Standard errors are clustered at the firm level.

Table 2.1: Summary Statistics

Panel A: iXBRL Adoption by Fiscal Year	Adoption Year	Num. of Firms	Percent
	2019	1,097	73.57
	2020	351	23.54
	2021	43	2.88

Panel B: Summary Statistics for Main Variables	N	Mean	SD	P25	P50	P75
Bloomberg Disclosure Score						
ESG Disclosure Score	6,562	39.53	10.85	31.98	34.54	44.90
Environmental Disclosure Score	6,562	14.56	19.45	0	2.08	24.80
Social Disclosure Score	6,562	18.22	11.51	11.03	13.60	22.73
Governance Disclosure Score	6,562	85.62	5.18	84.29	84.98	87.48
ESG Rating Disagreement						
ESG Disagreement	6,497	0.19	0.14	0.08	0.16	0.27
Environmental Disagreement	6,497	0.17	0.13	0.07	0.14	0.24
Social Disagreement	6,497	0.22	0.15	0.11	0.20	0.32
Governance Disagreement	6,497	0.22	0.15	0.11	0.21	0.32
Sustainability Outcomes						
ESG Risk Incidents (1/0)	6,313	0.40	0.49	0	0	1
ESG Risk Incidents (num.)	6,313	6.09	23.35	0	0	3
Total GHG Emissions (log)	6,443	12.57	2.11	11.18	12.60	13.92
Direct GHG Emissions (log)	6,439	10.08	2.45	8.49	10.01	11.45
Indirect GHG Emissions (log)	6,443	12.37	2.05	11.03	12.42	13.66
Firm Characteristics						
Firm Size	6,562	7.99	1.69	6.80	7.79	8.98
ROA	6,562	0.09	0.16	0.06	0.11	0.16
Leverage	6,562	0.29	0.25	0.11	0.27	0.42

Note. This table presents summary statistics for the sample. Panel A shows the distribution of the 1,491 unique firms by the fiscal year of iXBRL adoption. Fiscal year t is defined as the fiscal year ending between June 15 of calendar year t–1 and June 14 of calendar year t. Panel B reports summary statistics for firm-year level variables. The Bloomberg ESG, environmental, social, and governance disclosure scores are on a scale from 0 to 100. The construction of ESG rating disagreement measures is described in Section 2.4.1.3. The indicator variable *ESG Risk Incidents (1/0)* is equal to 1 if a firm experiences at least one ESG risk incident in a given year and 0 otherwise. *ESG Risk Incidents (num.)* is the number of ESG risk incidents. *Total GHG Emissions* is the natural logarithm of a firm's total greenhouse emissions in a year (in tCO2e). *Direct GHG Emissions* is the natural logarithm of a firm's Scope 1 greenhouse emissions in a year (in tCO2e). *Indirect GHG Emissions* is the natural logarithm of a firm's Scope 2 and Scope 3 greenhouse emissions in a year (in tCO2e). *Firm Size* is the natural logarithm of book value of assets (in $ million). *ROA* is return on assets, calculated as the operating income before depreciation divided by book value of total assets. *Leverage* is measured as the book value of total debt divided by book value of total assets.

Table 2.2: iXBRL Adoption and the Extent of Sustainability Disclosure

	Dep. var = *Bloomberg Disclosure Score*				Textual
	ESG	Env.	Soc.	Gov.	Measure
	(1)	(2)	(3)	(4)	(5)
Panel A: Static Treatment Effect					
Post-iXBRL	3.151***	6.337***	2.154***	0.968***	0.009**
	(0.518)	(1.080)	(0.805)	(0.135)	(0.004)
Panel B: Post-iXBRL Dynamic Effect					
Post-iXBRL (Rel. Yr = 0)	1.106***	2.445***	0.872	0.004	0.002
	(0.376)	(0.712)	(0.614)	(0.142)	(0.001)
Post-iXBRL (Rel. Yr = 1)	5.815***	11.408***	3.824**	2.223***	0.020**
	(1.073)	(2.301)	(1.647)	(0.206)	(0.009)
Obs.	6,562	6,562	6,562	6,562	5,100
Controls	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes

Note. This table presents the Callaway & Sant'Anna estimates from the static DiD regression: $y_{i,t} = \beta_0 + \beta_1 Post\text{-}iXRBL_{i,t} + \boldsymbol{\gamma}'\boldsymbol{X} + \boldsymbol{FE} + \epsilon_{i,t}$, and from its dynamic specification. The event window covers [–3, +1] years relative to the first year of iXBRL adoption. Panel A reports static treatment effect estimates, and Panel B reports dynamic effect estimates for post-iXBRL periods. In columns (1)-(4), the dependent variable $y_{i,t}$ is the Bloomberg ESG disclosure score, environmental disclosure score, social disclosure score, and governance disclosure score for fiscal year t of firm i, respectively. All disclosure scores are on a scale from 0 to 100. Column (5) applies a textual disclosure measure: $y_{i,t}$ is the fraction of ESG-related sentences to the total number of sentences in Item1, Item 1A and Item 7 of firm i's 10-K report for fiscal year t. $Post\text{-}iXBRL_{i,t}$ is a dummy equal to one if firm i files its 10-K report for fiscal year t in iXBRL, and zero otherwise. All specifications control for pre-treatment firm characteristics $\boldsymbol{X}$, including *Firm Size*, *ROA*, and *Leverage*, as defined in Table 2.1. Standard errors are clustered at the firm level and are reported in parentheses. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

Table 2.3: iXBRL Adoption and ESG Rating Disagreement

	Dep. var = *ESG Rating Disagreement*					
	Env.		Soc.		Gov.	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Static Treatment Effect						
Post-iXBRL	-0.067**	-0.007	-0.002	-0.036***	0.057	-0.047***
	(0.031)	(0.008)	(0.025)	(0.011)	(0.070)	(0.014)
Panel B: Post-iXBRL Dynamic Effect						
Post-iXBRL (Rel. Yr = 0)	-0.026	-0.000	0.015	0.004	-0.019	0.021
	(0.018)	(0.011)	(0.020)	(0.016)	(0.031)	(0.022)
Post-iXBRL (Rel. Yr = 1)	-0.109*	-0.016**	-0.019	-0.092***	0.134	-0.145***
	(0.060)	(0.007)	(0.045)	(0.008)	(0.137)	(0.008)
Obs.	2,931	3,127	2,931	3,127	2,931	3,127
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Rater-set FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Level of ESG disclosure	Low	High	Low	High	Low	High

Note. This table presents the Callaway & Sant'Anna estimates from the static DiD regression: $y_{i,t} = \beta_0 + \beta_1 Post\text{-}iXRBL_{i,t} + \boldsymbol{\gamma}'\boldsymbol{X} + \boldsymbol{FE} + \epsilon_{i,t}$, and from its dynamic specification. The event window covers [–3, +1] years relative to the first year of iXBRL adoption. Panel A reports static treatment effect estimates, and Panel B reports dynamic effect estimates for post-iXBRL periods. The dependent variable $y_{i,t}$ is the level of disagreement in the environmental, social, and governance performance scores for firm i's performance in fiscal year t. The construction of the disagreement measure is described in Section 2.4.1.3. The high-disclosure (low-disclosure) group consists of firms whose median Bloomberg ESG disclosure score over 2016–2018 is above (below) the sample median calculated over the same period. $Post\text{-}iXBRL_{i,t}$ is a dummy equal to one if firm i files its 10-K report for fiscal year t in iXBRL, and zero otherwise. All specifications control for pre-treatment firm characteristics $\boldsymbol{X}$, including *Firm Size*, *ROA*, and *Leverage*, as defined in Table 2.1. Standard errors are clustered at the firm level and are reported in parentheses. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

Table 2.4: Predictive Power of ESG score for Next Period's Greenhouse Gas Emissions

	Dep. var = log(*GHG Emissions*)					
	Toal		Direct		Indirect	
	After iXBRL (1)	Before iXBRL (2)	After iXBRL (3)	Before iXBRL (4)	After iXBRL (5)	Before iXBRL (6)
Panel A: MSCI						
log(*ESG Score*)	0.007	-0.036	-0.033	-0.129**	0.019	-0.032
	(0.057)	(0.024)	(0.099)	(0.054)	(0.060)	(0.022)
Difference in Coefficients	0.043		0.096		0.051	
p-value	0.460		0.369		0.386	
Obs.	5,936	5,936	5931	5931	5,936	5,936
Panel B: Refinitiv ASSET4						
log(*ESG Score*)	0.037	0.070*	-0.155	0.032	0.042	0.064*
	(0.061)	(0.039)	(0.123)	(0.055)	(0.060)	(0.037)
Difference in Coefficients	-0.033		-0.187		-0.022	
p-value	0.651		0.165		0.755	
Obs.	6,296	6,296	6291	6291	6,296	6,296
Panel C: S&P Global						
log(*ESG Score*)	-0.051	0.037	-0.259	0.008	-0.040	0.025
	(0.065)	(0.032)	(0.158)	(0.055)	(0.066)	(0.031)
Difference in Coefficients	-0.088		-0.267		-0.065	
p-value	0.218		0.108		0.375	
Obs.	4,129	4,129	4,127	4,127	4,129	4,129
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes

Note. This table reports the estimates from regression: $y_{i,t+1} = \beta_0 + \beta_1 \log(ESG\ Score_{i,t}) + \boldsymbol{\gamma' X_{i,t}} + \boldsymbol{FE} + \epsilon_{i,t}$. The dependent variable $y_{i,t+1}$ is the natural logarithm of greenhouse gas emissions of firm i in year $t+1$. Total, direct, and indirect emissions are defined in Table 2.1. $\log(ESG\ Score_{i,t})$ is the natural logarithm of the ESG score assigned to firm i for its year t's performance, provided by MSCI (Panel A), Refinitiv ASSET4 (Panel B), and S&P Global (Panel C). The regressions are estimated separately for the periods before and after iXBRL adoption. "Difference in Coefficients" is calculated as $\beta_1^{After\ iXBRL} - \beta_1^{Before\ iXBRL}$. All specifications control for firm characteristics $\boldsymbol{X}$, including *Firm Size*, *ROA*, and *Leverage*, as defined in Table 2.1. Standard errors are clustered at the firm level and are reported in parentheses. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

Table 2.5: Predictive Power of ESG score for Next Period's ESG Risk Incidents

	Dep. var = *ESG Risk Incidents*			
	Indicator (0/1)		Number (log)	
	After iXBRL (1)	Before iXBRL (2)	After iXBRL (3)	Before iXBRL (4)
Panel A: MSCI				
log(*ESG Score*)	0.038*	0.005	-0.092	0.053
	(0.023)	(0.016)	(0.071)	(0.065)
Difference in Coefficients	0.033		-0.144*	
p-value	0.161		0.062	
Obs.	5,785	5,785	2,378	2,378
Panel B: Refinitiv ASSET4				
log(*ESG Score*)	0.111***	0.116***	0.148	0.288***
	(0.027)	(0.019)	(0.113)	(0.077)
Difference in Coefficients	-0.005		-0.140	
p-value	0.865		0.219	
Obs.	6,160	6,160	2,526	2,526
Panel C: S&P Global				
log(*ESG Score*)	0.114***	0.121***	0.205*	0.132
	(0.034)	(0.029)	(0.123)	(0.096)
Difference in Coefficients	-0.007		0.072	
p-value	0.871		0.599	
Obs.	4,006	4,006	1,603	1,603
Controls	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes

Note. This table reports the estimates from regression: $y_{i,t+1} = \beta_0 + \beta_1 \log(ESG\ Score_{i,t}) + \boldsymbol{\gamma' X_{i,t}} + \boldsymbol{FE} + \epsilon_{i,t}$. Columns (1) and (2) are based on the full sample, and the dependent variable $y_{i,t+1}$ is an indicator equal to one if firm i experiences at least one ESG risk incident in year $t+1$. Columns (3) and (4) restrict the sample to firms that experience at least one ESG risk incident in year $t+1$, and the dependent variable $y_{i,t+1}$ is the natural logarithm of the number of incidents for firm i in year $t+1$. $\log(ESG\ Score_{i,t})$ is the natural logarithm of the ESG score assigned to firm i for its year t's performance, provided by MSCI (Panel A), Refinitiv ASSET4 (Panel B), and S&P Global (Panel C). The regressions are estimated separately for the periods before and after iXBRL adoption. "Difference in Coefficients" is calculated as $\beta_1^{After\ iXBRL} - \beta_1^{Before\ iXBRL}$. All specifications control for firm characteristics $\boldsymbol{X}$, including *Firm Size*, *ROA*, and *Leverage*, as defined in Table 2.1. Standard errors are clustered at the firm level and are reported in parentheses. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

Table 2.6: iXBRL Adoption and ESG Commitments

	Dep. var = *Fraction of ESG Commitment Sentences*		
	Total	Non-specific	Specific
	(1)	(2)	(3)
Panel A: Static Treatment Effect			
Post-iXBRL	0.037**	0.031**	0.006
	(0.016)	(0.014)	(0.020)
Panel B: Post-iXBRL Dynamic Effect			
Post-iXBRL (Rel. Yr = 0)	-0.001	-0.006	0.005
	(0.014)	(0.014)	(0.003)
Post-iXBRL (Rel. Yr = 1)	0.086***	0.078***	0.008
	(0.033)	(0.025)	(0.046)
Obs.	5,082	5,082	5,082
Controls	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes
Rater-set FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes

Note. This table presents the Callaway & Sant'Anna estimates from the static DiD regression: $y_{i,t} = \beta_0 + \beta_1 Post\text{-}iXRBL_{i,t} + \boldsymbol{\gamma' X} + \boldsymbol{FE} + \epsilon_{i,t}$, and from its dynamic specification. The event window covers [–3, +1] years relative to the first year of iXBRL adoption. Panel A reports static treatment effect estimates, and Panel B reports dynamic effect estimates for post-iXBRL periods. The dependent variable $y_{i,t}$ is the fraction of ESG commitment sentences to the total number of ESG-related sentences in Item1, Item 1A and Item 7 of firm i's 10-K report for fiscal year t. Columns (1), (2), and (3) report results for total, non-specific, and specific ESG commitments, respectively. $Post\text{-}iXBRL_{i,t}$ is a dummy equal to one if firm i files its 10-K report for fiscal year t in iXBRL, and zero otherwise. All specifications control for pre-treatment firm characteristics $\boldsymbol{X}$, including *Firm Size*, *ROA*, and *Leverage*, as defined in Table 2.1. Standard errors are clustered at the firm level and are reported in parentheses. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

2.7 Appendix

Figure A2.1: Example - Etsy's Reporting of Workforce Diversity

(a) Pre-iXBRL: Unstructured Narrative Text

Our Team

We pride ourselves on our action-oriented, values-based and purpose-driven work culture. Etsy's employees work hard to bring innovative ideas and energy every day to strengthen the experience for sellers and buyers on Etsy.com. As of December 31, 2017, we had 744 employees worldwide, with 452 employees located in our headquarters in Brooklyn, New York. Of those employees, we had 237 in engineering, 97 in product, 161 in member operations, 102 in marketing and 147 in facilities, IT and other corporate teams.

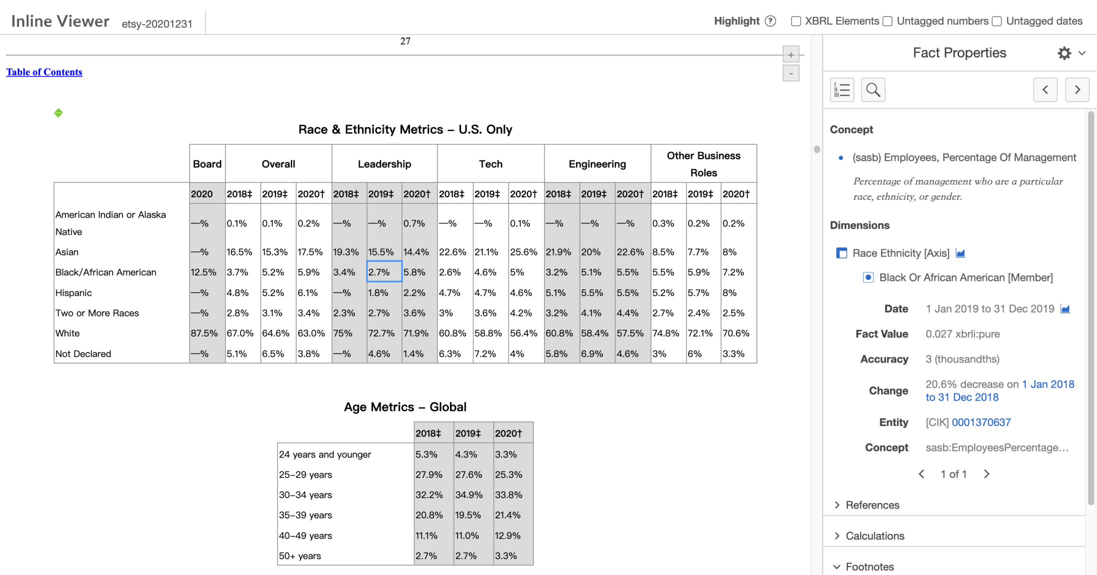
We focus on maximizing our employees' engagement, and their professional and personal well-being.

We believe employee engagement comes from fulfilling work focused on serving the needs of our sellers and buyers and from ample personal and professional growth opportunities. We invest heavily in employee development by offering coaching, skills workshops and training. We also offer our employees paid time off to volunteer so that they can support the causes and organizations they are passionate about. In 2016, we introduced a 26-week gender-blind parental leave policy that is available to all Etsy employees globally. Through this policy we aim to support and enable parents to play equal roles in building successful companies and nurturing their families.

Etsy engineering is widely known for its thought-leading approaches to software development as well as its unique engineering culture. Our engineering team coined the phrase "Code as Craft" to describe our love for building software and our melding of engineering discipline and individual craftsmanship. We believe our engineers have the skills, practices and experience needed to embrace the change the future inevitably brings.

We believe that a diverse workforce makes us a better company, and we strive to create opportunities for underrepresented groups to join, thrive and advance at Etsy. As part of our impact strategy, we have implemented new recruiting guidelines that are intended improve diversity at all levels of our company. As of December 31, 2017, 55% of employees identified as women. As of December 31, 2017, 60% of managers and 38% of product, engineering and technical operations employees identified as women. We are committed to improving the diversity within our workforce, and we publish an annual report that details our progress toward this goal.

(b) Post-iXBRL: Tagged Structured Table



Race & Ethnicity Metrics – U.S. Only

	Board	Overall			Leadership			Tech			Engineering			Other Business Roles		
	2020	2018‡	2019‡	2020†	2018‡	2019‡	2020†	2018‡	2019‡	2020†	2018‡	2019‡	2020†	2018‡	2019‡	2020†
American Indian or Alaska Native	—%	0.1%	0.1%	0.2%	—%	—%	0.7%	—%	—%	0.1%	—%	—%	—%	0.3%	0.2%	0.2%
Asian	—%	16.5%	15.3%	17.5%	19.3%	15.5%	14.4%	22.6%	21.1%	25.6%	21.9%	20%	22.6%	8.5%	7.7%	8%
Black/African American	12.5%	3.7%	5.2%	5.9%	3.4%	2.7%	5.8%	2.6%	4.6%	5%	3.2%	5.1%	5.5%	5.5%	5.9%	7.2%
Hispanic	—%	4.8%	5.2%	6.1%	—%	1.8%	2.2%	4.7%	4.7%	4.6%	5.1%	5.5%	5.5%	5.2%	5.7%	8%
Two or More Races	—%	2.8%	3.1%	3.4%	2.3%	2.7%	3.6%	3%	3.6%	4.2%	3.2%	4.1%	4.4%	2.7%	2.4%	2.5%
White	87.5%	67.0%	64.6%	63.0%	75%	72.7%	71.9%	60.8%	58.8%	56.4%	60.8%	58.4%	57.5%	74.8%	72.1%	70.6%
Not Declared	—%	5.1%	6.5%	3.8%	—%	4.6%	1.4%	6.3%	7.2%	4%	5.8%	6.9%	4.6%	3%	6%	3.3%

Age Metrics – Global

	2018‡	2019‡	2020†
24 years and younger	5.3%	4.3%	3.3%
25–29 years	27.9%	27.6%	25.3%
30–34 years	32.2%	34.9%	33.8%
35–39 years	20.8%	19.5%	21.4%
40–49 years	11.1%	11.0%	12.9%
50+ years	2.7%	2.7%	3.3%

Note. This figure presents another example from Etsy, Inc.'s 10-K filings to illustrate how the adoption of iXBRL changes the presentation and accessibility of sustainability-related information. Panel (a) is a screenshot of Etsy's form 10-K for the fiscal year ended December 31, 2017. Prior to iXBRL adoption, the company disclosed its workforce diversity through unstructured narrative text. Panel (b) displays the corresponding section from Etsy's form 10-K for the fiscal year ended December 31, 2020, viewed in an iXBRL viewer. Following iXBRL adoption, Etsy reported the racial and ethnic composition of its workforce in a structured table with iXBRL tags. These tags specify data elements such as the reporting concept (e.g., "Percentage of Management Who Are Black or African American"), relevant dimensions (e.g., fiscal period, race/ethnicity category, and employee group), and associated percentage changes. This structured tagging enables efficient and consistent extraction by automated systems and facilitates comparability across firms and time.

Figure A2.2: Example - Etsy's Reporting of Impact Strategy

(a) Pre-iXBRL: Unstructured Image

For 2018, we have set key performance indicators ("KPIs") in order to measure our impact progress.

OVERARCHING GOAL	**Economic Impact** Make creative entrepreneurship a path to economic security and personal empowerment	**Social Impact** Enable equitable access to the opportunities we create	**Ecological Impact** Build long-term resilience by eliminating our carbon impacts and fostering responsible resource use
KEY INITIATIVES	— Ensure the economic opportunities Etsy creates meaningfully benefit across a broad swath of our seller community — Foster economic security and personal empowerment for creative entrepreneurs through charitable and in-kind contributions — Advance public policies that increase economic security and reduce administrative burdens for creative entrepreneurs	— Meaningfully increase representation of underrepresented groups and ensure equity in Etsy's workforce — Build a diverse, equitable, and sustainable supply chain to support our operations and bring value to both Etsy and our vendors — Increase the presence of underrepresented populations within the Etsy seller community	— Utilize and source energy responsibly so that we can power our operations with 100% renewable electricity by 2020 and reduce the intensity of our energy use by 25% by 2025 — In 2018, develop a plan and set a goal to mitigate the carbon impacts of our marketplace that aligns with business growth — Run zero waste operations by 2020
2018 KPI	*Increase the number of sellers whose sales on Etsy serve as a meaningful source of personal income*	*Interview at least two candidates from underrepresented backgrounds for each role filled*	*Reduce the carbon dioxide emissions generated from our offices by at least 5% in 2018 compared to 2016*

(b) Post-iXBRL: Structured HTML Table

2020 Impact Highlights

Economic Impact	**Social Impact**	**Ecological Impact**
		
Make creative entrepreneurship a path to economic security and personal empowerment.	***Enable equitable access to the opportunities that we create.***	***Build long-term resilience by eliminating our carbon impacts and fostering responsible resource use.***
We have met our goal to double Etsy sellers' U.S. economic output by 2023. U.S. Etsy sellers contributed $13 billion to the U.S. economy in 2020, up 142% from our 2018 baseline, created 2.6 million jobs, enough to employ the entire city of Houston, Texas while generating nearly $4 billion in income to U.S. households.	Etsy continued to attract and retain world-class talent in 2020, with a keen focus on diversity and gender balance. In 2020, Etsy (excluding Reverb) more than doubled the percentage of our leadership level employee population who identify as an underrepresented minority (Black, Latinx, or Native American; collectively, "URM"). URM hires constituted 20% of U.S. hires in 2020, and our U.S. URM employee population has increased from 8.5% in 2018 to 12.5% in 2020.	We met our 2020 goal to source 100% of our electricity from renewable energy. This includes electricity used to power our global offices, remote electricity used by employees working from home in the United States as a result of COVID-19, and our computing load in colocated data centers and Google Cloud. We also met our goal to run a carbon neutral business for 2020 by investing in over 400,000 verified emissions reductions that protect forests, sponsor wind and solar farms, and help develop greener methods for producing auto parts.

Note. This figure provides an example from Etsy, Inc.'s 10-K filings to illustrate how the adoption of iXBRL changes the presentation and accessibility of sustainability-related information. Panel (a) is a screenshot of Etsy's form 10-K for the fiscal year ended December 31, 2017. Prior to iXBRL adoption, the company disclosed its impact strategy using a static image, which was readable by humans but invisible to machines. Panel (b) shows the corresponding section from Etsy's form 10-K for the fiscal year ended December 31, 2020. Following iXBRL adoption, the same content was reformatted into structured HTML tables, making it easily extractable by algorithms.

Table A2.1: Robustness: iXBRL Adoption and the Extent of Sustainability Disclosure (Including Voluntary Adopters)

	Dep. var = *Bloomberg Disclosure Score*				Textual
	ESG	Env.	Soc.	Gov.	Measure
	(1)	(2)	(3)	(4)	(5)
Post-iXBRL	2.692***	5.466***	1.747**	0.866***	0.008**
	(0.485)	(0.950)	(0.733)	(0.141)	(0.003)
Obs.	7,718	7,718	7,718	7,718	5,948
Controls	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes

Note. This table replicates the static difference-in-differences analysis in Table 2.2 but includes firms that voluntarily adopted iXBRL prior to their mandatory compliance date. Standard errors are clustered at the firm level and are reported in parentheses. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

Table A2.2: Robustness: iXBRL Adoption and ESG Rating Disagreement (Including Voluntary Adopters)

	Dep. var = *ESG Rating Disagreement*					
	Env.		Soc.		Gov.	
	(1)	(2)	(3)	(4)	(5)	(6)
Post-iXBRL	-0.059**	-0.008	0.005	-0.045***	0.054	-0.040***
	(0.028)	(0.007)	(0.022)	(0.010)	(0.063)	(0.013)
Obs.	3,339	3,534	3,339	3,534	3,339	3,534
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Rater-set FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Level of ESG disclosure	Low	High	Low	High	Low	High

Note. This table replicates the static difference-in-differences analysis in Table 2.3 but includes firms that voluntarily adopted iXBRL prior to their mandatory compliance date. Standard errors are clustered at the firm level and are reported in parentheses. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

Table A2.3: Robustness: iXBRL Adoption and ESG Commitments (Including Voluntary Adopters)

	Dep. var = *Fraction of ESG commitment sentences*		
	Total	Non-specific	Specific
	(1)	(2)	(3)
Post-iXBRL	0.038**	0.031***	0.007
	(0.015)	(0.012)	(0.018)
Obs.	5,778	5,778	5,778
Controls	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes
Rater-set FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes

Note. This table replicates the static difference-in-differences analysis in Table 2.6 but includes firms that voluntarily adopted iXBRL prior to their mandatory compliance date. Standard errors are clustered at the firm level and are reported in parentheses. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

Table A2.4: Probability of ESG Risk Incidents by Fiscal Year

Fiscal Year	Mean of *ESG Risk Incidents (1/0)*
2016	0.378
2017	0.399
2018	0.349
2019	0.379
2020	0.340
2021	0.334

Note. This table reports the annual mean of the indicator variable *ESG Risk Incidents (1/0)*, which is equal to one if a firm experiences at least one ESG risk incident in a given fiscal year and 0 otherwise. Fiscal year t is defined as the fiscal year ending between June 15 of calendar year t–1 and June 14 of calendar year t.

Chapter 3

Common Ownership, Public Attention, and Corporate Sustainability Performance

Co-authored with Huiyun Li and Qianying Liu

Abstract

This paper investigates the association between common ownership and corporate sustainability performance, as well as the moderating role of public attention to environmental issues. Using data on Chinese A-share listed firms, we show that common ownership is positively associated with firms' sustainability performance, and that this relationship is positively moderated by local public attention to environmental issues. Moreover, the channel through which common ownership is associated with sustainability performance varies with the level of public attention: the information transmission channel dominates in regions with high public attention to environmental issues, whereas the governance channel becomes more pronounced in regions with low public attention.

3.1 Introduction

In recent years, the rising focus on sustainability issues has reshaped corporate behavior and investment practices globally. Investors, regulators, and consumers are increasingly concerned with how firms manage sustainability-related risks and responsibilities. This shift has been amplified by information about sustainability issues and corporate behavior becoming more accessible and visible to the public. As public awareness and scrutiny grow, investors and firms are under increasing pressure to align their operations with sustainability principles.

While prior research on firms' sustainability outcomes has emphasized the role of either firm ownership structures or societal pressures in isolation, much less is known about how these two forces interact to influence sustainability outcomes. Understanding this interaction can have important practical implications. For investors, it helps better tailor their engagement strategies. For policy makers, it offers insights into how regulatory interventions and public awareness campaigns can be more effectively targeted. For firms, it helps guide efficient strategic alignment with evolving stakeholder expectations. Motivated by these considerations, we explore how ownership structures and societal pressures interact and jointly shape firms' sustainability outcomes.

Specifically, this paper investigates how common ownership and local public attention to environmental issues jointly predict the sustainability performance of Chinese public firms. We first test the association between common ownership and firms' sustainability performance, and the moderating role of public attention to environmental issues. We then explore two potential channels through which common ownership may predict better sustainability performance: (i) by facilitating the transmission of sustainability-related knowledge across industry peers (the information transmission channel), and (ii) by strengthening internal governance oversight (the governance channel). Furthermore, we assess how the strength of these channels varies with the level of local public attention.

Using a comprehensive sample of Chinese public firms from 2011 to 2022, our empirical analysis document three main findings. First, common ownership is positively associated with firms' sustainability performance, and this association is strengthened in cities with higher public attention to environmental issues. Second, we provide evidence consistent with both the information transmission channel and the governance channel. Third, the relative strength of these two channels depends on public attention: the information transmission channel dominates in cities with higher public attention to environmental issues, while the governance channel plays a more prominent role in cities with lower public attention.

Our findings are consistent with theories of common ownership. These theories suggest that common owners have incentives to maximize the value of their overall portfolios by reducing negative externalities or increasing positive externalities across portfolio firms (e.g., Hansen and Lott Jr (1996); López and Vives (2019)). Since sustainability issues are increasingly recognized as a source of systematic risk, common owners, who are typically large institutional investors, are subject to heightened public and regulatory scrutiny. As a result, they may have incentives to mitigate negative externalities from regulatory interventions and reputational damage, and generate positive externalities, such as reputational gains, by promoting stronger sustainability performance among their portfolio firms. These incentives are likely to be amplified in regions with higher public attention to environmental issues. When sustainability issues receive greater social visibility, the reputational costs of inaction and worse sustainability performance increase, while the positive externalities of promoting sustainability performance increase. In addition, common owners are often universal owners who hold highly-diversified and long-term portfolios with significant stakes, which means that their portfolios are inevitably exposed to systematic risks, such as climate change. Therefore, they have incentives to internalize exposure to climate change by improving the sustainability outcomes of their portfolio firms.

An alternative explanation for the observed positive association between common ownership and firms' sustainability performance is reverse causality. That is, firms with stronger sustainability performance might attract more common owners. While this paper does not aim to provide causal evidence, we attempt to rule out reverse causality as an alternative explanation by leveraging the Green Finance Reform and Innovation Pilot Policy, which was launched by the Chinese government in 2017. This policy promoted green financial instruments, reduced corporate greenhouse gas emissions, increased firm green innovation and improved firms' sustainability-related risk management. If reverse causality were the primary explanation, we would expect a significant increase in common ownership in firms headquartered in pilot provinces. To test this, we conduct a difference-in-differences analysis and find no significant increase in common ownership following the policy. This result suggests that reverse causality is unlikely to drive our baseline findings.

Turning to the positive moderating role of public attention, a potential confounding factor is the intensity of local environmental regulations. A higher intensity of environmental regulations may induce the local public to pay more attention to environmental issues. Meanwhile, firms and common owners are subject to stricter regulatory compliance requirements. We address this concern by splitting the sample into high- and low-intensity subsamples based on city-level regulatory intensity, measured using the frequency of keywords related to environmental regulations in annual reports of city governments. The positive moderating effect of public attention remains robust in both subsamples, with no significant difference between them, suggesting that the observed moderating effect cannot be entirely attributable to local regulatory enforcement.

To further explore the link between common ownership and sustainability performance, we examine two potential channels: the information transmission channel and the governance channel.

The information transmission channel emphasizes that common owners facilitate the diffusion of sustainability-related knowledge and practices across their portfolio firms. This idea builds on existing studies showing that firms' disclosure generates informational spillovers that improve peers' information environments, leading to higher liquidity and lower costs of capital for peer firms (Admati and Pfleiderer, 2000; Bushee and Leuz, 2005; Chen et al., 2013; Shroff et al., 2017). To internalize these externalities, common owners encourage their portfolio firms to provide more voluntary disclosure (Park et al., 2019). Overall, common ownership can reduce information frictions and facilitate the spread of sustainability knowledge and resources across firms, thereby allowing portfolio firms to adopt industry-leading practices more efficiently and reduce the costs of trial-and-error in implementing sustainability initiatives.

The governance channel emphasizes the role of common owners in monitoring and influencing their portfolio firms' decisions. Common owners are typically large institutional investors who hold significant stakes in their portfolio firms. They have the capacity to strengthen managerial oversight, discourage short-termism, and promote long-term investments in sustainability through mechanisms such as active engagement and divestment. For example, Kang et al. (2018) show that institutional owners holding multiple blocks in the same industry are more likely to perform effective monitoring. Dyck et al. (2019) find that institutional investors improve firms' environmental and social performance, particularly when the investors are from foreign countries with strong sustainability norms. They also show that institutional investors promote such improvements primarily through private engagement with firms they already hold.

The relative dominance of the two channels is likely to vary with the level of public attention to environmental issues. When public attention is high, firms face greater scrutiny and social pressure from local stakeholders, such as consumers, media, and policymakers. These external pressures translate into societal expectations for firms' sustainability behavior, and firms that fail to respond risk reputational damage. Heightened reputational concerns therefore increase firms' incentives to improve their sustainability performance. In such contexts, the primary constraint for firms is not the willingness to act, but the lack of sufficient knowledge and resources to implement effective practices. Common owners can alleviate this constraint by transmitting sustainability-related knowledge and resources, making the information transmission channel particularly effective, while reducing the need to rely on costly governance interventions.

By contrast, when public attention to environmental issues is low, external scrutiny and pressure from stakeholders is weaker and firms face limited reputational consequences for poor sustainability performance. In this setting, firms have fewer incentives to actively improve their sustainability practices, even when relevant knowledge or resources are available. As a result, the information transmission channel may not effectively induce significant changes in sustainability practices. Nevertheless, common owners, and especially universal owners, may still have incentives to push for improvements in sustainability performance, with the aim to internalize systematic externalities such as climate change. Common owners may need to rely more heavily on governance mechanisms to influence corporate sustainability behavior. Consequently, we expect the governance channel to dominate under low public attention.

To test the information transmission and governance channels, we conduct a mediation analysis following the framework of Baron and Kenny (1986) and estimate the statistical significance of the mediated effect using the bootstrapping method. The bootstrapping method is recommended because it does not require the mediated effects to follow a normal distribution (Shrout and Bolger, 2002).

We proxy the information transmission channel using the natural logarithm of the average Bloomberg ESG disclosure score of peer firms that are commonly held with the focal firm. We first show that common ownership predicts greater ESG disclosure by peer firms. We then show that, when regressing firm ESG performance on common ownership, the coefficient on common ownership becomes statistically significantly smaller after controlling for the average ESG disclosure of commonly held peer firms. These results are consistent with the hypothesis that common ownership is positively associated with firms' sustainability performance, at least partially, via the information transmission channel.

We capture the governance channel using the focal firms' internal control quality, measure by China's Dibo Internal Control Index. The Dibo Internal Control Index covers multiple dimensions of corporate governance, including compliance with laws and regulations, operational efficiency, reliable financial reporting, and effective risk management. We first show that common ownership predicts stronger internal control quality. We then find that, when regressing firm ESG performance on common ownership, the coefficient on common ownership becomes statistically significantly smaller after controlling for the focal firm's internal control quality. These results are consistent with the hypothesis that common ownership is positively associated with firms' sustainability performance, at least partially, through the governance channel.

To test the relative dominance of the two channels under different levels of public attention to environmental issues, we split the sample based on the sample mean of city-level public attention. Firms headquartered in cities with public environmental attention above (below) the sample mean are categorized into the subsample of high (low) public attention group. We then repeat the mediation analysis of each channel within these subsamples. Our findings are consistent with our predictions: the information transmission channel is more prominent in high-attention regions, whereas the governance channel dominates in low-attention regions.

This paper contributes to two strands of literature. The first strand examines the impact of societal pressure on corporate sustainability performance. Scrutiny from governments and the public has been shown to encourage more voluntary reporting on corporate social responsibility (e.g., Cho and Patten (2007); Delmas and Toffel (2008); Reid and Toffel (2009); Marquis and Qian (2014); Cho et al. (2015)). In addition, higher levels of public attention also incentivize firms to invest in green innovation, energy efficiency, and emissions reduction (Pan and He, 2022; Zhou and Ding, 2023). The second strand of literature examines the role of ownership structure in corporate sustainability. Prior studies suggest that institutional ownership motivates firms to have more their CSR reporting and activities (e.g., Dhaliwal et al. (2011); Solomon et al. (2011); Dyck et al. (2019); Chen et al. (2020)). As for common ownership, existing evidence is mixed regarding whether it promotes or hinders corporate sustainability performance.(e.g., Dai and Qiu (2021); Cheng et al. (2022); Hirose and Matsumura (2022))

While prior research has examined ownership structure or public pressure in isolation, this paper highlights their interaction by analyzing how public attention to environmental issues moderates the association between common ownership and firms' sustainability performance. In addition, this paper advances the understanding of the mechanisms linking common ownership and sustainability performance. It distinguishes between the information transmission and governance channels and demonstrates that their relative importance varies with the level of public attention.

The rest of this paper is organized as follows. Section 3.2 describes the data sources, variable construction, and sample. Section 3.3 conducts baseline analysis. Section 3.4 examines the information transmission and governance channels. Section 3.5 concludes.

3.2 Data and Sample

3.2.1 Data and Variables

3.2.1.1 Independent Variable: Firm Common Ownership

We obtain firm ownership data from the CSMAR database. Following He and Huang (2017), we define a common owner of a firm as a shareholder who holds at least 5% of the outstanding shares in the focal firm as well as at least 5% of the outstanding shares in one or more other firms within the same industry. We measure firm common ownership by the number of common owners. Specifically, we obtain the number of common owners of each firm on a quarterly basis, and compute the annual common ownership of a firm ($COMMON$) as the natural logarithm of the average number of common owners across the four quarters in a year. As discussed by Gerardi et al. (2023), this measure does not depend on firm market shares or control rights, thereby reducing endogeneity and measurement validity issues.

We use 5% as the threshold because previous literature has generally noted that investors with a 5% stake have the motivation and ability to monitor management behavior (Shleifer and Vishny, 1986). In addition, this threshold aligns with company laws in China, where a 5% equity holding is frequently recognized as a significant stake that grants substantial influence over firm governance. We show in Section 3.3.4 that our results are robust to alternative measures of common ownership.

3.2.1.2 Dependent Variable: Firms' Sustainability Performance

We measure firms' sustainability performance by the firm's overall environmental, social, and governance performance score (ESG) provided by Sino-Securities Index ESG ratings (hereinafter referred to as SINO). SINO ESG scores are on a scale of 0 (worst) to 100 (best). Compared with other major ESG rating agencies, SINO has the most comprehensive coverage of China's A-share listed firms. Hence, using SINO ESG scores ensures that our analysis includes the most complete and representative dataset for China's A-share market. Nevertheless, we show in Section 3.3.4 that our results are robust to ESG scores provided by alternative rating agencies.

3.2.1.3 Moderating Variable: Public Attention to Environmental Issues

Following Pan and He (2022), Zhou and Ding (2023), and Barwick et al. (2024), we use Baidu Search Index to measure public attention. Baidu Search Index, which is similar to Google Trends, is a search intensity index provided by China's largest search engine service provider, Baidu. Baidu Search Index tracks city-level frequency of keyword search queries from both desktop and mobile users on a daily basis. A higher index indicates a higher search frequency. We measure public attention to environmental issues ($ATTN$) in a given city in a given year by the annual total Baidu Search Index of the keyword "environmental pollution" in that city.

3.2.1.4 Control Variables

We control for the following firm characteristics that potentially affect firms' sustainability performance and common ownership: firm size (*SIZE*), age (*AGE*), leverage (*LEV*), return on assets (*ROA*), Tobin's Q (*TOBINQ*), whether the firm is audited by a Big Four accounting firm (*BIG4*), cash flow ratio (*CASH*), management expense ratio (*MEXP*), management shareholding ratio (*MSHARE*), institutional ownership (*INST*), and the degree of industrial concentration (*HHI*). Data for all control variables are obtained from the CSMAR database. All variables are defined in Appendix Table A3.1.

3.2.2 Sample Construction and Summary Statistics

Our sample begins in 2011, the first year that Baidu Search Index becomes available, and ends in 2022 based on data availability of SINO ESG scores. We start with all A-share listed firms in China from 2011 to 2022. After excluding ST companies, financial institutions, and observations with missing values for main variables, our final sample consists of 33,043 firm-year observations.

Table 3.1 presents the summary statistics of the entire sample. The ESG score ranges from 57.9 to 84.1. An average firm has a ESG score of 73.4, which corresponds to a B rating[1], indicating that the sustainability performance of Chinese firms is relatively poor. The Baidu Search Index of "environmental pollution" ranges from 8.5 to 1,148, with a standard deviation of 216.4, indicating that significant variation exists in the level of public attention to environmental pollution across cities. *COMMON* ranges from 0 to 1.0, indicating that the average number of common owners in a firm in a given year ranges from 0 to 2 in our sample.

[1]SINO ESG ratings: Leaders-AAA, AA, A, BBB; Average-BB, B, CCC; Laggard-CC, C.

3.3 Baseline Analysis

3.3.1 Common Ownership and Firms' Sustainability Performance

To examine the association between common ownership and Firms' Sustainability Performance, we estimate the following regression:

$$ESG_{i,t} = \beta_0 + \beta_1 COMMON_{i,t} + \boldsymbol{\gamma' X_{i,t}} + IND + PROV + YEAR + \epsilon_{i,t}, \tag{3.1}$$

where $ESG_{i,t}$ is the natural logarithm of firm i's overall ESG performance score from SINO in year t. $COMMON_{i,t}$ is common ownership of firm i in year t. The vector $\boldsymbol{X_{i,t}}$ stacks all the control variables listed out in Section 3.2.1. All variables are defined in Appendix Table A3.1. We include industry (IND) and province ($PROV$) fixed effects to account for unobservable industry-specific characteristics and province-level regulations that might influence baseline sustainability performance and common ownership patterns. We also include year fixed effects ($YEAR$) to control for common trends in sustainability, such as policy shifts and evolving social norms towards sustainable development.

Results are presented in column (1) of Table 3.2. The coefficient on *COMMON* is 0.913 and is significant at the 1% level. This finding indicates that common ownership is positively associated with firms' sustainability performance.

The positive association is consistent with theories of common ownership, which suggest that common owners seek to maximize the value of their overall portfolios by reducing negative externalities or fostering positive spillovers across the firms they hold (e.g., Hansen and Lott Jr (1996); López and Vives (2019)). As sustainability has become a central concern for policymakers and the public, common owners, who are typically large institutional investors, are subject to heightened public and regulatory scrutiny. Such scrutiny increases common owners' incentives to mitigate negative externalities from regulatory interventions and reputational damage, and generate positive externalities, such as reputational gains, by promoting stronger sustainability performance among their portfolio firms. In addition, since many common owners are universal investors who hold highly diversified and long-term portfolios with significant stakes, they are inevitably exposed to systematic risks. This risk exposure further strengthens their incentives to internalize sustainability-related systematic risks, such as climate change, by improving sustainability outcomes of their portfolio firms.

3.3.2 The Moderating Role of Public Attention to Environmental Issues

To test the moderating effect of public attention to environmental issues, we estimate the following regression:

$$\begin{aligned} ESG_{i,t} = \beta_0 + \beta_1 COMMON_{i,t} + \beta_2 COMMON_{i,t} \times ATTN_{c,t} + \beta_3 ATTN_{c,t} \\ + \boldsymbol{\gamma' X_{i,t}} + IND + PROV + YEAR + \epsilon_{i,c,t}, \end{aligned} \tag{3.2}$$

where the moderating variable, $ATTN_{c,t}$, captures public attention to environmental issues in year t in city c where firm i is headquartered. Control variables, stacked in the vector $\boldsymbol{X_{i,t}}$, are the same as in Equation (3.1). The coefficient β_2 captures the moderating effect.

Results are reported in column (2) of Table 3.2. The coefficient on the interaction term $COMMON \times ATTN$ is 0.476 and is significant at the 1% level.This result indicates that the positive association between common ownership and firms' sustainability performance becomes stronger in cities with higher levels of public attention to environmental issues. This positive moderating effect of public attention is consistent with the view that heightened societal scrutiny increases the reputational and political costs of weak sustainability performance, while amplifying the benefits of stronger practices. Under such conditions, common owners have stronger incentives to promote sustainability across their portfolio firms, thereby reinforcing the positive relationship between common ownership and firms' sustainability outcomes.

3.3.3 Alternative Explanations

3.3.3.1 Reverse Causality

One potential concern is that the positive association between common ownership and sustainability performance may be driven by reverse causality. While this paper does not aim to provide causal evidence, we attempt to mitigate reverse causality as an alternative explanation by leveraging the Green Finance Reform and Innovation Pilot Policy launched by the Chinese government. This policy promoted green financial instruments, reduced corporate greenhouse gas emissions, increased firm green innovation and improved firms' sustainability-related risk management. If common owners intentionally select firms with outstanding sustainability performance, we should observe a significant increase in common ownership in firms located in pilot provinces. To test this, we conduct a difference-in-differences analysis using the following specification:

$$COMMON_{i,t} = \beta_0 + \beta_1 TREAT_i \times POST_t + \boldsymbol{\gamma' X_{i,t}} + FIRM + YEAR + \epsilon_{i,c,t}. \tag{3.3}$$

The policy was lunched in 2017, and the dummy variable $POST_t$ is equal to one for the years 2018-2022 and zero otherwise. The dummy variable $TREAT_i$ is equal to one if firm i is headquartered in a pilot province and zero otherwise. The specification includes firm and year fixed effects. The control variables are the same as in Equation (3.1) and Equation (3.2).

The results are reported in column (1) of Table 3.3. The estimated coefficient on the interaction term $TREAT_i \times POST_t$ is not significantly positive, indicating no significant increase in common ownership in following the policy. This finding suggests that reverse causality is unlikely to drive our baseline results.

3.3.3.2 Environmental Regulations

The moderating effect of public attention may be confounded by different intensities of environmental regulatory enforcement across cities. Cities with stronger environmental regulations may experience both higher levels of public attention and more sustainable corporate behavior. To address this concern, we split the sample based on the intensity of city-level environmental regulations and re-examine the moderation effect within each subsample.

To proxy the intensity of city-level environmental regulations, we take the proportion of the total counts of 14 keywords[2] related to environmental regulations in city government annual reports. We then categorize firms headquartered in cities with environmental regulation intensity higher (lower) than the sample median into the strong (weak) environmental regulation subsample. Equation (3.2) is estimated for each subsample, and the results are reported in columns (2) and (3) of Table 3.3. The coefficients of the interaction term $COMMON \times ATTN$ are significantly positive in both groups, with no significant difference (p-value of Chow test = 0.897) between the two groups. These results imply that the moderating effect of public attention holds regardless of the strength of environmental regulations and cannot be explained by strong local environmental regulations.

3.3.4 Robustness Checks

We next conduct a series of robustness checks to show that our results are robust to alternative measures of common ownership, firms' sustainability performance and public attention to environmental issues.

3.3.4.1 Alternative Measure of Common Ownership

Following He and Huang (2017), we use the total percentage holdings by common owners in a firm ($COMSHARE$) and the natural logarithm of the number of cross owners holding at least a 10% stake in a firm ($COMMON10$) as two alternative measures of common ownership. We re-estimate Equations (3.1) and (3.2) with the alternative measures and report the estimates in Appendix Ta-

[2] The 14 keywords are: environmental protection, pollution, energy consumption, emission reduction, sewage discharge, ecological, green, low carbon, air, chemical oxygen demand, sulfur dioxide, carbon dioxide, PM10, PM2.5.

ble A3.2. According to columns (1) and (3), the coefficients on *COMSHARE* and *COMMON10* are both positive and significant the 1% level. According to columns (2) and (4), the coefficients of the interaction terms between common ownership and public attention to environmental issues, *COMSHARE×ATTN* and *COMMON10×ATTN*, respectively, are both positive and statistically significant. Hence, our results are robust to the alternative measures of common ownership.

3.3.4.2 Alternative Measure of Firms' Sustainability Performance

We replace the SINO ESG score with the ESG score provided by another ESG rating agency in China, WIND. We re-estimate Equations (3.1) and (3.2) with the alternative measures and report the estimated coefficients in Appendix Table A3.3. According to column (1), the coefficient on *COMMON* remains significantly positive. According to column (2), the coefficient on the interaction term *COMMON×ATTN* is positive and significant at the 1% level. These results suggest that our main findings are robust across different ESG rating methodologies and agencies.

3.3.4.3 Alternative Measure of Public Attention to Environmental Issues

In China, haze has been one of the most serious environmental issues and at the forefront of public concern. Therefore, we use Baidu Search Index of the keyword "haze" as an alternative proxy for the public attention to environmental issues (ATTN_H). The estimated coefficients of Equations (3.1) and (3.2) are reported in Appendix Table A3.4. Column (2) shows that the coefficient of the interaction term *COMMON×ATTN_H* remains positive and significant at the 1% level, indicating that the moderating effect of public attention to environmental issues is robust to the alternative measure of the public attention.

3.4 Channels

This section investigates the channels through which common ownership is positively associated with firms' sustainability performance: the information transmission channel and the governance channel.

3.4.1 Information Transmission Channel

As discussed in Section 3.1, common ownership may help reduce information frictions and promotes the diffusion of sustainability knowledge and practices across firms, enabling portfolio firms to adopt leading practices more effectively and to lower the costs associated with trial-and-error in implementing sustainability initiatives. Common owners, by holding stakes in multiple firms within the same industry, are well-positioned to transfer knowledge from one firm to others. Moreover, information disclosure by one firm tends to generate spillovers of positive market outcomes, such as higher liquidity and lower cost of capital, to its peer firms (Admati and Pfleiderer, 2000; Bushee and Leuz, 2005; Shroff et al., 2017). To internalize these positive externalities, common owners may demand more public disclosure from their portfolio firms.

This reasoning predicts that common ownership induces firms to provide more disclosure, and Park et al. (2019) provide evidence consistent with this prediction. Therefore, we capture the information transmission channel using the natural logarithm of the average Bloomberg ESG disclosure score of commonly held peer firms within the same industry of firm i (DIS). The Bloomberg ESG disclosure score, on a scale from 0 to 100, measures the comprehensiveness of firms' ESG disclosure. A higher (lower) score indicates that more (less) ESG information is disclosed by the given firm.

We then perform a mediation analysis following the framework of Baron and Kenny (1986) and estimate the statistical significance of the mediated effect using the bootstrapping method. The bootstrapping method is recommended because it does not require the mediated effects to follow a normal distribution (Shrout and Bolger, 2002). The results are reported in Table 3.4.

The mediation analysis is based on the sample without missing values for the variable *DIS*. First, we verify that the baseline positive association between common ownership and firms' sustainability performance still holds in this sample. Column (1) of Table 3.4 shows that the coefficient on *COMMON* is 0.831 and significant at the 1% level, confirming the baseline result. Then, column (2) shows that *COMMON* is positively associated with *DIS*, validating that common ownership predicts greater ESG disclosure by commonly held peer firms. Finally, column (3) re-estimates the specification in column (1) but additionally controls for *DIS*. The coefficient on *COMMON* decreases by 0.01 relative to column (1). Although the change in magnitude is marginal, the reduction is statistically significant at the 1% level. Therefore, we cannot reject the hypothesis that common ownership is positively associated with firms' sustainability performance, at least partially, via the information transmission channel.

3.4.2 Governance Channel

As discussed in Section 3.1, the governance channel highlights how common owners monitor and influence the decisions of the firms in their portfolios. Common owners are typically large institutional investors with the capacity to strengthen managerial oversight, discourage short-termism, and promote long-term investments in sustainability through mechanisms such as active engagement and divestment.

We capture the governance channel using firm-level internal control quality (*INTERNAL*), measured by China's Dibo Internal Control Index. The Dibo Internal Control Index covers multiple dimensions of corporate governance, including compliance with laws and regulations, operational efficiency, reliable financial reporting, and effective risk management, with a higher index indicating stronger internal control quality. This approach builds on the assumption that if common owners rely heavily on active engagement or divestment to intervene in firms' management, such interventions should be reflected in improvements in corporate governance. Prior literature provides both theoretical and empirical support for this assumption. For instance, Edmans et al. (2019) develop a model showing that common ownership can enhance governance through both voice and exit, while Dimson et al. (2015), using proprietary data from a single institutional investor, show that corporate governance improves following successful ESG engagements.

We conduct a mediation analysis and present the results in table 3.5. The mediation analysis is based on the sample without missing values for the variable *INTERNAL*. Column (1) of Table 3.5 shows that the baseline positive association between common ownership and firms' sustainability performance continues to hold in this sample, with the coefficient on *COMMON* being positive (0.923) and significant at the 1% level. Column (2) then shows that *COMMON* is positively associated with *INTERNAL*, validating that common ownership predicts improvements in corporate governance. Finally, column (3) re-estimates the specification in column (1) but additionally controls for *INTERNAL*. The coefficient on *COMMON* decreases by 0.038 relative to Column (1), and the reduction is statistically significant at the 1% level. These results are consistent with the hypothesis that common ownership is positively associated with firms' sustainability performance, at least partially, via the governance channel.

3.4.3 Dominant Channel under Different Levels of Public Attention

This section tests the hypothesis that the relative strength of the two channels depends on the level of public attention to environmental issues. Specifically, the information transmission channel is expected to be more effective when public attention is high, while the governance channel is expected to dominate when public attention is low. To test these predictions, we split the sample based on the mean level of city-level public attention. Firms headquartered in cities with public environmental attention above (below) the sample mean are categorized into the subsample of high (low) public attention group.

We first repeat the mediation analysis of the two channels in the high-public-attention subsample and present the results in Table 3.6. Columns (1) and (2) report the results for the information transmission channel. Comparing column (2) to column (1), the coefficient on *COMMON* decreases by 0.014 after controlling for *DIS*. Despite that the change in magnitude is small, the reduction is statistically significant at the 5% level. Columns (3) and (4) show the results for the governance channel. Comparing column (4) to column (3), the coefficient on *COMMON* decreases by 0.027 after controlling for *INTERNAL*, but the reduction is not statistically significant. These results suggest that, relative to the governance channel, information transmission channel tend to dominantly drive the positive association between common ownership and firms' sustainability performance when public attention to environmental issues is high, consistent with our prediction.

We then repeat the mediation analysis of the two channels in the low-public-attention subsample and present the results in Table 3.7. Columns (1) and (2) report the results for the information transmission channel. Comparing column (2) to column (1), the coefficient on *COMMON* decreases by only 0.006 after controlling for *DIS*, and the change is statistically insignificant. Columns (3) and (4) show the results for the governance channel. Comparing column (4) to column (3), the coefficient on *COMMON* decreases by 0.039 after controlling for *INTERNAL*, and the reduction is statistically significant at the 5% level. These results suggest that, relative to the information transmission channel, the governance channel primarily drive the positive association between common ownership and firms' sustainability performance when public attention to environmental issues is low, consistent with our prediction.

3.5 Conclusion

This paper examines the association between common ownership and corporate sustainability performance, as well as the moderating role of public attention to environmental issues. Using data on Chinese A-share listed firms, we find a positive association between common ownership and firms' sustainability performance. We then provide evidence consistent with the following two channels. First, common owners facilitate the transmission of sustainability-related knowledge and resources across their portfolio firms, improving firms' efficiency to adopt industry-leading sustainability practices. Second, common owners promoting firms' sustainability performance by exerting governance forces.

Moreover, we find that the relative importance of these two channels depends on the external social environment: the information transmission channel dominates in cities with higher public attention to environmental issues, while the governance channel plays a more prominent role in cities with lower public attention.

Overall, our findings shed light on the interaction between common ownership and public pressure in shaping firms' sustainability performance, with practical implications for investors and policymakers. Recognizing that the dominant channel varies with the level of public pressure can help investors better tailor their engagement strategies: in regions with high public attention, facilitating information sharing and peer learning may be more effective, whereas in low-attention regions, strengthening direct governance oversight becomes more critical. Furthermore, policymakers aiming to promote corporate sustainability may consider enhancing public awareness and scrutiny by increasing public access to local and firm-level sustainability-related information.

Table 3.1: Summary Statistics

Variable	N	Mean	SD	Min	Median	Max
ESG (raw)	33,043	73.386	5.018	57.87	73.66	84.15
ATTN (raw)	33,043	224.503	215.742	8.486	171.907	1147.888
COMMON	33,043	0.104	0.255	0	0	1.099
SIZE	33,043	22.188	1.278	19.956	21.988	26.21
AGE	33,043	2.016	0.939	0	2.197	3.367
LEV	33,043	0.409	0.204	0.051	0.399	0.885
ROA	33,043	0.044	0.063	-0.204	0.042	0.227
TOBINQ	33,043	2.008	1.256	0.848	1.6	8.215
BIG4	33,043	0.059	0.236	0	0	1
CASH	33,043	0.048	0.068	-0.152	0.047	0.243
MEXP	33,043	0.086	0.066	0.008	0.069	0.399
MSHARE	33,043	0.148	0.203	0	0.015	0.691
INST	33,043	0.431	0.25	0.002	0.445	0.909
HHI	33,043	0.084	0.089	0.016	0.053	0.59

Note. This table presents the summary statistics for the main variables used in this study. All variables are defined in Appendix Table A3.1. For ease of interpretation, this table reports the summary statistics of the raw score value of *ESG* and the raw index value of *ATTN*.

Table 3.2: Baseline Results

Dep. Var = *ESG*	(1)	(2)
COMMON	**0.913***** **(0.188)**	**-1.524*** **(0.848)**
COMMON×ATTN		**0.478***** **(0.163)**
ATTN		-0.063 (0.088)
SIZE	1.325*** (0.062)	1.323*** (0.062)
AGE	-0.809*** (0.068)	-0.774*** (0.068)
LEV	-4.372*** (0.311)	-4.339*** (0.310)
ROA	9.590*** (0.761)	9.527*** (0.753)
TOBINQ	-0.104*** (0.037)	-0.106*** (0.037)
BIG4	0.372* (0.210)	0.340 (0.213)
CASH	-0.180 (0.536)	-0.390 (0.528)
MEXP	-3.387*** (0.769)	-2.910*** (0.763)
MSHARE	1.613*** (0.354)	1.547*** (0.353)
INST	0.592** (0.286)	0.574** (0.283)
HHI	0.513 (0.732)	0.480 (0.738)
Constant	46.829*** (1.296)	47.110*** (1.355)
Industry FE	Yes	Yes
Province FE	Yes	Yes
Year FE	Yes	Yes
N	33,043	33,043
Adj. R^2	0.206	0.215

Note. This table reports baseline results. Column (1) estimates Equation (3.1). Column (2) estimates Equation (3.2). Standard errors are clustered at the firm level and are reported in parentheses. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

Table 3.3: Alternative Explanations

	Dep. Var = *COMMON*	Dep. Var = *ESG*	
		Environmental Regulations	
	Reverse Causality	Strong	Weak
	(1)	(2)	(3)
TREAT×POST	**-0.028***		
	(0.016)		
COMMON		-1.588	-1.358
		(1.036)	(1.071)
COMMON×ATTN		**0.461****	**0.465****
		(0.199)	**(0.206)**
ATTN		-0.076	-0.034
		(0.102)	(0.110)
Controls	Yes	Yes	Yes
Firm FE	Yes	No	No
Industry FE	No	Yes	Yes
Province FE	No	Yes	Yes
Year FE	Yes	Yes	Yes
N	33,043	15,507	17,536
Adj. R^2	0.709	0.213	0.218
***p*-value of Chow Test**		**0.897**	

Note. This table tests alternative explanations: reverse causality in Column (1) and environmental regulations in Columns (2) and (3). Column (1) presents the estimates of Equation (3.3). Columns (2) and (3) report the estimates of Equation (3.2) across subsamples defined by the intensity of local environmental regulations. Firms headquartered in cities with regulation intensity above (below) the sample median form the strong (weak) regulation subsample. Standard errors are clustered at the firm level and are reported in parentheses. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

Table 3.4: Information Transmission Channel

	Dependent Variable		
	ESG	*DIS*	*ESG*
	(1)	(2)	(3)
COMMON	**0.831*** **	**0.432** **	**0.821*** **
	(0.206)	**(0.196)**	**(0.206)**
DIS			**0.024** **
			(0.011)
SIZE	1.271***	0.083	1.269***
	(0.070)	(0.060)	(0.070)
AGE	-0.729***	0.135*	-0.732***
	(0.076)	(0.072)	(0.076)
LEV	-4.082***	-0.513*	-4.070***
	(0.348)	(0.310)	(0.348)
ROA	9.892***	0.960	9.870***
	(0.870)	(0.708)	(0.870)
TOBINQ	-0.150***	-0.029	-0.149***
	(0.040)	(0.040)	(0.040)
BIG4	0.404*	0.652***	0.388*
	(0.227)	(0.238)	(0.226)
CASH	-0.070	-1.170**	-0.042
	(0.598)	(0.506)	(0.597)
MEXP	-2.534***	1.329	-2.565***
	(0.884)	(0.829)	(0.882)
MSHARE	1.999***	0.369	1.990***
	(0.375)	(0.340)	(0.375)
INST	0.961***	-0.088	0.963***
	(0.312)	(0.289)	(0.312)
HHI	0.040	-0.548	0.053
	(0.980)	(1.115)	(0.980)
Constant	47.628***	29.712***	46.928***
	(1.462)	(1.231)	(1.493)
Industry FE	Yes	Yes	Yes
Province FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
N	25,839	25,839	25,839
Adj. R^2	0.194	0.774	0.194
Difference in Coefficients ((3)-(1)) = -0.010* **			
Bootstrapped *p*-value = 0.009			

Note. This table presents the test results of the information transmission channel. The difference in coefficients is calculated as the coefficient on *COMMON* when controlling for *DIS* (Column (3)) minus the coefficient on *COMMON* without controlling for *DIS* (Column (1)). Standard errors are clustered at the firm level and are reported in parentheses. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

Table 3.5: Governance Channel

	Dependent Variable		
	ESG	*INTERNAL*	*ESG*
	(1)	(2)	(3)
COMMON	**0.923*** **	**0.020** **	**0.885*** **
	(0.196)	**(0.009)**	**(0.193)**
INTERNAL			**1.872*** **
			(0.116)
SIZE	1.374***	0.030***	1.318***
	(0.065)	(0.003)	(0.064)
AGE	-0.790***	-0.027***	-0.739***
	(0.086)	(0.004)	(0.085)
LEV	-4.518***	-0.127***	-4.281***
	(0.326)	(0.021)	(0.318)
ROA	9.486***	1.388***	6.888***
	(0.794)	(0.072)	(0.790)
TOBINQ	-0.089**	-0.018***	-0.056
	(0.038)	(0.003)	(0.038)
BIG4	0.429*	0.018*	0.395*
	(0.219)	(0.009)	(0.217)
CASH	-0.279	-0.123***	-0.048
	(0.568)	(0.041)	(0.561)
MEXP	-3.544***	-0.291***	-3.000***
	(0.806)	(0.058)	(0.796)
MSHARE	1.661***	0.025	1.614***
	(0.391)	(0.017)	(0.388)
INST	0.674**	0.002	0.670**
	(0.303)	(0.016)	(0.299)
HHI	0.362	0.108**	0.161
	(0.777)	(0.045)	(0.763)
Constant	45.708***	1.408***	43.071***
	(1.352)	(0.066)	(1.334)
Industry FE	Yes	Yes	Yes
Province FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
N	30,673	30,673	30,673
Adj. R^2	0.205	0.127	0.218
Difference in Coefficients ((3)-(1)) = -0.038* **			
Bootstrapped *p*-value = 0.007			

Note. This table presents the test results of the governance channel. The difference in coefficients is calculated as the coefficient on *COMMON* when controlling for *DIS* (Column (3)) minus the coefficient on *COMMON* without controlling for *DIS* (Column (1)). Standard errors are clustered at the firm level and are reported in parentheses. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

Table 3.6: Dominant Channel under High Public Attention

	Information Transmission		Governance	
Dep. Var = *ESG*	(1)	(2)	(3)	(4)
COMMON	**1.179*****	**1.165*****	**1.150*****	**1.123*****
	(0.285)	**(0.285)**	**(0.277)**	**(0.275)**
DIS		**0.028**		
		(0.017)		
INTERNAL				**1.939*****
				(0.173)
SIZE	1.343***	1.342***	1.452***	1.395***
	(0.085)	(0.085)	(0.085)	(0.084)
AGE	-0.594***	-0.597***	-0.642***	-0.590***
	(0.097)	(0.097)	(0.120)	(0.119)
LEV	-4.086***	-4.069***	-4.288***	-4.082***
	(0.451)	(0.450)	(0.449)	(0.443)
ROA	10.473***	10.409***	10.182***	7.685***
	(1.135)	(1.136)	(1.137)	(1.134)
TOBINQ	-0.125**	-0.124**	-0.108**	-0.084
	(0.055)	(0.055)	(0.054)	(0.054)
BIG4	0.475*	0.461*	0.425*	0.363
	(0.253)	(0.252)	(0.251)	(0.250)
CASH	-1.217	-1.216	-1.080	-0.767
	(0.808)	(0.808)	(0.809)	(0.810)
MFEE	-1.695*	-1.749*	-1.951**	-1.488
	(1.015)	(1.014)	(0.972)	(0.965)
MSHARE	2.277***	2.269***	2.087***	2.080***
	(0.511)	(0.510)	(0.558)	(0.554)
INST	1.678***	1.685***	1.620***	1.630***
	(0.426)	(0.426)	(0.441)	(0.437)
HHI	0.170	0.243	-0.176	-0.456
	(1.323)	(1.321)	(1.117)	(1.101)
Constant	45.405***	44.513***	43.205***	40.471***
	(1.780)	(1.824)	(1.765)	(1.752)
Indsutry FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Province FE	Yes	Yes	Yes	Yes
N	12,197	12,197	12,560	12,560
Adj. R^2	0.234	0.234	0.249	0.260
Difference in Coefficients	**-0.014****		**-0.027**	
Bootstrapped *p*-value	**0.044**		**0.197**	

Note. This table presents the results of channel analysis under the high level of public attention to environmental issues. Firms headquartered in cities with public environmental attention above the sample mean are categorized into the high-public-attention group. Columns (1) and (2) test the information transmission channel, and the mediator variable is *DIS*. Columns (3) and (4) test the governance channel, and the mediator variable is *INTERNAL*. For each channel, the difference in coefficients is calculated as the coefficient on *COMMON* when controlling for the mediator minus the coefficient on *COMMON* without controlling for the mediator. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

Table 3.7: Dominant Channel under Low Public Attention

	Information Transmission		Governance	
Dep. Var = *ESG*	(1)	(2)	(3)	(4)
COMMON	**0.478***	**0.472***	**0.660*****	**0.621****
	(0.267)	**(0.267)**	**(0.253)**	**(0.251)**
DIS		**0.020**		
		(0.014)		
INTERNAL				**1.815*****
				(0.147)
SIZE	1.157***	1.159***	1.278***	1.228***
	(0.106)	(0.106)	(0.089)	(0.087)
AGE	-0.818***	-0.822***	-0.833***	-0.785***
	(0.109)	(0.109)	(0.113)	(0.111)
LEV	-4.020***	-4.019***	-4.535***	-4.292***
	(0.499)	(0.500)	(0.436)	(0.423)
ROA	9.897***	9.892***	9.475***	6.812***
	(1.271)	(1.271)	(1.065)	(1.061)
TOBINQ	-0.206***	-0.205***	-0.090*	-0.049
	(0.056)	(0.056)	(0.052)	(0.050)
BIG4	0.144	0.135	0.227	0.244
	(0.381)	(0.381)	(0.343)	(0.338)
CASH	0.980	1.013	0.453	0.627
	(0.837)	(0.837)	(0.754)	(0.740)
MFEE	-3.673**	-3.652**	-5.031***	-4.361***
	(1.477)	(1.478)	(1.211)	(1.193)
MSHARE	1.659***	1.662***	1.479***	1.420***
	(0.514)	(0.514)	(0.504)	(0.497)
INST	0.291	0.292	0.061	0.055
	(0.424)	(0.424)	(0.391)	(0.384)
HHI	0.617	0.571	0.755	0.627
	(1.467)	(1.470)	(1.029)	(1.008)
Constant	50.655***	50.009***	48.187***	45.535***
	(2.201)	(2.265)	(1.869)	(1.841)
Indsutry FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Province FE	Yes	Yes	Yes	Yes
N	13,637	13,637	18,114	18,114
Adj. R^2	0.168	0.168	0.182	0.195
Difference in Coefficients	**-0.006**		**-0.039****	
Bootstrapped *p*-value	**0.136**		**0.041**	

Note. This table presents the results of channel analysis under the low level of public attention to environmental issues. Firms headquartered in cities with public environmental attention below the sample mean are categorized into the high-public-attention group. Columns (1) and (2) test the information transmission channel, and the mediator variable is *DIS*. Columns (3) and (4) test the governance channel, and the mediator variable is *INTERNAL*. For each channel, the difference in coefficients is calculated as the coefficient on *COMMON* when controlling for the mediator minus the coefficient on *COMMON* without controlling for the mediator. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

3.6 Appendix

Table A3.1: Variable Definitions

Variable name	Variabe definition
Independent Variables	
COMMON	Natural logarithm of one plus the average number of common owners of firm i across the four quarters in year t. A common owner is defined as a shareholder who hold at least 5% of the firm's outstanding shares as well as at least 5% of the outstanding shares in one or more other firms within the same industry.
COMSHARE	Natural logarithm of one plus the average number of common owners of firm i across the four quarters in year t. A common owner is defined as a shareholder who hold at least 5% of the firm's outstanding shares as well as at least 5% of the outstanding shares in one or more other firms within the same industry.
COMMON10	Natural logarithm of one plus the average number of common owners of firm i across the four quarters in year t. A common owner is defined as a shareholder who hold at least 10% of the firm's outstanding shares as well as at least 10% of the outstanding shares in one or more other firms within the same industry.
Dependent Variables	
ESG	Natural logarithm of firm i's overall ESG performance score from SINO in year t. The SINO ESG score is on a scale of 0 to 100.
WIND	Natural logarithm of firm i's overall ESG performance score from WIND in year t. The WIND ESG score is on a scale of 0 to 10.
Moderating Variables	
ATTN	Natural logarithm of annual total Baidu Search Index of the keyword "environmental pollution" in the city where firm i is headquartered in year t
ATTN_H	Natural logarithm of annual total Baidu Search Index of the keyword "haze" in the city where firm i is headquartered in year t
Mediating Variables	
DIS	Natural logarithm of average Bloomberg ESG disclosure score of commonly held peers firms operating in the same industry as firm i in year t
INTERNAL	Dibo Internal Control Index of firm i in year t
Control Variables	
SIZE	Natural logarithm of total book value of assets
AGE	Natural logarithm of the number of years since the firm's establishment
LEV	Total book value of debt/Total book value of assets
ROA	Return on assets
TOBINQ	Tobin's Q
BIG4	Indicator variable that equals 1 if the firm is audited by a Big Four accounting firm in a given year, and 0 otherwise
CASH	Net cash flow from operating activities/Total assets
MEXP	Management expenses/Operating income
MSHARE	Management shareholding/Total equity
INST	The proportion of institutional investors' shareholdings
HHI	Herfindahl-Hirschman Index

Table A3.2: Robustness: Alternative Measure of Common Ownership

Dep. Var = ***ESG***	(1)	(2)	(3)	(4)
COMSHARE	**1.711*** **	**-2.900**		
	(0.460)	**(1.954)**		
COMSHARE×ATTN		**0.888** **		
		(0.365)		
CMMON10			**1.101*** **	**-0.775**
			(0.247)	**(1.086)**
COMMON10×ATTN				**0.368** *
				(0.207)
ATTN		-0.045		-0.044
		(0.087)		(0.087)
SIZE	1.344***	1.343***	1.338***	1.337***
	(0.062)	(0.062)	(0.062)	(0.061)
AGE	-0.810***	-0.774***	-0.817***	-0.780***
	(0.068)	(0.068)	(0.068)	(0.068)
LEV	-4.390***	-4.361***	-4.387***	-4.358***
	(0.312)	(0.311)	(0.311)	(0.310)
ROA	9.614***	9.526***	9.636***	9.560***
	(0.761)	(0.754)	(0.761)	(0.753)
TOBINQ	-0.095**	-0.096***	-0.094**	-0.095**
	(0.037)	(0.037)	(0.037)	(0.037)
BIG4	0.393*	0.373*	0.373*	0.354*
	(0.210)	(0.213)	(0.209)	(0.212)
CASH	-0.171	-0.389	-0.171	-0.389
	(0.536)	(0.529)	(0.535)	(0.529)
MEXP	-3.377***	-2.922***	-3.414***	-2.962***
	(0.769)	(0.762)	(0.768)	(0.760)
MSHARE	1.609***	1.545***	1.625***	1.557***
	(0.355)	(0.353)	(0.354)	(0.353)
INST	0.585**	0.569**	0.592**	0.571**
	(0.287)	(0.284)	(0.286)	(0.283)
HHI	0.497	0.469	0.472	0.445
	(0.732)	(0.738)	(0.730)	(0.737)
Constant	46.443***	46.590***	46.567***	46.724***
	(1.290)	(1.349)	(1.288)	(1.349)
Industry FE	Yes	Yes	Yes	Yes
Province FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
N	33,043	33,043	33,043	33,043
Adj. R^2	0.205	0.215	0.205	0.215

Note. This table repeats the baseline analysis with alternative measures of firm common ownership. Columns (1) and (2) estimate Equations (3.1) and (3.2), respectively, with *COMMON* and *COMMON×ATTN* replaced by *COMSHARE* and *COMSHARE×ATTN*. Columns (3) and (4) estimate Equations (3.1) and (3.2), respectively, with *COMMON* and *COMMON×ATTN* replaced by *COMMON10* and *COMMON10×ATTN*. All variables are defined in Appendix Table A3.1. Standard errors are clustered at the firm level and are reported in parentheses. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

Table A3.3: Robustness: Alternative Measure of Sustainability Performance

Dep. Var = ***WIND***	(1)	(2)
COMMON	**0.347***** **(0.040)**	**0.004** **(0.009)**
COMMON×ATTN		**0.192***** **(0.001)**
ATTN		-1.128*** (0.013)
SIZE	0.148*** (0.012)	0.019*** (0.004)
AGE	-0.097*** (0.013)	-0.013** (0.006)
LEV	-0.367*** (0.058)	-0.029** (0.013)
ROA	0.267** (0.130)	-0.037** (0.018)
TOBINQ	0.024*** (0.007)	0.004*** (0.001)
BIG4	0.258*** (0.046)	0.004 (0.014)
CASH	0.208* (0.106)	0.005 (0.014)
MEXP	0.380** (0.175)	-0.007 (0.031)
MSHARE	0.175*** (0.065)	0.001 (0.013)
INST	0.107** (0.053)	-0.004 (0.013)
HHI	0.128 (0.321)	-0.129*** (0.043)
Constant	2.836*** (0.242)	5.506*** (0.114)
Industry FE	Yes	Yes
Province FE	Yes	Yes
Year FE	Yes	Yes
N	17,275	17,275
Adj. R^2	0.222	0.989

Note. This table repeats the baseline analysis with an alternative measure of firm sustainability performance. Columns (1) and (2) estimate Equations (3.1) and (3.2), respectively, with ESG replaced by *WIND*. All variables are defined in Appendix Table A3.1. Standard errors are clustered at the firm level and are reported in parentheses. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

Table A3.4: Robustness: Alternative Measure of Public Attention to Environmental Issues

Dep. Var = *ESG*	(1)	(2)
COMMON	**0.913***** **(0.188)**	**-0.718** **(0.688)**
COMMON×ATTN_H		**0.297**** **(0.128)**
ATTN_H		0.173 (0.154)
SIZE	1.325*** (0.062)	1.162*** (0.096)
AGE	-0.809*** (0.068)	-1.027*** (0.108)
LEV	-4.372*** (0.311)	-4.058*** (0.325)
ROA	9.590*** (0.761)	3.232*** (0.680)
TOBINQ	-0.104*** (0.037)	0.006 (0.034)
BIG4	0.372* (0.210)	0.159 (0.275)
CASH	-0.180 (0.536)	-1.883*** (0.440)
MEXP	-3.387*** (0.769)	-4.456*** (0.746)
MSHARE	1.613*** (0.354)	2.319*** (0.430)
INST	0.592** (0.286)	-0.108 (0.349)
HHI	0.513 (0.732)	-0.331 (0.620)
Constant	46.829*** (1.296)	50.396*** (2.201)
Industry FE	Yes	Yes
Province FE	Yes	Yes
Year FE	Yes	Yes
N	33,043	33,043
Adj. R^2	0.206	0.514

Note. This table repeats the baseline analysis with an alternative measure of public attention to environmental issues. Columns (1) and (2) estimate Equations (3.1) and (3.2), respectively, with *COMMON×ATTN* and *ATTN* replaced by *COMMON×ATTN_H* and *ATTN_H*. All variables are defined in Appendix Table A3.1. Standard errors are clustered at the firm level and are reported in parentheses. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

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