

The Effect of Consumer Bankruptcy Generosity on Discretionary Expenditure: Evidence from Individual Campaign Contribution

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Abstract

We study whether expanding personal bankruptcy protection changes households' discretionary behavior. Using itemized Federal Election Commission contribution records, we exploit staggered state-level increases in homestead exemption limits in a stacked difference-in-differences design. We interpret these reforms as shocks to the generosity of balance-sheet insurance that reduce households' wealth at risk in adverse states by protecting home equity from unsecured creditors. We find that exemption increases raise individual campaign contributions, primarily through larger contribution amounts, with effects concentrated in locations where the new limits more fully cover households' home equity. Consistent with an insurance mechanism, responses are stronger in areas with greater medical expenditure risk and weaker where household credit is tighter. Overall, the results imply that debtor-protection reforms can affect not only default outcomes but also discretionary, money-intensive political participation.

1 Introduction

Personal bankruptcy is a core piece of the U.S. household finance system with over hundreds of thousands of households file for bankruptcy protection each year. In the twelve months ending September 2024, there were approximately 504,000 bankruptcy cases filed, up more than 16% from the previous year, with consumer cases accounting for the major part of filings¹. Beyond the raw number of filings, in consumer bankruptcy cases filed in 2024, debtors reported about \$85.9 billion in total liabilities, and roughly \$77.2 billion in net scheduled liabilities - a scheduled-based liability for potentially dischargeable debts ². Prior study also documents that American households receive more resources through the bankruptcy system than through all state unemployment insurance programs combined Lefgren et al. (2010). Economically, bankruptcy matters as it provides relief for households facing large idiosyncratic shocks by limiting repayments to the creditors in bad stats, operating as a social insurance Livshits et al. (2007).

Crucially, the insurance value of bankruptcy depends on state exemption generosity. Following Mahoney (2015), bankruptcy provides implicit insurance because it limits a household’s financial exposure in the bad state to the assets that can be seized in bankruptcy; exemption limits determine the size of that exposure by defining what is seizable. Homestead exemption is especially consequential because it governs whether home equity—often the largest asset on the household balance sheet—is protected, and therefore how much wealth remains at risk when shocks hit. This paper examines whether increases in homestead exemption limits affect household discretionary spending.

A growing literature studies the insurance properties of personal bankruptcy. Mahoney (2015) shows that the exemptions act as implicit health insurance, given that households living in regions with more generous exemptions tend to have lower private health insurance coverage. Pattison (2020) documents that debt defaults entails sizable consumption declines, but legal protections for debtors meaningfully mitigate these drops, consistent with the idea that debtor protections provide consumption insurance for bankruptcy filers. More recent work by Indarte (2023) highlights that insurance value of the bankruptcy is greater among households with higher liquidity constraints.

Despite the progress on the insurance value of personal bankruptcy, direct causal evidence on how increases in exemption generosity affect households’ spending, especially discretionary spending - remains limited. Grant (2010) finds that higher exemptions are associated with decreases in consumption

¹<https://www.uscourts.gov/data-news/judiciary-news/2024/11/07/bankruptcy-filings-rise-162-percent/>

²https://www.uscourts.gov/sites/default/files/document/bapcpa_alltables_dec2024.pdf

volatility and average consumption growth. Lehnert and Maki (2007), using state-level exemption heterogeneity, show that more generous exemptions are associated with greater consumption insurance - consumption is less sensitive to income shocks. More recent study by Pattison (2020), using PSID panel data, documents that household consumption falls about 6% in years of default, and the decline in consumption is larger for informal (non-bankruptcy) defaults than for formal bankruptcy. However, the author does not provide direct causal evidence on personal bankruptcy generosity on household consumptions. Additionally, Indarte (2023) shows that bankruptcy exemption provides sizable consumption-smoothing benefits for the marginal filers, inferred from the facts that fillings respond much more to liquidity fillers than to moral-hazard driving filers. Neither of these studies, however, estimates the direct causal response of household consumption, more specifically discretionary spending, to changes in exemption generosity induced by policy reforms.

We focus on household discretionary spending since insurance should affect the margin of spending that is most easily adjustable. As commitment spending (e.g. housing, utilities) is costly and difficult to adjust, households absorb shocks by changing non-essential, non-commitment spending Chetty and Szeidl (2007). Consistent with this idea, prior studies show that consumption declines following housing wealth losses concentrate in non-essential spending categories Mian and Sufi (2014), and monetary-policy shocks transmit mainly through mortgagors' non-essential spending Cloyne et al. (2020), highlighting that the adjustment to household shocks is disproportionately reflected in discretionary categories. Accordingly, if higher homestead exemptions reduce loss severity and precautionary saving needs, the effects should be most visible on discretionary margins.

We use individual campaign donation as a proxy for discretionary spending for three reasons. First, prior literature shows that individual political giving is highly resource-elastic and closely tied to household income and wealth, consistent with the idea that donations reflect discretionary spending capacity rather than subsistence needs (Ansolabehere et al., 2003; Barber et al., 2019). Second, individual political donation is a freely adjustable margin as it has no subsistence component, is typically made in small and timing-flexible increments, and can be set to zero at low cost ³.

Our study instead exploits state homestead exemption reforms in a stacked DID setting with LTV-based heterogeneity to estimate causal effects on discre-

³Transaction-level consumption data with national coverage, long time span, and consistent geographic identifiers are rarely available and typically come from proprietary samples with limited representativeness and changing coverage over time. In contrast, campaign contribution records are mandated, standardized, and geo-coded, and they span decades and all states, making them well-suited for studying policy-induced changes in a freely adjustable discretionary spending margin. We therefore use donation as a proxy for discretionary spending rather than as a comprehensive measure of household consumption.

tionary spending such as political donations and charitable donations. We use the staggered timing of homestead exemption increases across states as a source of exogenous variation in the insurance value of housing wealth, and compare changes in political donations in adopting states around their reform dates to changes in states that do not reform. With a comprehensive set of control variables, this design allows us to identify the effect of higher exemption generosity on discretionary consumption separately from contemporaneous movements in income, house prices, or local economic conditions. We collect data on time-varying changes in state homestead exemption limits from 2006 to 2017 and link these policy shocks to comprehensive data on itemized campaign contributions reported to the Federal Election Commission (FEC), aggregated at the donor-year level. This design allows us to test directly whether stronger statutory protection of household assets translates into greater donation behavior.

A central feature of our empirical strategy is to identify households by their exposure to exemption coverage. Households whose home equity is newly or fully covered by the revised exemption limits are the most affected by higher statutory protection (Gropp et al., 1997). To capture this variation in exposure, we classify households by loan-to-value (LTV) ratios at the ZIP-level, where LTV is an inverse proxy for home equity. This stratification allows us to test whether the effects of exemption reforms are concentrated among households most directly exposed to the change in downside protection.

Our findings are threefold. First, we find that higher personal bankruptcy exemptions increase political donations among households living in higher LTV regions. For households in low-equity (high-LTV) areas, an average increase in homestead personal bankruptcy exemption raises itemized giving by about 5–6 percent per year. In real dollar terms, this corresponds to roughly \$65–\$90 more per donor-year. Given that average itemized contributions in our sample are about \$1,300–\$1,500 per year, these changes are economically meaningful and politically consequential. By contrast, households with substantial equity above the cap, who remain exposed even after reforms, do not adjust their contributions. These results confirm that the insurance value of exemptions, rather than changes in wealth per se, drives participation: when downside risk is reduced, less-affluent households can reallocate resources from precautionary saving toward discretionary political activities.

Second, we find that the effects of exemption on individual donations are stronger in ZIP codes with lower pre-reform insurance coverage, where households face greater medical expenditure risk. In these areas, an average increase in homestead exemption raises contributions by 12.7 percent in the highest-LTV quintile and by 9.3 percent in the second quintile, a relative \$120 to \$187 increase in individual annual donations among households living in lower home equity regions. The economic magnitudes are more than double those in the baseline estimates. By contrast, no effects emerge in higher-insurance ZIPs. The findings are consistent with the interpretation that personal bankruptcy

exemptions substitute for incomplete insurance market (Mahoney, 2015).

Third, we examine heterogeneity by local credit constraints. Following the incomplete-markets literature (e.g. (Aiyagari, 1994; Zeldes, 1989)), households with tighter borrowing limits are less able to smooth shocks and therefore maintain higher precautionary buffers. Consistent with this prediction, we find no effect of raising exemptions among households living in ZIP codes with high pre-reform mortgage loan denial rates. By contrast, in areas with easier credit access, donations rose sharply by 12.9% in the highest-LTV quintile and 8.5% in the second quintile, corresponding to a \$115 to \$191 increase per donor-year. This evidence suggests households with higher credit constraints continue to hold a higher precautionary savings and re-allocate less resources into political donations, whereas those with easier credit access can reallocate resources into political donations once statutory protections lower their downside risk.

Our study first contributes to the personal bankruptcy exemption literature by providing direct causal evidence that increasing in homestead exemption generosity raise discretionary spending, as reflected in higher spending on political and charitable donations. By showing that reducing households' downside risk increases these donation flows, our baseline estimates document that stronger bankruptcy protections lead to greater discretionary spending. Our channel analyses further reveal that this effect operates through the relaxation of precautionary saving motives - donations rise most in areas with higher medical expenditure risk, where exemptions substitute for missing insurance, and in regions with easier credit access, where households can more readily reallocate freed resources. These results highlight that financial policies which insure key household assets—such as homestead exemptions in bankruptcy—can broaden political participation not by raising income or wealth, but by stabilizing families against adverse shocks. In doing so, such policies lower the need for large precautionary buffers and enable less-affluent households to engage more actively in money-intensive politics, thereby narrowing the participation gap between wealthy and middle-income citizens.

Our study also contributes to the debate of political participation by empirically testing whether relaxing households' precautionary saving motives stimulates political participation on the money-intensive margin of campaign donations. Previous research shows that political participation, especially individual campaign contribution, is inherently costly and are highly associated with the resources of the household. However, raising household resources might not always guaranteed political participation. Previous study (e.g. Broockman et al. (2024); Brännlund et al. (2024)) show that cash windfall do not always guarantee higher political participation among those who received the cash windfall. Additionally, previous literature shows factors along with financial resources that are also matter for political participation. For example, Brooks (2014) shows that lack of insurance protecting income losses and physical risks are associated with lower participation rates in Brazil. We test a different channel to

see whether reducing household’s uninsurable risks due to incomplete market encourages household participation in politics, especially in campaign finance, which is a costly form of political participation and are highly tie to the resources of the households. Additionally, Yorgason (2025) finds that reducing the cost of campaign donations by giving households in Washington vouchers increase donation participation among areas with higher prior voting and higher income. We examine reforms to personal bankruptcy law that raised homestead exemption limits, thereby increasing the share of assets protected from creditors in the event of bankruptcy without directly affecting the cash flows of the households on campaign donations. We find that higher exemptions increase campaign contributions among households whose home equity is newly or fully covered by the revised limits. In contrast to effects documented by Yorgason (2025), who shows that reducing the donation costs have greater effects on households with higher income, we find that the effects of improving asset protections and reduce household uninsurable risks have stronger effects among households with higher LTV ratio and lower income, increasing the diversification of the donor pool.

2 Hypothesis Development

Households facing greater economic uncertainty tend to hold larger precautionary buffers to self-insure against future adverse shocks. In Carroll (1997) buffer-stock saving model, higher income or asset value uncertainty induces greater savings, whereas a reduction in risk encourages households to draw down reserves and increase discretionary spending. Empirical studies have also provide evidence for the theory. Lusardi (1998), Kazarosian (1997), and Carroll and Samwick (1998) find that greater income risk is associated with higher savings rates, while risk reductions lead to lower savings and higher consumption. Since political participation — particularly through campaign contributions — is a discretionary activity (Ansolabehere et al., 2003), higher precautionary saving crowds out the financial resources households can allocate to politics. Conversely, when risk declines and precautionary buffers are drawn down, households can channel freed resources toward political activities, including donations.

A concrete example of such a risk-reducing mechanism is the homestead exemption in Chapter 7 bankruptcy. Homestead exemption allows borrowers to keep home assets up to the ex-emption limits when individuals file for Chapter 7 bankruptcy; this prevents assets from being seized. The exemption provides households with an implicit high-deductible insurance: households are exposed to financial risk from any negative shocks up to the level of asset that can be seized in bankruptcy and insured against financial risk above this level (Mahoney, 2015). By capping the share of home equity creditors can seize in Chapter 7 bankruptcy, exemptions shield a portion of a household’s primary residence from liquidation. This protection functions as state-provided insur-

ance: in worst-case scenarios — such as job loss, divorce, or large medical bills — households can discharge unsecured debts while retaining their home, preserving the single largest asset for many middle- and lower-wealth families. By reducing the scope of potential losses, higher exemption limits lower the incentive to hold large buffer stocks or to self-insure themselves (e.g.(Pavan, 2008; Mahoney, 2015)), releasing funds that can be redirected to discretionary spending, including political giving.

Moreover, Fay et al. (2002) shows that households with equity just at or slightly below the exemption limit receive the largest marginal benefit from a higher cap, because it sharply reduces the potential loss they would face in bankruptcy. By contrast, households far above the limit remain substantially exposed. Hence, the asset protection effect of personal bankruptcy exemption should be stronger among households who have home equity equal to or slightly lower than the exemption limits.

Together, these insights suggest that raising the homestead exemption limit reduces uninsured downside risk for households whose home equity is largely or fully covered by the new limit. This risk reduction should allow them to reallocate resources from precautionary savings toward discretionary activities, including political contributions.

H1: When a state raises its homestead exemption limit, campaign donations increase among households whose equity is largely or fully protected by the new limit.

Households treat medical expenses as a salient source of downside risk, and they respond by accumulating precautionary buffers. Canonical theory and evidence show that higher medical-expenditure risk raises saving and slows decumulation (Hubbard et al., 1995; Starr-McCluer, 1996; De Nardi et al., 2010; Kopecky and Koreshkova, 2014). Cross-country and life-cycle evidence further indicates that differences in medical-expense risk help explain consumption and wealth paths at older ages (Banks et al., 2019). Conversely, when medical risk is insured, households rely less on self-insurance through buffer stocks.

Medical shocks also push households toward financial distress and bankruptcy, especially when uninsured (Dobkin et al., 2018), while expansions of formal insurance reduce bankruptcy incidence (Gross and Notowidigdo, 2011). Homestead exemptions operate as implicit insurance by limiting the home equity that can be seized in Chapter 7, thereby capping wealth losses from large medical bills and related shocks (Mahoney, 2015). If exemptions substitute for private self-insurance, their effect on discretionary outlays—such as campaign giving—should be stronger precisely where ex-ante medical risk is higher (i.e., where health-insurance coverage is lower).

H2: The positive effect of a higher homestead exemption on campaign dona-

tions is larger in regions with lower health-insurance coverage (indicating higher medical-expenditure risk).

Economic theory predicts that when households face tighter borrowing limits, they increase self-insurance through higher precautionary or buffer-stock saving, thereby reducing current consumption. In incomplete-markets models with idiosyncratic risk, borrowing constraints raise desired asset holdings because households cannot rely on credit to smooth adverse income shocks (Aiyagari, 1994). Cross-country evidence from Jappelli and Pagano (1994) similarly shows that liquidity constraints are associated with higher saving rates and a stronger growth–saving relationship. In modern heterogeneous-agent frameworks, credit crunches mechanically induce greater precautionary saving and lower aggregate demand (Guerrieri and Lorenzoni, 2017).

Empirical evidence corroborates these mechanisms. Zeldes (1989) finds that liquidity-constrained households deviate from the permanent-income hypothesis, consuming less in the short run to maintain precautionary reserves. Gross and Souleles (2002) show that exogenous increases in credit limits trigger immediate increases in borrowing and spending, implying the mirror effect: when credit is tight, discretionary spending contracts and precautionary saving rises. These patterns suggest that local credit conditions can shape the extent to which households adjust their spending in response to changes in insured risk.

In the context of homestead exemptions, prior work shows that stronger asset-protection rules in bankruptcy increase lenders’ loss-given-default, prompting credit tightening—particularly for households with lower assets (Gropp et al., 1997; Berkowitz and Hynes, 1999; Pavan, 2008). If pre-reform credit availability is already low, households may hold larger buffer stocks and make less discretionary expenditures even after a reform reduces uninsured downside risk. Conversely, in high-credit environments, households tend to hold smaller buffers, so a reduction in risk - such as that from a higher homestead exemption - can free more liquidity resources for discretionary uses, including political contributions.

H3: The positive effect of a higher homestead exemption on campaign donations is larger in areas with greater pre-reform credit availability (lower mortgage-denial rates) and smaller in areas with tighter credit markets.

3 Institutional Setting

3.1 *Personal Bankruptcy Law*

When a household files for bankruptcy in the United States, an automatic stay is imposed that immediately halts collection efforts by creditors. This stay

stops wage garnishments, foreclosures, repossessions, and lawsuits, giving households temporary protection while the court supervises the resolution of debts.

There are two main forms of personal bankruptcy: Chapter 7 and Chapter 13. Chapter 7 is a liquidation procedure in which a trustee sells the debtor’s non-exempt assets and distributes the proceeds to creditors. Most unsecured debts, such as credit card balances and medical bills, are discharged at the end of the process, and debtors are allowed to keep all income earned after filing. Chapter 13 is a reorganization procedure in which debtors keep their assets but must commit to a repayment plan lasting three to five years. Under this plan, a portion of future income is used to repay creditors, and remaining unsecured debts are discharged once the plan is completed.

The 2005 Bankruptcy Abuse Prevention and Consumer Protection Act (BAPCPA) introduced several restrictions designed to limit strategic filings. The reform established a means test that prevents higher-income households from filing under Chapter 7 and directs them to Chapter 13 instead. In practice, most households file under Chapter 7 because it provides a faster discharge of debts while preserving future income. Roughly two-thirds of personal bankruptcy cases today are still filed under Chapter 7, while Chapter 13 is used more often by households with steady income who want to prevent foreclosure or restructure secured debts ⁴.

3.2 *Statutory Homestead Exemption*

A central feature of the U.S. personal bankruptcy system is the set of exemptions that determine which assets debtors can keep when they file. The purpose is to ensure that debtors retain a minimum level of property needed for basic living and the possibility of economic recovery after bankruptcy. Common exemptions include key households’ assets and home equity up to a certain value.

The homestead exemption is the most important exemption— as it protects the household’s primary residence, often the single largest asset—and has the most variations across states and over time (Cole et al., 2025; Indarte, 2023; Auclert et al., 2019). Under the homestead exemption, if a household’s home equity falls below the exemption limit, the home cannot be seized in Chapter 7. Any equity above the exemption limit, however, remains exposed to creditors. In this way, exemption rules effectively determine how much of a household’s wealth is protected in bankruptcy. Exemptions are set primarily at the state level, and the limits vary widely across states and over time. Some states offer relatively modest protections—such as a few thousand dollars of home equity—while others, including Texas and Florida, provide unlimited homestead

⁴<https://www.fool.com/money/research/personal-bankruptcy-statistics/>

protection. Federal law provides a set of default exemptions, but most states have opted out of the federal scheme, requiring debtors to use their state’s rules.

This variation across states creates large differences in the amount of wealth households can shield when they file for bankruptcy. Exemption levels have also changed over time as states revise their statutes, either by raising nominal exemption limits or adjusting them to account for housing market conditions. These reforms create plausibly exogenous changes in the extent to which households’ assets are protected, making exemptions an important institutional feature for understanding household behavior in the face of financial shocks. Table 1 provides a summary of the changes of homestead exemption limits post-BACPA regulation from 2006 to 2017. Within the time window, several jurisdictions provide unlimited protection, while limited-exemption states range from low five-figure limits to very high caps in a few cases. Within this window, states enacted a series of dated statutory increases in exemption limits. Most reforms are modest to mid-sized, consistent with incremental updates or cost-of-living catch-up. A smaller set undertook large, discrete jumps: New York in 2009 by about \$153,000, Massachusetts in 2010 by \$64,000, Rhode Island in 2012 by \$200,000. The timing clusters around the Great Recession and early recovery years, with relatively few changes after 2014; the last reform in the table occurs in 2017. California has two forms of homestead exemption, and we keep the one most available for all courts and with the highest exemption limits. These dated changes are the source of our event-time variation.

[Insert Table 1 about here]

We use the statewide exemption series assembled in Indarte (2023), which records homestead and wildcard exemptions for all fifty states, the District of Columbia, and the federal schedule from 2000–2021, with statutory sources and effective dates. Homestead exemptions protect equity in a primary residence; wildcard exemptions can be applied to any property. Limits are set separately for single and joint filers, and some states permit debtors to elect the federal schedule instead of the state schedule.

Our identification focuses on homestead reforms for two reasons. First, the homestead exemption has the largest exemption limits and insures the asset that constitutes the dominant component of household wealth. Second, homestead caps exhibit discrete, economically meaningful statutory jumps that are well suited to event-study and stacked difference-in-differences designs, whereas wildcard limits are generally smaller and change less sharply (Cole et al., 2025; Romeo and Sandler, 2023).

4 Identification Strategy

We follow previous literature (Cole et al., 2025; Romeo and Sandler, 2023) to exploit cross-sectional and time-varying state bankruptcy exemption limits as form of asset protection post-BAPCPA regulation from 2006 to 2017 to explain how asset protection laws induce political participation. The data of homestead exemption limits and dates that each state reformed its homestead exemption are collected using database provided by Indarte (2023). We follow Cerqueiro and Penas (2017) and Cole et al. (2025) by considering a state as “treated” if it reformed its homestead exemption limits the first time within our sample period.

One of the empirical challenges is that the homestead reforms are staggered across different States and over time. When treatment timing is staggered and effects are heterogeneous, standard DID and two-way fixed-effects model estimates combine a weighted average of cohort- and time-specific treatment effects and can sometimes load negative weights, biasing the direction of the true average treatment effect. In our setting, this issue arises because, under the standard staggered DID, some comparisons for later reforms implicitly benchmark later-adopting states against states that have already implemented a homestead reform, rather than against states that have not yet reformed, so the estimated coefficient mixes effects from different adoption cohorts and event times in a way that is difficult to interpret (Roth and Sant’Anna, 2023).

To resolve the issue, we implement a stacked DID estimator approach (Baker et al., 2022; Cengiz et al., 2019). Under the stacked DID approach, for each treatment (reform) cohort, we construct an event-time panel and restrict control units to never-treated units, excluding already-treated units. In this framework, identification comes only from comparisons in which the control group is never exposed to the reform over the sample period, so the estimated coefficients can be interpreted as average treatment effects for treated cohorts relative to a consistently untreated benchmark.

To implement the stacked DID approach, we restructure the data into a series of cohort-specific experiments. Each state is first assigned a treatment year, defined as the first year in which it reformed its homestead exemption limit, while states that never adopted a reform during the sample period are classified as never-treated. For each treatment year, we then construct a separate cohort consisting of the states that reformed in that year as the treated group, combined with all never-treated states as the control group. Within each cohort, we generate three key variables: an event-time measure defined as the calendar year minus the reform year, a post indicator equal to one in years at or after reform, and a treated indicator marking states that reformed in that cohort year. To ensure clean identification, we exclude states that implemented multiple reforms within the $[-3,+3]$ window, since overlapping policy changes would confound the estimation of treatment effects. Finally, we append all co-

horts into a single dataset, which allows us to estimate the baseline regression:

For each LTV quintile $q \in \{1, 2, 3, 4, 5\}$, we estimate:

$$Y_{i,t} = \beta(Treated_s \times Post_t) + F_{i,s,t} + \gamma_{s,c,d,z,t} + \epsilon_{i,t} \quad (1)$$

where $Y_{i,t}$ is the main dependent variable, which measures individual campaign donations. $Treated_s$ equals 1 if the individual resides in a state that reforms its homestead exemption in year t and 0 otherwise. $Post_t$ equals 1 after the homestead reform and 0 otherwise. To test whether effects are strongest where household home equity is largely or fully covered by the new exemption, we split the sample into five LTV quintiles—an inverse proxy for home equity—and estimate the reform effect within each quintile. Households in the top LTV quintiles (lowest home equity) are most likely to have their assets newly or fully covered by the revised limits, while those in the bottom quintiles (highest equity) remain partially exposed. We estimate the stacked DiD separately for each LTV quintile to capture this heterogeneity. We control for state, year, and individual-level fixed effects and include state, county, district, and ZIP code control variables. β is the key coefficient of interest, and it estimates the differential effect of each event (reform) on individual campaign donations, representing the causal effect of homestead reforms on individual campaign donations.

5 Data Description

5.1 Individual Campaign Contribution

We use OpenSecrets’ compilation of itemized individual contributions reported to the Federal Election Commission (FEC). Federal committees itemize a donor once that donor’s cumulative giving exceeds the disclosure threshold in a cycle or calendar year; gifts below the threshold are not individually identified and therefore are not in our microdata. For each itemized receipt, the file reports the donor’s city, state, and five-digit ZIP code; the amount and date of the contribution; the recipient committee’s name, party, and FEC identifier; and the donor’s reported employer and occupation. We use these fields as follows. ZIP codes and states are used to link donors to local economic conditions (ZIP-level income, housing, credit, and demographics) and to assign state-year treatment status under homestead-exemption reforms. Congressional district is assigned from ZIP–district crosswalks using contemporaneous boundaries. Employer and occupation are retained for heterogeneity and control specifications. We aggregate all itemized receipts to the individual \times calendar-year, netting

refunds and other negative entries so that the annual total reflects dollars actually retained by recipient committees (we exclude negative amounts—refunds to contributors—following Bouton et al. (2022)). We retain observations with valid state and ZIP codes and non-missing baseline controls; continuous covariates are winsorized at the one-percent tails.

We aggregate Federal Election Commission (FEC) records to each donor-year and define the outcome LnAmount as the natural log of total dollars contributed by individual i in calendar year t . We retain observations with valid residential ZIP code, county, state, and congressional district identifiers.

5.2 *Housing Value Data*

We supplement the contribution data with information on local housing markets and mortgage loans. We obtain housing values from Zillow’s Home Value Index (ZHVI) dataset which provides monthly estimates of the typical home value at the ZIP code level. The dataset is constructed by Zillow from its own property level valuation and summarizes the middle tier (roughly 35th to 65th percentile) home values to represent a market’s typical home value rather relying on the median sale price. We use the single-family home ZHVI dataset, aggregated to the calendar year.

5.3 *Home Mortgage Data*

HMDA requires many depository and non-depository mortgage lenders to report loan-level information on applications and origination for home purchase, refinance, and home improvement loans. The public release database includes, for each application, the action taken (e.g., originated, denied), loan amount, purpose, occupancy, lien status, applicant income and demographics, and detailed property location coded to the census tract. For our usage, we aggregate the loan amounts for originated loans, loan denial rates, and median debt-to-income ratio to the ZIP-year level and link to the residence of each individual donor from the FEC individual campaign contribution dataset. Our main proxy for home equity exposure is the loan-to-value ratio (LTV), defined as the ratio of mortgage amount originated to housing values (ZHVI) at the ZIP-year. The LTV ratio is an inverse proxy for home equity as higher LTV corresponds to smaller home equity, making households more sensitive to statutory changes in homestead exemption limits. In addition, we use the median debt-to-income ratio and the loan denial rate in each ZIP-year as controls for local credit tightness. Together, these two datasets allow us to quantify both sides of the household balance sheet—housing wealth and mortgage leverage—at fine geographic resolution.

5.4 *Local Characteristics*

We further incorporate a set of geographic economic and political indicators to capture local conditions beyond housing and credit markets. State-level real gross domestic product (GDP) is drawn from the U.S. Bureau of Economic Analysis (BEA), providing a benchmark for overall state economic activity. County-level labor market conditions are measured using unemployment rates from the Bureau of Labor Statistics’ Local Area Unemployment Statistics (LAUS) program. Household income at the ZIP level is obtained from the IRS Statistics of Income (SOI), which reports average adjusted gross income based on tax returns filed by residents of each ZIP code. Finally, congressional district political control is drawn from the MIT Election Data and Science Lab (MEDSL), which provides election results and officeholder partisanship across all congressional districts. Demographic and insurance-coverage covariates at the ZIP level are drawn from the American Community Survey (ACS) 5-year estimates, the Census Bureau’s recommended source for small geographies such as ZCTAs ⁵. The ACS provides rolling averages of household and population characteristics based on 60 months of survey responses, including age distribution, educational attainment, family size, and health-insurance coverage. We link the local characteristics back to the individual donation data and assign each donor to the party holding their district’s House seat in year t . Together, these datasets allow us to control for and exploit variation in local macroeconomic conditions, household income, political context, and demographic characteristics alongside our main measures of home equity and mortgage leverage.

5.5 *Summary Statistics*

Table 2 reports the summary statistics of the final sample set. Our unit of analysis is the individual \times year \times cohort. We require non-missing covariates for baseline controls. Continuous variables are winsorized at the 1% tails. All dollar amounts are converted to real terms based on the 2017 dollar value prior to logging. Appendix Table A1 provides definition for each of the variable used. The resulting panel contains 11,647,616 individual-year-cohort observations. On average, individuals contribute an amount corresponding to a log value (Ln Amount) of 6.49; the median is 6.31. In level terms, state-level economic productivity (Log GDP) averages 13.35, with a median of 13.52. Local individual average income at the ZIP level (Ln Income) averages 11.31, with

⁵Because ZIP-level ACS 5-year estimates are first available for 2008–2012, we assign pre-2012 sample years to this earliest release as a baseline proxy and match post-2012 sample years to the contemporaneous 5-year release (e.g., 2012 outcomes linked to the 2008–2012 ACS). This approach follows previous practice in ZIP-level empirical work (e.g., (Kim, 2022)).

a median of 11.35. The liquid assets over income ratio averages 0.05, with a median of 0.04. County-level unemployment rate has a mean of 6.27, with a median unemployment rate of 5.90%, and local debt-to-income ratio averages 2.03, with a median of 2.01. Local credit supply, measured by the mortgage loan denial rate, averages 0.19, with a median of 0.178; while housing value, measured by the log Zillow Home Value Index, averages 12.59, with a median of 12.54. Political and demographic characteristics are in line with national patterns. The indicator for residence in a Republican-held congressional district takes the value one for 59 percent of observations, and its median is one. The share of working-age population averages 0.63, with a median of 0.62. The share with a college degree averages 0.44, with a median of 0.44. Average family size is 3.02, with a median of 3.03.

[Insert Table 2 about here]

6 Main Results

Table 3 reports the results from the regressions effects of homestead reforms on individual campaign donations. The main explanatory variable is the natural logarithm of one plus total amount donated by individual donor per year. In our baseline results, LTV Q1 represents households living in ZIP codes with the highest LTV ratio, lowest home equity, while LTV Q5 represents households living in ZIP codes with the lowest LTV ratio. We control the state by cohort, year by cohort, and individual donor by cohort fixed effects. The treatment effect is concentrated in high-LTV (low-equity) regions, indicating that the homestead reform has significant effect only on households whose home equity is largely and fully covered by the new exemption limits. Based on the results in the first two columns, the average treated donors living in regions with highest LTV increase total donations by 5.8% per year while the average treated donors living in regions with the second highest LTV increase total donations by 4.9% percent per year. The diminishing effects of the homestead reform on individual campaign donations from high-LTV quintiles to low-LTV quintiles support our prediction: reforms that raise the exemption limits relax downside-loss exposure primarily for households most likely to have their equity largely or fully protected by the new limit, freeing liquid resources that can be reallocated to discretionary spending, including political contributions. In terms of real dollar value, compared to the average donation amount, this implies roughly \$86 increase donation per donor-year in LTV Q1 and \$64 increase in LTV Q2. By contrast, in low-LTV regions with more home equity above the exemption limits, the exemption offers limited additional protection, and the donation response is negligible. The test indicates that homestead reforms stimulate households to become more active in political participation, in terms of political donations. Taken together, the test supports our first hypothesis and indicate that expanding the homestead exemption limits increase political participation, in terms

of individual donations, for households whose home equity is largely or fully covered by the new exemption cap, with little or no effect on households whose equity remains above the limit.

[Insert Table 3 about here]

Taken together, the estimates in Table 3 provide several economic insights about how households respond to changes in homestead exemption generosity. First of all, the fact that the increase in donations is concentrated in the higher LTV quintiles indicates that the effect of the reform on discretionary spending is highly targeted as margin lies among households whose home equity is plausibly close or below the initial exemption limits, rather than among all households. This pattern is difficult to reconcile with a story in which reforms simply proxy for improved household economic conditions or general shift in political interest, since such forces would be expected to affect donors across the LTV distribution more uniformly. Instead, the pattern that we document here is consistent with the idea that the legal changes in exemption generosity matters where it meaningfully alters potential loss households would face under severe adverse shocks.

Secondly, the pattern of the estimates is consistent with the buffer-stock theory where changes in household downside risk can influence discretionary spending. In buffer-stock model, a reduction in uninsured downside risk lowers target wealth and lowers the incentive to hold greater precautionary buffer, leading to higher household consumption. Our main results are consistent with this prediction, where we see higher campaign donations for households whose home equity is largely or fully protected after a reform, and not for households for whom the change in the exemption is unlikely to alter their exposure to large losses. This alignment between the theoretical prediction and the empirical pattern is suggestive of a precautionary-saving channel, while still leaving room for other mechanisms that we explore in subsequent analysis.

6.1 *Robustness Checks*

6.1.1 *Parallel Trend Test*

Our stacked difference-in-differences estimates capture the causal effect of homestead reform on individual political donations. A key identifying assumption of our difference-in-differences design is that, absent the reform, the increase in campaign donations in treated states would have followed the same trajectory as in control states—the so-called parallel trends assumption. To assess the validity of this assumption, we test whether pre-treatment donation patterns evolve similarly across treated and untreated unit of observations prior to and after the reform. Specifically, we estimate an event-study specification that

allows us to trace the dynamics of donations relative to the reform year and examine whether the pre-reform leads are statistically indistinguishable from zero.

Table 4 reports the dynamic effect test for homestead exemption reforms on individual political donations across LTV quintiles. We omit the first year ($t = 3$) as the baseline and report the joint F-test p-values for the two pre-treatment coefficients at $t = -2$ and $t = -1$. In both the fourth and fifth LTV quintiles, these coefficients are individually insignificant, and the joint F-test cannot reject the null hypothesis that they are jointly zero, supporting the parallel-trends assumption. When the reform takes effect, we observe an immediate increase in individual donation amounts, followed by a further rise in the next year, where the effect peaks. This pattern is consistent with the buffer-stock saving theory and provides additional support to our main mechanism which indicates that households quickly relaxing some precautionary savings upon the introduction of the homestead exemption, and then gradually converging to a new buffer-stock target rather than responding to a one-off transitory income shock.

[Insert Table 4 about here]

6.1.2 *Excluding States by Economic Conditions*

Another concern in the baseline design is that very large economies may disproportionately shape national patterns of political participation and campaign finance, potentially biasing estimates of reform effects. Political science research has shown that campaign contributions are highly concentrated in wealthy states and metropolitan areas, where donor networks and mobilization infrastructures are most entrenched. For example, Gimpel et al. (2006) document that political giving is spatially clustered, with metropolitan areas in high-income states serving as disproportionate sources of contributions. Bonica (2014) similarly demonstrates that donor activity is skewed toward regions with higher income concentration, creating systematic imbalances in the geography of campaign finance. Relatedly, Brown et al. (1995) show that large states dominate the supply of campaign funds to national candidates, while Powell et al. (2003) emphasize how institutionalized fundraising networks in economically stronger states enhance donor participation. These findings suggest that wealthier states contribute not only a substantial share of aggregate donations but also shape the organization of campaign finance in ways that may not generalize to the rest of the country. To mitigate the concern that our results are disproportionately driven by such states, we re-estimate our models excluding the ten largest economies (measured by post Great Recession GDP and income per capita states) and assess whether the treatment effects persist in the broader sample.

The results presented in Tables 5 provide evidence that the main findings are

not disproportionately driven by the largest state economies. Panel A of Table 10 presents the results excluding the top 10 GDP states with a reference year in 2012 post the Great Recession, while panel B of Table 10 presents the results excluding the top 10 income per capita states with a reference year in 2012. After excluding the ten highest-GDP and per capita states, the estimated treatment effects remain positive, statistically significant, and concentrated among higher LTV quintiles, while for lower LTV quintiles the estimates are close to zero and statistically indistinguishable from zero. This attenuation pattern across quintiles is fully consistent with the theoretical mechanism: donors in high-equity areas gain little from expanded exemptions and therefore show no systematic response in political contributions.

Importantly, the persistence of strong and statistically significant results in Q1 and Q2, even after removing the politically and economically dominant states, suggests that the observed effects are not artifacts of idiosyncratic dynamics in large economies but reflect a broad-based behavioral response across the wider set of states. In other words, the findings demonstrate robustness to concern that the baseline estimates are mechanically driven by donors in a handful of mega-states with disproportionately large political fundraising networks.

[Insert Table 5 about here]

7 Mechanisms and Heterogeneity Tests

7.1 Channel Analysis

7.1.1 Insurance coverage heterogeneity

A large body of work shows that medical expenditure risk is a major driver of household precautionary saving. When households face greater exposure to uninsured medical shocks, they accumulate larger buffer stocks and de-cumulate wealth more slowly (Hubbard et al., 1995; De Nardi et al., 2010; Kopecky and Koreshkova, 2014; Banks et al., 2019). Conversely, when this risk is insured, households rely less on self-insurance. Relatedly, medical shocks are a common trigger of financial distress and bankruptcy, particularly among the uninsured (Dobkin et al., 2018), while expansions of health insurance lower bankruptcy filings (Gross and Notowidigdo, 2011).

We build on this literature to test whether the donation response we documented operates through a reduction in medical-expenditure risk. Specifically, homestead exemptions function as implicit insurance by capping the home equity that can be seized in medical-debt-induced bankruptcy (Mahoney, 2015). If reforms relax precautionary saving needs, the effect should be strongest in ZIP codes with lower health-insurance coverage, where households face higher

ex-ante medical risk. In those areas, we expect larger post-reform increases in campaign donations relative to high-coverage ZIPs.

To test the medical-expenditure risk channel, we examine heterogeneity in donation responses across areas with differing pre-reform levels of health insurance coverage. Since children and elderly households are disproportionately covered by publicly provided programs such as Medicaid and Medicare, we focus on insurance rates among the working-age population, which provides a sharper proxy for households' exposure to uninsured medical risk. We classify ZIP codes into high and low insurance coverage groups based on the pre-reform median coverage level. The heterogeneity test aligns with our second prediction that if homestead exemptions substitute for private self-insurance, they should have stronger effects where medical risk is higher (low insurance coverage), allowing households with protected home equity to reallocate resources from precautionary buffers into political donations. The absence of significant effects in high-coverage ZIPs supports the interpretation that the reform's donation response operates through the reduction of uninsured downside medical risk rather than through unrelated channels. The results are also consistent with Mahoney (2015) showing that homestead exemption have a crowd out effect on private insurance.

A potential concern with this stratification is that areas with systematically lower insurance coverage may differ along other socioeconomic and financial dimensions, such as income levels, credit supply, or demographic composition, which could confound comparisons. To mitigate these concerns, we implement propensity score matching (PSM) prior to estimation. We estimate a logit model predicting whether a ZIP code belongs to the low versus high insurance coverage group using a broad set of pre-reform covariates. The set of predictors includes the full suite of controls from our baseline specification—GDP, unemployment rate, average aggregate income, liquid assets over income, debt-to-income ratio, local credit supply, housing value, and loan-to-value ratios (Gropp et al., 1997)—as well as demographic characteristics plausibly related to insurance purchasing decisions, including the percentage of working-age residents, educational attainment, and household size (Currie and Gruber, 1996). We then match each low-coverage ZIP to its nearest high-coverage neighbor without replacement, to ensure high-quality matches. This procedure yields treatment and control strata that are more comparable to observed characteristics.

The matched-sample results are presented in Table 6. Panel A of Table 6 shows the estimates for ZIP codes with higher insurance coverage. Across all LTV quintiles, the estimated treatment effects are small and statistically indistinguishable from zero, consistent with the interpretation that in regions where medical risks are already broadly insured, homestead reforms do not meaningfully alter households' precautionary motives and their political giving. Panel B of Table 4 presents the results for lower insurance coverage ZIP codes, where households face greater ex-ante exposure to medical shocks. Here, the treatment effects are concentrated among more households with lower home equity:

donations increase by 12.7% in the highest LTV quintile and by 9.3% in the second-highest quintile, with both estimates statistically significant. In real dollar values, homestead reform raise individual donations by about \$187 per donor-year in LTV Q1 and about \$120 per donor-year in LTV Q2. Relative to the baseline estimates, the magnitude of the effects is almost double in LTV Q1 and in LTV Q2, indicating that the marginal insurance value of the homestead exemption is greater in where other types of insurance coverage is lower. Effects are economically and statistically negligible in the lower LTV quintiles. Economically, these findings are consistent with the mechanism that homestead reforms function as an insurance substitute when formal coverage is scarce, thereby reducing households' precautionary saving needs and crowding in discretionary political contributions.

[Insert Table 6 about here]

7.1.2 *Credit supply heterogeneity*

Prior research highlights that the insurance effect of personal bankruptcy law is mitigated when credit supply is tight, and households keep higher precautionary savings when they have limited access to credits in regions with higher exemptions (Pavan, 2008). Consistent to this intuition, theoretical models of incomplete markets Aiyagari (1994) and empirical evidence Zeldes (1989) and Gross and Souleles (2002) show that households with limited access to new credit rely more heavily on precautionary saving, reducing their willingness to reallocate resources toward discretionary spending. Additionally, studies of U.S. credit markets show that stronger exemption regimes increase lenders' expected losses and prompt credit tightening, especially for low-asset households (Gropp et al., 1997; Berkowitz and Hynes, 1999).

Building on this literature, we test whether the observed donation response to homestead reforms is mediated by local credit conditions. Specifically, we use pre-reform ZIP code-level mortgage denial rates as a proxy for credit tightness. If donations rise because reforms reduce households' need for precautionary saving, then the effect should be strongest in areas with looser credit markets (lower denial rates), where households maintain smaller precautionary buffers and can more easily reallocate freed resources. By contrast, in high-denial ZIPs with tighter credit, households should continue to self-insure heavily, muting the impact of reforms on political giving. To ensure that differences in donation responses between high- and low-denial ZIP codes are not confounded by other socioeconomic disparities, we again implement propensity score matching. Specifically, we estimate a logit model predicting assignment into the low- versus high-denial group using the same set of pre-reform covariates as in the insurance coverage analysis, excluding the loan denial rate itself since it is the stratifying variable.

The results in Table 7 below provide direct evidence that local credit conditions mediate the effect of homestead exemption reforms on political giving. In Panel A, which reports estimates for ZIP codes with higher pre-reform loan denial rates—indicative of tighter credit markets—the coefficients of the interaction term are uniformly small and statistically insignificant across the LTV distribution. This absence of response is consistent with the prediction of Pavan (2008), who emphasizes that the insurance value of personal bankruptcy is muted when households face tight borrowing constraints. More broadly, it aligns with the theoretical insights of Aiyagari (1994), and the empirical findings of Zeldes (1989) and Gross and Souleles (2002), who show that households with limited credit access rely more heavily on self-insurance through precautionary saving and thus have less scope to redirect resources toward discretionary expenditures such as political donations.

By contrast, Panel B demonstrates that in ZIP codes with lower denial rates, where credit markets are relatively looser, reforms produce sizable and statistically significant increases in donations among more leveraged households. Specifically, high-LTV households in the top two quintiles increase their giving by approximately 8.9–12.9% post-reform, while effects for lower-LTV quintiles remain negligible. Relative to the baseline estimates, the lower denial rate sub-sample yields about 12.9% increase in donations in LTV Q1 and 8.9% increase in LTV Q2, roughly double and 75% increase in the two LTV quintiles respectively. The dollar value is also sizable as the homestead reform increases individual donations by roughly \$191 and \$115 on average among the higher LTV quintiles. This pattern is consistent with the evidence from Gropp et al. (1997) and Berkowitz and Hynes (1999), who document that stronger exemption regimes raise lenders’ expected losses and alter credit supply in ways that disproportionately affect low-asset borrowers. Our results extend this literature by showing that where credit is more abundant, the protective effect of homestead exemptions translates into greater political participation, as households can maintain smaller precautionary buffers and respond more elastically to reductions in uninsured downside risk.

[Insert Table 7 about here]

7.2 Channel Analysis

7.2.1 Initial exemption limits heterogeneity

An additional source of heterogeneity arises from cross-state differences in the initial generosity of homestead exemptions. Theory suggests that the insurance value of exemption reforms is highly nonlinear: households in states with low initial protections are more vulnerable to downside shocks and thus benefit more from incremental increases. Dávila (2020) shows that variation in

state bankruptcy laws alters household exposure to risk, with the marginal insurance value of exemption increases being greatest where baseline protections are smallest. Consistent with this, Gropp et al. (1997) find that stronger exemptions raise households' borrowing constraints, particularly affecting those with limited collateral, while Berkowitz and Hynes (1999) emphasize that the asset-shielding function of exemptions is more salient where households would otherwise risk losing nearly all home equity in bankruptcy. Mahoney (2015) further demonstrates that exemption levels shape households' ex-ante portfolio choices, with more generous protections reducing exposure to uninsured medical shocks; this mechanism is arguably most powerful where initial protections are minimal. Taken together, these studies suggest that reforms enacted in low-protection states provide a disproportionately larger shift in effective insurance, reducing precautionary saving motives more strongly than in high-protection states. We therefore test whether the positive effect of homestead exemption reforms on campaign donations is concentrated in states that began with relatively low initial exemption limits.

[Insert Table 8 about here]

Table 8 shows the results of the heterogeneity test. The heterogeneity analysis across states with low versus high initial exemption levels provides further insight into the insurance channel underlying the baseline results. Panel A shows that the increase in donations following exemption reforms is concentrated in states with relatively lower than the average level of initial exemption limits. Specifically, the treatment effect is positive and statistically significant in the bottom two LTV quintiles, with magnitudes of 6.4% and 5.9% increase in donations following the homestead reform, respectively, while effects for higher LTV groups are economically small and statistically indistinguishable from zero. This pattern is consistent with the idea that households in low-protection environments are more vulnerable to adverse shocks and therefore place a higher marginal value on expanded exemption coverage. By alleviating the need for precautionary buffers in these states, reforms appear to free up resources that can be redirected toward political giving.

By contrast, Panel B shows no meaningful effect in states that already had high initial exemption limits. Across all LTV quintiles, the estimated coefficients are small in magnitude and lack statistical significance. This suggests that where pre-existing protections were already generous, additional increases yielded little incremental insurance value and thus had limited consequences for household liquidity allocation and political spending. The asymmetry across Panels A and B strengthens the interpretation that the observed effects are not merely mechanical but instead reflect differences in the marginal insurance value of reforms, aligning with theoretical predictions (Dávila, 2020; Mahoney, 2015).

7.2.2 *Small and large change in exemption heterogeneity*

Another source of the heterogeneity is the size of the increase in the homestead exemption itself. Empirical studies of the exemption generosity find that households in high-exemption regions are better able to smooth consumption against shocks than those facing weaker protection, precisely because more assets can be kept out of creditors' reach (Lehnert and Maki, 2007; Pattison, 2020; Mahoney, 2015; Indarte, 2023). On the other hand, the buffer-stock theory predicts that a household's target buffer decreases as insurance generosity raises, indicating that when household downside risk is reduced, the optimal target falls and households optimally run down precautionary savings and raise current consumption Carroll (1997). These studies imply a does-response effects of the homestead exemption - greater insurance value of the homestead exemption produces a larger fall in the target buffer, leading to a larger increase in discretionary spending. Motivated by this intuition, we split the reforms into terciles based on the size of the exemption change and test whether the positive effects of the homestead exemption on political donations is concentrated in larger reforms.

[Insert Table 9 about here]

Table 9 reports of the results of the level-change heterogeneity. Table 9 shows a clear pattern in household discretionary spending response to different levels of exemption generosity, where small reforms (bottom tercile) has no detectable change in individual donation across all LTV quintiles; median-size reforms raise political donations by roughly 5% for households in the top two LTV quintiles, but the effects are only marginally significant. In contrast, the largest reforms (top tercile) show a clear and statistically significant increase in individual donations of about 9% for LTV Q5 and about 5% for LTV Q4. This heterogeneous pattern is consistent with the buffer-stock mechanism as large increases in bankruptcy protection reduce downside risk more, inducing a larger decline in precautionary savings or self-insurance, and thereby relaxing the budget available for discretionary spending such as political donations.

8 Homestead Exemption Reforms and New Donors

If homestead reforms can promote political participation, especially in campaign donations, by reducing the uninsurable risks of the households, we should also expect to see increase in new donors. We focus on new donors emerging at the ZIP-year level to show whether homestead bankruptcy reform promotes new donors entering the donor pool. Table below examines whether homestead reform promotes entry of new donors at the ZIP-year level. The explanatory

variable is the inverse hyperbolic sin of the number of first-time donors in ZIP code z and in year t . We add an additional control of local population using the natural log of total tax filers at the ZIP codes to control for the local population to purge mechanical scale effects, given that entry counts rise purely with the number of potential donors; this isolates changes in the entry rate rather than differences in total population. Specifications include ZIP by cohort and year by cohort fixed effects.

[Insert Table 10 about here]

Table 10 shows the estimates on new donor counts. The extensive margin on new donor counts estimate similar patterns documented by the intensive margin. The treatment effect is concentrated in high-LTV (low-equity) ZIPs, indicating that households whose home equity is largely covered by the new exemption limits are more likely to start giving once their home equity is protected under the state law. The first two columns of Table 6 shows that the average treated ZIPs with highest LTV experience a 12.6% increase in new donors while ZIPs with the second highest LTV experience a 7.6% increase in new donors. Effects are statistically indistinguishable from zero in ZIPs with lower LTV (column 3 - 5). Boatright et al. (2006) show that a statewide RCT that mailed Ohio taxpayers about Ohio's political-contribution credit increased participation by roughly 5 - 6%. In the state level, using California as an example, the homestead reform, on average, promotes approximately 225 more donors from both LTV quintile regions per year. As indicated by previous research (e.g. (Ansolabehere et al., 2003; Barber et al., 2019)) that campaign donations are highly skewed to higher income households, the two highest LTV regions account roughly 2,395 new itemized donors in California per year, and the homestead reform explains approximately 9.4% of the raises in new itemized donors in these regions. Also, this number only accounts for itemized donors as un-itemized small donors are not reported by FEC ruling. This cross-sectional gradient The differential effects across LTV quintiles mirror the baseline results pattern and is consistent with our prediction that average treated ZIPs experienced new donors entering the donor pool once households have assets protected and face less uninsured downside risk. Where equity level thick (lower LTV), the exemption provides little protection, and no entry response is detectable.

Taken together, precedent evidence show that homestead exemption reforms unlock meaningful amounts of discretionary spending from precautionary savings. We next examine which giving margin move and whether the effect is partisan. We estimate the same regression model of the extensive margin on both Republican and Democratic new donors. We find that the increases of new donor entry are broadly symmetric across parties, with increases in both new Democratic and Republican donors in high-LTV communities.

[Insert Table 11 about here]

Table 11 documents the results of the new partisan donors, where Panel A shows the estimates of new Republican donors while Panel B shows the estimates of new Democratic donors. In the highest LTV regions, the number of new Republican donors raise by 12.5% and new Democratic donors raise by 8.7%. In the second highest LTV region, we also detect a positive entry response for Republican party by 8.9%, while coefficients in lower LTV regions are small and statistically indistinguishable from zero for both parties. The pattern documented is somewhat partisan-symmetric in sign and location - effects concentrated in high LTV regions - consistent with the new donor entry results.

9 Alternative Explanation: Policy Feedback

Previous studies by Mahoney (2015) and Dobbie and Song (2015) indicate that households are well aware of the debt-relief legislation and policies. If households are well aware of the legislation reforms and the politicians associated with the reforms, this might suggest a potential policy-feedback or policy-blowback channel for the increase in donations. On the other hand, Clinton and Sances (2018) indicates that households less likely to be motivated by policy changes such as medicaid expansions to participate in politics. Also, if policy-feedback does matter to households, we should observe similar effects among both higher and lower insurance coverage regions. In addition to that, if precautionary saving is the channel driving the donation increases, we should also expect other forms of discretionary expenditures to raise as well. However, if policy-feedback channel is the case, then we should only expect to see increase in campaign donations rather than other forms of discretionary expenditures. In order to test whether homestead reforms also increase other forms of consumptions, we aggregate charitable donations at the ZIP codes from the SOI IRS dataset and average the amounts by the tax filers at the ZIP code.

[Insert Table 12 about here]

Table 12 shows the results of the increasing in charitable donations at the ZIP code level. $IHSChartiable$ is the inverse hyperbolic sin of the average donations per tax filers at the ZIP code. The results below shows that after the homestead reform, charitable donation per capita increased by about 6.3% in the highest LTV regions and about 3.6% in the second highest LTV regions. The results indicates that after homestead reform, regions with highest LTV ratio experience an increase in charitable givings per capita by about \$71.80 while regions with the second highest LTV ratio experience an increase in charitable givings per capita by about \$41.03.

We replicate the baseline results using average donations per capita (tax filers) at the ZIP code in Table 13, where $IHSPolitical$ represents the average political donations per tax filers at the ZIP code. The ZIP-level result show

similar patterns to the baseline specification with 0.130% increase of average political donation per filer in LTV Q5 and 9.2% increase of average political donation per filer in LTV Q4. Compared to the raise in charitable giving after the homestead reform, political donations raised by \$0.60 per capita in LTV Q5 and by \$0.40 in LTV Q4. This small size is due to smaller political donation amounts and political donor pools than charitable donation and donors. Most recent anecdotal evidence show that individual charitable giving in 2024 is about \$392.45 billion ⁶, and more than half of the Americans have participated in charitable giving ⁷. However, both the size of political donations and the donor pool is disproportionally smaller than charitable donations. For instance, in the most recent presidential election cycle, political donations collected are about \$10 billion and only about 1.03% of the Americans have contributed during the election cycle ⁸. The tiny fraction of the donor pool mitigate the magnitude of the per capita average political donations when comparing the per capita average charitable donations. Because the charitable giving has a much larger donor base and higher baseline per-filer giving than political giving, the level response naturally larger than for political giving. The fact that both margins move in the same direction and in the same LTV regions is what our reducing buffer-stock mechanism predicts.

[Insert Table 13 about here]

The results in Table 12 also indicate that the increase in donations are not driven by policy-feedback or policy-blowback channels, as these channels might only impact campaign donations rather than charitable donations. The raise in charitable donations, on the other hand, also mitigate the concerns that the effects are driven by other forms of political mobilizations rather and suggest more of a reducing precautionary savings channel as Yildirim et al. (2024) indicates that political donations and charitable donations are substitutes: when natural disasters occurred, there is an increase in county-level charitable donations accompanied by an decrease in political donations; vice versa, if there is a political advertisements, there is an increase in political donations accompanied by and decrease in charitable donations. If the increase in donations are driven by other forms of political mobilization, we should expect to see a negative impacts on average charitable donations per capita accompanied with the positive impacts on political donations. However, under Carroll (1997) buffer-stock saving theory, as precautionary savings decreased, we should expect to see both charitable and political donation increases as forms of discretionary spending outlets.

⁶<https://givingusa.org/giving-usa-2025-u-s-charitable-giving-grew-to-592-50-billion-in-2024-lifted-by-stock-market-gains/>

⁷<https://www.developmentaid.org/api/frontend/cms/file/2025/09/The-Giving-Environment-Fall-2024-Final-remediated.pdf>

⁸<https://www.opensecrets.org/elections-overview/donor-demographics?cycle=2024&display=A>

Additionally, homestead exemption reforms are state statutes which require a governor’s approval or can be vetoed. In some states, the reforms were explicitly linked to the governor in official communications. If our new donor entry effects partly reflect policy feedback, as reforms mobilize new donors toward the policy sponsor, then donor entry should rise disproportionately for the governor’s party when the reform is publicly associated with the governor. Within our sample, we identified 8 states with official announcements linking the policy reforms with the governor, including North Carolina, Wisconsin, Louisiana, New York, Georgia, Utah, Alabama, and New Hampshire. We restrict the sample to these states and re-estimate a stacked difference-in-difference-in-differences model:

For each LTV quintile $q \in \{1, 2, 3, 4, 5\}$, we estimate:

$$Y_{z,t}^P = \beta (Treat_s \times Post_t \times GovParty_{s,t}^P) + \theta + F_{s,t} + \gamma_{s,c,d,z,t} + \epsilon_{z,t}. \quad (2)$$

where $Y_{z,t}^P$ represents new donor entry to either Republican or Democratic party. $GovParty_{s,t}^P$ is the dummy variable indicating the partisanship of the governor. θ captures all the lower-order terms of the DDD estimator.

[Insert Table 14 and 15 about here]

Table 14 shows the results of new Republican donor entry in the eight states where official state announcements explicitly linked the governor to the homestead reform. The baseline reform effect remains positive and significant in higher LTV regions, while effect in lower LTV regions are small and statistically indistinguishable from zero. Critically, the coefficients of the governor alignment triple interaction are insignificant across all LTV quintiles, implying no partisan amplification when the governor is Republican, thus little evidence for the policy-feedback channel.

Table 15 shows the results of new Democratic donor entry in the eight states. The table shows a similar pattern where the baseline effect is concentrated in higher LTV regions. However, the coefficients of the governor alignment triple interaction are insignificant across all quintiles, indicating that there is no post-reform jump in new Democratic donors when the governor is Democrat. Taken together, even in states that publicly associated the reform with the governor, partisan new-entry rises in high LTV quintiles regardless of gubernatorial partisan, providing no evidence of governor-aligned policy feedback effect.

10 Conclusion

This paper studies how changes in personal bankruptcy exemption generosity affect household discretionary spending. We focus on statutory increases in state homestead exemption limits, which expand the share of home equity that is protected from creditors in Chapter 7 bankruptcy. We exam whether homestead reforms increase discretionary outlays, using political and charitable donations as observable, highly adjustable components of consumption. In standard buffer-stock models, such a reduction in uninsured downside risk lowers target wealth and weakens the incentive to maintain large precautionary buffers, freeing resources that can be redirected toward nonessential spending.

Our individual-level analyses show that increasing homestead exemption generosity leads to higher individual campaign contributions among households living in higher LTV areas where households' home equity is newly or fully covered by the new exemption limits. Donors in low-LTV regions remain unaffected. Moreover, our ZIP-code-level analyses show that raising homestead exemption generosity also drives new donors entering into the local donor pool as well as leads to higher average charitable donation per tax fillers, and the effects are concentrated among ZIP codes with higher LTV. Together, these patterns indicate that stronger asset protection translates into higher discretionary spending among those who are mainly affected.

Second, we find that the donation response is concentrated in where the exposure to medical-expenditure risk is higher. In the matched comparison by working-age insurance coverage, treatment effects are negligible in high-coverage ZIPs, but in low-coverage ZIPs—where ex-ante medical risk is greater—the effects on donation are positive and significant. This result is consistent with the buffer stock saving theory - when uninsured risk is high, the reform relaxes precautionary-saving needs, and households reallocate resources to political giving; when risk is already insured, there is less or no response. Additionally, we find that the donation response depends on local credit access, where the effects are near zero in high-denial ZIPs but positive in low-denial ZIPs, especially among high-LTV households. Moreover, our further analyses testing new donor counts to either Republican and Democratic party responding to the announcement of governor approval of the homestead exemption reveals that the effects are less likely driven by policy-feedback mechanism. Taken these evidence together, our study suggests that raising homestead exemption generosity leads to higher household discretionary spending by reducing precautionary buffers against adverse shocks.

These results highlight the critical role of personal bankruptcy exemptions as a form of background insurance that shapes households' discretionary spending, rather than only affecting outcomes at the point of default. An important implication of our study is that the design of homestead exemption policy can

have meaningful spillovers for how households allocate resources between precautionary saving and non-essential spending, including politically and socially salient categories such as campaign and charitable giving.

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Table 1: Increases in State Homestead Exemptions, 2007–2017

Table reports every statutory increase in single-filer homestead-exemption limits between 2007 and 2017. Each row is one enacted increase, and a state may appear multiple times if it reformed its homestead exemption multiple times within the sample period. Year is the first year the new limit took effect. Δk reports the size of the changes, expressed in thousand dollars. California have two distinct exemption limits, and we report the changes on the exemption limit that has greater exemption amounts and are accepted across all courts.

State	Year	Δk	State	Year	Δk
AK	2008	2.7	MN	2007	100.0
AK	2012	2.7	MN	2013	90.0
AL	2015	10.0	MT	2007	150.0
CA	2010	25.0	WY	2012	10.0
CO	2007	15.0	NC	2009	16.5
CO	2015	15.0	ND	2009	20.0
DE	2010	25.0	NE	2007	47.5
DE	2011	25.0	NH	2016	20.0
GA	2012	11.5	NM	2007	30.0
IN	2010	2.3	NY	2011	25.0
IN	2016	1.7	OH	2010	152.7
LA	2009	10.0	OH	2013	111.3
MD	2010	21.6	OH	2016	4.0
MD	2013	1.4	OR	2007	5.0
MD	2016	0.7	RI	2012	200.0
ME	2008	12.5	SC	2008	1.4
MI	2008	1.5	SC	2010	1.9
MI	2011	1.6	SC	2012	2.8
MI	2014	3.3	SC	2014	2.1
MI	2017	0.4	SC	2016	0.9
WI	2009	35.0	UT	2013	10.0
VT	2009	50.0			

Table 2: Summary Statistics

Variable	N	Mean	Median	Min	Max	Std. Dev.
LnAmount	11,647,616	6.49	6.31	3.24	10.17	1.30
Log GDP	11,647,616	13.35	13.52	10.97	14.63	0.84
Ln AGI	11,647,616	11.31	11.35	4.38	13.26	1.24
Liquid Asset / AGI	11,647,616	0.05	0.04	0.00	0.18	0.03
Unemployment Rate	11,647,616	6.27	5.90	2.60	12.20	2.20
Median DTI	11,647,616	2.03	2.01	1.14	3.27	0.40
Loan Denial Rate	11,647,616	19.10	17.87	8.92	40.24	6.35
Ln Home Value	11,647,616	12.59	12.54	11.09	14.51	0.72
Pct. Working Age	11,647,616	0.63	0.62	0.40	0.86	0.07
Republican Dist.	11,647,616	0.59	1.00	0.00	1.00	0.49
Avg. Family Size	11,647,616	3.02	3.03	2.25	3.74	0.27
Pct. College	11,647,616	0.44	0.44	0.09	0.81	0.19

Notes: This table presents descriptive statistics of total observations, mean, median, minimum, and maximum values, standard deviation for variables with non-missing values. Unit of observation is at the donor-year-cohort level. All variables are measured at the annual frequency from 2006 to 2017. Monetary variables are inflation-adjusted to 2017 dollars. Variables are defined in the Appendix Table A1.

Table 3: Baseline Results: Homestead Exemption on Individual Donations

This table examines the effect of raising homestead exemption limits on individual campaign donations and shows the results of the model specification:

$$\text{LnAmount}_{i,t} = \beta(\text{Treat}_s \times \text{Post}_t) + F_{i,s,t} + \gamma_{s,c,d,z,t} + \epsilon_{i,t}$$

where the table is separately estimated on different samples across LTV quintiles, which group donors based on the home equity of households in their ZIP code to capture different household exposure to homestead exemption. LTV is an inverse proxy for home equity and is calculated by dividing the median mortgage amount at the origination by the Zillow reported housing value at the ZIP code. Columns (1) through (5) reports the results from each regression across LTV quintiles, where LTV Q1 represents donors residing in ZIPs with highest LTV while LTV Q5 represents donors residing in ZIPs with lowest LTV. The dependent variable, $\text{LnAmount}_{i,t}$, is the natural logarithm of one plus the amount of individual donations per year. Treat equals one for donors in states that reformed its homestead exemption and zero otherwise. Post equals one for years after the homestead reform and zero otherwise. The interaction term $\text{Treat} \times \text{Post}$ captures the effect of homestead reform on individual donations. $F_{i,s,t}$ is the matrix of fixed effects, including state-by-cohort (s), year-by-cohort (t), and individual donor-by-cohort (i) fixed effects. γ is the matrix of control variables including local economic conditions (GDP, unemployment rate, household income), household finance indicators (debt to income, LTV, mortgage loan denial rate, home value), demographic composition (working age share, college graduate share, family size), and local political alignment (Republican vote share). The unit of observation is at the individual donor-year-cohort level. Standard errors are clustered at the state-year level with t -statistics in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

	(1)	(2)	(3)	(4)	(5)
	<i>LnAmount</i>	<i>LnAmount</i>	<i>LnAmount</i>	<i>LnAmount</i>	<i>LnAmount</i>
	LTV Q5	LTV Q4	LTV Q3	LTV Q2	LTV Q1
Median LTV	[1.13]	[0.93]	[0.84]	[0.77]	[0.63]
Treat × Post	0.058*** (2.88)	0.049*** (2.78)	0.008 (0.42)	-0.019 (-0.82)	0.013 (0.64)
Log GDP	0.360 (1.25)	0.131 (0.53)	0.593** (2.29)	-0.259 (-0.88)	0.099 (0.49)
Ln Income	0.053 (1.57)	0.095* (1.69)	0.143*** (3.47)	0.247*** (4.13)	0.066*** (3.53)
Liquid Asset over Income	0.450 (1.30)	0.361 (0.81)	-0.506 (-1.23)	0.071 (0.15)	-0.100 (-0.50)
Unemployment Rate	-0.005 (-0.60)	-0.007 (-0.86)	-0.003 (-0.34)	-0.041** (-2.30)	-0.010 (-1.27)
DTI	0.004 (0.16)	-0.160* (-1.87)	0.039 (1.23)	0.078 (1.39)	-0.002 (-0.07)
Loan Denial Rate	0.000 (0.07)	-0.002 (-1.58)	-0.003** (-2.42)	-0.004 (-1.14)	-0.002* (-1.72)
Ln Home Value	-0.073 (-1.54)	0.087 (0.98)	-0.090 (-1.38)	-0.336*** (-2.69)	0.030 (1.27)
Pct. Working Age	-0.107 (-0.41)	-0.329 (-0.69)	0.205 (0.97)	0.074 (0.28)	0.087 (0.98)
Republican Dist.	0.025 (1.28)	0.032* (1.84)	-0.012 (-0.84)	0.006 (0.51)	-0.007 (-0.54)
Pct. College Grad.	0.219 (1.12)	-0.135 (-0.61)	-0.147 (-1.31)	0.422 (1.53)	-0.051 (-0.99)
Avg. Family Size	-0.049 (-0.87)	-0.017 (-0.28)	-0.121** (-2.28)	-0.063 (-0.55)	0.023 (0.98)
Observations	441041	519153	764183	1146510	2503721
Adjusted R^2	0.677	0.689	0.691	0.656	0.637
State-Cohort FE	✓	✓	✓	✓	✓
Year-Cohort FE	✓	✓	✓	✓	✓
Contributor-Cohort FE	✓	✓	✓	✓	✓

Table 4: Parallel Trends Test by LTV Quintile

This table estimates the dynamic effect of homestead-exemption reforms using a stacked event-study specification, reported separately by LTV quintiles:

$$LnAmount_{i,t} = \sum_{T \neq -3} \beta_T \cdot (Treat_s \times RelTime_T) + F_{i,s,t} + \gamma_{s,c,d,z,t} + \epsilon_{i,t}$$

where $RelTime_k$ are relative year indicators from $T = -3$ to $T = 3$, omitting $T = -3$ as the baseline period. The dependent variable is the natural logarithm of one plus the amount of individual donations per year. $Treat$ equals one for donors in states that reformed its homestead exemption and zero otherwise. $Post$ equals one for years after the homestead reform and zero otherwise. Columns (1) through (5) report results for each LTV quintile. $F_{i,s,t}$ is the matrix of fixed effects, including state-by-cohort (s), year-by-cohort (t), and individual donor-by-cohort (i) fixed effects. γ is the matrix of control variables including local economic conditions, household finance indicators, demographic composition, and local political alignment. Coefficients β_T for T less than 0 test for pre-trends while coefficients for T greater or equal to 0 test the post-reform dynamics. The unit of observation is at the individual donor-year-cohort level. Standard errors are clustered at the state-year level with t -statistics in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

	(1) <i>LnAmount</i> LTV Q5	(2) <i>LnAmount</i> LTV Q4	(3) <i>LnAmount</i> LTV Q3	(4) <i>LnAmount</i> LTV Q2	(5) <i>LnAmount</i> LTV Q1
T = -2	0.003 (0.12)	-0.027 (-1.43)	0.013 (0.56)	0.039* (1.72)	0.030 (1.03)
T = -1	0.034 (1.20)	0.011 (0.40)	0.042 (1.64)	0.053 (1.64)	-0.021 (-0.62)
T = 0	0.066** (2.53)	0.048* (1.66)	0.036 (1.25)	0.026 (0.88)	-0.014 (-0.51)
T = 1	0.107*** (3.17)	0.065** (2.34)	0.032 (1.13)	0.039 (1.23)	0.032 (1.07)
T = 2	0.075** (2.17)	0.038 (1.19)	0.027 (0.85)	-0.014 (-0.35)	0.032 (0.95)
T = 3	0.046 (1.19)	0.016 (0.38)	0.033 (0.99)	0.007 (0.17)	0.015 (0.46)
Observations	453301	537417	790710	1185938	2572224
Adjusted R^2	0.677	0.687	0.691	0.657	0.636
Joint test p-value	0.484	0.129	0.255	0.152	0.290
Controls	✓	✓	✓	✓	✓
State-Cohort FE	✓	✓	✓	✓	✓
Year-Cohort FE	✓	✓	✓	✓	✓
Contributor-Cohort FE	✓	✓	✓	✓	✓

Table 5: Robustness: Exclude Wealthy States

This table examines the effect of raising homestead-exemption limits on individual campaign donations under exclusions of wealthy states and shows the results of the model:

$$\text{LnAmount}_{i,t} = \beta(\text{Treat}_s \times \text{Post}_t) + F_{i,s,t} + \gamma_{s,c,d,z,t} + \epsilon_{i,t}$$

where we restrict the sample to exclude wealthy states as follow: Panel A drops units from the top 10 GDP states while Panel B drops the top 10 income-per-capita states using 2012 as the reference year. The table is separately estimated on different samples across LTV quintiles, which group donors based on the home equity of households in their ZIP code to capture different household exposure to homestead exemption. LTV is an inverse proxy for home equity and is calculated by dividing the median mortgage amount at the origination by the Zillow housing value at the ZIP code. Columns (1) through (5) reports the results from each regression across LTV quintiles, where LTV Q1 represents donors residing in ZIPs with highest LTV while LTV Q5 represents donors residing in ZIPs with lowest LTV. The dependent variable, $\text{LnAmount}_{i,t}$, is the natural logarithm of one plus the amount of individual donations per year. Treat equals one for donors in states that reformed its homestead exemption and zero otherwise. Post equals one for years after the homestead reform and zero otherwise. The interaction term $\text{Treat} \times \text{Post}$ captures the effect of homestead reform on individual donations. $F_{i,s,t}$ is the matrix of fixed effects, including state-by-cohort (s), year-by-cohort (t), and individual donor-by-cohort (i) fixed effects. γ is the matrix of control variables including local economic conditions, household finance indicators, demographic composition, and local political alignment. The unit of observation is at the individual donor-year-cohort level. Standard errors are clustered at the state-year level with t -statistics in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

Panel A: Exclude Top GDP States					
	(1)	(2)	(3)	(4)	(5)
	<i>LnAmount</i>	<i>LnAmount</i>	<i>LnAmount</i>	<i>LnAmount</i>	<i>LnAmount</i>
	LTV Q5	LTV Q4	LTV Q3	LTV Q2	LTV Q1
Mean LTV	1.22	0.93	0.85	0.77	0.61
Treat × Post	0.068*** (2.93)	0.069*** (2.98)	0.052** (2.19)	0.008 (0.32)	0.017 (0.58)
Observations	160188	220551	283882	355708	539367
Adjusted R^2	0.662	0.687	0.692	0.667	0.638
Controls	✓	✓	✓	✓	✓
State-Cohort FE	✓	✓	✓	✓	✓
Year-Cohort FE	✓	✓	✓	✓	✓
Contributor-Cohort FE	✓	✓	✓	✓	✓
Panel B: Exclude Top Income per Capita States					
	(1)	(2)	(3)	(4)	(5)
	<i>LnAmount</i>	<i>LnAmount</i>	<i>LnAmount</i>	<i>LnAmount</i>	<i>LnAmount</i>
	LTV Q5	LTV Q4	LTV Q3	LTV Q2	LTV Q1
Mean LTV	1.22	0.94	0.84	0.76	0.59
Treat × Post	0.056*** (2.69)	0.044** (2.38)	0.015 (0.76)	-0.047 (-1.43)	0.011 (0.45)
Observations	428524	494625	701261	957721	1838101
Adjusted R^2	0.675	0.687	0.689	0.651	0.633
Controls	✓	✓	✓	✓	✓
State-Cohort FE	✓	✓	✓	✓	✓
Year-Cohort FE	✓	✓	✓	✓	✓
Contributor-Cohort FE	✓	✓	✓	✓	✓

Table 6: Channel Analysis: Insurance Coverage Heterogeneity

This table tests whether the effect of raising homestead exemption limits on individual campaign donations varies with health-insurance coverage, using the model:

$$\text{LnAmount}_{i,t} = \beta(\text{Treat}_s \times \text{Post}_t) + F_{i,s,t} + \gamma_{s,c,d,z,t} + \epsilon_{i,t}$$

where we split the sample by pre-reform working-age insurance coverage, averaged over the pre-reform years. Panel A reports results for ZIP codes with above-median pre-reform insurance coverage; Panel B for below-median pre-reform insurance coverage. Within each panel, we estimate the model separately across ZIP-level loan-to-value (LTV) quintiles, an inverse proxy for home equity. Columns (1) through (5) report results for each LTV quintile. The dependent variable, $\text{LnAmount}_{i,t}$, is the natural logarithm of one plus the amount of individual donations per year. Treat equals one for donors in states that reformed its homestead exemption and zero otherwise. Post equals one for years after the homestead reform and zero otherwise. The interaction term $\text{Treat} \times \text{Post}$ captures the effect of homestead reform on individual donations. $F_{i,s,t}$ is the matrix of fixed effects, including state-by-cohort (s), year-by-cohort (t), and individual donor-by-cohort (i) fixed effects. γ is the matrix of control variables including local economic conditions (GDP, unemployment rate, household income), household finance indicators (debt to income, LTV, mortgage loan denial rate, home value), demographic composition (working age share, college graduate share, family size), and local political alignment (Republican vote share). All regressions are estimated on a propensity-score-matched sample constructed with pre-reform averages of the control variables listed in above as covariates. Each below-median insurance-coverage ZIP is matched one-to-one to a unique above-median coverage ZIP without replacement; unmatched ZIPs are dropped, and only the matched pairs are used in the estimations. The unit of observation is at the individual donor-year-cohort level. Standard errors are clustered at the state-year level with t -statistics in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

Panel A: Higher Insurance Coverage					
	(1)	(2)	(3)	(4)	(5)
	<i>LnAmount</i>	<i>LnAmount</i>	<i>LnAmount</i>	<i>LnAmount</i>	<i>LnAmount</i>
	LTV Q5	LTV Q4	LTV Q3	LTV Q2	LTV Q1
Treat × Post	0.026 (0.75)	0.018 (0.68)	-0.045 (-1.41)	-0.005 (-0.15)	-0.024 (-0.77)
Observations	65,549	97,311	146,180	171,520	191,440
Adjusted R ²	0.675	0.704	0.701	0.649	0.644
Controls	✓	✓	✓	✓	✓
State-Cohort FE	✓	✓	✓	✓	✓
Year-Cohort FE	✓	✓	✓	✓	✓
Contributor-Cohort FE	✓	✓	✓	✓	✓
Panel B: Lower Insurance Coverage					
	(1)	(2)	(3)	(4)	(5)
	<i>LnAmount</i>	<i>LnAmount</i>	<i>LnAmount</i>	<i>LnAmount</i>	<i>LnAmount</i>
	LTV Q5	LTV Q4	LTV Q3	LTV Q2	LTV Q1
Treat × Post	0.127*** (3.90)	0.093*** (3.06)	0.017 (0.51)	-0.009 (-0.30)	-0.001 (-0.05)
Observations	111,487	152,886	184,073	176,592	314,678
Adjusted R ²	0.678	0.677	0.676	0.658	0.642
Controls	✓	✓	✓	✓	✓
State-Cohort FE	✓	✓	✓	✓	✓
Year-Cohort FE	✓	✓	✓	✓	✓
Contributor-Cohort FE	✓	✓	✓	✓	✓

Table 7: Channel Analysis: Loan Denial Rate Heterogeneity

This table tests whether the effect of raising homestead exemption limits on individual campaign donations varies with credit access, using the model:

$$\text{LnAmount}_{i,t} = \beta(\text{Treat}_s \times \text{Post}_t) + F_{i,s,t} + \gamma_{s,c,d,z,t} + \epsilon_{i,t}$$

where we split the sample by pre-reform mortgage denial rates, averaged over the pre-reform years. Panel A reports results for ZIP codes with above-median pre-reform mortgage denial rates; Panel B for below-median pre-reform mortgage denial rates. Within each panel, we estimate the model separately across ZIP-level loan-to-value (LTV) quintiles, an inverse proxy for home equity. Columns (1) through (5) report results for each LTV quintile. The dependent variable, $\text{LnAmount}_{i,t}$, is the natural logarithm of one plus the amount of individual donations per year. Treat equals one for donors in states that reformed its homestead exemption and zero otherwise. Post equals one for years after the homestead reform and zero otherwise. The interaction term $\text{Treat} \times \text{Post}$ captures the effect of homestead reform on individual donations. $F_{i,s,t}$ is the matrix of fixed effects, including state-by-cohort (s), year-by-cohort (t), and individual donor-by-cohort (i) fixed effects. γ is the matrix of control variables including local economic conditions (GDP, unemployment rate, household income), household finance indicators (debt to income, LTV, mortgage loan denial rate, home value), demographic composition (working age share, college graduate share, family size), and local political alignment (Republican vote share). All regressions are estimated on a propensity-score-matched sample constructed with pre-reform averages of the control variables listed in above as covariates excluding loan denial rate. Each below-median insurance-coverage ZIP is matched one-to-one to a unique above-median coverage ZIP without replacement; unmatched ZIPs are dropped, and only the matched pairs are used in the estimations. The unit of observation is at the individual donor-year-cohort level. Standard errors are clustered at the state-year level with t -statistics in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

Panel A: Higher Loan Denial Rates					
	(1)	(2)	(3)	(4)	(5)
	<i>LnAmount</i> LTV Q5	<i>LnAmount</i> LTV Q4	<i>LnAmount</i> LTV Q3	<i>LnAmount</i> LTV Q2	<i>LnAmount</i> LTV Q1
Treat × Post	0.009 (0.30)	0.046 (1.59)	-0.087 (-1.52)	0.023 (0.52)	-0.017 (-0.59)
Observations	98,911	109,279	90,593	97,932	263,610
Adjusted R ²	0.659	0.685	0.684	0.670	0.657
Controls	✓	✓	✓	✓	✓
State-Cohort FE	✓	✓	✓	✓	✓
Year-Cohort FE	✓	✓	✓	✓	✓
Contributor-Cohort FE	✓	✓	✓	✓	✓
Panel B: Lower Loan Denial Rates					
	(1)	(2)	(3)	(4)	(5)
	<i>LnAmount</i> LTV Q5	<i>LnAmount</i> LTV Q4	<i>LnAmount</i> LTV Q3	<i>LnAmount</i> LTV Q2	<i>LnAmount</i> LTV Q1
Treat × Post	0.101*** (2.95)	0.068** (2.08)	-0.022 (-0.83)	0.026 (1.02)	0.005 (0.17)
Observations	77,106	128,847	163,904	143,791	137,956
Adjusted R ²	0.698	0.675	0.687	0.662	0.645
Controls	✓	✓	✓	✓	✓
State-Cohort FE	✓	✓	✓	✓	✓
Year-Cohort FE	✓	✓	✓	✓	✓
Contributor-Cohort FE	✓	✓	✓	✓	✓

Table 8: **Initial Exemption Limit Heterogeneity**

This table tests whether the effect of raising homestead exemption limits on individual campaign donations varies with states' initial exemption levels, using the model:

$$\text{LnAmount}_{i,t} = \beta(\text{Treat}_s \times \text{Post}_t) + F_{i,s,t} + \gamma_{s,c,d,z,t} + \epsilon_{i,t}$$

where we split the sample by states' initial homestead exemption limit, the statutory exemption before any reform. We compute a single threshold using the average cross-state average initial exemption limits within our sample, excluding states with unlimited exemption limits. Panel A reports results for states with initial exemptions at or above this average; Panel B for states below this average. Within each panel, we estimate the model separately across ZIP-level loan-to-value (LTV) quintiles, an inverse proxy for home equity. Columns (1) through (5) report results for each LTV quintile. The dependent variable, $\text{LnAmount}_{i,t}$, is the natural logarithm of one plus the amount of individual donations per year. Treat equals one for donors in states that reformed its homestead exemption and zero otherwise. Post equals one for years after the homestead reform and zero otherwise. The interaction term $\text{Treat} \times \text{Post}$ captures the effect of homestead reform on individual donations. $F_{i,s,t}$ is the matrix of fixed effects, including state-by-cohort (s), year-by-cohort (t), and individual donor-by-cohort (i) fixed effects. γ is the matrix of control variables including local economic conditions, household finance indicators, demographic composition, and local political alignment. The unit of observation is at the individual donor-year-cohort level. Standard errors are clustered at the state-year level with t -statistics in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

Panel A: Individual Donations by States with Higher Initial Exemption Levels					
	(1)	(2)	(3)	(4)	(5)
	LnAmount LTV Q1	LnAmount LTV Q2	LnAmount LTV Q3	LnAmount LTV Q4	LnAmount LTV Q5
Treat × Post	0.025 (0.52)	0.030 (0.75)	0.037 (0.95)	-0.073 (-1.22)	-0.028 (-0.59)
Observations	320075	395430	583405	854049	1632179
Adjusted R^2	0.692	0.701	0.707	0.673	0.657
Controls	✓	✓	✓	✓	✓
State-Cohort FE	✓	✓	✓	✓	✓
Year-Cohort FE	✓	✓	✓	✓	✓
Contributor-Cohort FE	✓	✓	✓	✓	✓
Panel B: Individual Donations by States with Lower Exemption Levels					
	(1)	(2)	(3)	(4)	(5)
	LnAmount LTV Q1	LnAmount LTV Q2	LnAmount LTV Q3	LnAmount LTV Q4	LnAmount LTV Q5
Treat × Post	0.059*** (2.81)	0.053*** (2.72)	-0.003 (-0.16)	0.003 (0.14)	0.021 (0.92)
Observations	393688	451073	660114	1000962	2269992
Adjusted R^2	0.661	0.671	0.674	0.638	0.621
Controls	✓	✓	✓	✓	✓
State-Cohort FE	✓	✓	✓	✓	✓
Year-Cohort FE	✓	✓	✓	✓	✓
Contributor-Cohort FE	✓	✓	✓	✓	✓

Table 9: Exemption Change Heterogeneity

This table tests whether the effect of raising homestead exemption limits on individual campaign donations varies with states' initial exemption levels, using the model:

$$\text{LnAmount}_{i,t} = \beta(\text{Treat}_s \times \text{Post}_t) + F_{i,s,t} + \gamma_{s,c,d,z,t} + \epsilon_{i,t}$$

where we split the sample by states' initial homestead exemption limit, the statutory exemption before any reform. We split the sample based on the tercile of the exemption reform size, where the bottom tercile includes states having the smaller exemption changes while the top tercile includes states having larger exemption changes. Panel A reports results for smaller change states (bottom tercile); Panel B for median change states (middle tercile); Panel C for larger change states (Top tercile). Within each panel, we estimate the model separately across ZIP-level loan-to-value (LTV) quintiles, an inverse proxy for home equity. Columns (1) through (5) report results for each LTV quintile. The dependent variable, $\text{LnAmount}_{i,t}$, is the natural logarithm of one plus the amount of individual donations per year. Treat equals one for donors in states that reformed its homestead exemption and zero otherwise. Post equals one for years after the homestead reform and zero otherwise. The interaction term $\text{Treat} \times \text{Post}$ captures the effect of homestead reform on individual donations. $F_{i,s,t}$ is the matrix of fixed effects, including state-by-cohort (s), year-by-cohort (t), and individual donor-by-cohort (i) fixed effects. γ is the matrix of control variables including local economic conditions, household finance indicators, demographic composition, and local political alignment. The unit of observation is at the individual donor-year-cohort level. Standard errors are clustered at the state-year level with t -statistics in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

Panel A: Individual Donations by Bottom Exemption Change Tercile Sub-sample					
	(1)	(2)	(3)	(4)	(5)
	LnAmount LTV Q5	LnAmount LTV Q4	LnAmount LTV Q3	LnAmount LTV Q2	LnAmount LTV Q1
Treat × Post	0.052 (1.31)	0.035 (0.96)	0.027 (0.87)	-0.055 (-1.44)	-0.031 (-0.85)
Observations	425742	503057	734977	1081471	2113514
Adjusted R^2	0.681	0.691	0.695	0.661	0.644
Controls	✓	✓	✓	✓	✓
State-Cohort FE	✓	✓	✓	✓	✓
Year-Cohort FE	✓	✓	✓	✓	✓
Contributor-Cohort FE	✓	✓	✓	✓	✓
Panel B: Individual Donations by Middle Exemption Change Tercile Sub-sample					
	(1)	(2)	(3)	(4)	(5)
	LnAmount LTV Q5	LnAmount LTV Q4	LnAmount LTV Q3	LnAmount LTV Q2	LnAmount LTV Q1
Treat × Post	0.048* (1.89)	0.054* (1.88)	0.017 (0.46)	-0.042 (-0.86)	0.011 (0.24)
Observations	430215	504066	738507	1096348	2162406
Adjusted R^2	0.679	0.690	0.694	0.660	0.645
Controls	✓	✓	✓	✓	✓
State-Cohort FE	✓	✓	✓	✓	✓
Year-Cohort FE	✓	✓	✓	✓	✓
Contributor-Cohort FE	✓	✓	✓	✓	✓
Panel C: Individual Donations by Top Exemption Change Tercile Sub-sample					
	(1)	(2)	(3)	(4)	(5)
	LnAmount LTV Q5	LnAmount LTV Q4	LnAmount LTV Q3	LnAmount LTV Q2	LnAmount LTV Q1
Treat × Post	0.088*** (2.99)	0.054** (2.58)	-0.010 (-0.39)	0.015 (0.70)	0.018 (0.72)
Observations	420422	505160	747368	1112485	2464223
Adjusted R^2	0.681	0.691	0.693	0.658	0.636
Controls	✓	✓	✓	✓	✓
State-Cohort FE	✓	✓	✓	✓	✓
Year-Cohort FE	✓	✓	✓	✓	✓
Contributor-Cohort FE	✓	✓	✓	✓	✓

Table 10: Extensive Margin: ZIP-Code New Donors by LTV quintile

This table examines the effect of raising homestead exemption limits on new donor entry at the ZIP-level and shows the results of the model specification:

$$\text{LnNewDonor}_{z,t} = \beta(\text{Treat}_s \times \text{Post}_t) + F_{s,t} + \gamma_{s,c,d,z,t} + \epsilon_{z,t}$$

where the table is separately estimated on different samples across LTV quintiles as an inverse proxy for home equity and is calculated by dividing the median mortgage amount at the origination by the Zillow's home value at the ZIP code. Columns (1) through (5) report the results from each regression across LTV quintiles, with LTV Q1 represents the ZIPs with the lowest LTV while LTV Q5 represents ZIPs with the highest LTV. The dependent variable $\text{LnNewDonor}_{z,t}$ is the natural log of one plus the number of new donors residing in ZIP codes z in year t . A new donor is defined as a unique contributor who makes a recorded donation in year t and has no donation in any prior year since 2000. Treat equals one for ZIP code in reformed states and zero otherwise. Post equals one for years after the homestead reform and zero otherwise. $F_{s,t}$ is the matrix of fixed effects, including State-by-cohort (z) and year-by-cohort (t). γ is the matrix of control variables including local economic conditions, household finance indicators, demographic composition, and local political alignment. We add an additional control variable, LnPopulation to control for changes in local population, and LnPopulation is measured as the total tax filers at the ZIP-level. The unit of observation is at ZIP-year-cohort level Standard errors are clustered at the state-year level with t -statistics in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

	(1) <i>LnNewDonor</i> LTV Q5	(2) <i>LnNewDonor</i> LTV Q4	(3) <i>LnNewDonor</i> LTV Q3	(4) <i>LnNewDonor</i> LTV Q2	(5) <i>LnNewDonor</i> LTV Q1
Treat × Post	0.126*** (2.95)	0.076** (2.07)	0.032 (0.87)	-0.014 (-0.34)	-0.030 (-0.67)
Log GDP	0.718 (1.18)	0.756 (1.44)	0.462 (1.03)	0.542 (1.14)	0.824** (1.99)
Ln Income	0.801*** (11.67)	0.776*** (13.53)	0.685*** (12.45)	0.752*** (18.16)	0.770*** (18.86)
Liquid Asset over Income	11.983*** (8.05)	14.078*** (8.51)	13.471*** (14.52)	10.606*** (13.80)	7.582*** (13.93)
Unemployment Rate	0.012* (1.95)	0.010 (1.41)	-0.004 (-0.58)	0.017** (2.29)	0.012* (1.71)
DTI	-0.138*** (-3.71)	-0.318*** (-8.59)	-0.254*** (-7.33)	-0.230*** (-5.89)	-0.125*** (-4.57)
Loan Denial Rate	0.000 (0.05)	0.005** (2.56)	0.005*** (2.68)	0.001 (0.34)	-0.000 (-0.04)
Ln Home Value	0.048 (1.29)	0.208*** (4.77)	0.207*** (4.56)	0.179*** (3.96)	0.142*** (4.84)
Pct. Working Age	1.261*** (5.83)	1.315*** (6.68)	1.135*** (5.52)	0.767*** (3.97)	0.801*** (4.53)
Republican Dist.	-0.019 (-1.02)	0.020 (0.96)	-0.008 (-0.49)	-0.023* (-1.84)	0.003 (0.20)
Pct. College Grad.	2.687*** (19.06)	2.018*** (16.98)	1.968*** (18.97)	1.858*** (17.03)	1.586*** (16.42)
Avg. Family Size	-0.187*** (-4.92)	-0.158*** (-3.70)	-0.205*** (-5.94)	-0.178*** (-4.68)	-0.131*** (-4.36)
Ln Population	0.773*** (41.90)	0.848*** (52.10)	0.891*** (73.88)	0.892*** (84.36)	0.884*** (71.96)
Observations	60871	58865	58524	59257	60278
Adjusted R^2	0.717	0.765	0.812	0.834	0.873
State-Cohort FE	✓	✓	✓	✓	✓
Year-Cohort FE	✓	✓	✓	✓	✓

Table 11: New Donor Entry Analysis by Parties

This table examines the effect of raising homestead exemption limits on new donor entry at the ZIP-level and shows the results of the model specification:

$$IHSNewParty_{z,t} = \beta(Treat_s \times Post_t) + F_{s,t} + \gamma_{s,c,d,z,t} + \epsilon_{z,t}$$

where the table is separately estimated on different samples across LTV quintiles as an inverse proxy for home equity and is calculated by dividing the median mortgage amount at the origination by the Zillow's home value at the ZIP code. Columns (1) through (5) report the results from each regression across LTV quintiles, with LTV Q1 represents the ZIPs with the highest LTV while LTV Q5 represents ZIPs with the lowest LTV. The dependent variable $IHSNewParty_{z,t}$ is the inverse hyperbolic sin of the number of new donors to either Republican or Democratic party residing in ZIP codes z in year t , where $IHSNewRep$ represents new Republican donor and $IHSNewDem$ represents new Democratic donor. A new democratic donor is defined as a new unique donor who has a partisanship leaning to the democratic party. Donor partisan leaning is defined as the ratio of the difference between her Democratic and Republican donations over her total donations. If the ratio is less than 0, then the donor is defined as a Democratic donor. $Treat$ equals one for ZIP code in reformed states and zero otherwise. $Post$ equals one for years after the homestead reform and zero otherwise. $F_{s,t}$ is the matrix of fixed effects, including State-by-cohort (z) and year-by-cohort (t). γ is the matrix of control variables including local economic conditions, household finance indicators, demographic composition, and local political alignment. We add an additional control variable, $LnPopulation$ to control for changes in local population, and $LnPopulation$ is measured as the total tax filers at the ZIP-level. The unit of observation is at ZIP-year-cohort level Standard errors are clustered at the state-year level with t -statistics in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

Panel A: New Republican Donors					
	(1) <i>IHSNewRep</i> LTV Q5	(2) <i>IHSNewRep</i> LTV Q4	(3) <i>IHSNewRep</i> LTV Q3	(4) <i>IHSNewRep</i> LTV Q2	(5) <i>IHSNewRep</i> LTV Q1
Treat × Post	0.125*** (2.79)	0.089* (1.93)	0.046 (1.04)	-0.023 (-0.47)	-0.046 (-0.91)
Observations	60871	58865	58524	59257	60278
Adjusted R^2	0.611	0.676	0.732	0.755	0.794
Controls	✓	✓	✓	✓	✓
State-Cohort FE	✓	✓	✓	✓	✓
Year-Cohort FE	✓	✓	✓	✓	✓
Panel B: New Democratic Donors					
	(1) <i>IHSNewDem</i> LTV Q5	(2) <i>IHSNewDem</i> LTV Q4	(3) <i>IHSNewDem</i> LTV Q3	(4) <i>IHSNewDem</i> LTV Q2	(5) <i>IHSNewDem</i> LTV Q1
Treat × Post	0.087** (2.09)	0.067 (1.58)	0.039 (0.91)	0.051 (1.05)	0.034 (0.58)
Observations	60871	58865	58524	59257	60278
Adjusted R^2	0.577	0.631	0.693	0.720	0.803
Controls	✓	✓	✓	✓	✓
State-Cohort FE	✓	✓	✓	✓	✓
Year-Cohort FE	✓	✓	✓	✓	✓

Table 12: Homestead Exemption on Charitable Donations

This table examines the effect of raising homestead exemption limits on individual campaign donations and shows the results of the model specification:

$$IHSCCharitable_{z,t} = \beta(Treat_s \times Post_t) + F_{s,t} + \gamma_{s,c,d,z,t} + \epsilon_{z,t}$$

where the table is separately estimated on different samples across LTV quintiles, which group donors based on the home equity of households in their ZIP code to capture different household exposure to homestead exemption. LTV is an inverse proxy for home equity and is calculated by dividing the median mortgage amount at the origination by the Zillow reported housing value at the ZIP code. Columns (1) through (5) reports the results from each regression across LTV quintiles, where LTV Q1 represents donors residing in ZIPs with lowest LTV while LTV Q5 represents donors residing in ZIPs with highest LTV. The dependent variable, $IHSCCharitable_{z,t}$, is the inverse hyperbolic sin of ZIP-code level average charitable donations per year. The average charitable donation is measured by dividing the total charitable donations at the ZIP code by the total tax filers at the ZIP code. $Treat$ equals one for ZIP codes in states that reformed its homestead exemption and zero otherwise. $Post$ equals one for years after the homestead reform and zero otherwise. The interaction term $Treat \times Post$ captures the effect of homestead reform on individual donations. $F_{s,t}$ is the matrix of fixed effects, including state-by-cohort (s) and year-by-cohort. γ is the matrix of control variables including local economic conditions (GDP, unemployment rate, household income), household finance indicators (debt to income, liquid asset to income, mortgage loan denial rate, home value), demographic composition (working age share, college graduate share, family size), and local political alignment (Republican vote share). We also include the natural logarithm of the tax filers to control for size effects since the average charitable donation is measured based on local population. The unit of observation is at the ZIP-year-cohort level. Standard errors are clustered at the state-year level with t -statistics in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

	(1) <i>IHSCCharitable</i> LTV Q5	(2) <i>IHSCCharitable</i> LTV Q4	(3) <i>IHSCCharitable</i> LTV Q3	(4) <i>IHSCCharitable</i> LTV Q2	(5) <i>IHSCCharitable</i> LTV Q1
Treat × Post	0.060*** (3.26)	0.039*** (3.01)	0.015 (1.61)	0.004 (0.41)	0.000 (0.02)
Observations	60871	58865	58524	59257	60278
Adjusted R^2	0.826	0.919	0.947	0.950	0.949
Controls	✓	✓	✓	✓	✓
State-Cohort FE	✓	✓	✓	✓	✓
Year-Cohort FE	✓	✓	✓	✓	✓

Table 13: Homestead Exemption on Average Political Donation per Capita

This table examines the effect of raising homestead exemption limits on individual campaign donations and shows the results of the model specification:

$$IHSPolitical_{z,t} = \beta(Treat_s \times Post_t) + F_{s,t} + \gamma_{s,c,d,z,t} + \epsilon_{z,t}$$

where the table is separately estimated on different samples across LTV quintiles, which group donors based on the home equity of households in their ZIP code to capture different household exposure to homestead exemption. LTV is an inverse proxy for home equity and is calculated by dividing the median mortgage amount at the origination by the Zillow reported housing value at the ZIP code. Columns (1) through (5) reports the results from each regression across LTV quintiles, where LTV Q1 represents donors residing in ZIPs with lowest LTV while LTV Q5 represents donors residing in ZIPs with highest LTV. The dependent variable, $IHSPolitical_{z,t}$, is the inverse hyperbolic sin of ZIP-code level average political donations per year. The average political donation is measured by dividing the total political donations at the ZIP code by the total tax filers at the ZIP code. $Treat$ equals one for ZIP codes in states that reformed its homestead exemption and zero otherwise. $Post$ equals one for years after the homestead reform and zero otherwise. The interaction term $Treat \times Post$ captures the effect of homestead reform on individual donations. $F_{s,t}$ is the matrix of fixed effects, including state-by-cohort (s) and year-by-cohort. γ is the matrix of control variables including local economic conditions (GDP, unemployment rate, household income), household finance indicators (debt to income, liquid asset to income, mortgage loan denial rate, home value), demographic composition (working age share, college graduate share, family size), and local political alignment (Republican vote share). We also include the natural logarithm of the tax filers to control for size effects since the average charitable donation is measured based on local population. The unit of observation is at the ZIP-year-cohort level. Standard errors are clustered at the state-year level with t -statistics in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

	(1) <i>IHSPolitical</i> LTV Q5	(2) <i>IHSPolitical</i> LTV Q4	(3) <i>IHSPolitical</i> LTV Q3	(4) <i>IHSPolitical</i> LTV Q2	(5) <i>IHSPolitical</i> LTV Q1
Treat × Post	0.130*** (3.68)	0.092*** (2.75)	0.027 (0.86)	0.008 (0.22)	0.000 (0.01)
Observations	60871	58865	58524	59257	60278
Adjusted R^2	0.436	0.462	0.554	0.651	0.777
Controls	✓	✓	✓	✓	✓
State-Cohort FE	✓	✓	✓	✓	✓
Year-Cohort FE	✓	✓	✓	✓	✓

Table 14: New Republican Donors to Republican Governors

This table examines the effect of raising homestead exemption limits on individual campaign donations and shows the results of the model specification:

$$IHSNewRep_{z,t} = \beta(Treat_s \times Post_t \times GovParty_{s,t}^{REP}) + \theta + F_{s,t} + \gamma_{s,c,d,z,t} + \epsilon_{z,t}. \quad (3)$$

where the table is separately estimated on different samples across LTV quintiles, which group donors based on the home equity of households in their ZIP code to capture different household exposure to homestead exemption. LTV is an inverse proxy for home equity and is calculated by dividing the median mortgage amount at the origination by the Zillow reported housing value at the ZIP code. Columns (1) through (5) reports the results from each regression across LTV quintiles, where LTV Q1 represents donors residing in ZIPs with lowest LTV while LTV Q5 represents donors residing in ZIPs with highest LTV. We restrict the sample to the 8 states that have officially link governors to the homestead exemption reforms. The dependent variable, $IHSNewRep_{z,t}$, is the inverse hyperbolic sin of new Republican donors entry per ZIP code per year. $GovParty_{s,t}^{REP}$ is a dummy variable equals 1 for Republican governor and 0 otherwise. $Treat$ equals one for ZIP codes in states that reformed its homestead exemption and zero otherwise. $Post$ equals one for years after the homestead reform and zero otherwise. The interaction term $Treat \times Post$ captures the effect of homestead reform on individual donations. θ captures the lower-order terms of the DDD estimator. $F_{s,t}$ is the matrix of fixed effects, including state-by-cohort (s) and year-by-cohort. γ is the matrix of control variables including local economic conditions, household financial indicators, demographic composition, and local political alignment. We also include the natural logarithm of the tax filers to control for size effects. The unit of observation is at the ZIP-year-cohort level. Standard errors are clustered at the state-year level with t -statistics in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

	(1) <i>IHSNewRep</i> LTV Q5	(2) <i>IHSNewRep</i> LTV Q4	(3) <i>IHSNewRep</i> LTV Q3	(4) <i>IHSNewRep</i> LTV Q2	(5) <i>IHSNewRep</i> LTV Q1
Treat × Post	0.152** (2.07)	0.183** (2.41)	0.131* (1.85)	0.014 (0.20)	0.043 (0.62)
governor_rep	0.067 (1.13)	0.067 (1.02)	0.032 (0.66)	-0.006 (-0.11)	-0.017 (-0.24)
Treat × governor_rep	-0.042 (-0.35)	0.068 (0.48)	0.264* (1.66)	0.338** (1.98)	0.228 (1.49)
Post × governor_rep	0.080 (1.44)	0.034 (0.60)	-0.008 (-0.17)	-0.005 (-0.11)	0.018 (0.35)
Treat × Post × governor_rep	-0.002 (-0.02)	-0.161 (-1.13)	-0.246 (-1.65)	-0.123 (-0.75)	-0.079 (-0.56)
Observations	40192	37926	37700	38239	38049
Adjusted R^2	0.619	0.680	0.736	0.760	0.796
Controls	✓	✓	✓	✓	✓
State-Cohort FE	✓	✓	✓	✓	✓
Year-Cohort FE	✓	✓	✓	✓	✓

Table 15: New Democratic Donors to Democratic Governors

This table examines the effect of raising homestead exemption limits on individual campaign donations and shows the results of the model specification:

$$IHSNewDEM_{z,t} = \beta (Treat_s \times Post_t \times GovParty_{s,t}^{DEM}) + \theta + F_{s,t} + \gamma_{s,c,d,z,t} + \epsilon_{z,t}. \quad (4)$$

where the table is separately estimated on different samples across LTV quintiles, which group donors based on the home equity of households in their ZIP code to capture different household exposure to homestead exemption. LTV is an inverse proxy for home equity and is calculated by dividing the median mortgage amount at the origination by the Zillow reported housing value at the ZIP code. Columns (1) through (5) reports the results from each regression across LTV quintiles, where LTV Q1 represents donors residing in ZIPs with lowest LTV while LTV Q5 represents donors residing in ZIPs with highest LTV. We restrict the sample to the 8 states that have officially link governors to the homestead exemption reforms. The dependent variable, $IHSNewDEM_{z,t}$, is the inverse hyperbolic sin of new Republican donors entry per ZIP code per year. $GovParty_{s,t}^{DEM}$ is a dummy variable equals 1 for Democratic governor and 0 otherwise. $Treat$ equals one for ZIP codes in states that reformed its homestead exemption and zero otherwise. $Post$ equals one for years after the homestead reform and zero otherwise. The interaction term $Treat \times Post$ captures the effect of homestead reform on individual donations. θ captures the lower-order terms of the DDD estimator. $F_{s,t}$ is the matrix of fixed effects, including state-by-cohort (s) and year-by-cohort. γ is the matrix of control variables including local economic conditions, household financial indicators, demographic composition, and local political alignment. We also include the natural logarithm of the tax filers to control for size effects. The unit of observation is at the ZIP-year-cohort level. Standard errors are clustered at the state-year level with t -statistics in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

	(1) <i>IHSNewDem</i> LTV Q5	(2) <i>IHSNewDem</i> LTV Q4	(3) <i>IHSNewDem</i> LTV Q3	(4) <i>IHSNewDem</i> LTV Q2	(5) <i>IHSNewDem</i> LTV Q1
Treat × Post	0.139* (1.76)	0.030 (0.29)	0.123 (1.12)	0.078 (0.70)	0.147 (1.38)
governor_dem	-0.053 (-0.61)	0.015 (0.20)	-0.070 (-1.22)	-0.052 (-0.91)	-0.045 (-0.61)
Treat × governor_dem	0.053 (0.36)	-0.181 (-1.18)	0.007 (0.04)	-0.257* (-1.69)	-0.129 (-0.99)
Post × governor_dem	0.005 (0.07)	-0.029 (-0.46)	-0.015 (-0.31)	-0.021 (-0.42)	0.021 (0.37)
Treat × Post × governor_dem	-0.145 (-1.19)	0.065 (0.45)	-0.096 (-0.74)	-0.094 (-0.69)	-0.172 (-1.34)
Observations	40192	37926	37700	38239	38049
Adjusted R^2	0.581	0.631	0.692	0.719	0.799
Controls	✓	✓	✓	✓	✓
State-Cohort FE	✓	✓	✓	✓	✓
Year-Cohort FE	✓	✓	✓	✓	✓

Appendix Table A1: Definitions of Variables

This table reports the definitions of all variables. The first column reports the notation of the variables. The second column reports the definition of the variables. Percent variables are expressed in percentage points.

Variable	Definition
LnAmount	Natural log of one plus the total dollar amount donated by donor i in year t .
IHSNewDonor	Inverse Hyperbolic sin of new donors at ZIP z in year t (no donation recorded in any prior panel year since 2000).
IHSNewRep	Inverse Hyperbolic sin of new Republican donors at ZIP z in year t (no donation recorded in any prior panel year since 2000).
IHSNewDem	Inverse Hyperbolic sin of new Democratic donors at ZIP z in year t (no donation recorded in any prior panel year since 2000).
IHSChartible	Inverse Hyperbolic sin of charitable donation deduction divided by tax filers at ZIP z in year t (no donation recorded in any prior panel year since 2000).
IHSPolitical	Inverse Hyperbolic sin of total political donation divided by tax filers at ZIP z in year t (no donation recorded in any prior panel year since 2000).
Ln GDP	Natural log of real GDP in state s in year t .
Ln Income	Natural log of average household income at ZIP z in year t .
Liquid Asset over Income	income from annuity and equity assets over total income at ZIP z in year t
Unemployment Rate	Unemployment rate (percent) in county c in year t .
DTI	Median level of income divided by mortgage amount at origination at ZIP z in year t .
LTV	Mortgage amount at origination divided by Zillow housing value index at ZIP z in year t .
Loan Denial Rate	Percentage of mortgage applications denied at ZIP z in year t .
Ln Home Value	Natural log of median home value at ZIP z in year t .
Republican Dist.	A dummy variable equal to one if the district is held by a republican representative in district d in year t , otherwise zero
Pct. Working Age	Percentage of population aged 18–64 at ZIP z in year t .
Pct. College	Percentage of adults with bachelor's degree or higher at ZIP z in year t .
Avg. Family Size	Average persons per family at ZIP z in year t .
Ln Population	Natural log of the number of tax filers at ZIP z in year t .

Inventor Entrepreneurship

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Abstract

This paper examines how founders' prior inventive experience affects startup performance. Using a leave-one-out shift-share design, we provide causal evidence that technological change-induced inventor entrepreneurship increases the likelihood of high-value acquisition while reducing the likelihood of going public. These effects are driven by inventor founders with up-to-date and industry-relevant technological expertise. We also find that inventor-founded startups generate more novel patents and are more likely to produce top-cited patents early in their life cycle. We find no support for a killer-acquisition hypothesis. Instead, the results are consistent with a technology-acquisition explanation, where acquiring firms become more likely to cite the innovations of inventor-founded targets after the acquisition.

Keywords: Entrepreneurship, Inventor founders, Technology, Performance

JEL classification: L26, J24, O33, L25

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I. INTRODUCTION

Startups play an important role in modern economy through job creation and innovation. Yet entrepreneurial outcomes are highly skewed: nearly half of new businesses fail within five years, and only a small fraction grow to make sustained economic contributions (Decker et al., 2014).¹ This stark heterogeneity raises a fundamental question: what makes startups successful? Understanding what differentiates successful startups from those that fail remains a fundamental question at the intersection of entrepreneurship, innovation, and finance. Prior studies emphasize the value of entrepreneurial human capital (e.g., Queiró, 2022; Smith et al., 2019), and document the importance of technical talent for innovative startups (Balsmeier et al., 2025; Gu et al., 2022). However, causal evidence on the effect of entrepreneurial technical human capital on startup performance remains limited (Guzman and Stern, 2020), and the underlying mechanisms are even less understood.

In this paper, we address this gap by focusing on a specific form of entrepreneurial technical human capital: inventive experience. This focus allows us to identify founders' technological fields and track the evolution of their knowledge over time. Intuitively, inventive experience helps founders recognize and exploit emerging technological opportunities, creating competitive advantages. However, inventive expertise may also depreciate rapidly in fast-moving technological environments (Aghion et al., 2024; Deming and Noray, 2020), potentially placing technically specialized founders at a disadvantage relative to their peers. Therefore, whether founders' prior inventive experience ultimately enhances or hinders startup performance is an open empirical question.

The key challenge in establishing a causal relationship between founder inventive experience and startup performance lies in the self-selection of inventors into entrepreneurship. Unobservable characteristics (e.g., intelligence) may both motivate inventors to become founders and influence the performance of their firms. To address potential omitted variable issue, we adopt a shift-

¹This is also true for VC-backed firms. According to Carta, 48% of startups founded in 2018 that raised at least \$1 million went out of business within five years of founding. https://www.linkedin.com/posts/peterjameswalker_cartadata-shutdowns-startups-activity-7176621421048074241-1c5T

share instrumental variable (SSIV) approach, a method increasingly used in economics and finance research to address endogeneity (e.g., [Autor et al., 2013](#); [Borusyak et al., 2022](#); [Mohnen, 2025](#)). Specifically, we construct our instrument using weighted technology changes, where the weights reflect each U.S. county's inventor endowments. The rationale for our SSIV is that technology changes provide future inventor founders with comparative advantages, as their technical expertise enables them to better understand emerging trends and exploit potential technological opportunities ([Acemoglu et al., 2006](#); [Nelson and Phelps, 1966](#)). While local supply of technologically relevant inventors captures the county's exposure to technological change. Counties with more inventors possessing expertise in the technology fields undergoing dynamic changes are more likely to see those inventors respond to technological shocks by establishing inventor-founded firms.

We implement our identification strategy as follows. We proxy technology change by using the intensity of breakthrough patents, which reflect the discontinuous advances in the technological frontier ([Kelly et al., 2021](#)). Our identification relies on the assumption that the timing of breakthrough innovation is exogenous to individual inventors. The local inventor supply is predetermined to avoid potential endogeneity from technological changes. We specifically consider inventors in their 40s at the time of firm founding as potential inventor founders, based on the mean founder age in patenting startups reported by [Azoulay et al. \(2020\)](#). The local inventor supply is therefore calculated as the proportion of inventors who will be aged 40-49 at the time of founding, specialized in a specific technological field within a given county, relative to the total number of such inventors in the U.S.

Given our focus on innovative startups, we restrict the sample to patenting firms, with a particular focus on high-tech industries. Our main analysis sample includes 6,643 innovative startups founded between 2005 and 2015, 54.2% of which are founded by inventors. Our analysis first suggests that firms founded by inventors are more likely to be acquired and less likely to go public. Economically, being inventor-founded leads to 15.7% lower likelihood of achieving an IPO and a 45.5% higher likelihood of being acquired. The results are robust when considering only high-value acquisitions or when using a longer observation window for IPO outcomes. Our findings

suggest that exit outcomes are partly shaped by a startup's founding structure, with technology-motivated ventures exhibiting a greater propensity to exit via acquisition. This pattern is also consistent with the broader shift in startup exits over recent decades, during which acquisitions overtook IPOs as the dominant exit route (Ederer and Pellegrino, 2023; Huang et al., 2023).

To understand how inventor founders influence startup success, we examine heterogeneity in their technological knowledge. We find that inventors with up-to-date, industry-relevant technological expertise are more responsive to technological change by establishing firms. Startups founded by these inventors are more likely to be acquired and less likely to go public. Overall, our findings suggest that inventor-founded firms are more likely to be successful, with inventor founders' up-to-date and relevant knowledge playing a key role in their firms' success.

We next explore why firms acquire inventor-founded startups. We first examine two channels that could facilitate the acquisition of inventor-founded firms: innovation and venture capital (VC) funding, and then investigate whether acquirers integrate the targets' innovations into their own patents after the acquisition. Our results indicate that inventor-founded firms outperform their counterparts in both the quantity and quality of innovation early in their life cycle. Specifically, being inventor-founded is associated with a 264% increase in patents and a 126% increase in novel patents in the first year after founding. In addition, inventor-founded firms have a 34.4% and 44% higher likelihood of producing top-cited patents in the first and second year after founding, respectively. In terms of VC funding, we find that inventor-founded firms are more likely to receive corporate venture capital (CVC) funding, though no significant effect is found for VC funding. However, inventor-founded firms receive a larger first-round VC investment compared to their VC-backed peers. To differentiate the motive for acquiring inventor-founded firms, we employ a stacked event-study design comparing acquirers' citations to the acquired inventor-founded firms' patents with citations to a matched set of "twin" patents before and after acquisition. The results indicate that acquirers are more likely to cite the target's patents after acquisition, particularly their novel patents. This pattern is inconsistent with the killer acquisition hypothesis, which predicts that acquirers suppress novel innovations and are less likely to integrate them.

We further investigate the local impacts of inventor entrepreneurship. We find that counties with a greater concentration of inventor-founded firms attract larger inflows of technologically proximate inventors, including star inventors. Moreover, such counties exhibit higher levels of patenting activity and generate more radical patents. These findings also point to a mechanism underlying the agglomeration of inventors and the formation of technology clusters: inventor-founded firms draw technologically related inventors into the county, and the resulting expansion of the local inventor pool, in turn, fosters subsequent rounds of inventor entrepreneurship.

This research contributes to several strands of literature. First, our study adds to the emerging literature on the role of inventors in the economy. For example, [Balsmeier et al. \(2025\)](#) find that the inflow of inventors increases both the number and quality of local VC-backed firms. [Akcigit and Goldschlag \(2025\)](#) show that inventor-founded firms exhibit higher employment growth rates. Our research differs by specifically focusing on inventor founders and their impact on other dimensions of firm outcomes, such as startup exit and innovation productivity. A closely related work is by [Islam and Zein \(2020\)](#), who find that firms led by inventor CEOs are associated with better innovation performance. However, there are key differences between their study and ours. Their research focuses on publicly listed U.S. firms, whereas our study examines firms from their very inception. Theoretical framework suggests that entrepreneurs are critical in the early stages of a firm's development, where they leverage their human capital to differentiate the firm and create value ([Rajan, 2012](#)). Therefore, we investigate the role of inventor leaders at different stages of the firm's development. Additionally, [Kaplan et al. \(2009\)](#) show that most founders of VC-backed firms are no longer serving as CEO by the time of the IPO. Hence, the identity, role and impact of post-IPO CEOs may differ from those of the founders that we investigate in our sample. Thus, our paper complements and extends the study by [Islam and Zein \(2020\)](#) by providing important lessons for the investors and stakeholders of unlisted startups, including for the small subset of startups that are eventually listed.

Second, our research also relates to research on knowledge obsolescence. Technological advances can render human capital obsolete while providing younger workers with entry opportu-

nities (MacDonald and Weisbach, 2004). Empirically, Deming and Noray (2020) find that young workers earn more during periods of rapid skill change, particularly in STEM occupations. Relatedly, Aghion et al. (2024) suggest that the arrival of new inventions negatively affects the wage returns of employees who are further from the human capital frontier and increases their probability of exiting employment. In the context of innovation, Kaltenberg et al. (2023) show that the quality and disruptiveness of patents decline as inventors age. We contribute to this literature by showing that technological change creates opportunities for inventor founders with up-to-date knowledge, which in turn contributes to startup success. We also provide evidence on the underdeveloped area of how entrepreneurs' knowledge affects startup success (Gofman and Jin, 2024).

Third, we contribute to the literature examining how entrepreneur characteristics shape entrepreneurial success. Previous studies have explored various founder characteristics, such as age (Azoulay et al., 2020), prior business experience (Lafontaine and Shaw, 2016), and schooling (Queiró, 2022). Our study aims to establish a causal relationship between founders' inventive experience and startup success, and to identify the underlying mechanism driving this relationship. Moreover, rather than focusing on self-employed entrepreneurship broadly, this research specifically examines startups in high-tech industries, aligning with the burgeoning literature on high-growth entrepreneurship (e.g., Azoulay et al., 2020; Decker et al., 2014).

Finally, we add to the literature on the factors that influence individuals' selection into entrepreneurship. Behavioural research suggests that entrepreneurs may have heterogeneous preferences on risk, nonpecuniary benefits (Åstebro et al., 2014). Non-behavioural research examines the impact of external environment. Recently, researchers have emphasized advanced knowledge as an important driver of high-growth entrepreneurship. For example, Gofman and Jin (2024) suggest that the brain drain of professors reduces the AI-related knowledge of future startup founders, which in turn negatively impacts firm formation. One closely related study is by Bernstein et al. (2022), who find that young and skilled individuals are more likely to respond to locally arising business opportunities, highlighting the role of the local population's demographic composition in shaping entrepreneurship. Our study specifically examines how technological change influences

inventors' selection into entrepreneurship, with the demographic composition of local inventors playing a key role.

The rest of the paper is organized as follows. Section II details the methodology, including data and the identification strategy. Section III reports the empirical findings. Section IV presents robustness checks, and Section V concludes.

II. METHODOLOGY

A. Data

A.1. Startup

Our sample includes US patenting firms that founded between 2005 and 2015. The starting year, 2005, is chosen due to constraints in patents application data. We primarily utilize startups' patent applications to identify inventor founders. While both the granted and failed patent applications only became available following the enactment of the American Inventors Protection Act (AIPA) in late 2000. We limit the truncation bias by focusing on firms founded after 2005. Given the lag between patent application and grant, our patent data ends in 2020 (Lerner and Seru, 2022). We set the cutoff founding year to 2015 to ensure at least five years for analysing innovation performance. Other firm information, such as exit outcomes, is observed from 2005 through 2024.

To identify U.S. patenting startups, we first extract firms founded between 2005 and 2015 from the dataset provided by Ewens and Marx (2024), which reports founding years for U.S.-based assignees of USPTO patents granted between 1976 and 2021. Assignee names are then fuzzy matched with companies' names on Crunchbase, using both current and previous firm names.² Matches are dropped if the founding years in the two datasets differ by more than 5 years, if the patent filing state misalign with the firm's headquarter state, or if the founder or industry information is missing. We double-check and correct the founding year for the remaining matches based

²Crunchbase's broad coverage of innovative startups is well credited in previous studies (e.g., Gofman and Jin, 2024)

on other data sources (e.g., pitchbook, company website).

Crunchbase classifies a startup’s industry by multiple different tags without providing a standard industry index. We use Python package BERTopic to cluster firms, which initially generates 22 industry clusters. As our focus is on high-tech industries, we exclude firms from traditional sectors such as commerce.³ The detailed industry composition is provided in [Appendix A](#). We then match these industries to 4-digit International Standard Industrial Classification (ISIC) codes to map their corresponding technology classes ([Lybbert and Zolas, 2014](#)).⁴

We collect firm information from Crunchbase, such as founding team, headquarter location, funding history, and exit events. We only focus on the first exit event of startups. Firm’s exit event information is checked and supplemented with SDC platinum and capital IQ. All acquisition events in our sample involve majority acquisitions or transactions made to obtain majority control. For IPOs, we only include listings on major stock exchanges and exclude SPACs. Our main analysis sample includes 6,643 innovative startups headquartered in 124 US counties.

A.2. Patent and Inventor

Our granted patent data is sourced from [Ewens and Marx \(2024\)](#). We supplement this with unsuccessful patent application data from the Patent Assignment Dataset, which provides detailed assignment information for patents and patent applications from 1970 to 2023. Using fuzzy matching between standardized assignee and startup names, we extract matches with scores above 90. We only keep applications where the filing state is consistent with the firm’s headquarter state and manually verify the remaining pairs that are not exact matches.

The inventor information is sourced from PatentsView, an open data platform that offers disambiguated inventor data, including unique identification numbers (IDs), gender, and locations. We match the names of founders with inventors who had filed patent applications in the respective

³We exclude 926 firms in commerce (e.g., consumer goods), 126 in agriculture, and 80 in energy (primarily oil and gas). We also exclude firms classified as Nanotechnology (72) and 3D printing (98), as these categories are too broad to assign to a single industry and would introduce noise if included

⁴We thank Nikolas Zolas for generously providing the replication files. We construct the ISIC–CPC concordance using 1990–2003 USPTO utility patents.

firms. There is concern that some founders may have filed patents before founding the firm but haven't filed any after founding the firm. For those founders without any inventing history in their founded firms, we collect self-reported patent information on their LinkedIn profiles. We track the inventing history of inventor IDs associated with identified patents and applications. An inventor founder is defined as a founder with prior inventive experience before founding the firm.

To construct the dataset of local inventors, we use the location submitted by inventors when filing patents. Following the practice of previous studies (e.g., [Balsmeier et al., 2025](#)), we assume that inventors remain in the county where they were previously located before filing a patent in another county. For inventors who file patents in multiple counties within a given year, we take the most frequent filing location. An inventor's specialized technology is defined as the set of technology classes in which she has filed patents up to a given year. We access inventors' birthyear data from the dataset provided by [Kaltenberg et al. \(2023\)](#), which covers the birthyear of 1.4 million US inventors active between 1976 and 2018. We identify 506,245 inventors with available birth year information and filed at least one patent during 1999 and 2009 across 2,803 US counties.

B. Identification Strategy

B.1. Shift-share Design

Our motivation for shift-share IV comes from both demand and supply for inventor founders. On the demand side, firms need entrepreneurs who can understand and implement new technologies when industries experience dramatic changes in underlying technological knowledge ([Nelson and Phelps, 1966](#); [Acemoglu et al., 2006](#)). The demand for an inventor on the founding team is greater when the technology is approaching frontier, because their technical expertise helps firms navigate rapid technology change and capture emerging opportunities. In our setting, technology change serves as the “shift” (“shock”) for the instrument.

We proxy technology change by using the intensity of breakthrough patents, which reflect the discontinuous advances in the technological frontier ([Kelly et al., 2021](#)). Specifically, the “shift” captures the average intensity of breakthrough patents in a given technology area across US. To

avoid any mechanical correlations, we use a leave-one-out strategy and exclude breakthrough patents in the focal county. The construction of technology change index is as follows:

$$TechChange_{-c,k,t} = \frac{\sum_{q=1, q \neq c}^N \frac{TotalBreakthroughPatent_{k,q,t}}{TotalPatent_{k,q,t}}}{N-1} \quad (1)$$

where k, q , and t denote technology, county, and year, respectively. Technology change affects industries to different extents. To capture technology change relevant to each industry, we weight technology change by the importance of each technology to each industry, using the patent-industry concordance constructed with the Algorithmic Links with Probabilities (ALP) method (Lybbert and Zolas, 2014).

The local supply of technologically relevant inventors captures the county’s exposure to technological change. Counties with more relevant inventors are more exposed to technology change and thus more likely to create inventor-founded firms. We focus on the supply of inventors in their forties at the founding year, based on the evidence on the average age of founders of patenting firms (Azoulay et al., 2020).⁵ This focus also helps mitigate concerns that our instrument may capture the effects of inventor employees, since middle-aged inventors are more likely to be employed by large firms, whereas young firms tend to attract younger inventors (Akcigit and Goldschlag, 2025). We calculate the local inventor supply five years prior to technology change (six years before the founding year) to avoid the impact of technology change on inventor mobility.⁶ Specifically, the local inventor supply is the proportion of inventors who will be aged 40-49 at the time of founding, specialized in a specific technological field within a given county, relative to the total number of such inventors in the U.S. For example, for the 2006 founding year cohort, we focus on inventors

⁵We plot the distribution of breakthrough innovation intensity across inventor founder age groups in Appendix B. Across all technology classes, founders in their forties are no less responsive to technological change. Notably, inventors specializing in medical and biochemical fields are especially responsive to technological change within this age group.

⁶Inventors are immobile and become less likely to change jobs (Akcigit and Goldschlag, 2025). Therefore, inventors who filed patents in their late 30s in a given county are likely to remain in that county during their 40s. In addition, for each county and technology class, we examine the correlation between the predicted ratio of inventors and the actual ratio of inventors. The correlation coefficient is 0.99 and is statistically significant at the 1% level.

born between 1957 and 1966 and examine their distribution across U.S. counties in the year 2000.

We formally define our shift-share IV as follows:

$$SSIV_{j,c,t-1} = \sum_{k=1}^K \underbrace{w_{k,j} \cdot \frac{Inv_{k,c,t-6}}{Inv_{k,t-6}}}_{shares} \cdot \underbrace{TechChange_{-c,k,t-1}}_{shifts} \quad (2)$$

where k, c, j , and t denote technology, county, industry, and founding year, respectively. $w_{k,j}$ represents the frequency of technology k used in industry j . $Inv_{k,c,t-6}$ denotes the number of targeted inventors in county c in year $t-6$. $SSIV_{j,c,t-1}$ is thus a weighted sum of technology changes, where the weights reflect the local inventor endowments. In the empirical analyses, we standardize $SSIV_{j,c,t-1}$ to have a mean of 0 and a variance of 1.

Our identification relies on exogenous shifts, assuming the timing of breakthrough innovations is exogenous to individual inventors. In such setting, the exogeneity of the “shares” doesn’t constitute a necessary condition for shift share design as long as the “shifts” are plausibly randomly assigned (Borusyak et al., 2022). Our preferred model for testing the causal relationship between founder prior inventive experience and startup outcomes is as follows:

$$InvFounded_{i,t} = \alpha_0 + \beta_1 SSIV_{j,c,t-1} + X_{j,c,t-6} + \beta_{j,c} + \gamma_{c,t} + \theta_{i,t} \quad (3)$$

$$Y_i = \alpha_0 + \beta_2 \widehat{InvFounded}_{i,t} + X_{j,c,t-6} + \beta_{j,c} + \gamma_{c,t} + \theta_{i,t} \quad (4)$$

where i, c, j , and t denote firm, county, industry, and founding year, respectively. $SSIV_{j,c,t-1}$ is our shift-share instrument variable. Y_i includes a battery of measures for the firm outcomes. $InvFounded_{i,t}$ is a dummy variable equal to 1 if the founding team includes at least one inventor, and 0 otherwise. We include county-industry fixed effects to absorb time-invariant county-industry characteristics, such as local labour market specialization. Additionally, we include county-founding year fixed effects to control for county-level shocks in the founding year. The term $X_{j,c,t-6}$ controls for the sum of exposure shares, $\sum_{k=1}^K w_{k,j} \cdot \frac{Inv_{k,c,t-6}}{Inv_{k,t-6}}$, addressing the “incomplete shares” issue noted by Borusyak et al. (2022). It controls for local inventor endowments,

since counties specialized in an industry will naturally host more inventors active in related technological fields. Standard errors are clustered at the county level. The variable of interest is β_2 , which measures the effect of founder inventive experience on the firm outcomes. For instance, if inventor-founded firms are more likely to complete an IPO, β_2 should be significantly positive.

B.2. The Validity of Shift-share Design

Following [Borusyak et al. \(2022\)](#), we implement several checks to evaluate the validity of $SSIV_{j,c,t-1}$.

First, a valid shift share research design require many uncorrelated shifts. Table 1 reports summary statistics for the “shifts”. The “shifts”, *TechChange*, is residualized on exposure share controls, county-industry fixed effects, and county-founding year fixed effect. The residualized “shifts” are aggregated to technology-year level. The residualized “shifts” have a mean of 0.044 and a standard deviation of 0.036. $1/HHI$ is the inverse of the Herfindahl-Hirschman Index of average exposure shares, which measures the effective sample size. In our data, the effective sample size is 43, suggesting we still have sizeable shock variation.

Insert Table 1 here.

Second, we present a balance test at the “shift” level. Specifically, we apply the same residualization procedure to predetermined county characteristics and regress the resulting residualized predetermined county characteristics on the residualized “shift”. The results are presented in Table 2. Panel A shows no statistically significant correlation between predetermined macro county characteristics and technology change. In Panel B, we conduct county industry balance test. We still do not find any evidence that technology change predicts predetermined county-industry strength. Overall, the “shift” are balanced with respect to observed county characteristics.

Insert Table 2 here.

III. EMPIRICAL RESULTS

A. Summary Statistics

Table 3 shows summary statistics for both inventor-founded and non-inventor-founded firms. The instrumental variable *SSIV* is normalized to have a mean of 0 and standard deviation of 1, which is not presented in the table. Among the 6,643 innovative startups, 54.2% of them are inventor-founded. Averagely, there are two founders in the founding team. In terms of exit outcomes, 4.1% of firms complete an IPO and 16.9% are acquired within nine years of founding. Acquisitions remain the most common exit route for startups, consistent with recent evidence showing a growing trend of startup sellouts and a decline in IPO activity (Ederer and Pellegrino, 2023). In addition, 8.5% of startups fail, while the majority remain active and privately held.⁷ In terms of funding sources, 80.2% of firms receive VC funding, which aligns with VC's preference for high-tech industries. CVC investment is rare; only 1.8% receive CVC funding.

Insert Table 3 here.

B. Main Results

Table 4 presents the ordinary least squares (OLS) estimates. From the naive OLS regressions in Column 1-3, inventor-founded firms are more likely to complete an IPO or be acquired. However, these regressions should not be interpreted causally. Our analysis primarily focuses on the results from the 2SLS regressions. Columns 4-6 report the reduced-form estimates that regress each outcome directly on the instrument. The results indicate that firms founded following greater technological change are more likely to be acquired but less likely to complete an IPO. This supports the argument that technological changes make selling to a larger organization more attractive, as it allows firms to benefit from economies of scope and accelerate product launches (Gao et al., 2013).

⁷If a firm's operating status is labelled as closed in Crunchbase by 2024, we classify it as out of business. For firms labelled closed but with no recorded close date, we follow previous practice (e.g., González-Urbe, 2020; Li et al., 2023) and assign the close date as five years after the later of the last investment round date or the last patent filing date. If this calculated year exceeds 2024, we set it to 2024.

Insert Table 4 here.

Table 5 reports 2SLS regression results. As expected, there is a significant positive relationship between the instrument and the possibility of a firm being inventor founded in Column 1. For a county with a high number of eligible inventors in a specific technology area, it's more likely to have firms founded by inventors when substantial knowledge changes occur within that technology domain. The first-stage F-statistics is 81, which well exceeds the rule-of-thumb threshold of 10, suggesting that weak instrument is not a concern.⁸ Columns 2-5 report the second-stage estimates. Column 2 shows that inventor-founded firms are more likely to exit via IPO or acquisition. However, the positive effect is driven by acquisition rather than IPO. Column 3 reports the estimate for IPO likelihood, and the coefficient is significantly negative.⁹ While the coefficient on the likelihood of being acquired in Column 4 is positive and statistically significant at the 1% level, suggesting that inventor-founded firms are more likely to be acquired. Specifically, being inventor-founded leads to 15.7% lower likelihood of completing an IPO and a 45.5% higher likelihood of being acquired. Given that only 4% of firms exit via IPO and 17% via acquisition, the effects are economically meaningful. Column 5 reports the estimate for failure likelihood, and no significant effect is found.

Insert Table 5 here.

The finding that inventor-founded firms are more likely to be acquired and less likely to go public raises the question of whether these firms are of high-quality. IPOs are typically viewed as clear indicators of startup success, whereas acquisitions can be used to disguise failure (Balsmeier et al., 2025). To answer this question, we use two alternative measures of high-value acquisitions. The first defines acquisitions with a deal value greater than \$25 million (in 2010 dollars) as high-value, while those with missing deal values or values below this threshold are classified as low-value. The

⁸We also compute the effective F-statistic using stata command *weakivtest*, which is 67.9 and well above the threshold of 23.1.

⁹Given that firms might require a longer period to achieve an IPO, we extend the observation window to ten and eleven years as a robustness check. To do so, we have to exclude the last and last-two founding year cohort, respectively. The results remain consistent with the main findings.

second measure focuses on VC-backed firms, for which we compare the acquisition deal value with the total venture capital raised prior to acquisition. Acquisitions with a deal value-to-investment multiple greater than 1.25 are classified as high-value (Balsmeier et al., 2025). Tabel 6 reports the results. In both Columns, the coefficients are significantly positive, suggesting inventor-founded firms are more likely to exit through high-value acquisitions.

To summarize, we provide evidence that inventor-founded firms are more likely to be acquired and less likely to achieve an IPO. The results remain robust across alternative observation windows and alternative acquisition measures. Our findings suggest that exit outcomes are partly shaped by a startups founding structure, with technology-motivated ventures exhibiting a greater propensity to exit via acquisition. These results also relate to the broader shift in startup exits since the early 1990s, during which acquisitions overtook IPOs as the dominant exit route (Ederer and Pellegrino, 2023; Huang et al., 2023). In our high-tech startup context, an IPO may not always be the optimal choice for entrepreneurs. Going public requires mandatory information disclosure (e.g., clinical trial data), which can expose firms to the risk of revealing technological secrets to competitors and makes tech firms reluctant to pursue an IPO (Ewens and Farre-Mensa, 2022). In contrast, being acquired by a large firm can not only alleviate concerns about tech information disclosure but also allow startups to leverage the strength of the acquirer and benefit from economies of scope (Aggarwal and Hsu, 2014; Eckbo and Lithell, 2025; Gao et al., 2013). Our results also indicate that merging IPOs and acquisitions into one “successful” exit measure may fail to capture meaningful differences in firms exit patterns.

Insert Table 6 here.

C. Inventor Founder Knowledge Heterogeneity

In this section, we explore two sources of heterogeneity in inventor-founders’ technological knowledge: knowledge up-to-dateness and knowledge relevance.

C.1. Knowledge Up-to-dateness

Human capital becomes important when the need for understanding new technology is greater (Acemoglu et al., 2006; Nelson and Phelps, 1966). However, as technology evolves, existing knowledge becomes obsolete, making it more difficult to keep pace with new technological trends. If the above effects are driven by inventor founders' knowledge transfer, one would expect those with more up-to-date knowledge to better understand technology changes and exploit emerging opportunities, thereby enhancing firm quality.

Following the logic used to construct firm-level technological obsolescence (Ma, 2025), we develop a measure of founder knowledge *Up-to-dateness*. The underlying logic is that obsolete knowledge becomes less cited over time, while up-to-date knowledge attracts more citations. To construct this measure, we first collect all patents filed by an entrepreneur at least two years before the firm's founding year t . We then identify the references made by those patents, which form the entrepreneur's knowledge base. Next, we count the forward citations to the knowledge base at time $t - 2$ and $t - 1$.¹⁰ To avoid self-citation biases, we exclude entrepreneur's own patents from both the knowledge base and citations to it. The *Up-to-dateness* measure is defined as:

$$Up-to-dateness_{i,t-1} = \ln(1 + forward\ citations_{i,t-1}) - \ln(1 + forward\ citations_{i,t-2}) \quad (5)$$

where i denotes the founder and t denotes founding year, respectively. Trending knowledge is defined as knowledge with an *Up-to-dateness* value above the median. To analyse whether firms founded by inventors with up-to-date knowledge contribute to firms' success, we classify inventor-founded firms into two groups: firms founded by at least one inventor possessing trending knowledge, and firms founded by inventors with obsolete knowledge. We run separate 2SLS regressions for each subsample, both including non-inventor-founded firms.

Insert Table 7 here.

¹⁰For robustness, we also examine the citation changes between $t - 4$ and $t - 1$. The results still hold.

We report the results in Table 7. Panel A includes both non-inventor-founded firms and firms founded by inventor(s) with up-to-date knowledge, Panel B includes non-inventor-founded firms and firms founded by inventor(s) with obsolete knowledge. The first-stage F-statistic is 31 in Panel A, whereas the instrument is weak in Panel B. This suggests that inventors with up-to-date knowledge are more responsive to technological change by establishing firms. In Column 3 of Panel A, the coefficient is positive and significant at the 5% level, indicating that firms founded by inventors with up-to-date knowledge are more likely to be acquired. Interestingly, we find no significant effect on the likelihood of IPO.

C.2. Knowledge Relevance

In addition to knowledge *Up-to-datedness*, another important dimension to consider is the relevance of knowledge. It's natural to expect that inventor founders with industry-relevant technological expertise are better able to understand the evolution of technology. We classify inventor founders as having relevant knowledge if their technological focus in the founding year falls within technology classes associated with the firm's industry, based on the industry-patent concordance. We then divide inventor-founded firms into two groups: those founded by inventors with relevant knowledge and those founded by inventors with irrelevant knowledge. Separate 2SLS regressions are estimated for each subsample, both of which include non-inventor-founded firms.

We report the results in Table 8. In Panel A, the instrument is strong and positively associated with the likelihood of being inventor-founded, suggesting that inventors with relevant knowledge are more responsive to technological change. In Column 3, the coefficient is positive and significant at the 1% level, indicating that firms founded by inventors with relevant knowledge are more likely to be acquired. The coefficient on IPO likelihood is significantly negative in Column 4, implying that such firms are less likely to go public. Panel B presents a different pattern: the instrument is weak in this subsample, suggesting that inventors with irrelevant knowledge are not responsive to technological change.

Insert Table 8 here.

In summary, we identify a knowledge transfer mechanism through which founders' inventive experience influences firm exit outcomes. Our results suggest that inventors with up-to-date and relevant knowledge are more responsive to technological change by establishing firms, and that firms founded by such inventors are more likely to be acquired. In terms of IPO, we find that firms founded by inventors with relevant knowledge are less likely to exit through an IPO, whereas no significant effect is found for those founded by inventors with up-to-date knowledge.

D. Why Do Firms Acquire Inventor-Founded Startups?

Our results above indicate that inventor-founded firms are not only more likely to be acquired but also more likely to achieve high-value acquisitions. In this section, we examine why firms acquire inventor-founded firms. On one hand, inventor-founded firms may possess deep technical expertise which makes them attractive targets for acquiring technical know-how ([Bena and Li, 2014](#)). On the other hand, they can be associated with disruptive innovations, prompting incumbent firms to acquire them in order to stifle future competition ([Cunningham et al., 2021](#)). Below, we first examine the channels that could facilitate the acquisition of inventor-founded firms, and then investigate whether acquirers integrate the targets innovations into their own patents after the acquisition.

D.1. Startup Innovation Performance

Technical knowledge from executives has a positive effect on firms' innovation performance. For example, [Islam and Zein \(2020\)](#) find that public firms led by inventor CEOs produce more valuable patents in the technological field of their CEOs. Based on this, we expect inventor-founded firms to demonstrate superior innovation performance, which makes them attractive targets.

Table 9 presents the second-stage results on startup innovation performance, measured over one to five years after founding. Panels A analyses the number of patents. The results show that inventor-founded firms receive more granted patents within the first five years. Specifically,

being inventor-founded leads to a 264% increase in patents one year after founding.¹¹ The large magnitude is due to the low patenting activity in the first year: only 28% of firms in our sample file patent applications, and 24% receive granted patents during that period. Panel B focuses on the novelty of patents. We use the RETech score introduced by [Bowen et al. \(2023\)](#), which captures whether a patent belongs to a rapidly evolving or stable technological area. Patents with a higher RETech score within the same filing year and technology class are classified as novel patents. We find that inventor-founded firms produce more novel patents in the first two years after founding. Specifically, being inventor-founded leads to a 126% and 844% increase in the number of novel patents in the first and second year after founding, respectively. This pattern is consistent with our prediction for the instrument *SSIV*. That's, inventor-founded firms emerge in response to breakthrough innovations, so their early patents are more likely to fall into rapidly evolving technological domains. In Panel C, we examine the likelihood of filing top-cited patents, defined as patents among the top 10% most cited patents within the same filing year and technology class. We again find a significant positive effect in the first two years. Specifically, being inventor-founded leads to a 34.4% and 44% higher likelihood of filing top-cited patents in the first and second year after founding, respectively. There is a significant negative effect in Year 4. However, the results in [Table A1](#) and [Table A2](#) suggest that this effect is not driven by inventor founders possessing up-to-date or industry-relevant technological expertise knowledge.

Insert [Table 9](#) here.

Taken together, our evidence suggests that inventor-founded firms exhibit a rapid innovation process. They produce more patents and more novel patents early in their life cycle. Moreover, they are more likely to file top-cited patents in the early years, suggesting that their innovations are of high-quality.

¹¹As the dependent variable is inverse hyperbolic sine transformed, the approximate percentage change in dependent variable is $100(e^{\beta} - 1)\%$ ([Bellemare and Wichman, 2020](#)).

D.2. VC Funding

Entrepreneurial human capital helps firms differentiate from peers in early stage, while VC financing is important for firms to expand as they grow (Rajan, 2012). VC funding not only helps alleviate the financial constraints faced by many innovative startups but also offers valuable non-financial support. For example, venture capital firms play an important role in helping bridging startups and potential acquirers (Amornsiripanitch et al., 2019; González-Uribe, 2020). CVC facilitates the acquisition of portfolio firms and improve acquisition efficiency (Ma, 2020). In this section, we investigate whether inventor-founded firms perform better in securing VC funding.

Insert Table 10 here.

Table 10 reports the results. In Column 1, the coefficient on CVC investment is positive and significant at the 10% level, suggesting that being inventor-founded increases the likelihood of receiving CVC funding by 12.6%. Given that only 1.8% of firms receive CVC funding, this magnitude is large relative to the mean. In Column 2, we examine the effect on receiving VC investment, and no significant effect is found. It's likely because the majority of firms in our sample receive VC funding. We then examine the effect on the amount of VC investment. In Column 3, the coefficient on the first-round VC investment amount is significantly positive. Specifically, being inventor-founded leads to a 268% increase in the size of the first VC round. Economically, changing from non-inventor-founded to inventor-founded raises the first-round VC investment by about \$24.5 million, relative to a mean of \$9.14 million. In Column 4, we continue to find a positive effect on the total VC investment amount, although the significance level drops to 10%. This weaker significance may reflect that later VC rounds are influenced by other confounding factors, such as the reputation of existing investors, which can dilute the founder effect.

In summary, we find that inventor-founded firms are more likely to receive CVC investment and attract larger first-round VC investments.

D.3. Killer Acquisition or Innovation Integration?

The evidence above shows that inventor-founded firms exhibit stronger innovation performance in both innovation quantity and quality. Acquiring innovation is one key reason for M&A (Bena and Li, 2014; Ma, 2020). It can also be possible that firms may also engage in “killer acquisitions” to pre-empt competition threat (Cunningham et al., 2021). To differentiate the motive for acquiring inventor-founded firms, we employ a stacked event study to compare acquirer’s citation to target’s patents and to their equivalent patents before and after acquisition.

Specifically, the treated group comprises patents filed by target firms at least three years before acquisition. The control group comprises “twin” patents filed by firms never acquired by the same acquirer, also at least three years before the “acquisition”. Twin matches are drawn from Arts et al. (2021), which identifies similar patents for all U.S. utility patents granted between 1969 and 2018. We further limit matches to pairs whose filing years differ by no more than five years. The final sample includes 1,483 treated patents from 386 inventor-founded targets and 2,035 control patents from 934 firms. Each set of similar patents is treated as a patent family. The estimation specification is:

$$Cit_{i,j,t} = \alpha_0 + \sum_{k \neq -1} \beta_e \cdot Treated_i \cdot \mathbf{1}[e = k] + \varepsilon_{c,t} + \gamma_{c,i} + \theta_{i,j,t} \quad (6)$$

where i , j , t , and c denote patent, the acquirer, year and acquisition deal, respectively. $Treated_i$ is a dummy equal to 1 for treated patents and 0 otherwise. $\mathbf{1}[e = k]$ is the event-time dummy. $Cit_{i,j,t}$ is a dummy equal to 1 if acquirer j cites patent i . We include deal-by-year fixed effects and deal-by-patent fixed effects. The standard errors are clustered at patent family level.

We plot the estimated coefficients in Figure 1. Figure 1a reports results for the full sample, whereas Figure 1b focuses on novel patents. In both panels, there is no significant difference in citations between the treated and control groups before the acquisition. Following acquisition,

acquirers are more likely to cite the target's patents, especially their novel patents.¹² The results indicate that acquirers are more likely to integrate targets' novel innovations after acquisition. This pattern is inconsistent with the killer acquisition hypothesis, which predicts that acquirers suppress novel innovations and are less likely to integrate them.

Insert Figure 1 here.

In summary, we show that inventor-founded firms exhibit stronger innovation performance, which makes them appealing targets for acquiring innovation. We find no evidence that such acquisitions are intended to eliminate novel innovations. Instead, the results are consistent with a technology-acquisition explanation, where acquiring firms become more likely to cite the innovations of inventor-founded targets after the acquisition.

E. Local Impacts of Inventor Entrepreneurship

We show that counties endowed with a larger stock of inventors in a given technological domain exhibit a higher propensity for inventor-founded firm entry following breakthrough innovations within that domain. Moreover, those inventor-founded firms exhibit higher quality in terms of high-value acquisition, innovation, and securing VC investment. How do these firms affect the local economy? In this section, we analyse two potential spillover effects: inventor inflows and innovations.

To investigate these effects, we aggregate inventor-founded firms at the county-industry level and examine how inventor entrepreneurship affects local inventor inflows and innovation. Table 11 reports the results. Column 1 presents the first-stage estimates, indicating that counties with greater exposure to industry-level technological change experience a higher rate of inventor-founded firm entry. Columns 2-5 report the second-stage results. In Columns 2 and 3, we examine the inflow of inventors and star inventors, respectively. Both coefficients are positive and statistically significant

¹²We also present the Difference-in-Difference estimates in Table A9. The results indicate that acquirers are more likely to cite targets' patents after the acquisition, with stronger effects for novel patents.

at the 1% level, suggesting that the establishment of inventor-founded firms in a given county induces greater inventor in-migration. Specifically, a 1% increase in the number of inventor-founded firms is associated with a 15.6% and 17.2% increase in the inflow of inventors and star inventors, respectively. These findings point to a mechanism underlying the formation of inventor agglomerations and technology clusters: inventor-founded firms attract technologically proximate inventors to relocate to the county, and the resulting expansion of the local inventor pool, in turn, fosters subsequent rounds of inventor entrepreneurship.

We next examine the effect of inventor entrepreneurship on local innovation productivity, with results presented in Columns 4 and 5. Column 4 examines the impact of inventor entrepreneurship on the total number of industry-related patents filed within the county. The coefficient is positive and statistically significant at the 1% level, indicating inventor entrepreneurship leads to more industry-related patents. In Column 5, we focus on high-impact innovation, defined as those among the top 10% most cited within their technology class and filing year. The positive and statistically significant coefficient indicates that inventor entrepreneurship not only increases the quantity of local innovation but also enhances its quality by generating more radical inventions. Quantitatively, a 1% increase in the number of inventor-founded firms is associated with a 20% increase in top-cited patents.

Insert Table 11 here.

In summary, we show that inventor entrepreneurship generates substantial local spillovers. Counties with a greater concentration of inventor-founded firms attract larger inflows of technologically proximate inventors, including star inventors. Furthermore, these counties exhibit higher levels of both overall and radical innovation output.

IV. ROBUSTNESS

This section discusses alternative explanations and reports findings from various robustness checks.

First, our instrument may still capture the effect of inventor employees, rather than inventor founders. Individuals differ in their preferences for becoming founders versus employees, and early employees also play a critical role in firm performance (Choi et al., 2023; Roach and Sauermann, 2015). To address this concern, we exclude inventor-founded firms with experienced inventor inflows during their founding or first year to avoid confounding effects from early experienced joiners. Experienced inventors are defined as those who have prior inventive experience before joining the firm. We then examine the effect on firm exits using the shortlisted sample, with results reported in Table A3. Our results remain robust after excluding inventor-founded firms that experience either experienced-inventor inflows or senior-inventor inflows during their founding or first year. Hence, our main results are not driven by early inventor employees.

Second, inventor founders may be particularly important in hard-tech industries such as biotechnology, we re-estimate the results after excluding biotechnology firms. Table A4 shows that our results remain consistent with our main findings.

Third, we show that our results are robust to exclude three county-industry clusters with the highest concentration of companies: the software industry in Santa Clara County (CA) and San Francisco County (CA), and the biotechnology industry in Middlesex County (MA). The results are presented in Table A5. We still find a significant positive effects on acquisition. However, the first-stage regression becomes weaker, which may suggest that inventors in these counties are less responsive to technological change compared to those in hot clusters.

Fourth, we re-estimate the main results using alternative instrument constructions. Table A8 reports the results. In Panel A, we replace the shift component with the ratio of breakthrough innovations in the U.S., excluding patents from the focal county. In Panel B, we measure inventor supply using the share of targeted inventors from $t - 9$ to $t - 6$. The results remain robust under these alternative instruments

Finally, we vary the observation windows in several tests. In Table A7, we extend the IPO observation windows to account for the fact that some firms go public later (Ewens and Farre-Mensa, 2020). Consistent with the main results, we still find a significant negative effect on IPO

likelihood. In Table A8, we measure *Up-to-datedness* using a 3-year window. The acquisition results remain robust. However, we find a significant negative effect on IPO likelihood, which is inconsistent with Table 7. A plausible explanation is that a shorter window captures more recent knowledge held by inventor founders at the time of founding, which is more valuable for firm success. In contrast, longer windows inherently include less recent knowledge, which is more likely to be obsolete.

V. CONCLUSIONS

This paper examines the effects of founders’ prior inventive experience on startup performance. Using a leave-one-out shift-share research design, we provide causal evidence that inventor-founded firms are more likely to be acquired and less likely to go public. Acquisitions of inventor-founded firms are also more likely to be high-value. Our findings suggest that exit outcomes are partly shaped by a startups founding structure, with technology-motivated ventures exhibiting a greater propensity to exit via acquisition. These results also relate to the broader shift in startup exits in recent decades, during which acquisitions overtook IPOs as the dominant exit route (Ederer and Pellegrino, 2023; Huang et al., 2023). We further establish a founder knowledge transfer mechanism. We find that inventors with up-to-date and industry-relevant technological expertise are more responsive to technology change by founding firms, and that startups founded by such inventors are more likely to be acquired.

To understand why firms acquire inventor-founded startups, we first examine two channels that could facilitate their acquisitions and then investigate whether acquirers integrate the targets innovations into their own patents after the acquisition. Our results indicate that inventor-founded firms produce more novel patents and are more likely to generate top-cited patents early in their life cycle. In terms of VC funding, we find that inventor-founded firms are more likely to receive CVC funding, though no significant effect is found for VC funding. However, inventor-founded firms receive a larger first-round VC investment compared to their VC-backed peers. To differentiate the motive for acquiring inventor-founded firms, we employ a stacked event-study design comparing

acquirers' citations to the acquired inventor-founded firms' patents with citations to a matched set of "twin" patents before and after acquisition. We find that acquirers are more likely to cite the targets' patents after acquisition, particularly their novel patents. These findings suggest that acquisitions of inventor-founded firms are undertaken to acquire innovation rather than to suppress it.

Inventor entrepreneurship generates substantial local spillovers. Counties with a greater concentration of inventor-founded firms attract larger inflows of technologically proximate inventors, including star inventors. Furthermore, these counties produce more radical patents. These findings suggest a self-reinforcing mechanism underlying the agglomeration of inventors and the formation of technology clusters. We also provide meaningful policy implications for efforts aimed at fostering vibrant technological ecosystems. For example, subsidies targeting inventors with up-to-date knowledge and policies that lower barriers to inventor mobility may prove effective in stimulating local innovation and fostering entrepreneurial activity.

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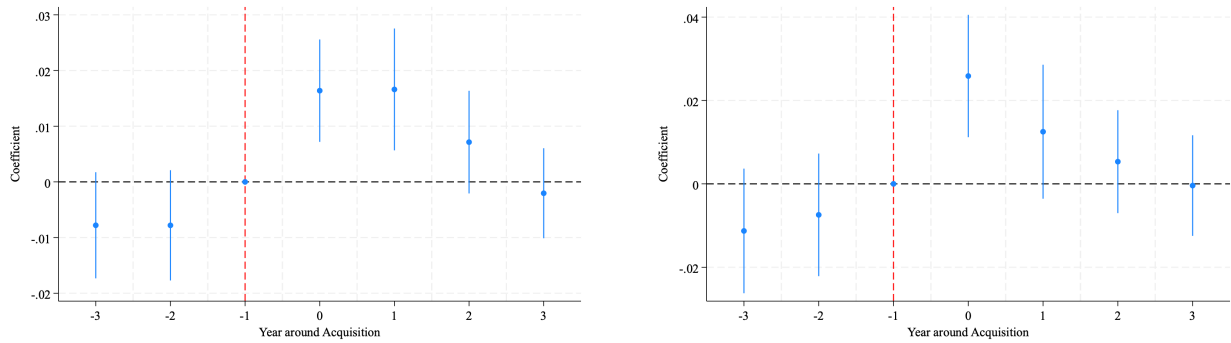
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Figure 1: Difference in acquirer's citation to twin patents around acquisition



(a) Full patent Sample

(b) Novel patent Sample

Note: The figure plots the coefficients estimated from Equation 6. The omitted category is one year prior to acquisition. Vertical bars represent 95% confidence intervals. Panel (a) includes patents of acquired inventor-founded firms and a matched set of “twin” patents, whereas Panel (b) includes only novel patents of acquired inventor-founded firms and a matched set of “twin” patents.

Table 1: Shock summary statistics

Mean	0.044
SD	0.036
Max	0.117
Min	0
1/HHI	43.388
Shocks	319

Notes: This table reports the distribution for shock, *TechChange*, at technology-year level. *TechChange* represents the intensity of breakthrough innovations in a given technology class. The detailed construction is described in Section B.1.

Table 2: Shock Balance Tests

Panel A: County Balance		
	Coef	SE
Unemployment rate	.159	.305
GDP	-.017	.033
College-educated population	-.007	.013
Panel B: County Industry Balance		
Manufacturing	-.042	.081
Information	-.005	.010
Financial activities	-.004	.007
Education and health services	-.036	.070
Observations	319	319

Note: This table reports the balance test for shocks. Panel A analyses macro county characteristics. *Unemployment rate* is county unemployment rate. *GDP* is the natural logarithm of 1 plus county GDP, adjust to 2017 dollars. *College-educated population* is the natural logarithm of 1 plus the number of individuals with a bachelor's degree or higher in 2000, in a given county. Panel B analyses county industry specialization, which is measured by the concentration of total wages in a given county for a specific industry relative to the concentration of total wages for that industry in the U.S. County characteristics are lagged by five years prior to the shocks (or three years when data availability is limited). Both lagged county characteristics and the shocks are residualized on exposure share controls, county-industry fixed effects, and county-founding year fixed effects. Following [Borusyak et al. \(2022\)](#), all observations are weighted by average exposure shares computed using the Stata command *ssaggregate*. Standard errors are clustered at technology level.

Table 3: Summary Statistics

	Mean	SD	Min	Median	P75	Max	N
Founder Num	2.031	1.109	1	2	3	11	6643
Inventor Founder Num	0.712	0.786	0	1	1	7	6643
InvFounded	0.542	0.498	0	1	1	1	6643
CVC-backed	0.018	0.133	0	0	0	1	6643
VC-backed	0.802	0.398	0	1	1	1	6643
IPO	0.041	0.198	0	0	0	1	6643
Acquired	0.169	0.375	0	0	0	1	6643
Failed	0.085	0.279	0	0	0	1	6643

Note: This table reports firm-level summary statistics. All firms in the sample were founded between 2005 and 2015 and received at least one utility patent between 2005 and 2020. For the variable definition and construction, see [Appendix C](#).

Table 4: Founder inventive experience and Startup Performance (OLS Results)

	OLS			Reduced-Form		
	(1) IPO/Acq	(2) IPO	(3) Acquired	(4) IPO/Acq	(5) IPO	(6) Acquired
InvFounded	0.032** (0.014)	0.009* (0.004)	0.023* (0.013)			
SSIV				0.015** (0.006)	-0.008** (0.004)	0.023*** (0.006)
Control	Y	Y	Y	Y	Y	Y
Cty-Ind FE	Y	Y	Y	Y	Y	Y
Cty-FoundingYr FE	Y	Y	Y	Y	Y	Y
Observations	6643	6643	6643	6643	6643	6643
R-squared	0.184	0.195	0.180	0.183	0.195	0.179

Note: This table presents the ordinary least squares (OLS) estimates. Column 1-3 report the correlation between founder inventive experience and firm exits. Columns 4-6 show the reduced-form estimates that regress the exit outcomes on the instrument. *SSIV* is the shift-share instrumental variable; the construction is detailed in Section [B.1](#). *InvFounded* is a dummy variable equal to 1 if the founding team includes at least one inventor, and 0 otherwise. *IPO/Acq* is a dummy variable equal to 1 if the firm completed an IPO or was acquired within nine years after founding, and 0 otherwise. *IPO* is a dummy variable equal to 1 if the firm completed an IPO within nine years after founding, and 0 otherwise. *Acquired* is a dummy variable equal to 1 if the firm was acquired within nine years after founding, and 0 otherwise. All specifications include exposure share controls, county-industry fixed effect, and county-founding year fixed effect. Industry is defined using four-digit ISIC. Standard errors are clustered at the county level. ***, **, and * denote significance at the 1%, 5% and 10% levels, respectively.

Table 5: Founder inventive experience and Startup Performance

	(1) InvFounded	(2) IPO/Acq	(3) IPO	(4) Acquired	(5) Failed
SSIV	0.052*** (0.006)				
InvFounded		0.298** (0.132)	-0.157** (0.064)	0.455*** (0.129)	-0.178 (0.157)
Control	Y	Y	Y	Y	Y
Cty-Ind FE	Y	Y	Y	Y	Y
Cty-FoundingYr FE	Y	Y	Y	Y	Y
Observations	6643	6643	6643	6643	6643
First-stage F-stat		80.895	80.895	80.895	80.895
R-squared	0.195				

Note: This table reports the two-stage least square (2SLS) results for startup performance. Column 1 presents the first-stage estimates from Equation 3, and the remaining columns report the second-stage estimates from Equation 4. *SSIV* is the shift-share instrumental variable; the construction is detailed in Section B.1. *InvFounded* is a dummy variable equal to 1 if the founding team includes at least one inventor, and 0 otherwise. *IPO/Acq* is a dummy variable equal to 1 if the firm completed an IPO or was acquired within nine years after founding, and 0 otherwise. *IPO* is a dummy variable equal to 1 if the firm completed an IPO within nine years after founding, and 0 otherwise. *Acquired* is a dummy variable equal to 1 if the firm was acquired within nine years after founding, and 0 otherwise. *Failed* is a dummy variable equal to 1 if the firm failed by 2024, and 0 otherwise. All specifications include exposure share controls, county-industry fixed effect, and county-founding year fixed effect. Industry is defined using four-digit ISIC. Standard errors are clustered at the county level. The test for weak instrument is Kleibergen-Paap F-statistic. ***, **, and * denote significance at the 1%, 5% and 10% levels, respectively.

Table 6: Founder inventive experience and Startup Acquisition

	(1) Acquired (<i>multiple</i> >1.25)	(2) Acquired (>\$25 M)
<i>InvFounded</i>	0.125* (0.063)	0.159** (0.079)
Control	Y	Y
Cty-Ind FE	Y	Y
Cty-FoundingYr FE	Y	Y
Observations	5103	6643
First-stage F-stat	221.181	80.895

Note: This table reports the second-stage estimates for startup acquisition, using two different measures of acquisition. *InvFounded* is a dummy variable equal to 1 if the founding team includes at least one inventor, and 0 otherwise. *Acquired (multiple>1.25)* is a dummy variable equal to 1 if the firm was acquired with a multiple greater than 1.25 within nine years after founding, and 0 otherwise. *Acquired(>\$25M)* is a dummy variable equal to 1 if the firm was acquired with a deal value greater than 25 million dollars (in 2010 dollars) within nine years after founding, and 0 otherwise. All specifications include exposure share controls, county-industry fixed effect, and county-founding year fixed effect. Industry is defined using four-digit ISIC. Standard errors are clustered at the county level. The test for weak instrument is Kleibergen-Paap F-statistic. ***, **, and * denote significance at the 1%, 5% and 10% levels, respectively.

Table 7: Inventor Founder Knowledge Up-to-datedness and Startup Performance

Panel A: Up-to-date Knowledge				
	(1) InvFounded	(2) IPO/Acq	(3) Acquired	(4) IPO
SSIV	0.070*** (0.012)			
InvFounded		0.293* (0.168)	0.386** (0.169)	-0.093 (0.083)
Control	Y	Y	Y	Y
Cty-Ind FE	Y	Y	Y	Y
Cty-FoundingYr FE	Y	Y	Y	Y
Observations	4655	4655	4655	4655
First-stage F-stat		32.256	32.256	32.256
R-squared	0.210			
Panel B: Obsolete Knowledge				
	InvFounded	IPO/Acq	Acquired	IPO
SSIV	0.024* (0.014)			
InvFounded		0.028 (0.428)	0.253 (0.451)	-0.225 (0.209)
Control	Y	Y	Y	Y
Cty-Ind FE	Y	Y	Y	Y
Cty-FoundingYr FE	Y	Y	Y	Y
Observations	4618	4618	4618	4618
First-stage F-stat		2.967	2.967	2.967
R-squared	0.213			

Note: This table reports 2SLS estimates of startup performance, separately for the two subsamples. Panel A includes non-inventor-founded firms and firms founded by inventor(s) with up-to-date knowledge. Panel B includes non-inventor-founded firms and firms founded by inventor(s) with obsolete knowledge. The detailed construction for inventor founder knowledge up-to-datedness is described in Equation 5. *SSIV* is the shift-share instrumental variable; the construction is detailed in Section B.1. *InvFounded* is a dummy variable equal to 1 if the founding team includes at least one inventor, and 0 otherwise. *IPO/Acq* is a dummy variable equal to 1 if the firm completed an IPO or was acquired within nine years after founding, and 0 otherwise. *IPO* is a dummy variable equal to 1 if the firm completed an IPO within nine years after founding, and 0 otherwise. *Acquired* is a dummy variable equal to 1 if the firm was acquired within nine years after founding, and 0 otherwise. All specifications include exposure share controls, county-industry fixed effect, and county-founding year fixed effect. Industry is defined using four-digit ISIC. Standard errors are clustered at the county level. The test for weak instrument is Kleibergen-Paap F-statistic. ***, **, and * denote significance at the 1%, 5% and 10% levels, respectively.

Table 8: Inventor Founder Knowledge Relevance and Startup Performance

Panel A: Industry-relevant Technological Knowledge				
	(1)	(2)	(3)	(4)
	InvFounded	IPO/Acq	Acquired	IPO
SSIV	0.072*** (0.006)			
InvFounded		0.204 (0.125)	0.338*** (0.113)	-0.134** (0.058)
Control	Y	Y	Y	Y
Cty-Ind FE	Y	Y	Y	Y
Cty-FoundingYr FE	Y	Y	Y	Y
Observations	5547	5547	5547	5547
First-stage F-stat		171.147	171.147	171.147
R-squared	0.230			
Panel B: Industry-irrelevant Technological Knowledge				
	InvFounded	IPO/Acq	Acquired	IPO
SSIV	0.002 (0.023)			
InvFounded		-0.443 (11.279)	-1.337 (18.686)	0.894 (8.350)
Control	Y	Y	Y	Y
Cty-Ind FE	Y	Y	Y	Y
Cty-FoundingYr FE	Y	Y	Y	Y
Observations	3724	3724	3724	3724
First-stage F-stat		0.010	0.010	0.010
R-squared	0.214			

Note: This table reports 2SLS estimates of startup performance, separately for the two subsamples. Panel A includes non-inventor-founded firms and firms founded by inventor(s) with industry-relevant technological knowledge. Panel B includes non-inventor-founded firms and firms founded by inventor(s) with industry-irrelevant technological knowledge. *SSIV* is the shift-share instrumental variable; the construction is detailed in Section B.1. *InvFounded* is a dummy variable equal to 1 if the founding team includes at least one inventor, and 0 otherwise. *IPO/Acq* is a dummy variable equal to 1 if the firm completed an IPO or was acquired within nine years after founding, and 0 otherwise. *IPO* is a dummy variable equal to 1 if the firm completed an IPO within nine years after founding, and 0 otherwise. *Acquired* is a dummy variable equal to 1 if the firm was acquired within nine years after founding, and 0 otherwise. All specifications include exposure share controls, county-industry fixed effect, and county-founding year fixed effect. Industry is defined using four-digit ISIC. Standard errors are clustered at the county level. The test for weak instrument is Kleibergen-Paap F-statistic. ***, **, and * denote significance at the 1%, 5% and 10% levels, respectively.

Table 9: Founder inventive experience and Startup Innovation

Panel A: Patents					
	(1) Year 1	(2) Year 2	(3) Year 3	(4) Year 4	(5) Year 5
InvFounded	1.291*** (0.171)	2.671*** (0.818)	0.995*** (0.193)	2.564*** (0.829)	1.213** (0.563)
Panel B: Novel patents					
	Year 1	Year 2	Year 3	Year 4	Year 5
InvFounded	0.814*** (0.182)	2.245*** (0.567)	0.108 (0.263)	1.521** (0.692)	0.449 (0.449)
Panel C: Top-cited patents					
	Year 1	Year 2	Year 3	Year 4	Year 5
InvFounded	0.344** (0.145)	0.440** (0.182)	0.203 (0.134)	-0.379*** (0.121)	0.123 (0.286)
Control	Y	Y	Y	Y	Y
Cty-Ind FE	Y	Y	Y	Y	Y
Cty-FoundingYr FE	Y	Y	Y	Y	Y
Observations	6640	6607	6509	6374	6143
First-stage F-stat	80.912	55.834	44.914	30.243	17.760

Note: This table presents the second-stage regression estimates for innovation performance over one to five years after founding, based on the full sample. Panel A shows results for inverse hyperbolic sine transformed number of granted patents. Panel B reports inverse hyperbolic sine transformed number of novel patents. A novel patent is defined as one with a higher RETech score within the same filing year and technology class. Panel C reports the likelihood of filing top-cited patents. A top cited patent is among the top 10% most cited patents within the same filing year and technology class. *InvFounded* is a dummy variable equal to 1 if the founding team includes at least one inventor, and 0 otherwise. All specifications include exposure share controls, county-industry fixed effect, and county-founding year fixed effect. Industry is defined using four-digit ISIC. Standard errors are clustered at the county level. The test for weak instrument is Kleibergen-Paap F-statistic. ***, **, and * denote significance at the 1%, 5% and 10% levels, respectively.

Table 10: Founder inventive experience and VC Funding

	(1) CVC-backed	(2) VC-backed	(3) First VC	(4) Total VC
InvFounded	0.126* (0.070)	0.071 (0.173)	1.304** (0.542)	1.870* (1.073)
Control	Y	Y	Y	Y
Cty-Ind FE	Y	Y	Y	Y
Cty-FoundingYr FE	Y	Y	Y	Y
Observations	6643	6643	4042	4714
First-stage F-stat	80.895	80.895	71.459	53.453

Note: This table presents the second-stage regression estimates for VC funding. *InvFounded* is a dummy variable equal to 1 if the founding team includes at least one inventor, and 0 otherwise. *CVC-backed* is a dummy variable that equals 1 if the firm received corporate venture capital funding by 2024, and 0 otherwise. *VC-backed* is a dummy variable that equals 1 if the firm received venture capital funding by 2024, and 0 otherwise. *First VC* is natural logarithm of the first VC investment amount, adjusted to 2010 U.S. dollars. *Total VC* is natural logarithm of the total VC funding raised by startups as of 2024, adjusted to 2010 U.S. dollars. All specifications include exposure share controls, county-industry fixed effect, and county-founding year fixed effect. Industry is defined using four-digit ISIC. Standard errors are clustered at the county level. The test for weak instrument is Kleibergen-Paap F-statistic. ***, **, and * denote significance at the 1%, 5% and 10% levels, respectively.

Table 11: Local Impacts of Inventor Entrepreneurship

	(1)	(2)	(3)	(4)	(5)
	InvFoundedFirms	InvIn	StarInvIn	Pats	TopPats
SSIV	0.124*** (0.031)				
InvFoundedFirms		0.156*** (0.050)	0.172*** (0.060)	0.168*** (0.041)	0.200*** (0.040)
Control	Y	Y	Y	Y	Y
Cty-Ind FE	Y	Y	Y	Y	Y
CtyYearFE	Y	Y	Y	Y	Y
Observations	1192	1192	1192	1192	1192
States	23	23	23	23	23
Counties	44	44	44	44	44
First-stage F-stat		15.776	15.776	15.776	15.776
R-squared	0.741				

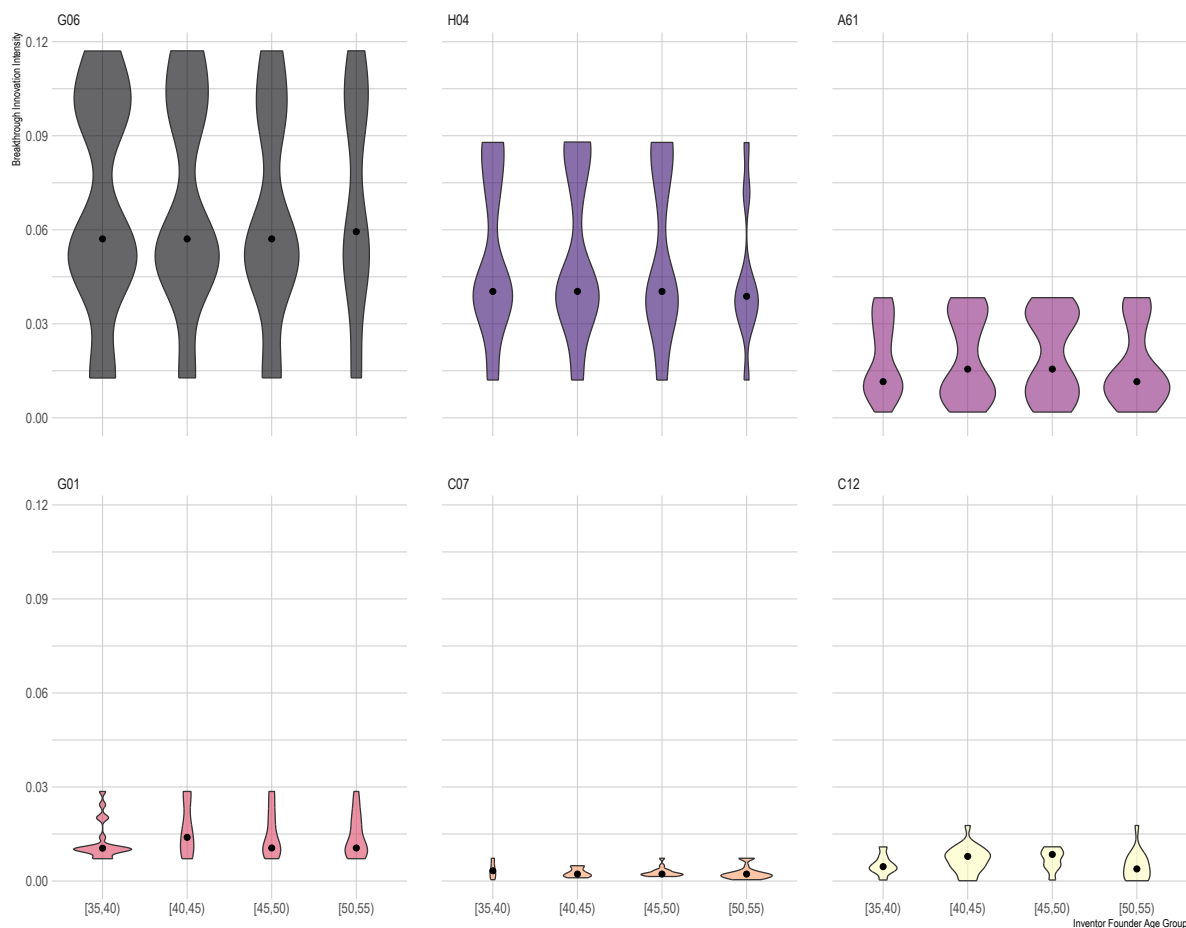
Note: This table presents 2SLS regression estimates for county-level outcomes. *InvFoundedFirms* is the natural logarithm of the number of inventor-founded firms in a given industry and county. *SSIV* is the shift-share instrumental variable; the construction is detailed in Section B.1. *InvIn* is the natural logarithm of the number of inventors with industry-related knowledge moving into a given county within a three-year period. *StarInvIn* is the natural logarithm of the number of star inventors with industry-related knowledge moving into a given county within a three-year period. Star inventors are those whose number of patents ranks in the top 10 within a given technology area by a given year. *Pats* is the natural logarithm of the number of patents in industry-related technology areas within a three-year period. *TopPats* is the natural logarithm of the number of top-cited patents in industry-related technology areas within a three-year period. A top cited patent is among the top 10% most cited patents within the same filing year and technology class. All specifications include exposure share controls, county-industry fixed effect, and county-year fixed effect. Industry is defined using four-digit ISIC. Standard errors are clustered at the county level. The test for weak instrument is Kleibergen-Paap F-statistic. ***, **, and * denote significance at the 1%, 5% and 10% levels, respectively.

Appendix A. INDUSTRY DISTRIBUTION

ISIC 4-digit Index	Industry	Number	Ratio
2100	Biotechnology	1433	21.57%
2610	Semiconductor	117	1.76%
2620	Hardware	128	1.93%
2640	Things of Internet	221	3.33%
2910	Autonomous Vehicles	277	4.17%
3030	Aerospace	43	0.65%
3250	Medical Devices	1022	15.38%
3510	Clean Tech	317	4.77%
5820	Gaming	139	2.09%
5820	Ed Tech	54	0.81%
6201	Augmented Reality	122	1.84%
6311	Big Data	2004	30.17%
6619	Fin Tech	230	3.46%
7310	Advertising	445	6.70%

Note: This table reports the industry distribution of the main sample. We cluster Crunchbase industry tags using the BERTopic package, and the resulting clusters are then matched to ISIC 4-digit index.

Appendix B. INVENTOR FOUNDER AGE DISTRIBUTION



Note: This violin plot presents the distribution of the breakthrough innovation intensity *TechChange* across inventor founder age groups, focusing on the six CPC technology classes with the highest concentration of inventor founders. Among 2,197 inventor founders with available birth year information, the mean age is 45, consistent with previous findings [Azoulay et al. \(2020\)](#). The majority of inventor founders specialize in six CPC technology classes: G06 (Computing; Calculation; Counting), H04 (Electric Communication Technique), A61 (Medical or Veterinary Science; Hygiene), G01 (Measuring; Testing), C07 (Organic Chemistry), and C12 (Biochemistry), representing 27.7%, 19.1%, 18.1%, 6.3%, 5.1%, and 4.1% of the sample, respectively. The area of each violin reflects the distribution of inventor founders within a given age group for each technology class. For G06, founder ages tend to be younger, while A61 is dominated by older founders. The black dot indicates the median breakthrough innovation intensity. Across these technology classes, founders in their 40s are no less responsive to technological change.

Appendix C. VARIABLE DEFINITION AND CONSTRUCTION

Variables	Definition and Construction
A. Startup	
<i>SSIV</i>	Shift-share instrumental variable. The construction is detailed in Section B.1.
<i>Founder Num</i>	The number of founder(s).
<i>Inventor Founder Num</i>	The number of founder(s) with prior inventing experience.
<i>InvFounded</i>	Dummy variable equal to 1 if the founding team includes at least one inventor, and 0 otherwise.
<i>IPO</i>	Dummy variable equal to 1 if the firm completed an IPO within nine years of founding, and 0 otherwise.
<i>Acquired</i>	Dummy variable equal to 1 if the firm was acquired within nine years after founding.
<i>Failed</i>	Dummy variable equal to 1 if the firm failed by 2024, and 0 otherwise.
<i>CVC-backed</i>	Dummy variable equal to 1 if the firm received corporate venture capital funding by 2024, and 0 otherwise.
<i>VC-backed</i>	Dummy variable equal to 1 if the firm received venture capital funding by 2024, and 0 otherwise.
<i>First VC</i>	Natural logarithm of the first VC investment amount as of 2024, adjusted to 2010 U.S. dollars.
<i>Total VC</i>	Natural logarithm of the total VC investment amount as of 2024, adjusted to 2010 U.S. dollars.
B. County	
<i>GDP</i>	Natural logarithm of 1 plus county GDP, adjusted to 2017 dollars.
<i>College-educated population</i>	Natural logarithm of 1 plus the number of individuals with a bachelor's degree or higher in 2000, in a given county.
<i>Unemployment rate</i>	The unemployment rate in a given county.

Table A1: Founder Knowledge Up-to-datedness and Startup Innovation

Panel A: Patents					
	(1) Year 1	(2) Year 2	(3) Year 3	(4) Year 4	(5) Year 5
InvFounded	0.664*** (0.132)	2.034*** (0.599)	0.899*** (0.339)	2.514*** (0.613)	1.460** (0.614)
Panel B: Novel patents					
	Year 1	Year 2	Year 3	Year 4	Year 5
InvFounded	0.568*** (0.113)	1.382*** (0.405)	0.018 (0.355)	1.129** (0.477)	0.209 (0.277)
Panel C: Top-cited patents					
	Year 1	Year 2	Year 3	Year 4	Year 5
InvFounded	0.069 (0.055)	0.364*** (0.113)	0.062 (0.074)	0.070 (0.081)	0.028 (0.269)
Control	Y	Y	Y	Y	Y
Cty-Ind FE	Y	Y	Y	Y	Y
Cty-FoundingYr FE	Y	Y	Y	Y	Y
Observations	4645	4621	4548	4461	4297
First-stage F-stat	30.679	25.755	29.880	27.956	21.958

Note: This table presents the second-stage regression estimates for innovation performance over one to five years after founding, using the subsample of non-inventor-founded firms and firms founded by inventors with up-to-date knowledge. Panel A shows results for inverse hyperbolic sine transformed number of granted patents. Panel B reports inverse hyperbolic sine transformed number of novel patents. A novel patent is defined as one with a higher RETech score within the same filing year and technology class. Panel C reports the likelihood of filing top-cited patents. A top cited patent is among the top 10% most cited patents within the same filing year and technology class. *InvFounded* is a dummy variable equal to 1 if the founding team includes at least one inventor, and 0 otherwise. All specifications include exposure share controls, county-industry fixed effect, and county-founding year fixed effect. Industry is defined using four-digit ISIC. Standard errors are clustered at the county level. The test for weak instrument is Kleibergen-Paap F-statistic. ***, **, and * denote significance at the 1%, 5% and 10% levels, respectively.

Table A2: Founder Knowledge Relevance and Startup Innovation

Panel A: Patents					
	(1) Year 1	(2) Year 2	(3) Year 3	(4) Year 4	(5) Year 5
InvFounded	0.837*** (0.155)	1.749*** (0.355)	0.643** (0.280)	2.274*** (0.443)	1.143*** (0.322)
Panel B: Novel patents					
	Year 1	Year 2	Year 3	Year 4	Year 5
InvFounded	0.622*** (0.125)	1.343*** (0.232)	0.112 (0.291)	1.618*** (0.357)	0.470 (0.288)
Panel C: Top-cited patents					
	Year 1	Year 2	Year 3	Year 4	Year 5
InvFounded	0.268*** (0.075)	0.443*** (0.114)	0.180*** (0.068)	-0.132 (0.081)	0.039 (0.174)
Control	Y	Y	Y	Y	Y
Cty-Ind FE	Y	Y	Y	Y	Y
Cty-FoundingYr FE	Y	Y	Y	Y	Y
Observations	5544	5514	5422	5305	5107
First-stage F-stat	170.917	269.751	217.468	215.343	145.324

Note: This table presents the second-stage regression estimates for innovation performance over one to five years after founding, using the subsample of non-inventor-founded firms and firms founded by inventors with industry-relevant technological knowledge. Panel A shows results for inverse hyperbolic sine transformed number of granted patents. Panel B reports inverse hyperbolic sine transformed number of novel patents. A novel patent is defined as one with a higher RETech score within the same filing year and technology class. Panel C reports the likelihood of filing top-cited patents. A top cited patent is among the top 10% most cited patents within the same filing year and technology class. *InvFounded* is a dummy variable equal to 1 if the founding team includes at least one inventor, and 0 otherwise. All specifications include exposure share controls, county-industry fixed effect, and county-founding year fixed effect. Industry is defined using four-digit ISIC. Standard errors are clustered at the county level. The test for weak instrument is Kleibergen-Paap F-statistic. ***, **, and * denote significance at the 1%, 5% and 10% levels, respectively.

Table A3: Excluding Experienced Inventor Employees Inflow

	Excluding experienced inventors		Excluding inventors in their 40s	
	(1) IPO	(2) Acquired	(3) IPO	(4) Acquired
InvFounded	-0.145** (0.066)	0.397*** (0.134)	-0.293** (0.116)	0.521* (0.292)
Control	Y	Y	Y	Y
Cty-Ind FE	Y	Y	Y	Y
Cty-FoundingYr FE	Y	Y	Y	Y
Observations	6467	6467	5973	5973
First-stage F-stat	53.440	53.440	40.701	40.701

Note: This table presents the second-stage regression estimates for startup exits, excluding inventor-founded firms with experienced inventor inflows during their founding or first year. *InvFounded* is a dummy variable equal to 1 if the founding team includes at least one inventor, and 0 otherwise. *IPO* is a dummy variable equal to 1 if the firm completed an IPO within nine years after founding, and 0 otherwise. *Acquired* is a dummy variable equal to 1 if the firm was acquired within nine years after founding, and 0 otherwise. All specifications include exposure share controls, county-industry fixed effect, and county-founding year fixed effect. Industry is defined using four-digit ISIC. Standard errors are clustered at the county level. The test for weak instrument is Kleibergen-Paap F-statistic. ***, **, and * denote significance at the 1%, 5% and 10% levels, respectively.

Table A4: Excluding Biotechnology Industry

	(1)	(2)	(3)
	IPO/Acq	IPO	Acquired
InvFounded	0.298** (0.132)	-0.157** (0.064)	0.455*** (0.129)
Control	Y	Y	Y
Cty-Ind FE	Y	Y	Y
Cty-FoundingYr FE	Y	Y	Y
Observations	6643	6643	6643
First-stage F-stat	80.895	80.895	80.895

Note: This table presents the second-stage regression estimates for startup exits, excluding biotechnology industry. *InvFounded* is a dummy variable equal to 1 if the founding team includes at least one inventor, and 0 otherwise. *IPO/Acq* is a dummy variable equal to 1 if the firm completed an IPO or was acquired within nine years after founding, and 0 otherwise. *IPO* is a dummy variable equal to 1 if the firm completed an IPO within nine years after founding, and 0 otherwise. *Acquired* is a dummy variable equal to 1 if the firm was acquired within nine years after founding, and 0 otherwise. All specifications include exposure share controls, county-industry fixed effect, and county-founding year fixed effect. Industry is defined using four-digit ISIC. Standard errors are clustered at the county level. The test for weak instrument is Kleibergen-Paap F-statistic. ***, **, and * denote significance at the 1%, 5% and 10% levels, respectively.

Table A5: Excluding Three Largest County-Industry Clusters

	(1) IPO/Acq	(2) IPO	(3) Acquired
InvFounded	0.663 (0.414)	-0.118 (0.127)	0.781** (0.372)
Control	Y	Y	Y
Cty-Ind FE	Y	Y	Y
Cty-FoundingYr FE	Y	Y	Y
Observations	5786	5786	5786
First-stage F-stat	18.573	18.573	18.573

Note: This table presents the second-stage regression estimates for startup exits, excluding the software industry in Santa Clara County (CA) and San Francisco County (CA), and the biotechnology industry in Middlesex County (MA). *InvFounded* is a dummy variable equal to 1 if the founding team includes at least one inventor, and 0 otherwise. *IPO/Acq* is a dummy variable equal to 1 if the firm completed an IPO or was acquired within nine years after founding, and 0 otherwise. *IPO* is a dummy variable equal to 1 if the firm completed an IPO within nine years after founding, and 0 otherwise. *Acquired* is a dummy variable equal to 1 if the firm was acquired within nine years after founding, and 0 otherwise. All specifications include exposure share controls, county-industry fixed effect, and county-founding year fixed effect. Industry is defined using four-digit ISIC. Standard errors are clustered at the county level. The test for weak instrument is Kleibergen-Paap F-statistic. ***, **, and * denote significance at the 1%, 5% and 10% levels, respectively.

Table A6: Alternative Instrument Constructions

Panel A: Alternative Shift			
	(1) IPO/Acq	(2) IPO	(3) Acquired
InvFounded	0.666** (0.267)	-0.115 (0.142)	0.780*** (0.193)
Control	Y	Y	Y
Cty-Ind FE	Y	Y	Y
Cty-FoundingYr FE	Y	Y	Y
Observations	6643	6643	6643
First-stage F-stat	24.558	24.558	24.558
Panel B: Alternative Share			
	IPO/Acq	IPO	Acquired
InvFounded	0.297** (0.137)	-0.187** (0.088)	0.483*** (0.124)
Control	Y	Y	Y
Cty-Ind FE	Y	Y	Y
Cty-FoundingYr FE	Y	Y	Y
Observations	6643	6643	6643
First-stage F-stat	80.526	80.526	80.526

Note: This table presents the second-stage regression estimates for startup exits, using alternative instrument constructions. Panel A uses an alternative measure of shift: the ratio of breakthrough innovations in the U.S., excluding patents from the focal county. Panel B uses an alternative measure of share: the share of targeted inventors from $t - 9$ to $t - 6$. *InvFounded* is a dummy variable equal to 1 if the founding team includes at least one inventor, and 0 otherwise. *IPO/Acq* is a dummy variable equal to 1 if the firm completed an IPO or was acquired within nine years after founding, and 0 otherwise. *IPO* is a dummy variable equal to 1 if the firm completed an IPO within nine years after founding, and 0 otherwise. *Acquired* is a dummy variable equal to 1 if the firm was acquired within nine years after founding, and 0 otherwise. All specifications include exposure share controls, county-industry fixed effect, and county-founding year fixed effect. Industry is defined using four-digit ISIC. Standard errors are clustered at the county level. The test for weak instrument is Kleibergen-Paap F-statistic. ***, **, and * denote significance at the 1%, 5% and 10% levels, respectively.

Table A7: Alternative IPO Window

	(1) IPO (<=10)	(2) IPO (<=11)
InvFounded	-0.110** (0.048)	-0.114* (0.069)
Control	Y	Y
Cty-Ind FE	Y	Y
Cty-FoundingYr FE	Y	Y
Observations	5990	5225
First-stage F-stat	134.354	125.385

Note: This table presents the second-stage regression estimates for IPO likelihood, using extended observation windows of 10 and 11 years. *InvFounded* is a dummy variable equal to 1 if the founding team includes at least one inventor, and 0 otherwise. *IPO* is a dummy variable equal to 1 if the firm completed an IPO within 10 (11) years after founding, and 0 otherwise. All specifications include exposure share controls, county-industry fixed effect, and county-founding year fixed effect. Industry is defined using four-digit ISIC. Standard errors are clustered at the county level. The test for weak instrument is Kleibergen-Paap F-statistic. ***, **, and * denote significance at the 1%, 5% and 10% levels, respectively.

Table A8: Alternative Windows for Measuring Knowledge Up-to-datedness

Panel A: Up-to-date Knowledge				
	(1) InvFounded	(2) IPO/Acq	(3) Acquired	(4) IPO
SSIV	0.084*** (0.010)			
InvFounded		0.137 (0.124)	0.280** (0.124)	-0.143** (0.066)
Control	Y	Y	Y	Y
Cty-Ind FE	Y	Y	Y	Y
Cty-FoundingYr FE	Y	Y	Y	Y
Observations	4629	4629	4629	4629
First-stage F-stat		65.932	65.932	65.932
R-squared	0.209			
Panel B: Obsolete Knowledge				
	InvFounded	IPO/Acq	Acquired	IPO
SSIV	0.009 (0.012)			
InvFounded		1.307 (1.425)	1.174 (1.324)	0.133 (0.583)
Control	Y	Y	Y	Y
Cty-Ind FE	Y	Y	Y	Y
Cty-FoundingYr FE	Y	Y	Y	Y
Observations	4613	4613	4613	4613
First-stage F-stat		0.505	0.505	0.505
R-squared	0.214			

Note: This table reports the estimation results for the effect of founder knowledge up-to-datedness on startup exits, using a 3-year window to measure *up-to-datedness*. Panel A includes non-inventor-founded firms and firms founded by inventor(s) with up-to-date knowledge. Panel B includes non-inventor-founded firms and firms founded by inventor(s) with obsolete knowledge. The detailed construction for inventor founder knowledge up-to-datedness is described in Equation 5. *SSIV* is the shift-share instrumental variable; the construction is detailed in Section B.1. *InvFounded* is a dummy variable equal to 1 if the founding team includes at least one inventor, and 0 otherwise. *IPO/Acq* is a dummy variable equal to 1 if the firm completed an IPO or was acquired within nine years after founding, and 0 otherwise. *IPO* is a dummy variable equal to 1 if the firm completed an IPO within nine years after founding, and 0 otherwise. *Acquired* is a dummy variable equal to 1 if the firm was acquired within nine years after founding, and 0 otherwise. All specifications include exposure share controls, county-industry fixed effect, and county-founding year fixed effect. Industry is defined using four-digit ISIC. Standard errors are clustered at the county level. The test for weak instrument is Kleibergen-Paap F-statistic. ***, **, and * denote significance at the 1%, 5% and 10% levels, respectively.

Table A9: Acquirer’s Citation to Twin Patents

Panel A: Full Sample	
	Cit
Treated × Post	0.009** (0.004)
Deal-Year FE	Y
Deal-Patent FE	Y
Observations	41132
R-squared	0.567
Panel B: Novel Patents	
	Cit
Treated × Post	0.013** (0.006)
Deal-Year FE	Y
Deal-Patent FE	Y
Observations	20890
R-squared	0.635

Note: This table reports the coefficients and standard errors estimated from Equation C.1:

$$Cit_{i,j,t} = \alpha_0 + \beta Treated_i \cdot Post_t + \varepsilon_{c,t} + \gamma_{c,i} + \theta_{i,j,t} \quad (C.1)$$

where i , j , t , and c denote patent, the acquirer, year and acquisition deal, respectively. $Treated_i$ is a dummy equal to 1 for treated patents and 0 otherwise. $Post_t$ is a dummy equal to 1 in years after the acquisition and 0 otherwise. $Cit_{i,j,t}$ is a dummy equal to 1 if acquirer j cites patent i . We include deal-by-year fixed effects and deal-by-patent fixed effects. The standard errors are clustered at patent family level. ***, **, and * denote significance at the 1%, 5% and 10% levels, respectively.

Corporate Political Connection and Misleading Communication Incidents around Sustainability

Guo Lu, George Voulgaris, and Alice Xu

Abstract: We examine the influence of political connections -measured using Political Action Committee (PAC) contributions- on US firms' misleading communication around sustainability issues. To identify incidents of misleading corporate communication, we use plausibly exogenous issues-and-event-driven data going beyond firms' self-reported disclosures. Our results show a positive and significant association between political connections and misleading sustainability communication. This is consistent with a political cost mechanism: Politically connected firms face greater pressure to protect allied politicians from being associated with their sustainability outcomes and hence respond by engaging in misleading communication. To strengthen identification, we exploit the exogenous shock created by senator promotions to powerful committee positions to capture the influence of political ties. The study provides new evidence on how political connections are linked to corporate misrepresentation events and highlights political ties as an additional risk factor for regulators and investors when assessing the credibility of corporate sustainability claims.

1. Introduction

US corporations actively participate in political campaigns and cultivate political connections with policymakers through political contributions and lobbying activities. Recent events, including the well-known FirstEnergy case in Ohio (Reuters, 2025), illustrate how such corporate financial support can be exchanged for political favours. These types of pay-to-play connections between corporations and public officials in US politics have drawn substantial attention from the public and academia. Prior research shows that these connections can generate various benefits for firms, such as better access to information, and lower financing costs (Ahmed, Duellman, & Grady, 2022; Christensen, Morris, Walther, & Wellman, 2023; Correia, 2014; Faccio, Masulis, & McCpnnell, 2006; Pham, 2019). However, some evidence also suggests that such connections can place firms under pressure to grant favours to politicians and expose them to increased scrutiny (Bertrand, Kramarz, Schoar, & Thesmar, 2018; Faccio & Hsu, 2017; Jung, 2024).

Using a financial disclosure angle, prior research also offers mixed findings on the impact of political connections: while some studies find that politically connected firms benefit from preferential SEC enforcement (Correia, 2014; Yu & Yu, 2011), others document increased scrutiny and higher incidence of SEC comment letters (Heese, Khan, & Ramanna, 2017). Although there is ample evidence on the relationship between political connections and financial disclosures, research on the impact of such connections on non-financial disclosures is rather scarce. In this study we are thus motivated to examine the relationship between political connections and corporate disclosures around sustainability, with a particular focus on misleading communications around these issues.

Recent evidence shows a sharp rise in misleading corporate sustainability claims, with environmental misleading communication incidents now accounting for one in four climate-

related Environmental, Social and Governance (ESG) incidents and around one-third of implicated firms also linked to social misleading communication conduct (RepRisk, 2023). Misleading communication incidents around sustainability have become a pressing concern for investors, regulators and consumers. High-profile cases such as Volkswagen's 'Clean Diesel' false-advertising scandal and Invesco Advisers' penalty for misleading ESG description further demonstrate the costly reputational and market-value losses once such behaviour is uncovered (FTC, 2016; SEC, 2024a; SEC, 2024b). Given this evidence and the potential impact of political connections, it is important to examine whether political ties encourage firms to engage in more or less ESG-related misrepresentation.

Political connections can provide firms with different benefits such as lower enforcement risks and better access to inside information (Ahmed et al., 2022; Christensen, Mikhail, Walther, & Wellman, 2017; Christensen et al., 2023; Correia, 2014; Liu, Hu, & Cheng, 2021), but this also increases the visibility for both the firm and the connected politician. Drawing on political cost theory (Jones, 1991; Watts & Zimmerman, 1978), we argue that this heightened visibility raises the expected costs of public scrutiny and regulatory action, giving politically connected firms stronger incentives to manage impressions about their sustainability performance through misleading communication practices. However, political ties can arguably insulate firms from external pressures, as supportive officials can soften regulatory responses and help reduce the criticism on firm's sustainability outcomes. When poor sustainability performance is less likely to trigger serious sanctions or political backlash, politically connected firms can thus have a lower need to resort to misleading communication practices.

We use Political Action Committee (PAC) contribution data from OpenSecrets as our primary measure of political connections. This measurement approach has been widely adopted in prior research on U.S. political connections (Akey, 2015; Bradley, Pantzalis, & Yuan, 2016; Christensen et al., 2017; Cooper, 2011; Correia, 2014). In the U.S., firms are not permitted to

donate directly to candidates or political parties. Instead, corporate PACs can be set up to represent specific firm interests in political campaigns and firms are allowed to cover almost all expenses incurred by their affiliated PACs. These PACs collect contributions from the firm's related stakeholders, including executives, employees, and shareholders, while the final decisions on how to allocate these funds are typically made by top executives.

To measure misleading communication conduct, we draw on RepRisk incident data. RepRisk monitors multilingual public and stakeholder sources to identify incident information and classifies these incidents into 28 ESG issue categories. Each incident is linked to at least one category. We rely on RepRisk's classification and focus on incidents categorised as misleading communication, including greenwashing, false advertising, off-label marketing and astroturfing. This incident-based approach differs from previous literature related to greenwashing and CSR decoupling in two ways. First, rather than relying on disclosure score measures that are largely under managerial control, we use externally identified incidents which are issue and event driven and go beyond firms' self-reported disclosures. Second, these incidents are not limited to disclosure practices but also include misleading communication at the product or service level and cover ESG issues, including but not limited to greenwashing and social washing.

Our results show that firms with political connections tend to have more misleading communication incidents. This result supports the view that politically connected firms are under greater pressure to shield allied politicians from associations with poor sustainability outcomes, leading them to engage in more misleading communication activities. The association remains robust when we control for firm and year fixed effects, and it also holds when we exploit promotions of firms' home state senators to powerful Senate committee positions as an exogenous shock to political influence.

Our study contributes to the literature on political connections by showing how special ties between firms and politicians are associated with misleading sustainability communication activities (Ahmed et al., 2022; Boubakri, Guedhami, Mishra, & Saffar, 2012; Christensen et al., 2023; Correia, 2014; Faccio & Hsu, 2017; Heese et al., 2017; Khwaja & Mian, 2005; Pham, 2019; Ramanna & Roychowdhury, 2010; Thompson, 2022). This paper also adds to existing work on the determinants of misleading corporate ESG communication activities (Gomes, Marsat, Peillex, & Pijourlet, 2024; Gull, Hussain, Khan, Khan, & Saeed, 2023; Pope, Peillex, El Ouadghiri, & Gomes, 2024; Yu, Luu, & Chen, 2020; Zhang, 2023a; Zhang, 2023b) by identifying political connections as an additional salient driver of such activities.

Our study also offers practical implications for regulators and investors. Firms with strong political ties appear to face distinctive pressure that can elevate sustainability misrepresentation. Recognising these dynamics can help regulators and investors better assess the risks posed by politically connected firms and underscore the need to promote transparency in corporate ESG communication and political activity.

2. Literature review

2.1. Political connection literature

The influence of political connections on corporate behaviour has attracted considerable research attention over the last two decades (Butler, Fauver, & Mortal, 2009; Chaney, Faccio, & Parsley, 2011; Chen, Li, Su, & Sun, 2011; Claessens, Feijen, & Laeven, 2008; Guedhami, Pittman, & Saffar, 2014; Khwaja & Mian, 2005; Leuz & Oberholzer-Gee, 2006; Ramanna & Roychowdhury, 2010). Prior studies document a wide range of benefits associated with political ties. For example, politically connected firms are more likely to receive government bailouts (Faccio et al., 2006), obtain procurement contracts (Goldman, Rocholl, & So, 2013), and enjoy tax advantages (Adhikari, Derashid, & Zhang, 2006; Kim & Zhang, 2016). They

also face lower SEC penalties (Ahmed et al., 2022; Correia, 2014) and gain access to inside information (Christensen et al., 2017; Christensen et al., 2023; Liu et al., 2021; Pham, 2019).

However, another line of previous literature also highlights the cost associated with corporate political connection. One set of studies shows that political ties can distort firms' operational and investment decisions. To maintain a long-term relationship with politicians, connected firms may hire additional employees or expand facilities during election years to stimulate the local economy and support affiliated candidates (Bertrand et al., 2018; Faccio & Hsu, 2017). Some other papers document agency problems and wealth transfers. Cheung, Rau, and Stouraitis (2009) provide evidence that minority shareholders bear wealth transfers to state-owned controlling shareholders. In addition, Wang (2015) suggests that the firms with politically connected directors are more likely to suffer the loss from related-party transactions and the over-investment problems, and Fan, Wong, and Zhang (2007) find that newly partially privatised firms with politically connected CEOs underperform those without such special ties in the post-IPO period. Political connections may also expose firms to greater public attention and increase regulatory scrutiny. Heese et al. (2017) show that politically connected firms are more likely to receive SEC comment letters, and they argue that SEC staff appear to target these firms because they view these connections as a risk indicator.

2.2. Misleading communication around sustainability

A growing number of studies have paid attention to misleading communication around sustainability, often using terms such as “greenwashing” or “CSR decoupling”. This prior literature can be grouped into three main streams based on how misleading behaviour is measured. Some studies focus on the selectivity of environmental disclosure, for example, the proportion of significant environmental information disclosed relative to the total information available (Gomes et al., 2024; Marquis, Toffel, and Zhou, 2016; Pope et al., 2024). Other work

refers to the gap between firms' symbolic (external) actions and substantive (internal) actions (Bothello, Ioannou, Porumb, and Zengin Karabrahimoglu, 2023; Hawn and Ioannou, 2016). Although the gap could arise in either direction, only situations in which symbolic efforts exceed substantive efforts are typically regarded as indicators of inappropriate behaviours. Some recent studies also look at the difference between a firm's relative disclosure performance and its relative actual performance (Gull et al., 2023; Tashman, Marano, and Kostova, 2019; Yu et al., 2020; Zhang, 2023b).

Our study is related to above studies on greenwashing and CSR decoupling, but differs in two important respects. First, rather than relying on disclosure or score measures that are largely under managerial control, we use incident data on misleading communication. These data are issue- and event-driven, based on external sources beyond firms' self-reported disclosures, and are therefore less directly managed by firms. Second, the incidents we study are not limited to disclosure practices but also capture misleading conduct at the product or service level, such as false labelling and false advertising, and cover environmental, social and governance issues, including but not limited to greenwashing and social washing.

3. Hypothesis development

Political connections can provide firms with benefits such as preferential access to inside information and more favourable treatment from regulators (Christensen et al., 2023; Correia, 2014; Mehta, Srinivasan, & Zhao, 2020; Pham, 2019). At the same time, these ties expose both the firm and the connected politician to heightened public and media scrutiny. When a politically connected firm performs poorly on environmental or social dimensions, stakeholders may interpret this underperformance as evidence of regulatory capture or misuse of public office, which increases the reputational and political costs related to the connection.

Political cost theory suggests that firms anticipate such costs and adjust their reporting and disclosure to reduce the likelihood of adverse political actions. Watts and Zimmerman (1978) argue that visible firms with high reported profits attract public attention and may face higher taxes and stricter regulation, giving managers more incentives to manage earnings downward to avoid these costs. Subsequent work shows that firms manage earnings to obtain direct favours from regulators, for example, during import relief investigations (Jones, 1991), or to reduce the political costs associated with high visibility and large profits (Pappas, Walker, Xu, & Zeng, 2024; Ramanna & Roychowdhury, 2010). Therefore, firms can use reporting strategies to avoid public scrutiny and to protect the benefits they receive from the politicians.

We extend this premise to sustainability communication. Political connections provide firms with different benefits, but they also increase the visibility of both the firm and the connected politician. Any negative information about a firm's sustainability performance is therefore more likely to attract public attention and be interpreted as evidence of regulatory capture or misuse of public office, raising the reputational and political costs of the connection. To reduce this risk and to protect the benefits associated with their political ties, politically connected firms have a stronger incentive to avoid visible associations between their sustainability performance and their connected politicians. Because genuine improvements in sustainability performance are costly and often slow to achieve, managers may instead resort to symbolic strategies such as misleading communication. By engaging in misleading communication activities, such as overstating their efforts in sustainability and selectively omitting unfavourable information, these firms seek to polish their public image and preserve the private benefits of the political relationship. These arguments lead to the following hypothesis:

Hypothesis 1a: Politically connected firms tend to have more misleading communication incidents around sustainability.

The relationship between political connections and misleading sustainability communication may operate in the opposite direction. Under a sheltering channel (Xiao & Shen, 2022), political ties insulate firms from some of the external pressures that would otherwise encourage them to manage impressions. Politically connected firms can rely on their relationships with public officials to soften regulatory actions (Correia, 2014; Thompson, 2022) and reduce part of the criticism associated with weak sustainability performance. Because poor performance is less likely to trigger severe sanctions or political backlash, these firms face lower political costs from not achieving strong sustainability outcomes and have less need to present themselves in an especially positive light. Therefore, misleading sustainability communication becomes less necessary as a tool to manage public perceptions. These arguments lead to the following alternative hypothesis:

Hypothesis 1b: Politically connected firms tend to have fewer misleading communication incidents around sustainability.

4. Methodology

4.1. Political connection measurement

We use corporate PAC contributions as our main measure of political connections. In the U.S., firms are not permitted to donate directly to parties and candidates. Instead, affiliated PACs can be established, which collect contributions from individuals associated with the firm, such as employees and executives. These PACs are typically overseen by a treasurer who is often a political specialist and is responsible for maximising the effectiveness of the PAC's funds.

We obtain PAC contribution data from OpenSecrets, a US non-profit organisation that collects and performs basic processing of raw data from the Federal Election Commission (FEC). Data from OpenSecrets is widely used in studies related to political connections and contributions (Ahmed et al., 2022; Bradley et al., 2016; Heese et al., 2017; Mehta et al., 2020).

Following prior literature (Heese et al., 2017; Knechel & Park, 2022; Krishnan, Li, & Park, 2022; Thompson, 2022), we measure political connections using the sum of PAC contribution amounts over the last three years as political connections tend to be persistent rather than one-off and are built gradually.

4.2. Misleading communication event measurement

We measure misleading communication activities using RepRisk incident dataset, a widely recognised data resource used in previous studies (Hrazdil, Anginer, Li, & Zhang, 2024; Kim & Zhang, 2016; Li & Wu, 2020; Lin, She, Yoon, & Zhu, 2025). RepRisk screens more than 150,000 public sources and stakeholder outlets every day across 23 languages, and it deliberately excludes company self-disclosures. Sources include print and news media, social media, government bodies, regulators, think tanks, and other outlets at international, national, regional, and local levels. Its methodology is issue- and event-driven rather than company-driven, and it scans sources for ESG risk incidents. RepRisk reports that its algorithm can flag any companies exposed to ESG risks. The dataset is organised into 28 ESG issue categories, with every incident linked to at least one category.

We rely on RepRisk's incident classification and focus on those labelled as misleading communication. In RepRisk, this type of incident covers cases where a firm manipulates the truth to present itself positively while its actions contradict that image, or where it misleads consumers about products or services. Examples include 'greenwashing', false advertising, off-label marketing, and 'astroturfing'.

4.3. Data and sample

Table 1 describes our sample selection process. To construct the political contribution dataset, we first build a PAC-firm linking table. We start from the PAC names in OpenSecrets and match them to firm names in Compustat Snapshot. We initially apply exact name matching where the

PAC name clearly contains the full firm name. For the remaining unmatched PACs, we use fuzzy matching between PAC names and Compustat firm name. This procedure yields a mapping from each PAC to a Compustat firm. In addition, we manually review the fuzzy matching results to ensure the quality of the mapping. Next, we process the contribution transactions and aggregate them to the firm-level. Using the transaction-level records in OpenSecrets, we first compute annual total contributions for each PAC by year. We then merge these PAC-year with the PAC-firm linking table and calculate contributions of each firm each year, producing a firm-year dataset of PAC contributions.

To process RepRisk incidents, we first aggregate incident-level observations to the firm-level and then assign a value of zero to the number of misleading communication incidents if a firm in the Compustat-OpenSecrets sample has no recorded incident in a given year. We do this because RepRisk states that it captures any company exposed to business conduct and reputational risks, regardless of the company’s size, sector, country of headquarters or operations, or listing status (RepRisk, 2021). Finally, we merge firm-year political contribution data with the incident data and other datasets. Our final sample includes 8,332 firm-year observations from 2007 to 2022.

4.4. Research design

We use the following baseline regression to examine our research questions:

$$\begin{aligned}
 Miscomm_t = & PC_{t-3,t-1} + Firm_Size_{t-1} + ROA_{t-1} + Leverage_{t-1} + Sales_Grow_{t-1} \\
 & + Cap_Inten_{t-1} + MB_{t-1} + RD_Inten_{t-1} + Advert_Inten_{t-1} + GSVI_{t-1} \\
 & + Analyst_Follow_{t-1} + Industry\ FE + State\ FE + Year\ FE + \varepsilon
 \end{aligned}$$

Regarding the dependent variable, we use the inverse hyperbolic sine of the number of incidents as our main proxies for misleading communication conducts. We apply the IHS transformation to the count-based outcome variables to address their skewed distribution and

to accommodate the frequent zero values in our sample (Glaeser & Omartian, 2022; Kim, Shi, & Verdi, 2025).

Our main measure for the political connection $PC_{t-3, t-1}$ is calculated as the sum of the corporate political contribution over the past three years, following previous papers (Heese et al., 2017; Knechel & Park, 2022; Krishnan et al., 2022; Thompson, 2022). We control for a set of variables capturing firm-level financial characteristics and visibility. We include firm size (*Firm_Size*) because larger firms tend to attract greater attention and are more likely to focus on sustainability (Bothello, Ioannou, Porumb, & Zengin-Karaibrahimoglu, 2023; Kim & Lyon, 2015; Pope et al., 2024). Following previous papers on sustainability communication (Gull et al., 2023; Marquis, Toffel, & Zhou, 2016; Pope et al., 2024), we also control for firm performance using return on asset (*ROA*), leverage (*Leverage*) and sales growth (*Sales_Grow*). Because the use of physical assets can affect a firm's environmental performance and related disclosure, we control for capital intensity (*Cap_Inten*). We include the market-to-book ratio (*MB*) as a proxy for growth opportunities and market expectations, which are important drivers of firms' attention to sustainability (Bothello et al., 2023). In addition, research and development intensity (*RD_Inten*) captures a firm's long-term orientation (Pope et al., 2024). Since advertising activities are related both to impression-building and public attention (Pope et al., 2024; Walls & Berrone, 2017), we control for advertising intensity (*Advert_Inten*). Finally, we account for the firm's visibility, including public attention (*GSVI*) and analyst following (*Analyst_Follow*), in line with prior studies (Bothello et al., 2023; Hawn & Ioannou, 2016).

4.5. Descriptive statistics

Table 2 reports the sample distribution by year. There is a declining trend in the number of observations after 2017. This trend is consistent with a reduction in the number of corporate

PACs registered with the FEC in recent years, even though the total number of all types of PACs has increased (Ballotpedia, 2025; FEC, 2025).

Table 3 presents summary statistics for all variables used in the baseline regression analysis. There are 1,359 firm-year observations with a non-zero number of incidents, and the distribution of incident counts is very similar to that reported in other studies using RepRisk incident data (Li & Wu, 2020; Lin et al., 2025). Table 3 also reports that the mean of the three-year sum of political contributions is 0.252 million dollars. The average firm size is 27,919 million dollars, and the mean of *ROA* is 3.5%. In addition, firms in our sample are followed one average by around 11 active analysts.

Table 4 reports the pairwise correlation coefficients of the variables. There is a significantly positive correlation between political connections (*PC*) and the number of misleading communication incidents (*Miscomm_Numb*). This positive correlation provides preliminary evidence that politically connected firms tend to have more misleading communication incidents. The correlation coefficients in Table 5 are generally moderate and mostly below 0.5, except for the correlations between firm size and political connections, and between firm size and analyst following, which are noticeably higher. These higher correlations indicate that larger firms tend to donate more to political campaigns and receive more analyst coverage.

5. Results

5.1. Baseline tests

Table 5 reports the baseline regression results on the association between political connections and misleading communication incidents around sustainability. In column (1), we use the inverse hyperbolic sine of the number of incidents as the dependent variable, as this transformation has advantages over a log transformation in dealing with skewed count data and frequent zero values (Glaeser & Omartian, 2022; Kim et al., 2025). The coefficient on the

political connections is positive and significant at 1% level (Coefficient = 0.031, t-statistics = 4.948). Column (2) uses the natural logarithm of one plus the number of incidents and again has a positive and significant coefficient on political connections at 1% level (Coefficient = 0.238, t-statistics = 4.873). Column (3) reports Poisson regressions using the raw incident count as the outcome variable to address the concern that transforming the count-based variable may bias the regression estimates (Chen & Roth, 2023; Cohn, Liu, & Wardlaw, 2022). The coefficient on $PC_{t-3, t-1}$ remains positive and significant (Coefficient = 0.261, t-statistics = 2.271). Overall, these results indicate that firms with stronger political connection tend to engage in more misleading communication incidents around sustainability.

Regarding control variables, firm size is positively and significantly associated with misleading communication, indicating that larger firms are more likely to involve in such incidents. Advertising intensity is also positively related to the number of incidents, consistent with the argument that firms investing more in promotion are more frequently associated with impression-building.

6. Robustness test

6.1. Firm fixed effect

Table 6 reports the results of regression that include firm and year fixed effects instead of industry, state and year fixed effects. We employ the IHS transformation of the incident count as the dependent variable. The coefficients on political connections remain positive and statistically significant, indicating that our main findings are robust to the inclusion of firm fixed effects.

6.2. Alternative Measure of Political Connection

Table 7 reports a robustness test using an alternative measure of firms' political connections. Instead of capturing political connections using the dollar value of PAC contributions made in the past three years, as in the baseline and prior tables, we follow Christensen et al. (2017) and proxy for political connections by the total number of political candidates to whom a firm contributed over the three-year window. The coefficient on political connections continues to be positive and statistically significant, indicating that our main finding is robust to using this alternative proxy.

6.3. Senator Promotion Shock

To further strengthen identification, we exploit exogenous variation created by promotions of state senators to chair influential Senate committees (Cohen, Coval, & Malloy, 2011; Jung, 2024). Our key variable $Senator_Promote_t$ is an indicator that equals one for firms headquartered in a state where a senator is appointed as chair or ranking minority member of a powerful Senate committee in a given year. The variable retains a value of one for the subsequent six years and is zero otherwise. We focus on the five most powerful Senate committees (e.g., Finance, Veterans' Affairs, Appropriations, Rules and Administration, Armed Services).

We obtain U.S. Senate committee assignment data from Charles Stewart III and Jonathan Woon (Congressional Committee Assignments, 103rd–114th Congresses, 1993–2017). Using these data, we first construct a state-year dataset that identifies the years in which each state's senator holds a powerful committee chair or ranking minority position. We then merge this state-year dataset with the other datasets using firms' historical headquarter states.

Table 8 reports the regression results using senator promotion as an alternative measure of political connections. The coefficient on $Senator_Promote_t$ is positive and statistically significant in column (1) and column (2), indicating that firms whose home-state senator is

promoted to a powerful committee experience more misleading sustainability misrepresentation incidents. These results are consistent with our baseline findings and suggest that exogenous increases in influence of political ties are associated with higher levels of misleading sustainability communication.

7. Forthcoming analysis and Conclusion

As part of additional analysis, we will also use lobbying expenses obtained from OpenSecrets as an alternative measure of political connections. In the U.S., firms spend billions of dollars annually lobbying Congress and federal agencies in addition to making political campaign contributions. Lobbying expenses have also been widely used as a measure of political connections in previous studies (Ahmed et al., 2022; Bradley et al., 2016; Heese et al., 2017; Mehta et al., 2020). We will construct a political connection variable by calculating the sum of lobbying expenses over the past three years.

This study examines whether firms with political connections engage in more misleading communications activities around sustainability. Our baseline analysis shows a positive and statistically significant association between political connections and misleading sustainability communication incidents. These findings are consistent with a political cost mechanism: politically connected firms face stronger incentives to manage impressions about their sustainability performance in order to protect the benefits associated with their political ties.

The study contributes to the literature on political connections by documenting how political ties are linked to corporate non-financial communication. It also adds to work on sustainability or ESG communication by relying on externally identified ESG incidents, rather than disclosure-based measures that are largely under managerial control. The results have

implications for regulators and investors, suggesting that political ties may be an additional risk factor when assessing the credibility of firms' sustainability related claims.

Appendix A Variable definition

Variable	Description
<i>Misleading communication incident variables</i>	
<i>Miscomm_Numb</i>	The number of misleading communication incidents
<i>Miscomm_IHS</i>	The inverse hyperbolic sine transformation of the number of misleading communication incidents
<i>Miscomm_Ln</i>	The natural log of the number of misleading communication incidents plus one
<i>Political connection variables</i>	
<i>PC</i>	The total amount of PAC contributions over the past three years
<i>PC_Cands</i>	The total number of unique political candidates to whom the firm's PAC made contributions over the past three years
<i>Senator_Promote</i>	A dummy variable equal to one if the firm is headquartered in a state where a senator is appointed as chair or ranking minority member of a powerful Senate committee in a year t , and for the subsequent six years; zero otherwise
<i>Control variables</i>	
<i>Advert_Inten</i>	Advertising expenses divided by sales
<i>Analyst_Follow</i>	The natural logarithm of the number of analysts providing earnings forecasts within the 30-day period preceding the firm's fiscal year-end date plus one
<i>Cap_Inten</i>	Net property, plant and equipment divided by total assets
<i>Firm_Size</i>	The natural logarithm of total assets plus one
<i>GSVI</i>	The natural logarithm of Google Search Volume Index plus one
<i>Leverage</i>	Long-term debt divided by total assets.
<i>MB</i>	The market value of equity divided by the book value of total assets.
<i>RD_Inten</i>	Research and development expenses divided by sales
<i>ROA</i>	Net income divided by total assets
<i>Sales_Grow</i>	The percentage change in sales revenue from year $t-1$ to year t

Table 1 Sample Selection

	Number of observations	Number of firms (PACs)
OpenSecrets corporate PAC contribution data from 1989 to 2022	2,923,353	5,904
After matching and merging the with Compustat from 1989 to 2022	27,063	2,211
After merging with RepRisk data from 2007 to 2022	10,277	988
After excluding observations with missing values	8,332	945

Table 2 Sample Distribution

Sample distribution by year	
Year	Number of firm-years
2007	541
2008	532
2009	542
2010	540
2011	590
2012	566
2013	557
2014	554
2015	539
2016	541
2017	491
2018	499
2019	472
2020	484
2021	441
2022	443
All years	8,332

Table 3 Summary Statistics

Variable	N	Mean	SD	p25	Median	p75
<i>Miscomm_Numb</i>	8,332	0.477	2.016	0.000	0.000	0.000
<i>PC (\$ m)</i>	8,332	0.252	0.414	0.023	0.086	0.272
<i>Firm Size (\$ m, unlogged)</i>	8,332	27,919	56,067	2,710	8,202	25,536
<i>ROA</i>	8,332	0.035	0.083	0.009	0.036	0.074
<i>Leverage</i>	8,332	0.267	0.189	0.122	0.252	0.371
<i>Sales Grow</i>	8,332	0.072	1.100	-0.033	0.041	0.118
<i>Cap Inten</i>	8,332	0.314	0.270	0.083	0.218	0.544
<i>MB</i>	8,332	2.917	5.213	1.280	2.075	3.599
<i>RD Inten</i>	8,332	0.025	0.056	0.000	0.000	0.017
<i>Advert Inten</i>	8,332	0.011	0.023	0.000	0.000	0.011
<i>GSVI</i>	8,332	4.480	2.320	3.902	5.440	6.120
<i>Analyst Follow (unlogged)</i>	8,332	11.120	8.564	4.000	10.000	17.000

Table 4 Pearson correlation

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
(1) <i>Miscomm_Numb</i>	1.000											
(2) <i>PC</i>	0.292	1.000										
(3) <i>Firm Size</i>	0.299	0.557	1.000									
(4) <i>ROA</i>	0.075	0.116	0.115	1.000								
(5) <i>Leverage</i>	-0.001	-0.009	0.013	-0.147	1.000							
(6) <i>Sales Grow</i>	-0.003	-0.014	0.000	0.022	0.001	1.000						
(7) <i>Cap Inten</i>	0.033	-0.005	0.005	-0.071	0.330	-0.006	1.000					
(8) <i>MB</i>	0.063	0.106	0.019	0.161	-0.014	0.002	-0.075	1.000				
(9) <i>RD Inten</i>	0.022	0.031	-0.047	0.028	-0.140	0.014	-0.262	0.154	1.000			
(10) <i>Advert Inten</i>	0.140	-0.008	0.014	0.063	-0.009	-0.008	-0.202	0.113	0.116	1.000		
(11) <i>GSVI</i>	0.036	0.099	0.167	0.053	0.035	-0.015	0.043	0.035	0.054	0.018	1.000	
(12) <i>Analyst Follow</i>	0.185	0.350	0.515	0.152	-0.011	0.004	-0.005	0.120	0.196	0.049	0.128	1.000

Notes: This table presents Pearson correlation coefficients between the variables used in our baseline regression model. Bold coefficients are statistically significant at the 1% level or below. See Appendix A for the detailed definitions of all variables. We winsorize all continuous variables at the 1st and 99th percentiles.

Table 5 Baseline tests: the impact of political connections on misleading communication incidents around sustainability

Dependent Variable:	<i>Misleading Communication</i>		
	<i>Miscomm_IHS</i>	<i>Miscomm_Ln</i>	<i>Miscomm_Numb</i>
	(1)	(2)	(3)
<i>PC_{t-3, t-1}</i>	0.301*** (4.948)	0.238*** (4.873)	0.261** (2.271)
<i>Firm_Size_{t-1}</i>	0.090*** (7.471)	0.070*** (7.397)	0.867*** (15.952)
<i>ROA_{t-1}</i>	0.071 (0.764)	0.055 (0.745)	2.000*** (2.807)
<i>Leverage_{t-1}</i>	-0.106 (-1.533)	-0.084 (-1.523)	-0.424 (-1.306)
<i>Sales_Grow_{t-1}</i>	0.002 (1.336)	0.001 (1.376)	0.011 (0.185)
<i>Cap_Inten_{t-1}</i>	0.084 (1.209)	0.071 (1.258)	0.264 (0.631)
<i>MB_{t-1}</i>	-0.001 (-0.372)	-0.001 (-0.360)	0.002 (0.323)
<i>RD_Inten_{t-1}</i>	0.047 (0.173)	0.038 (0.178)	-3.492** (-2.363)
<i>Advert_Inten_{t-1}</i>	2.067** (2.074)	1.646** (2.068)	9.881*** (4.315)
<i>GSVI_{t-1}</i>	-0.005 (-1.310)	-0.004 (-1.281)	0.024 (1.103)
<i>Analyst_Follow_{t-1}</i>	0.002 (0.914)	0.001 (0.951)	0.028*** (4.086)
<i>Constant</i>	-0.674*** (-7.460)	-0.528*** (-7.404)	-9.707*** (-18.717)
<i>Observation</i>	8,332	8,332	8,332
<i>Adj. R²</i>	0.358	0.356	
<i>Industry FE</i>	Yes	Yes	Yes
<i>State FE</i>	Yes	Yes	Yes
<i>Year FE</i>	Yes	Yes	Yes

Notes: This table reports the regression results for the impact of political connection on misleading communication incidents around sustainability. See Appendix A for the detailed definitions of all variables. Column (1) uses the inverse hyperbolic sine (IHS) transformation of the number of incidents as the dependent variable. Column (2) uses the natural logarithm of one plus the number of misleading communication incidents as the dependent variable. Column (3) report Poisson regression results, where the dependent variable is the raw count of incidents. *PC_{t-3, t-1}* denotes the total amount of political contributions over the past three years. We winsorize all continuous variables at the 1st and 99th percentiles. The numbers enclosed in parentheses are the t-statistics computed using firm-level clustered standard errors. ***, **, and * represent significant levels of 1%, 5%, and 10%, respectively.

Table 6 Robustness test: Firm fixed effect

Dependent Variable:	<u>Misleading Communication</u>
	(1)
<i>PC_{t-3, t-1}</i>	0.315*** (3.286)
<i>Firm_Size_{t-1}</i>	-0.006 (-0.201)
<i>ROA_{t-1}</i>	0.030 (0.370)
<i>Leverage_{t-1}</i>	0.183** (2.093)
<i>Sales_Grow_{t-1}</i>	0.002*** (2.842)
<i>Cap_Inten_{t-1}</i>	0.009 (0.066)
<i>MB_{t-1}</i>	-0.001 (-0.526)
<i>RD_Inten_{t-1}</i>	0.512 (0.990)
<i>Adver_Inten_{t-1}</i>	-1.183 (-0.997)
<i>GSVI_{t-1}</i>	-0.005 (-0.783)
<i>Analyst_Follow_{t-1}</i>	0.006*** (3.110)
<i>Constant</i>	0.110 (0.369)
<i>Observation</i>	8,332
<i>Adj. R²</i>	0.596
<i>Firm FE</i>	Yes
<i>Year FE</i>	Yes

Notes: This table reports the robustness test results using firm and year fixed effects. See Appendix A for the detailed definitions of all variables. We use the inverse hyperbolic sine (IHS) transformation of the number of incidents as the dependent variable. $PC_{t-3, t-1}$ denotes the total amount of political contributions over the past three years. We winsorize all continuous variables at the 1st and 99th percentiles. The numbers enclosed in parentheses are the t-statistics computed using firm-level clustered standard errors. ***, **, and * represent significant levels of 1%, 5%, and 10%, respectively.

Table 7 Robustness test: Alternative measure of political connection

Dependent Variable:	<i>Misleading Communication</i>
	(1)
<i>PC_Cands_{t-3, t-1}</i>	0.017*** (2.834)
<i>Firm_Size_{t-1}</i>	0.125*** (8.658)
<i>ROA_{t-1}</i>	0.076 (0.803)
<i>Leverage_{t-1}</i>	-0.156** (-2.159)
<i>Sales_Grow_{t-1}</i>	0.002* (1.822)
<i>Cap_Inten_{t-1}</i>	0.136*** (2.643)
<i>MB_{t-1}</i>	0.000 (0.078)
<i>RD_Inten_{t-1}</i>	0.286 (1.055)
<i>Adver_Inten_{t-1}</i>	2.270** (2.322)
<i>GSVI_{t-1}</i>	-0.006 (-1.554)
<i>Analyst_Follow_{t-1}</i>	0.002 (0.828)
<i>Constant</i>	-1.002*** (-8.322)
<i>Observation</i>	7,537
<i>Adj. R²</i>	0.347
<i>Industry FE</i>	Yes
<i>State FE</i>	Yes
<i>Year FE</i>	Yes

Notes: This table reports the regression results using the total number of unique political as an alternative measure. See Appendix A for the detailed definitions of all variables. We use the inverse hyperbolic sine (IHS) transformation of the number of incidents as the dependent variable. *PC_Cands_{t-3, t-1}* denotes the total number of unique political candidates to whom the firm's PAC made contributions over the past three years. We winsorize all continuous variables at the 1st and 99th percentiles. The numbers enclosed in parentheses are the t-statistics computed using firm-level clustered standard errors. ***, **, and * represent significant levels of 1%, 5%, and 10%, respectively.

Table 8 Robustness test: Senator promotion shock

Dependent Variable:	<i>Misleading Communication</i>
	(1)
<i>Senator_Promote_t</i>	0.014** (2.390)
<i>Firm_Size_{t-1}</i>	-0.013 (-1.297)
<i>ROA_{t-1}</i>	0.003 (0.670)
<i>Leverage_{t-1}</i>	0.043 (1.370)
<i>Sales_Grow_{t-1}</i>	-0.000 (-0.290)
<i>Cap_Inten_{t-1}</i>	-0.000 (-0.861)
<i>MB_{t-1}</i>	-0.034 (-0.530)
<i>RD_Inten_{t-1}</i>	0.003*** (3.173)
<i>Adver_Inten_{t-1}</i>	-0.018 (-0.134)
<i>GSVI_{t-1}</i>	-0.001 (-0.246)
<i>Analyst_Follow_{t-1}</i>	0.006*** (3.919)
<i>Constant</i>	0.142* (1.826)
<i>Observation</i>	28,484
<i>Adj. R²</i>	0.507
<i>Firm FE</i>	Yes
<i>Year FE</i>	Yes

Notes: This table reports the regression results using senator promotion as an alternative measure of political connections. See Appendix A for detailed definitions of all variables. We use the inverse hyperbolic sine (IHS) transformation of the number of incidents as the dependent variable. *Senator Promote_t* is an indicator that equals one for firms headquartered in a state whose senator becomes a chair or ranking minority member of a powerful Senate committee in year *t*, and in each of the subsequent six years, and zero otherwise. All continuous variables are winsorised at the 1st and 99th percentiles. t-statistics based on standard errors clustered by headquarters state and industry are reported in parentheses. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

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Regulating Sustainability Assurance: IAASB's Strategic Turn to ISSA 5000

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1. Introduction

The emergence of sustainability reporting and assurance as regulatory and professional concerns reflects a notable shift in contemporary accounting practice (Farooq and De Villiers, 2019; Gaudy and Malsch, 2023; Harrer and Lehner, 2024). Historically regarded as an optional, narrative-focused addition to financial reporting, sustainability reporting has progressively been integrated as an obligation within the fundamental architecture of financial markets. In this context, the EU's regulatory ambition, initiated by the European Green Deal in 2019 and formalised in the Corporate Sustainability Reporting Directive (CSRD), replacing NFRD, marks a significant regulatory inflection point. The reporting requirements begin with the 2024 reporting year (reports in 2025) as the CSRD phases in sustainability disclosure and assurance obligations and extends the assurance remit beyond statutory auditors to accredited practitioners operating under regulatory oversight (EU, 2022; EC, 2025). The associated European Sustainability Reporting Standards (ESRS) published in 2023, established by the European Financial Reporting Advisory Group (EFRAG), reinforce this framework by prioritising public interest, highlighting double materiality, and enhancing stakeholder accountability (EC, 2023). This regulatory development does not align with a conventional top-down pattern of international standard diffusion. The EU-led initiatives have positioned it as a key agenda-setter and required adjustments from international standard-setting institutions to meet the EU timeline. At the global level, the establishment of the International Sustainability Standards Board (ISSB) in 2021 and the release of IFRS S1 and S2 reflect this intensifying and largely responsive movement toward harmonised sustainability disclosure (IFRS Foundation, 2020; 2021; 2023). In this backdrop, the International Auditing and Assurance Standard Board (IAASB) released International Standard on Sustainability Assurance (ISSA) 5000 in 2024 as a framework-neutral and profession-agnostic global baseline standard, aiming to address the persistent fragmentation and credibility concerns in sustainability assurance (SA) endorsed by the International Organization of Securities Commissions (IOSCO) (IAASB, 2024a; IOSCO, 2024). Together with the International Ethics Standards Board for Accountants (IESBA)'s ethics and independence standard, ISSA 5000 serves as a global uniform benchmark for all practitioners to follow (IFEA, 2025), reframing discussions over the IAASB's institutional significance in a rapidly changing and complex assurance environment.

Assurance is commonly framed across sustainability frameworks as a governance mechanism intended to enhance the credibility of disclosures, reduce information asymmetry, and address legitimacy concerns arising from regulatory demands and greenwashing risk (Simnett, Vanstraelen and Chua, 2009; Boiral, Heras-Saizarbitoria and Brotherton, 2019a; Hazaea et al., 2022; Alsahali, 2025). However, unlike financial auditing, SA has evolved through a

fragmented and decentralised landscape, characterised by heterogeneous providers and competing frameworks (Perego and Kolk, 2012; Farooq and De Villiers, 2018; Channuntapipat, Samsonova-Taddei and Turley, 2020; Ge, Simnett and Zhou, 2024a). This diversity also manifests at the firm level, where auditors, consultants, and engineers draw on distinct professional logics and methodological repertoires, often resulting in uneven assurance practices (Farooq & De Villiers, 2019; Hazaea et al., 2022; Simnett et al., 2022). Related empirical work further suggests that traditional audit approaches are frequently ill-equipped to capture the qualitative, forward-looking, and stakeholder-orientated characteristics that increasingly define ESG reporting (Krasodomska, Simnett and Street, 2021; De Villiers et al., 2022). Scholars have scrutinised the adequacy of current IAASB assurance standards, including ISAE 3000, emphasising their insufficient ability to encompass the procedural and subjective aspects of sustainability performance (O'Dwyer, 2011; Simnett, 2012; Sonnerfeldt and Pontoppidan, 2020; Ge, Simnett and Zhou, 2024). The rise in SA adoption contrasts with its inconsistent and underdeveloped implementation compared to reporting practices. Although 54% of N100 and 69% of G250 companies currently secure some form of assurance (KPMG, 2024; 2025b), the assured scope remains heavily concentrated on GHG emissions, with other material sustainability dimensions receiving substantially less attention (IFAC, 2025b). This narrow focus suggests that assurance remains largely compliance-driven, reflecting limited organisational readiness rather than a fully embedded governance function grounded in internal commitment (KPMG, 2025a).

Considering shifting regulatory dynamics, SA is increasingly examined not simply as a technical exercise, but as a landscape of regulatory politics, professional jurisdiction, and legitimacy-building through which standard-setters seek to establish authority within a rapidly consolidating field (Lemma et al., 2024; Becker, Gronewold and Weiß, 2025; Carungu et al., 2025). Emerging developments in the mid-2020s include the extension of private standard-setting into areas such as sustainability reporting and assurance. In contrast, a significant portion of the accounting literature has approached the IAASB primarily through the lens of financial auditing, emphasising its role in the establishment of international audit standards and the institutional dynamics underpinning that process (Humphrey et al., 2004; Loft, Humphrey and Turley, 2006; Jedidi and Humphrey, 2024; Humphrey, Mardini and O'Dwyer, 2025). Agenda-setting studies further perceive persistent professional contestation, weak standardisation, and uneven assurance quality, even in regulated contexts (Free, Jones and Tremblay, 2024; Pizzi, Venturelli and Caputo, 2024; 2025). Despite introducing a dedicated SA standard only in 2024, more than two decades after the release of its general assurance framework in ISAE 3000, the IAASB's delayed engagement invites closer scrutiny of its strategic orientation as a financial audit standard setter. However, the way in which the IAASB has positioned itself vis-à-vis other institutional actors shaping SA remains underexplored, particularly as the field becomes increasingly regulated and contested. The convergence of recent regulatory developments has produced an unusually compressed window of change, rendering an examination of the IAASB's positioning strategies both timely and empirically revealing. Thus, this research investigates the following research question:

How and why did the IAASB, through ISSA 5000, seek to position itself within the emerging field of sustainability assurance, and what dynamics shaped the development and positioning of the standard?

The remainder of the chapter is organised as follows. Section 2 examines prior research on SA, fragmentation, and transnational standard setting. Section 3 develops the theoretical framework, drawing on concepts of regulatory space, institutional layering, and legitimacy. Section 4 outlines the research design and analytical approach. Section 5 analyses the IAASB's positioning strategies, tracing four phases from the late 1990s to 2024.

2 Literature Review

2.1 History of IAASB and governance structure prior to 2020:

The IAASB originated in the late 1970s, during a period when the global accountancy profession aimed to standardise auditing practices. After the establishment of the International Federation of Accountants in 1977, the International Auditing Practices Committee was created in 1978 as one of IFAC's standing committees (Camfferman and Zeff, 2007; 2018; Camfferman, 2020). Initially, the IAPC issued International Auditing Guidelines (IAGs) before moving toward more formalised International Standards on Auditing (ISAs) by 1991, reflecting growing demands for transparency in global auditing practices (Roussey, 1992; Humphrey, Loft and Samsonova-Taddei, 2014). This shift reflected the IAPC's maturation into a de facto global audit standard-setter, albeit one that remained profession-led, voluntarily adopted, and narrowly orientated toward financial statement assurance under IFAC's governance. However, triggered by corporate scandals in the 2000s, IFAC restructured and renamed the IAPC into the International Auditing and Assurance Standards Board (IAASB) in 2002 to tackle the credibility crisis from self-regulatory to explicit public-interest legitimacy (Loft and Humphrey, 2010; Lessambo, 2018). The Monitoring Group (MG), consisting of six international regulatory and institutional bodies (Basel Committee, European Commission, Financial Stability Board, IAIS, IOSCO, and World Bank), was established in 2002 through a regulator-led dialogue with IFAC. In 2003, a series of governance reforms were initiated that shifted IFAC's standard-setting activities to foster closer collaboration with international regulatory bodies, forming a coalition of public authorities responsible for ensuring the credibility and public-interest focus of international auditing standards. This process resulted in the establishment of the Public Interest Oversight Board (PIOB) in 2005, incorporating public interest considerations into the architecture of international standard setting. Changes to governance that followed contributed to the broader uptake of International Standards on

Auditing (ISAs), most visibly through the European Union's 2006 Statutory Audit Directive (Directive 2006/43/EC), which required member states to give effect to ISAs within their national statutory audit frameworks (Humphrey and Loft, 2009; Humphrey et al., 2014). With the introduction of a three-tier governance arrangement, the IAASB's institutional orientation shifted away from self-regulation and toward a structure grounded in public-interest oversight and formalised accountability.

IFAC acts as the global representative body of the accounting profession and hosts the IAASB as an independent standard-setting board, bringing together 180 professional accountancy organisations across more than 135 jurisdictions and representing millions of professional accountants worldwide as of 2024 (IFAC, 2024b). The IAASB's standard-setting board consists of 18 members, which include nine non-practising accountants nominated by IFAC member bodies, an independent chair, at least three public interest representatives, and other members nominated by the Transnational Auditors Committee (TAC). Operating within the Forum of Firms, the TAC brings together major international audit networks and functions as an interface between the International Federation of Accountants and the wider regulatory environment, advancing commitments to audit quality and cross-jurisdictional consistency (Humphrey and Loft, 2011; Dewing and Russell, 2014; Wymeersh, 2015a). Despite its formal independence, IAASB's financial dependence on IFAC and prominent international audit firms has raised scholarly concerns about the strength of its autonomy, as independence is safeguarded procedurally rather than substantively, leaving room for both perceived and structural influence by the profession it regulates, where arrangements may favour professional interests and perpetuate regulatory capture (Botzem and Quack, 2006; Humphrey and Loft, 2006; Burns and Fogarty, 2010; Malsch and Gendron, 2011). Stakeholder participation is formalised via Consultative Advisory Groups (CAG) without reallocating decision authority, reinforcing a governance model in which participation supports procedural legitimacy but rarely translates into substantive influence over standard-setting outcomes (Loft, Humphrey and Turley, 2006; Wymeersh, 2015b).

The PIOB operates as a secondary layer of governance in auditing standard setting, with responsibility for safeguarding the public interest across agenda-setting, priorities, and work programmes (PIOB, 2006; 2007; Wymeersh, 2015b). The board consists of a maximum of ten members who serve six-year terms and adhere to a stringent ethical code that forbids members from engaging in public practice as auditors or being employed by audit firms (IFAC, 2003b; PIOB, 2007; IOSCO, 2008). Funding operates on a multi-stakeholder basis, with a significant share historically coming via the IFAC alongside contributions from public authorities and other stakeholders, a configuration that has long fuelled debate over the credibility of independence within a system that remains partly profession-financed (Humphrey et al., 2006; Wymeersch, 2015). The PIOB's exclusion from technical draughting and operational decision-making restricts its ability to impact substantive outcomes, despite its oversight mandate (Botzem, 2012; Wymeersch, 2015b). The public interest representation is channelled through oversight and consultation rather than direct participation, a configuration that critics associate

with a continuing democratic deficit in transnational standard-setting governance (Humphrey et al., 2006; Loft, Humphrey and Turley, 2006). The MG sits at the apex of oversight within the global financial regulatory framework. Its involvement centres on supervision of the PIOB, including responsibility for appointments and ongoing review of performance, which places the MG at the interface between public authorities and auditing standard setting (Humphrey and Loft, 2006; Richardson and Eberlein, 2011). Alongside this role, the MG interacts with standard-setting bodies by providing regulatory and policy input and by commenting on technical proposals, including through Consultative Advisory Groups (Humphrey and Loft, 2011; Humphrey et al., 2011). However, scholars note that the MG's influence is exercised at a strategic and procedural level of system oversight rather than through intervention in technical standard setting, meaning that while it enhances regulatory legitimacy and acceptance, it leaves substantive professional autonomy largely intact (Botzem, 2012; Richardson & Eberlein, 2011; Wymeersch, 2015).

2.2 IAASB governance structure post 2020:

In 2015, the MG initiated a reform process addressing concerns regarding public-interest orientation and legitimacy in international audit and ethics standard setting, especially the impact of the accountancy profession. Concerns focused on IFAC's roles in funding and nominations, as well as the persistent influence of audit firms in standard-setting, resulting in the issuance of the MG's recommendations in 2020 (MG, 2020). The reform agenda focused on identified deficiencies in public-interest orientation, emphasising IFAC's role in supporting standard-setting boards and the impact of professional bodies via board composition and consultation processes. The MG also expressed concern that standards risked lacking sufficient relevance and timeliness in a rapidly changing audit and business environment. The implementation was planned through a transition strategy to be completed within nine months of the recommendations, with finalisation within three years. A key institutional outcome was the formation of the International Foundation for Ethics and Audit (IFEA), which was incorporated in October 2022 and became active in January 2023, reorganising the IAASB and IESBA under a separate legal body from IFAC (IFEA, 2023b; 2023a). However, the IFEA governance structure consists of three members: MG, PIOB, and IFAC. The foundation is governed by a six-member board of trustees, with four public interest trustees nominated by the PIOB and two private industry trustees nominated by IFAC. The MG also retains the right to appoint an observer. The governance of the International Foundation for Ethics and Audit is shared by three member organisations: the Monitoring Group, the Public Interest Oversight Board, and the International Federation of Accountants. Oversight is exercised through a six-member Board of Trustees, comprising four public-interest trustees nominated by the PIOB and two private-industry trustees nominated by IFAC. The MG does not sit on the board but retains the right to appoint an observer. The chairs of the IAASB and IESBA serve as IFEA co-CEOs. Following reform, the board appointments are now handled through the PIOB's Standard-Setting Board Nominations Committee, using an open call and skills-based selection process, with a reduction in board size from 18 to 16 members. While the PIOB's oversight role remains in place, the post-reform arrangements increase its visibility and influence through

greater transparency, including public reporting, open meetings, and direct control over nominations (PIOB, 2022). From 2024, stakeholder input is channelled through a Stakeholder Advisory Council (SAC), replacing the former CAGs. At the same time, funding remains a fundamental tension. IFEA’s disclosures indicate significant continuing reliance on IFAC-linked funding arrangements, including contributions associated with the FOF, despite IFEA’s stated objective of increasing external contributions (IFEA, 2023b; 2023a). This preserves a structural tension, as while formal independence has been strengthened through governance redesign, financial reliance remains partly profession-proximate, sustaining potential perception-based legitimacy risks. Although these reforms were initially driven by debates over audit and ethics legitimacy, they now shape the boards’ expanding sustainability responsibilities, raising questions about their institutional fit for SA.

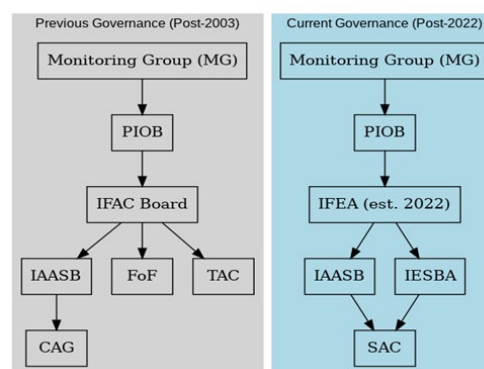


Figure 1 Change in IAASB governance structure over time

2.3 Competitive Dynamics in the Sustainability Assurance Market:

The sustainability assurance market is divided between accounting assurance providers (AAPs) and non-accounting assurance providers (NAAPs)(Farooq and De Villiers, 2017). AAPs primarily consist of the Big Four accounting firms (PwC, E&Y, Deloitte, and KPMG), whose longstanding ambitions to extend audit expertise into non-financial domains have been widely observed in the audit society literature (Power, 1996; O’Dwyer, 2011). By contrast, NAAPs are a much more varied group, including global engineering firms, certification bodies, boutique sustainability consultancies, and other types of assurance providers. This categorisation reflects the broad scope of expertise within the field (Manetti and Toccafondi, 2012; Perego and Kolk, 2012; Wong et al., 2016). However, neither provider type fits all the competence criteria (assurance process, industry understanding, and sustainability subject matter) for high-quality engagements(Adams and Evans, 2004; Farooq and De Villiers, 2019). AAPs provide procedural rigour, established quality control frameworks, and reputational capital from financial auditing (Canning, O’Dwyer and Georgakopoulos, 2019; Al-Shaar, Albitar and Hussainey, 2022). The familiarity with the reporting entity’s industry when the assurator serves as the statutory financial auditor facilitates efficiencies and integration between financial and SA (Gillet, 2012; Boiral and Heras-Saizarbitoria, 2020; Bouten and Hoozée,

2024). At the same time, their expertise in sustainability has been historically scrutinised, reflecting the profession's roots in financial rather than non-financial reporting (Gray, 2000; Boiral, Heras-Saizarbitoria and Brotherton, 2019b; Canning, O'Dwyer and Georgakopoulos, 2019). Conversely, NAAPs frequently provide enhanced sustainability and technical knowledge (particularly for emissions, engineering, and lifecycle data). Still, it may face credibility constraints where assurance users expect stronger procedural discipline, independence safeguards, and transparent assurance reporting fields (Gillet, 2012; Manetti and Toccafondi, 2012; Wong et al., 2016; Boiral et al., 2019). In practice, companies' choices reflect this ambiguity. Firms with well-established financial governance arrangements often gravitate toward Big Four audit firms, reflecting perceptions of methodological rigour and organisational familiarity, whereas entities with more specialised sustainability oversight structures may opt for boutique providers that bring deeper subject-matter expertise (Channuntapipat, 2021; Simpson, Aboagye-Otchere and Ahadzie, 2022; Lemma et al., 2024). Divergent client preferences indicate that credibility in SA remains subjective and expectation-contingent, with no provider fully meeting all stakeholder expectations (Moroney, 2016; Boiral et al., 2019; Channuntapipat, Samsonova-Taddei and Turley, 2019). Empirical research has not yet established whether any specific category of provider consistently yields superior sustainability disclosures or organisational enhancements, highlighting an evidentiary gap that sustains ongoing debate (Simpson, Aboagye-Otchere and Ahadzie, 2022; Gaudy and Malsch, 2023; Velte, 2025). The competitive duality has fragmented the field, creating a multidisciplinary gap that necessitates comprehensive expertise, which is currently unfulfilled.

Historically, assurance engagements have been dominated by AAPs, which accounted for 63% of global sustainability assurance engagements in 2019. Their share declined to 61% in 2020 and 57% in 2021 before stabilising at 58% in 2022 and easing again to 55% in 2023. Over the same period, NAAPs expanded their presence, increasing from 37% in 2019 to approximately 42% in 2022 and 45% in 2023, reflecting a gradual but persistent rebalancing of the sustainability assurance market (IFAC, 2022; EFAA, 2024; IFAC, 2025b). This pattern is consistent with a competitive segmentation dynamic in which NAAPs increasingly capture targeted, technically bounded engagements, particularly emissions-related assurance, while the Big Four remain preferred for broader, multi-topic ESG assurance (ICAEW, 2023; Gipper, Ross and Shi, 2024; Dachevski and Ackers, 2025). SA is most developed in Europe, where regulation has made it mandatory rather than voluntary, with much of the work undertaken by audit firms. The standardisation of ESG reporting fits well with audit firms' existing methods and control systems, and their early involvement has helped secure client relationships that are difficult for other providers to displace. In 2022, audit firms, primarily the Big Four, managed 82% of assurance engagements in Europe, an increase from 69% in 2019 and significantly higher than the approximately 58% global share (EFAA, 2024). Although the global sustainability assurance market has become more diverse, Europe has moved in the opposite direction, with ESG assurance increasingly concentrated among the Big Four. Across European jurisdictions, statutory auditors now perform a growing share of ESG assurance engagements, strengthening audit firms' incumbency. The UK and Ireland previously displayed greater openness to non-accounting assurance providers between 2019 and 2022 (ICAEW, 2023), yet

this position has shifted, as audit firms accounted for approximately 63% of FTSE 100 ESG assurance in 2023, pointing to a closer coupling between ESG assurance and financial reporting (EFAA, 2024; ESG News, 2024). By contrast, outside Europe sustainability assurance remains more fragmented. In the United States, audit firms were responsible for roughly one-quarter of sustainability assurance engagements in 2022–2023, with comparably low participation observed across much of the Asia-Pacific region, especially in environmental and GHG-related assurance work (ICAEW, 2023; Gipper, Ross and Shi, 2024; IFAC, 2024a). The extent to which emerging global standards and regulatory initiatives will reshape these patterns has yet to be determined.

2.4 Fragmentation and Pluralism in Sustainability Assurance Standards:

First issued in 2003 and revised in 2013, the International Standard on Assurance Engagements (ISAE) 3000 provides a principles-based framework for assurance engagements beyond the audit of financial statements. In sustainability assurance, accounting firms are commonly required by professional bodies or regulators to apply ISAE 3000, anchoring these engagements in established audit methods and expectations regarding evidence, documentation, and professional (Boiral, Heras-Saizarbitoria and Brotherton, 2019b; Ge, Simnett and Zhou, 2024c; Lehner, 2025). Drawing on the accounting profession's institutional position, ISAE 3000 has become widely adopted internationally (Venter and Van Eck, 2021; KPMG, 2024; 2024). By 2021, around 95% of audit firms providing sustainability assurance applied the standard, up from 88% in 2019, consolidating its status as the default reference point for accounting-led sustainability assurance engagements ((Venter and Krasodomska, 2024; Krasodomska et al., 2025; Sakchuenyos et al., 2025). Simultaneously, its origins in financial auditing have faced criticism, especially for limiting assurance to information chosen for disclosure by companies and for inadequately addressing wider stakeholder issues (Simnett, 2012; Simnett, Zhou and Hoang, 2022). The principles-based design facilitates application flexibility but has led to inconsistent practices among providers (Krasodomska & Simnett, 2021). Research indicates that practitioners utilise considerable professional judgement, with some providing limited assurance with minimal detail, while others embrace broader interpretations (Rossi and Tarquinio, 2017; Boiral, Heras-Saizarbitoria and Brotherton, 2019b; Larrinaga et al., 2020). The inherent subjectivity and lack of prescriptive guidance reduce the comparability and reliability of sustainability assurance outcomes (Carrington, 2018; Richard & Odendaal, 2020). On the other side of the spectrum, NAAPs often prefer the AA1000 Assurance Standard (AA1000AS) to ISAE 3000. By contrast, AA1000AS is designed specifically for sustainability assurance rather than derived from financial audit practice, with an emphasis on inclusivity, materiality, and responsiveness (Farooq and De Villiers, 2018; 2019; Farooq, Zaman and Nadeem, 2021). Engagements conducted under AA1000AS typically extend beyond checks of data accuracy to assess how organisations identify, engage with, and respond to material stakeholder concerns, resulting in more qualitatively orientated assurance statements (Channuntapipat, Samsonova-Taddei and Turley, 2020).

Following the 2013 revision of ISAE 3000, which opened its use to all competent practitioners, many NAAPs have adopted it to boost credibility. Its use is linked to greater transparency and clearer independence disclosures, supporting trust and market growth. Still, varied application and the use of other standards mean consistency is not guaranteed (Boiral, Heras-Saizarbitoria and Brotherton, 2019b; Ge, Simnett and Zhou, 2024c; Krasodomska et al., 2025). Stakeholders tend to favour assurance providers who demonstrate ethical rigour and independence in their practices (Manetti and Toccafondi, 2012; Boiral et al., 2019; Boiral, Heras-Saizarbitoria and Brotherton, 2019b). The impact of ISAE 3000 and AA1000AS on assurance rigour remains ambiguous, as both standards prioritise broad, principles-based guidance over detailed procedural requirements leaving procedural depth to professional judgement (Boiral et al., 2019). However, scholars argue that the scope of these two standards varies, suggesting they may be more complementary than substitute standards (Manetti and Toccafondi, 2012; Junior, Best and Cotter, 2014; Alsahali and Malagueño, 2022). In addition to ISAE 3000 and AA1000, various other standards and guidelines are also relevant. For instance, ISO 14064 serves as a widely recognised standard for GHG accounting and verification, particularly for NAAPs. Along with national standards, such as the ASAE 3000 series in Australia and AICPA's attestation standards in the U.S., there is also a framework such as the ISAE 3410, which is an IAASB standard specifically for GHG emissions assurance. A sustainability report may be assured according to one or more standards, based on the provider's preference and the focus of the engagement (KPMG, 2015; IFAC, 2024a). Recent studies indicate that NAAPs utilise a more flexible approach to the application of standards. In 2021, NAAPs referred to a mix of standards globally, with ISO 14064 (46%) leading for GHG assurance, followed by ISAE 3000 (revised) (38%) and AA1000 (24%) (IFAC, 2023) to strategically blend multiple standards to differentiate themselves and meet specialised client needs. The differences in assurance standards indicate that the selection of standards serves as a competitive differentiator (Farooq and De Villiers, 2018; Hummel, Schlick and Fifka, 2019), while the presence of various frameworks raises issues regarding consistency, comparability, and credibility in sustainability assurance. The development of ISSA 5000 by the IAASB can be read as a response to fragmentation and inconsistency in assurance practice, although it remains unclear whether a single global standard can resolve underlying differences in professional logics, expertise, and market structures.

2.5 Transnational Private Standard Setting in Accounting and Assurance:

Global economic governance has increasingly shifted toward transnational private regulation, with non-state actors exercising rule-making authority across jurisdictions (Cutler, Haufler and Porter, 1999; Djelic and Quack, 2018). In this context, the IASB and the IAASB exemplify privately constituted organisations whose standards function as de facto global regulation despite the absence of a formal legal mandate or electoral accountability (Cooper and Robson, 2006; Loft, Humphrey and Turley, 2006; Black, 2008). Although standard setters, despite lacking formal public mandates, wield significant influence via their technical expertise, ability to set agendas, and strategic alignment with states and market participants (Cutler, Haufler and Porter, 1999; Botzem, 2014a). The privatisation of rule-making has highlighted concerns

regarding legitimacy, leading scholars to prioritise legitimacy as a key analytical focus (Suchman, 1995; Djelic and Sahlin-Andersson, 2006). The authority of transnational standard setters is deeply bound to assess the legitimacy of transnational standard setters by examining input, throughput, and output legitimacy (who participates, how decisions are made, and what outcomes are produced) (Scharpf, 1999; Richardson and Eberlein, 2011; Schmidt, 2013). Procedural transparency and due process mechanisms (discussion papers, exposure drafts, comment letter solicitations, public board meetings, and multiple stages of public consultation) have become essential in pursuing legitimacy (Richardson and Eberlein, 2011; IFRS Foundation, n.d.). This approach has become central in seeking legitimacy aimed at making the process inclusive, transparent, and rational to counteract their lack of electoral legitimacy, resulting in standards that merit acceptance (Botzem, 2014a; Arce, Giner and Taleb, 2023; Thies, 2025). Complementing procedural measures, standard-setter officials also invoke rhetorical strategies to show how appeals to the public interest, alongside narratives of independence and objectivity, have functioned as rhetorical legitimisation strategies accompanying procedural reforms (Suddaby and Greenwood, 2005; Durocher and Fortin, 2010; Pelger and Spieß, 2017).

A key aspect of legitimacy is who gets to participate in standard setting (input legitimacy) as in principle, the IASB and IAASB solicit input, although the IAASB operates formal, open consultative due processes, soliciting input from global stakeholders and using advisory councils and working groups to incorporate diverse perspectives (IFRS Foundation, 2013; IAASB, 2019). In practice, research points to persistent imbalances in participation, with auditing standard-setting processes often more restricted than those in accounting and dominated by audit firms, professional bodies, and regulators, while investors and wider public-interest groups remain marginally (Humphrey, Loft and Woods, 2009; Gäumann and Dobler, 2019). Throughput legitimacy is closely linked to input legitimacy and pertains to the perceived fairness, balance, and transparency of the decision-making process. Even when participation is formally open, not all inputs have equal influence in determining final standards (Pelger and Spieß, 2017; Durocher et al., 2019). These patterns indicate that transnational standard setting is accessible mainly to well-resourced actors with major financial and intellectual resources (Burlaud and Colasse, 2011; Baudot, 2018; 2018; Miyauch and Sanada, 2019), with epistemic global networks that can influence the agendas and content of global accounting and auditing standards (Suddaby, Cooper and Greenwood, 2007; Ramirez, 2012) that align with their commercial and professional objectives (Camfferman and Zeff, 2007; Botzem, 2014a). The ability to influence technical discourse serves as a mechanism of power, allowing elite actors to define legitimate accounting and auditing knowledge while sidelining alternative viewpoints, especially those focused on non-investor or public-interest issues (Gallhofer and Haslam, 2007; Power, 2010; Botzem, 2014a). Consequently, the accounting profession and its associated standard-setters assert their legitimacy not via democratic representation but through technical proficiency, procedural knowledge, and ethical standards (Suddaby and Greenwood, 2005; Cooper and Robson, 2006; Pelger and Spieß, 2017).

Although standard setters present their outputs as global and harmonised, the diffusion of standards remains fragmented and politically mediated. The IAASB similarly depends on state and regulatory adoption of its standards within broader financial governance reforms (World Bank, 2019). While states often support international standards to promote market integration and regulatory harmonisation, they retain control through endorsement and oversight arrangements, producing an ongoing negotiation between public authority and private expertise (Humphrey, Loft and Woods, 2009; Abbott and Sindal, 2009; Botzem, 2014). States and regional blocs may tolerate delegation in normal times but intervene when distributional stakes rise or crises create pressure to act, revealing a conditional delegation logic. The IASB's authority was bolstered by the EU's 2005 adoption of IFRS, yet interventions such as the IAS 39 carve-out and G20 pressure during the financial crisis reveal how political actors can circumscribe its independence. In response, the G20's call for stronger oversight led to the creation of the Monitoring Board in 2009, re-politicising accounting standard setting (Kusano and Sanada, 2019). Similarly, following the Enron scandals, IFAC introduced PIOB (Loft et al., 2006; Humphrey et al., 2009), and proposing a new oversight model through the MG reforms in 2020 in IAASB (IFAC, 2025a) indicates attempts to shore up its legitimacy and public interest focus amid growing scrutiny of audit regulation. Although many jurisdictions formally adopt ISAs, national adaptations persist, indicating that despite the symbolic authority of international standards, substantive global uniformity remains limited. While the diffusion of IFRS and ISAs reflects sufficient legitimacy for broad acceptance, recurring local modifications underscore the context-dependent nature of output legitimacy (Samsonova-Taddei and Humphrey, 2014; Boolaky and Soobaroyen, 2017). Oversight reforms act as mechanisms of legitimation, indicating public supervision of professional self-regulation and highlighting the politically contingent character of private standard-setting authority (Mattli and Büthe, 2005; Botzem, 2014a; Hazgui and Gendron, 2015). As a result, standard setters' authority depends on continued state endorsement and remains subject to intervention when core public interests are implicated, making responsiveness to oversight and reform pressures central to maintaining autonomy and mitigating the risk of direct regulatory intervention (Camfferman and Zeff, 2018; Sanada, 2020). Thus, the quest to bolster legitimacy is an ongoing negotiated reality for transnational standard setters, which evolves as they confront new economic realities and stakeholder expectations (Mueckenberger and Jastram, 2010; Richardson and Eberlein, 2011).

3. Theoretical Framework

This chapter sets out the theoretical architecture of the study through the integration of three closely connected analytical perspectives: regulatory space (Hancher and Moran, 1989), institutional layering (Mahoney and Thelen, 2009), and legitimacy (Suchman, 1995; Scharpf, 1999; 1999; Schmidt, 2013). These concepts are not treated as standalone theoretical tools but are instead brought together as complementary analytical lenses, each shedding light on a different aspect of the IAASB's development of understanding of how transnational standard-setters operate, adapt, and position themselves within contested global governance arenas, particularly in the emerging field of sustainability assurance. In combination, they draw

attention to how the Board negotiates authority within a crowded regulatory space, pursues gradual institutional change through layering, and seeks to establish and maintain legitimacy in a context marked by uncertainty and competing governance claims.

3.1 Regulatory Space in Transnational Standard-Setting:

The concept of regulatory space, introduced by Hancher and Moran (1989), provides a foundational lens through which to analyse the environment in which transnational standard-setters operate. Hancher and Moran conceptualise regulation not solely as the purview of state actors but as a regulatory space inhabited by various entities, including states, professions, corporate organisations, and civil society, which engage in competition or collaboration to shape rule-making to hold and compete for various resources (such as expertise, legitimacy, or legal authority) that confer influence within this space. Regulatory space suggests a complex environment characterised by overlapping authorities, where the boundaries of influence are dynamic and subject to ongoing negotiation (Black, 2001; Vibert, 2014). Contemporary governance is characterised by decentralised regulation, where authority is dispersed across public and private actors, making coordination and negotiation more central than hierarchical control (Black, 2024). This perspective shifts attention away from formal rules and agencies towards the interactions through which regulatory influence is exercised. In sustainability assurance, the IAASB operates within a crowded transnational field that includes the EU through the CSRD, international bodies such as IOSCO and the ISSB, as well as national regulators and competing assurance frameworks. Taken together, sustainability assurance can be understood as a developing arena shaped by inter-institutional interactions and ongoing contests over influence (Hancher & Moran, 1989; Cutler et al., 1999). Regulatory space has been used in qualitative research to examine how authority is contested and negotiated among actors across a range of contexts, including sustainability reporting (Carungu et al., 2025), financial governance during periods of crisis (Botzem and Quack, 2006; Botzem, 2014a), and in conceptually related studies on IFRS for SMEs (Warren, Carter and Napier, 2019) and integrated reporting (Rowbottom and Locke, 2016). Drawing on this body of work, the present study adopts a regulatory space perspective to analyse how the IAASB navigates, positions, and asserts authority within the emerging and contested field of sustainability.

3.2 Institutional Layering as an evolutionary strategy:

Mahoney and Thelen's (2009) theory of institutional change, developed in *Explaining Institutional Change*, has become highly influential for its departure from earlier approaches that emphasise either path dependence and long-term stability (Pierson, 2000) or punctuated equilibrium driven by external shock (True et al., 2019). Instead of predominantly identifying change at critical junctures, they emphasise incremental transformations that arise through the gradual accumulation of smaller shifts over time (Djelic and Quack, 2003). It shifts attention away from shock-driven accounts of institutional change by showing how transformation often unfolds incrementally, through the cumulative effect of small adjustments (Djelic and Quack, 2003; Mahoney and Thelen, 2009). Mahoney and Thelen (2009) present a framework where

institutional change arises from two primary factors: the political environment, which influences the broader conditions affecting an institution, and the institution's nature, characterised by its rules and internal structure. These factors influence who becomes a change agent and what strategies they pursue to follow recognisable patterns rather than occurring suddenly. It outlines four main types of change: displacement, which involves replacing existing rules; layering, entailing the addition of new rules alongside old ones; drift, characterised by the persistence of rules despite changes in context; and conversion, where existing rules are repurposed for new objectives.

In contested regulatory environments, institutional change generally manifests as gradual adaptation rather than abrupt displacement. Institutional layering, a key concept in historical institutionalism, refers to the incremental change process wherein new rules or structures are added to existing ones rather than completely replacing them) (Streeck and Thelen, 2005). Layering describes a mode of institutional change in which new elements are grafted onto existing arrangements instead of replacing them outright (Schickler, 2001). This pattern emerges in contexts of strong institutional resistance, where powerful veto actors restrict the potential for comprehensive reform, thereby making incremental adjustments more viable (Hacker, 2004). In such settings, restricted interpretive flexibility further narrows the range of available options, encouraging change agents to work through additive modifications as a way of reshaping institutions over time (Mahoney and Thelen, 2009). Over time, new layers may gain significance compared to older ones, thus reconfiguring the institution's character with minimal conflict (Streeck & Thelen, 2005; Mahoney & Thelen, 2009). Layering provides a means of maintaining continuity with previous structures, satisfying conservative perspectives, while allowing change agents to gradually promote new agendas (Thelen, 2004; Mahoney & Thelen, 2009). Prior studies show that new standards are often introduced alongside existing regimes (Alon and Dwyer, 2016; Alexander and Alon, 2017). These examples demonstrate that layering can drive incremental institutional change even in contexts marked by resistance. This same logic has been applied to explain developments in transnational accounting and assurance standard-setting.

3.3 Legitimacy of Private Standard-Setters:

For a private transnational regulator such as the IAASB, which operates without a formal public mandate, authority therefore depends on its capacity to acquire and maintain legitimacy across multiple dimensions (Richardson and Eberlein, 2011). A complementary strand of political and institutional theory conceptualises these sources of legitimacy in terms of input, throughput, and output (Scharpf, 1999; Schmidt, 2013). Input legitimacy relates to who can participate in decision-making and the extent to which standard-setting processes are inclusive and accountable to those affected, for example, through exposure drafts, comment letters, and advisory groups. Throughput legitimacy, by contrast, centres on the internal quality of decision-making, including fairness, consistency, and the robustness of procedures (Dingwerth, 2007; Richardson and Eberlein, 2011). Output legitimacy refers to the consequences of standard-

setting activity, focusing on the quality, effectiveness, and broader acceptance of the standards produced. A standard setter achieves output legitimacy when its standards are seen as solving the intended problems and serving the public interest or needs of stakeholders (Bernstein and Cashore, 2007). Although input, throughput, and output legitimacy are analytically distinct, they frequently pull in different directions in practice (Scharpf, 1999; Schmidt, 2013). Emphasis on output, such as timely and effective decisions, can narrow opportunities for participation, raising concerns about technocracy or exclusion (Scharpf, 1999). Conversely, broad stakeholder participation can slow decision-making or dilute outcomes, thereby undermining output legitimacy (Schmidt, 2013). Similarly, a strong emphasis on procedural rigour may enhance fairness and transparency yet still prove insufficient if outcomes are judged to be ineffective (Black, 2008). Legitimacy thus demands continual balancing across these dimensions, as emphasis on one often entails trade-offs with others (Papadopoulos, 2010; Schmidt, 2013).

Within this literature, a number of studies concentrate on IASB processes and due process, exploring how legitimacy is assembled through consultation practices, procedural transparency, and justificatory devices such as agenda-setting and Bases for Conclusions (Pelger and Spieß, 2017; Arce, Giner and Taleb, 2023; Thies, 2025). A related strand focuses on sustainability and assurance, examining how authority takes shape in emerging and still unsettled regulatory domains through participation and procedural design (O'Dwyer, 2011). Other work broadens the analysis to multi-actor and institutional contexts, showing how legitimacy is negotiated over time across dispersed actors and multiple levels of governance, including regional and transnational arrangements (Botzem, 2014b; Becker, Gronewold and Weiß, 2025). Taken together, these studies highlight the value of legitimacy as an analytical lens for understanding how accounting standard setters articulate and sustain authority in complex and contested regulatory environments. Taken together, the integrated theoretical framework provides a useful basis for analysing the evolution of sustainability assurance standard-setting, as it connects institutional context, processes of change, and acceptance in a more holistic way.

4. Research Methodology

This study employs a qualitative, interpretive case study to examine how the International Auditing and Assurance Standards Board (IAASB) sought to establish credibility and position itself within the contested domain of sustainability assurance through the development of ISSA 5000. It adheres to a process-oriented framework concentrating on the evolution of field-level changes over time (Lukka and Modell, 2017). The interpretive paradigm enables a deeper understanding of how regulatory actors construct meaning and negotiate legitimacy in evolving assurance domains (Denzin and Lincoln, 2018). An interpretive lens facilitates understanding of how regulatory actors perceive and react to changing assurance expectations (Smith and Sparkes, 2016; Denzin and Lincoln, 2018). This case study employs a phased historical

narrative from 1997 to 2025, facilitating a process-tracing strategy that documents sequential institutional developments and mechanisms of change (Langley, 1999; George and Bennett, 2005). This approach facilitates a detailed analysis of the IAASB's strategic deployment over time consistent with existing literature that underscores the significance of historical and institutional context in comprehending standard development (Humphrey, Loft and Woods, 2009; Sonnerfeldt and Loft, 2018; Humphrey et al., 2021; O'Dwyer, Humphrey and Rowbottom, 2024).

4.1 Data Sources and selection:

This study draws extensively on publicly available documentary evidence analysed comprises official IAASB publications and archival records, including meeting agendas and minutes, exposure drafts, comment letters, consultation papers, official statements and press releases on websites, and finalised standards (e.g., ISAE 3000 and ISSA 5000). Additionally, official statements and press releases issued by international regulatory bodies and standard-setting organisations were analysed situating the IAASB's evolution within the broader global regulatory environment. The selection process emphasised authenticity, credibility, representativeness, and meaningfulness of the documents, as these dimensions critically affect the reliability and validity of historical documentary analyses (Flick, 2014). Applying these rigorous criteria enabled accurate reconstruction of IAASB's institutional timeline to enhance the overall credibility of the research findings (Bowen, 2009). A historical timeline was developed by studying IAASB archival sources to identify major events, pivotal moments, and decision-making processes during the creation of ISA 5000. This timeline method provided a structured, evidence-based overview of the progression, extending the analysis beyond final outcomes to include the evolving nature of debates and institutional responses. Timeline construction was used not only as a descriptive tool but as an analytic device to identify turning points, delays, accelerations, and shifts in strategy that would otherwise remain obscured in static analysis which helped to identify the four phases for analysis. This approach is consistent with historical-institutional research, which underlines the relevance of time in understanding regulatory change (Sonnerfeldt and Loft, 2018; O'Dwyer, Humphrey and Rowbottom, 2024). Documentary analysis constituted the primary empirical foundation, with interviews employed to contextualise and interpret archival material rather than serving as a distinct analytical component (Sonnerfeldt & Loft 2018).

4.2 Elite Interviews:

A total of 12 semi-structured elite interviews were conducted with current and former IAASB members, a PIOB member, an EU policymaker, representatives from professional accounting organisations, and regulators. Elite interviewing is particularly beneficial for research on standard-setting, because expertise is concentrated among a select group of powerful individuals participating in institutional decision-making (Dexter, 1970). Interviewees were anonymised with pseudonyms (e.g., IAASB member 1, AE1, MG1) and chosen according to their role in or proximity to the IAASB assurance development process. Interview material was

used to situate key events, reconstruct decision-making trajectories, and capture internal interpretations of institutional change. It also contributed to contextualising documentary analysis, highlighting voids in the archival record, and clarifying the reasons why specific options were dismissed, modified, or postponed during the standard-setting process (Sonnerfeldt and Loft, 2018).

4.3 Inductive Thematic Coding and Analytical Approach:

The qualitative material, including archival documents and interview transcripts, was analysed through inductive thematic coding. This approach prioritises the reconstruction of meaning from participants' perspectives and avoids imposing predefined theoretical categories at the initial stages of analysis (Braun and Clarke, 2006; Silverman, 2009). In accordance with Braun and Clarke's (2006) reflexive thematic analysis paradigm, codes were derived from meticulous, line-by-line examination of the texts and subsequently organised into overarching themes through iterative grouping based on similarity, frequency, or contextual significance. The analysis was conducted without predetermined categories, enabling themes to emerge from the data and to represent how principal actors expressed their interests, concerns, and interpretive stances (Silverman, 2009).

Inductive coding is widely used in accounting and auditing research and has been shown to enable the identification of phases, critical junctures, and longer-term patterns in institutional development without imposing theoretical frameworks at an early stage (Humphrey, Loft and Woods, 2009; O'Dwyer, Owen and Unerman, 2011). The present study inductively identified key turning points in the IAASB's development by analysing recurring coding patterns and reconstructing the timeline, in accordance with established methodologies employed in similar research (Sonnerfeldt and Loft, 2018; O'Dwyer, Humphrey and Rowbottom, 2024). Thematic coding therefore functioned not merely as a means of organising data but as an analytical device for generating conceptual insight, with theory introduced at a later stage to refine interpretation rather than to shape analysis from the outset (Timmermans and Tavory, 2012). Multiple sources were triangulated to strengthen the validity of the interpretive robustness by ensuring that emerging explanations were supported across sources rather than relying on a single perspective.

5. Finding and Analysis

IAASB Development Timeline: 1997–2025 (Sustainability and Assurance)

GREEN: Final Standard Releases Only RED: Regulatory Developments BLACK: All Other Developments (Projects, Transitions, Working Groups)

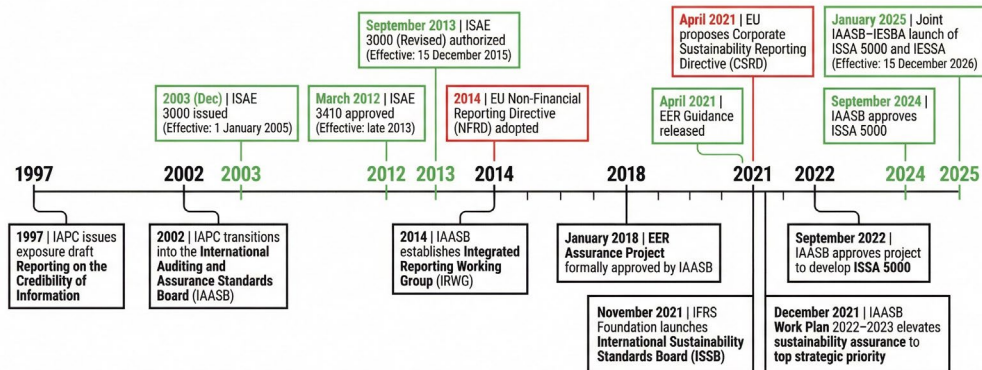


Figure 2 Timeline of IAASB journey of sustainability assurance standard setting

5.1 Phase I. Positioning the profession in a new space (late 1990s–2003)

The 1990s witnessed an expansion in corporate reporting on social and environmental issues, which in turn created a demand for voluntary credible third-party verification (O’Dwyer and Owen, 2005; Perez and Sanchez, 2009; Bebbington and Unerman, 2018). This phase is shaped by tensions between growing demand for verification, the absence of settled criteria, and competing claims over who can credibly provide ‘assurance’. This early uncertainty created an initiation for the accounting profession to position itself as a credible assurance provider. As early as 1986, the AICPA permitted CPAs to provide assurance over non-financial information (Brackney and Helms, 1996). This position was subsequently reinforced by the Special Committee on Assurance Services, which framed the extension of assurance into broader business reporting as a strategic opportunity for professional development (Jeppesen, 1998; Fédération des Experts-comptables Européens (FEE), 2003). The establishment of the Global Reporting Initiative (GRI) in 1997 further institutionalised this trajectory by introducing shared reporting criteria and heightening expectations for independent assurance. In Europe, the FEE practice was advancing faster than regulation, with limited standard-setting guidance. In response, the IAPC issued its first exposure draft (Reporting on the Credibility of Information) in 1997, extending audit concepts to non-financial engagements and allowing different levels of assurance, adopting the softer label *reporting service engagements*, given uncertainty around the term *assurance* (FEE, 1999;2002; IFAC, 2002). European commentators cautioned that audit terminology was already embedded in neighbouring fields (ISO 14001 certification and EDP audits) and warned that extending it to non-financial areas with unclear criteria risked confusion, recommending embracing “assurance” as a neutral label. The early struggle was both technical and jurisdictional, as audit concepts were applied to a domain with unsettled meanings and professional boundaries (FEE, 2003). In response, the

IAPC's subsequent exposure draft, Assurance Engagements, abandoned the earlier continuum model in favour of a distinction between reasonable and limited assurance. Although ISAE 100 was issued in draft form in 2000, weak guidance on moderate assurance and ongoing disagreement delayed its interim release. As consensus remained elusive, IFAC commissioned a special assurance research task force in 2002, drawing on academic expertise to clarify the theoretical foundations of assurance and stabilise meaning (IFAC, 2002; 2003a). In parallel, FEE argued that sustainability assurance had expanded beyond environmental issues into social and economic dimensions without corresponding standards, questioned the adequacy of ISAE 100, and called for a multidisciplinary approach that placed stakeholder engagement at the centre of scope, criteria, and materiality judgements (Fédération des Experts-comptables Européens (FEE), 2002; Fédération des Experts Comptables Européens (FEE), 2006).

Rather than displacing existing audit norms, the IAPC adopted a layering strategy (Thelen, 2003; Mahoney and Thelen, 2009), adding an overarching framework (International Framework for Assurance Engagements) which set out the core principles, objectives, and elements applicable to all assurance engagements. This allowed the profession to gradually extend its jurisdiction into non-financial domains without destabilising its foundational architecture. The IAPC released ISAE 3000 (replacing ISAE 100) in December 2003 (effective from 1 January 2005) as its first general comprehensive standard for non-financial assurance engagements after six years of deliberations. The jurisdictional ambition of this standard-setting project was explicit. IAPC Chairman Robert Roussey endorsed this stance, stating, *"The IAPC has therefore developed professional standards to establish the accountant as a primary provider of these assurance services and to assist practitioners in providing quality assurance services"* (AICPA, 1997). While ISAE 3000 recognised the rising demand for assurance over sustainability reporting, it largely emphasised procedural harmonisation and the extension of accountancy-based assurance logics into non-financial domains, permitting the involvement of experts under the oversight of professional accountants (IAASB, 2002; IFAC, 2003). Early IAASB legitimacy-building efforts were therefore anchored in profession-led processes, relying on extensive consultation, iterative exposure drafts, and expert-driven knowledge development as sources of throughput legitimacy that positioned accountants as authoritative assurance providers within an epistemically uncertain field. The transition from the IAPC to the IAASB in 2002 improved institutional independence, reinforced its focus on public interest, and expanded its scope to include non-financial assurance alongside financial auditing. Yet the field remained contested, with AccountAbility launching the AA1000 Assurance Standard in 2003, promoting a stakeholder-driven approach absent in ISAE 3000's original formulation, which challenged the profession's cautious, audit-based model (AccountAbility, 2008). The growing number of sustainability assurance frameworks reflects a field in formation, marked by tensions between rising market demand and epistemic uncertainty, professional authority and diverse expertise, and competing claims to legitimacy that challenge consistency and comparability across practices (KPMG, 2005).

5.2 Phase II. Co regulatory settlement and consolidation (2004–2013)

The period following the issuance of ISAE 3000 was characterised less by immediate convergence than by an emerging co-regulatory settlement, as standard-setting authority was negotiated across multiple institutional arenas. Its introduction coincided with European-led growth in sustainability assurance, with professional bodies issuing dedicated national guidance that implicitly signalled the generic framework's limited fit for sustainability engagements, reinforcing tensions between European pluralism and ambitions for global harmonisation (KPMG, 2005). In January 2005, the Dutch accounting body NBA issued ED 3410, Europe's first sustainability assurance standard, drawing on ISAE 3000; similar initiatives spread across Europe in the mid-2000s, reinforcing the pluralism. This signalled the accounting profession's growing role in sustainability assurance. Interview evidence indicates that calls for a dedicated sustainability assurance standard intensified in the mid-2000s, led by FEE's *Call for Action* (2004; reiterated 2006) (FEE, 2004; 2006), with the European profession offering support to the IAASB. As one interviewee stated, "*The Big Four were in high demand for one global framework, which made sense in terms of investor clarity and the costs of training and quality assurance...having one standard was 'extremely useful'*" (Interviewee 9). In response, the IAASB initially treated ED 3410 as a possible complement to ISAE 3000 but later reaffirmed ISAE 3000 as the governing architecture, positioning national initiatives as complementary rather than substitutive (IAASB, 2005c; 2005b). This move preserved ISAE 3000 as the centre of authority with a continued layering strategy (Thelen, 2004), as the IAASB reinforced its foundational framework while allowing new sustainability-focused interpretations and practices to evolve alongside it, rather than replacing existing standards.

This restraint coincided with the IAASB's broader institutional priorities, the IAASB's Clarity Project (initiated in 2004 and culminating in 2009), which emerged under post-scandal regulatory pressure to restructure and clarify the ISAs. In a 2007 communication on its future technical strategy, Chair John Kellas stated, "*The IAASB intends to continue to make auditing standards its first priority*" (IAASB, 2005a; 2007; Dennis, 2010). *An interview account similarly portrays, "After the framework was published, there was a proposal to develop an assurance standard for environmental reporting. But following the IOSCO endorsement, the focus shifted to clarifying financial auditing standards, and other projects were put on hold"* (Interviewee 3). While acknowledging IAASB's resource constraints, European stakeholders signalled that the public interest warranted IAASB leadership in this area to ensure consistency and credibility across borders. This reflects a reordering of priorities within the alignment with public oversight, and the repair of audit legitimacy took precedence over strengthening the board's throughput legitimacy with global expectations for standard setting, while sustainability assurance remained a deferred concern.

However, the EU regulatory framework increasingly emphasised non-financial information through capital market and transparency directives (2003/71/EC; 2004/109/EC), heightening

demand for non-financial assurance and exerting additional pressure on the IAASB to respond. It created demand for non-financial assurance, placing pressure on the IAASB to act while oversight actors constrained expansive standard-setting. In its 2007 consultation on the 2009–2011 strategy, the IAASB received strong calls to lead on sustainability assurance which urged the IAASB to build on existing frameworks and national experiences (AA1000A; GRI Guidelines NIVRA's 3410N), rather than allow fragmented, ad hoc approaches to continue. The message was that IAASB should not reinvent the wheel but rather harmonise and elevate these experiences into a single high-quality benchmark (IAASB, 2007; 2008a; 2008b). Yet calls for innovation were met with caution from the CAG and regulators such as IOSCO, who warned that early engagement with sustainability assurance could divert attention from core audit responsibilities or fail to secure market uptake. As one interviewee suggested, "I think the other angle, paradoxically, was basically the lack of real assurance skills and standard-setting capabilities within the institutions that were tasked with doing that, so they had to rely on the private sector, in a way, because they didn't really have the time, the skills or the competencies" (Interviewee 9). The IAASB was caught between demands for expansion and the limits of its organisational capacity, shaping a gradual, cautious trajectory.

While its 2009–2011 strategy acknowledged the growing importance of sustainability, it stopped short of committing to a standalone standard, instead opting for a middle path to address urgent needs ((GHG) assurance) within the existing ISAE 3000 framework and to undertake a revision of ISAE 3000. As an interviewee states, "*Our Dutch preference was for assurance on the full environmental report, as most assurance engagements at the time focused on environmental reporting. The board chose GHG information instead, partly because in several jurisdictions, such as Australia, GHG reporting was mandatory... Europe played a dominant role in the IAASB's development of environmental and sustainability assurance. There was considerable opposition and many comments, but the standard was eventually issued. It came relatively late, however, as Europe already had its own GHG verification protocol, and as a result it was not widely adopted... What was very positive was that this became the first standard that was more specific than the framework*" (Interviewee 4). As a result, the IAASB's turn to GHG assurance functioned as a pragmatic and low-risk entry point into sustainability assurance, enabling incremental action. The IAASB convened four international roundtables to gather input, bringing together auditors, companies, NGOs, environmental experts, non-accountant assurance providers, and regulators. The breadth of participation highlighted the inherently multidisciplinary nature of sustainability assurance, a point the IAASB had to acknowledge and accommodate, challenging its audit-centric approach. The IAASB's GHG Task Force went to work in 2009; an exposure draft was issued in January 2011; and ISAE 3410 was approved in March 2012, effective from late 2013 (IAASB, 2008a; 2012). As the IAASB chair Arnold Schilder noted then, "*carbon markets worth billions required reliable information, and providing assurance to enhance the reliability of emissions information...is also increasingly being required by regulation or presented on a voluntary basis*" (IAASB, 2011b). The prominence of GHG assurance thus reflected its growing salience for capital markets, rather than a comprehensive response to sustainability accountability, with ISAE 3410 marking the IAASB's first subject-specific assurance standard

since ISAE 3000 and providing a visible output-legitimacy signal in a highly relevant area. In parallel, the IAASB initiated a review of ISAE 3000 to strengthen input legitimacy, drawing on a late-2008 survey of national standard setters and IFAC member bodies to inform a revision project formally approved in March 2009 (IAASB, 2009b; 2009a). This illustrates a renewed input legitimacy strategy, mobilising field-level knowledge and consultation to inform revisions of ISAE 3000 while managing plural expectations. As chair emphasised, “*ISAE 3000 was intended to facilitate innovation in the evolving field of assurance, not act as an impediment*” (IAASB, 2011a). This strategy reflected concerns that sustainability assurance remained conceptually unsettled and that weaknesses in ISAE 3000 needed to be resolved to preserve legitimacy and jurisdiction before further standard-setting expansion through gradual layering.

One of the most delicate issues in revising ISAE 3000 concerned its intended audience. IAASB standards are traditionally written by and for professional accountants within the IFAC system, yet by around 2010 much sustainability assurance was being conducted by non-accounting providers. While FEE pushed to limit the standard to accountants, citing risks of quality dilution and misleading impressions of IAASB oversight, and even proposed dual versions. The IAASB task force agreed that accountants should remain the core audience, but the public interest of the standard meant other practitioners could not be formally excluded. The revision positioned the accounting profession’s ethical and quality-control requirements as the benchmark, allowing non-accounting practitioners to claim compliance with ISAE 3000 only where they could demonstrate measures that were “at least as demanding”, consistent with the IAASB’s approach to ethical requirements and ISQC 1 (IAASB, 2009a; 2010b; 2010a). Leading to a compromise of conditional inclusion, explicitly signalled exceptions rather than co-equal audiences were provided. This conditional inclusion positioned ISAE 3000 as a de facto global benchmark that non-accounting providers were expected to aspire to, while limiting full integration, as many continued to rely on alternative frameworks such as AA1000AS or ISO standards. This conditional inclusion reflects the IAASB’s effort to retain throughput legitimacy, balancing technical quality, professional boundaries, and public interest accountability in a fragmented assurance ecosystem. The IAASB authorised the revised edition of ISAE 3000 in September 2013 with an effective start date of December 15, 2015 (IAASB, 2013). Overall, Phase II saw a co-regulatory settlement emerge, as the IAASB relied on selective revisions and additions to ISAE 3000 while sustainability assurance practice remained plural.

5.3 Phase III. Fragmentation pressures and incremental layering (2014–2019)

The developments emerging from 2014 did not merely introduce an additional reporting initiative to the IAASB’s agenda; they exacerbated an existing issue present in Phases I and II specifically: how a standard setter can expand its authority into a domain where the objects of assurance proliferate faster than the criteria needed to stabilise them. The launch of the

principles-based <IR> Framework aimed at formalising integrated thinking through the combination of financial and non-financial information into a unified value-creation narrative that meets increasing stakeholder information requirements(IIRC, 2014). Alongside, European regulators strengthened non-financial reporting requirements via the Non-Financial Reporting Directive (2014/95/EU), which required ESG disclosures. Member States were permitted to require independent verification, even with restricted assurance at the EU level. Prior to the Directive, France mandated third-party verification through the Grenelle II law, leading to inconsistent regulatory implementation and diverse reporting frameworks in assurance practices(EU, 2014). Against this backdrop, FEE urged the IAASB to lead <IR> assurance development as a credibility solution in its July 2013 comment letter to IIRC in anticipation of growing market demand (FEE), 2013). Yet interviewees recalled a deliberate reluctance to intervene, *“the IIRC with its CEO, Paul Druckmann, and its chairman, Mervyn King, from South Africa. They were very outspoken in calling upon us to do something in these areas of integrated reporting...and at that time, we were quite reluctant, including myself, to do that too quickly, because we thought we really needed to see what's going on. There was also concern in the IAASB not to stifle innovation, because it was all under development... (Interviewee 2)”*. A further board member contributed, *“But here we were in really virgin territory... the Wild West... there was all this experimentation going on. But lots of people were saying there are lots of problems with doing this stuff. We are not sure how to do it. We need help... You know, to stop us having to work everything out for ourselves from first principles (Interviewee 1).”* This reluctance reflected not indifference to assurance but concern that premature intervention in an unstable reporting field could undermine the IAASB’s credibility as a standard setter, reflecting a growing tension between standard-setter caution and external pressures for early assurance engagement. In this context, calls for IAASB leadership can be understood less as technical requests than as efforts to secure an authoritative anchor for credibility claims under conditions of institutional plurality.

Rather than pursuing new standards immediately, the IAASB focused on research and consultation, establishing an Integrated Reporting Working Group (IRWG) in 2014 to explore the applicability of ISAE 3000 to integrated reports. It also engaged with the IIRC through forums and comment letters, thereby reinforcing input legitimacy through the incorporation of stakeholder perspectives to maintain its authority despite initial reluctance to commit. Subsequently, the IAASB prioritised research, consultation, and procedural deliberation over rapid output signalling engagement while remaining deliberately open-minded amid uncertainty thus enhancing a throughput-oriented, exploratory approach. Accordingly, this culminated into a staff paper *Exploring Assurance on Integrated Reporting and Other Emerging Developments in External Reporting which set out the board’s initial thinking on key <IR> assurance challenges, including suitable criteria, forward-looking and narrative information, and the scoping of multi-faceted engagements, framing assurance as an emerging and complex issue to be understood before any commitment to standard-setting (IAASB, 2015)*. However, when the IIRC issued discussion papers in 2014 and 2015 publishing an *Overview of Feedback and Call for Action* that explicitly pressed the audit profession and standard-setters to respond more decisively (IIRC, 2015b; 2015a).This marked as an escalation

in pressure, as an interviewee recalled , *“now it's really time for us to do something and do more than just what we did in exploring so far, because I think they might even have said that otherwise they would have to go to others, and one that will be a bit odd, given the global role of the IAASB (Interviewee 2)”*. Consequently, the transition from exploration to action was therefore informed by concerns that inaction could prompt stakeholders to seek assurance guidance elsewhere, thereby unsettling the IAASB’s claimed position as a global authority in assurance standard setting.

Nonetheless, uncertainty over how to proceed became increasingly noticeable, as one interviewee recalled, *“By about 2016, this was already starting to happen. Some of these other parties were supportive, and you could see them beginning to come together, even though they did not really know what to do. Given the range of activities underway, there were plenty of people willing to tell us what they saw as the key difficulties and issues, and what we decided to do was to go and ask them directly what the problems were” (Interviewee 1)*. Amid growing alignment without coordination, the IAASB responded to epistemic uncertainty by legitimising its intervention through stakeholder consultation and problem definition, grounding its authority in input legitimacy by translating dispersed and unsettled practices into a bounded set of issues articulated by affected actors and rendered suitable for consideration within its due process. This lack of clarity amid convergence helps explain the IAASB’s shift away from a narrow focus on integrated reporting toward the broader concept of emerging forms of external reporting. As one member noted. As a member noted, *“Talking to organisations developing the major frameworks, such as GRI and integrated reporting, was a really important part of the discussions. They actually came to IAASB meetings, and this was pivotal in realising how many reporting frameworks would ultimately require assurance” (Interviewee 4)*. Internal resistance to an integrated-reporting label further reinforced this shift, with one member recalling that *“We initially wanted to call it integrated reporting. The UK was opposing... because they had the strategic report, and other countries had a different type of report. It was renamed Emerging Forms of External Reporting” (Interviewee 3)*. Limiting the project to integrated reporting would therefore have misrepresented the diversity of emerging practices and assurance demands as part of a much larger shift encompassing sustainability reports, CSR reports, greenhouse gas statements, and other non-financial disclosures. These developments culminated in the Discussion Paper Supporting Credibility and Trust in Emerging Forms of External Reporting: Ten Key Challenges for Assurance Engagements, which reframed assurance on EER around a set of identifiable challenges grounded in ISAE 3000. The paper was presented to the IAASB in March 2016 and subsequently released for public consultation in August 2016 (IAASB, 2016a; 2016b; 2017b). The IAASB derived its authority from established assurance concepts and due process rather than domain-specific expertise, with legitimacy arising from its capacity to organise a fragmented reporting landscape through problem definition and procedural mechanisms, thereby extending its regulatory reach without binding itself to any single reporting regime.

Nevertheless, resource constraints shaped the limits of how far the IAASB could credibly move beyond exploratory activity, as an interviewee reflected, *“Sustainability assurance was not a very high priority. There was concern that it would become a ‘black hole’ absorbing significant resources, and both the Board and the CAG simply did not have the funding to do it properly. It could not be done as a half-hearted project. In fact, the work would have died without external funding. Securing that funding took time, as we were clear that funders could support the project but not influence it. In the end, they accepted those terms, and without that funding, we would not have done anything at all”* (Interviewee 1). As a result, the IAASB responded to epistemic uncertainty by collaborating with the World Business Council for Sustainable Development (WBCSD), a key participant in the Gordon and Betty Moore Foundation’s Conservation and Financial Markets Initiative (CFMI). WBCSD invited the IAASB to apply for a grant, which proved catalytic for the resource-constrained Board, enabling work to start in late 2017 rather than awaiting a later funding cycle. Collaboration broadened the IAASB’s input base beyond the accounting profession by incorporating major sustainability preparers and users, grounding its intervention in an input-legitimacy strategy that translated dispersed practices into issues processed through due process. WBCSD’s Managing Director for Redefining Value highlighted that *“unprecedented collaboration”* was needed to integrate financial and non-financial information and that WBCSD chose the IAASB *“for their internationally renowned methods of setting the highest standards”*(IAASB, 2017a). This external validation reinforced the IAASB’s institutional legitimacy in the EER domain, while its established output legitimacy, grounded in its reputation for high-quality international standards, positioned it as a preferred actor in sustainability assurance. However, the board’s initial reluctance stemmed less from opposition to sustainability assurance than from resource constraints. Without adequate capacity, the project could not be pursued in a manner consistent with the IAASB’s procedural standards, prompting the board to prioritise the preservation of throughput legitimacy. As one board member added, *“We got financial support from WBCSD and financially from the Harry and Betty Moore Foundation...” We need to be very careful with that, because there were a lot of time, staff and board member constraints... .. discuss all of this openly in the board... bring that to a consensus, and maybe with some minority views... have a voting procedure... at least two-thirds of the board need to be in favour.”* (Interviewee 1). This emphasis on openness, consensus-building, and voting framed the project’s acceptability through throughput legitimacy, signalling that external funding did not substitute for due process but instead heightened the need to be seen to follow it.

These pressures were amplified by agenda-setting dynamics. These constraints were reinforced by the board’s agenda-setting dynamics. As another interviewee recalled, *“It took a very long time before the EER was on the agenda... it’s very difficult to put something new on the agenda of the IAASB.... The point that a lot of people... were afraid of is about the subject matter (IAASB member 4)”*. Another member added a similar experience: *“There weren’t a lot of people joining in... that’s very unusual for the board...”* Eventually it came around to supporting it, but... not pretending to understand it... *‘We’ll support it, but we don’t really understand it’*(IAASB member 1). Taking them together, difficulties were simultaneously institutional and epistemic, the challenge of advancing EER within the IAASB’s crowded

agenda but also of stabilising an emerging domain whose boundaries and implications were not yet widely understood. Within this context, funding operated not merely as an enabler of action but as a constraint that shaped which institutional responses were considered viable. As a member reflected, “...with that funding in place, we managed to persuade the board for. That we should at least do some guidelines to help people. And so that was the genesis of the guidelines. I said at that meeting my preference would be to go straight to a standard... but I don't think the rest of the board, or actually most practitioners and repairers of accounts, were ready at that stage... So that's why we started the guidelines... practical guidance... that was the genesis... the approval... was the first pivotal step...” (IAASB member 4). At the same time, internal reservations persisted, as another EER guidance working group member stated, “If you want to do it properly or not in a pressure cooker, then it's easier to publish guidance. [...] IAASB members are always afraid that guidance dilutes the standards or that something in the guidance could be contradictory to the standards. Guidance never has the status of a standard, and there is a concern that people may nevertheless treat it as one” (IAASB member 3). Following the discussion paper, stakeholders broadly converged on the view that, despite growing demand for EER assurance, the continued evolution and lack of harmonisation across reporting frameworks (e.g., <IR>, GRI, SASB) made a binding EER-specific standard premature. Respondents instead favoured flexible, practical guidance on applying ISAE 3000 (Revised), often alongside calls for collaboration with other organisations, a position the paper itself explicitly endorsed. These concerns highlight the tension between responsiveness and control that accompanied the decision to proceed with guidance. Taken together, these accounts show that guidance emerged as a pragmatic compromise, functioning as a minimum viable intervention that delivered responsiveness and practical value in terms of output legitimacy while deferring the stronger institutional commitment implied by a dedicated standard until professional capacity and market conditions matured, thereby deliberately blurring the boundary between authoritative standards and non-authoritative outputs. This tension helps explain why guidance was deployed as a transitional instrument, with its authority deliberately constrained through non-authoritative framing, procedural safeguards, and close alignment with existing standards. In January 2018, the IAASB approved the EER Assurance Project to develop non-authoritative guidance on applying ISAE 3000 (revised) to emerging forms of external reporting. Using a phased approach, the IAASB advanced from a Phase 1 consultation issued in February 2019 to Phase 2 work, culminating in proposed guidance released for public consultation in March 2020 (IAASB, 2018; 2018; 2020), which allowed the Board to remain credible, present, and adaptable in an unsettled field.

5.4 Phase IV. IAASB's relevance race to sustainability assurance (2021–2025)

An unprecedented intensification of external pressure fundamentally reshaped the IAASB's pace and strategic stance with the EC's introduction of the European Green Deal in 2019 (EC, 2019) and later at the “*Financing the European Green Deal*” conference on 28 January 2020, where Executive Vice-President Valdis Dombrovskis underscored the EU's ambition to lead the development of non-financial reporting standards: “*The many overlapping international reporting standards and set-ups confuse companies and investors... They also find it expensive.*”

The EU is well placed to address this situation and show leadership in building consensus for a set of standards that can be widely accepted” (EC, 2020). Europe signalled its readiness to act independently by advancing robust disclosure regulation, placing clear pressure on international standard-setters such as the IAASB to adapt or risk marginalisation. EU officials moved swiftly from political intent to regulatory action, with consultation on the NFRD sought to improve the consistency and quality of sustainability reporting by addressing standardisation, materiality, and assurance. Europe’s early-2020 initiatives catalysed global coordination, as international standard-setters and regulators framed fragmented sustainability reporting as a shared problem requiring collaborative global standardisation. By 2021–2022, these dynamics had become global, as the U.S. SEC’s proposed climate disclosure rules, including potential assurance, and the IFRS Foundation’s creation of the International Sustainability Standards Board signalled the rising worldwide importance of sustainability reporting and assurance. As one interviewee reflected, *“Europe was a starting point, but it was possible only because the SEC had published its draft rule..It became a global topic. Everybody around the table was still thinking, ‘Oh no, we have enough with 3000 and EER,’ but once that happened, it was not only the EU but also the US needing a sustainability assurance standard. It totally changed the approach”* (Interviewee 5). This phase can therefore be understood as a clear case of externally induced institutional acceleration (Greenwood et al.,2011) whereby regulatory and political forces outpaced and reoriented the IAASB’s traditionally incremental, profession-led standard-setting approach

The EU advanced its Green Deal agenda through the April 2021 CSRD proposal, replacing the NFRD. This marked the first binding sustainability assurance requirement in EU law and reinforced the EU’s role as a regional orchestrator, alongside global responses such as the IFRS Foundation’s launch of the ISSB in November 2021. The IFRS Trustees and IOSCO explicitly noted that establishing the ISSB would “form the basis for the development of an audit and assurance framework” for sustainability information. Against this rapidly escalating regulatory backdrop, by the time the EER guidance was released in December 2021, the Board acknowledged that ISAE 3000 (Revised) and the guidance were underutilised due to their general terminology and lack of specific applicability, while also raising concerns about their capacity and agility to respond quickly, thereby casting doubt on their ability to keep pace with rapid external developments (IAASB, 2021a; 2021a; 2021b; 2021c). This underutilisation weakened the profession’s claim that an adequate toolkit already existed, even as interviewees described how political timelines redefined expectations. *“Until the CSRD got adopted and IFRS started producing sustainability reporting standards, it was not seen as a priority.... Before that, it was seen as a nice-to-have, not as a must-have”* (Interviewee 1). One interviewee recalls the profession initially seeking to defend the sufficiency of the existing framework: *“We told them, do not invent something. Look, you have ISAE 3000 available, and you have EER guidance. Just plug all together, and everything is there...but they wanted the standard, so we perceived the need for a specific sustainability assurance standard”* (Interviewee 6), signalling the necessity of a dedicated sustainability assurance standard rather than continued reliance on generic instruments. From the EU perspective, the IAASB’s position as the sole global assurance standard-setter became strategically salient under these time constraints. As one EU

official explained, “*The IAASB was the only shop in town... We left it open in the CSRD text it is not mentioned who should do it or how ... but we always had in mind that...it would make sense to rely on their standards as a source of inspiration*” (Interviewee 3). This implicit reliance simultaneously elevated the IAASB’s relevance and intensified pressure to deliver quickly, as delay risked prompting EU-specific standard-setting while retaining the right to replace it if its output proved too slow or unfit for purpose. While the IAASB experienced this conditional reliance as a source of acute time pressure, accelerating standard-setting under the threat of regulatory substitution (Abbott & Snidal 2009; Richardson & Eberlein 2011).

Within the IAASB, the acceleration of EU sustainability regulation was widely interpreted as a decisive turning point for the Board’s strategic relevance. As IAASB members reflected, “*Europe is, in reality, the main client.... It made sense if the IAASB wanted to remain strategically relevant, they had no choice but to engage with the EU. Otherwise they would have gradually become irrelevant, being just a standard-setter for the audit of financial statements* (Interviewee 9)”. This urgency was reinforced by concerns about missing regulatory deadlines. As one IAASB member explained, the board “*really felt that they needed to meet the deadlines of the EC, because they didn’t want to run the risk that the EC would... do their own thing. If [the EU doesn’t] think it’s fit for purpose... they’ll just make their own standard*” (IAASB member 5). As a result, ISA 5000 was developed under extreme temporal compression. One interviewee described the process as “*a kind of pressure cooker because of the EC developments,*” noting that “*they needed to have this standard out before the EC would start developing one*” (Interviewee 4). The IAASB’s internal response illustrates how standard-setting bodies confront institutional pressures through constrained strategic choices, seeking to reconcile external responsiveness with the preservation of procedural legitimacy (Greenwood et al., 2011; Richardson & Eberlein, 2011). This sense of urgency also explains why sustainability assurance, despite having appeared intermittently on the IAASB’s agenda for over a decade, only became a sustained priority once EU mandate was imminent. As another interviewee reflected, “*It is going to be mandated by the EU, so this is the right moment... if you compare that to 10–15 years before, it was already on their agenda, but every time it dropped off the priority list*” (Interviewee 11). In December 2021, the IAASB approved its 2022–2023 Work Plan, elevating sustainability and ESG assurance to a top strategic priority for the first time (IAASB, 2021d; 2021d). At this point, the IAASB’s input legitimacy is challenged, as stakeholders no longer view its existing tools as adequate. Its continued authority becomes contingent on demonstrating responsiveness to external regulatory demands.

In response to growing global concern, the IAASB engaged with key stakeholders between February and June 2022 through outreach, which served both as technical consultation and as a means of building input legitimacy to justify demand for a global sustainability assurance standard to address fragmentation linked to emerging mandatory reporting regimes. OSCO convened regulators, investors, and standard-setters in early 2022 to stress that credible sustainability reporting depends on independent assurance. This led to growing agreement on the need for a global baseline approach accommodating Europe’s leadership but remaining

applicable worldwide. On 15 September 2022, IOSCO issued its statement calling on the IAASB and IESBA to develop global, profession-agnostic standards by building on existing frameworks rather than taking a greenfield approach (IOSCO, 2021; 2022). As a professional accounting body highlighted “*The allowance for other assurance providers is a political decision... ISAE 3000... was already used by other practitioners, even when it was meant to be used by accountants*” (Interviewee 11). These standards needed to be delivered promptly to support the EU and ISSB-aligned jurisdictions and to avoid fragmented national approaches. As one oversight member noted, “*While Europe’s regulatory influence is significant, designing a standard solely around European requirements would risk excluding other jurisdictions and undermine the international comparability needed for a global assurance standard*” (MG 1). Another member explained, “*A fully greenfield approach was deliberately avoided because it would have taken many years; instead, the IAASB built on ISAE 3000 and existing assurance concepts to enable timely delivery*” (Interviewee 6). This intervention channelled political urgency into a clear standard-setting pathway. Rather than advocating a new institutional architecture, IOSCO emphasised the need to balance speed with throughput legitimacy, encouraging the IAASB to rely on its existing procedural structures. The ISSA 5000 project was formally initiated in September 2022, when the IAASB approved a dedicated project proposal to develop a global sustainability assurance standard, following IOSCO’s call for timely, profession-agnostic assurance frameworks (IAASB, 2022c; 2022a; 2022b). The avoidance of the greenfield approach by MG reflects institutional layering (Mahoney & Thelen, 2009), as the IAASB planned to develop ISSA by augmenting existing frameworks under time constraints; the board reconciled continuity with adaptation, representing a practical reaction to political exigency and institutional limitations.

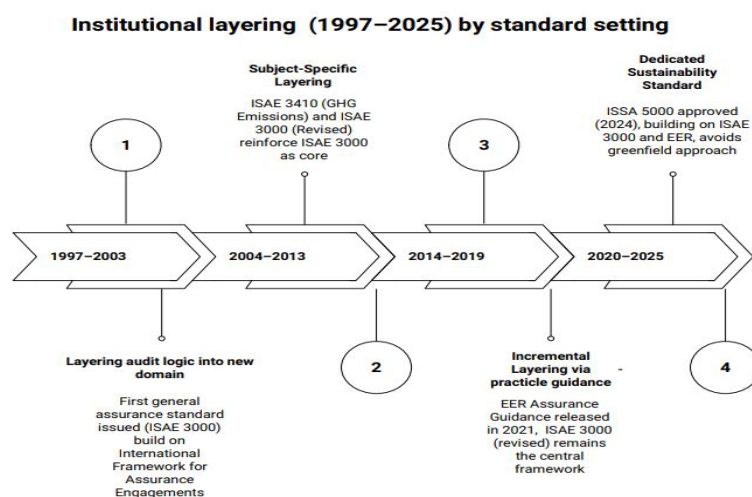


Figure 3 Layering strategy of IAASB over standard setting period.

Despite the extraordinary time pressure, the IAASB had to balance speed with its due process obligations. IOSCO’s March 2023 report and public statement sharpened this into a clear

timeline expectation. IOSCO stated that “the global community is looking for timely actions to put in place assurance standards and encouraged the IAASB and IESBA to deliver their framework for end-2024 disclosures”. As a MG observer noted, “*Exposure drafts, stakeholder consultation, and consideration of feedback were non-negotiable, even on an accelerated timeline*” (Interviewee 5). This represented a legitimacy dilemma as the IAASB had to deliver quickly without compromising due process but there was an explicit recognition within the board that the traditional pace had to be compressed dramatically. In direct response to IOSCO’s normative guidance, the IAASB proactively accelerated the ISSA 5000 timeline aiming to gather broad and timely input through a global outreach strategy and to support completion of the standard by the end of 2024 (IAASB, 2023). The IAASB released Exposure Draft 5000 in August with a firm deadline in December, making it clear that extensions would not be possible due to the tight timeline. To support meaningful engagement despite time constraints consultation period it organised a global outreach effort that included roundtables and webinars aimed at involving a broad and diverse range of stakeholders including non-accountant practitioners reference groups. This strategy helped the IAASB compensate for the compressed timeline by reinforcing both input and throughput legitimacy. As another oversight representative observed, “*There was an acceptance which is quite unusual for the IAASB that this was not going to be perfect when it went out, but that it had to be good enough, with learning continuing through implementation*” (Interviewee 10). This reflected a shift toward throughput legitimacy, in which established procedural expectations were deliberately adjusted to accommodate heightened political urgency while preserving the credibility of the process. In September 2024, the IAASB officially approved ISSA 5000 validated by PIOB for procedurally and ethically robust due process and IOSCO endorsed ISSA 5000 for the timely delivery of ISSA 5000, stating, “the IAASB for delivering sustainability assurance standards well on time with a view to enable the assurance of 2024 corporate disclosure” (IOSCO, 2024). This formal approval and external endorsement affirmed the IAASB’s output legitimacy by signalling responsiveness to both technical and political expectations, consistent with the literature emphasising how timely, problem-solving outputs underpin legitimacy in transnational standard-setting (Thies, 2025; Schmidt, 2013). The IAASB and IESBA published a coherent sustainability-related standards package on January 27, 2025, combining assurance (ISSA 5000) with ethical and independence requirements, with a shared effective date and implementation guidelines (IESBA, 2022; IAASB, 2024b). This strengthened output legitimacy by delivering a complete, integrated framework, and reinforced throughput legitimacy through standard coordination to preserve authority.

Discussion, contribution, and conclusion

The findings show that the IAASB's authority in SA was conditionally delegated rather than institutionally settled (Abbott et al., 2015; Humphrey et al., 2009). For much of the period under study, the IAASB had sufficient discretion not to act proactively because SA remained peripheral in agenda-setting and had not yet become a binding regulatory priority. This allowed the profession to preserve influence through general standards and selective addition, while deferring a dedicated standard. This discretion narrowed sharply once EU-led regulation and IOSCO support raised the stakes. The IAASB's global authority was not exercised above regional politics but through alignment with a dominant regional regulator while continuously framing itself as a global solution. This shift from profession-led discretion to regulator-conditioned responsiveness helps explain why ISSA 5000 emerged not as the outcome of autonomous professional leadership but as a relevance-preserving response to the threat of regulatory substitution (Richard et al., 2025). The IAASB mobilised fragmentation to preserve its authority, intervening strategically once formal endorsement became politically salient. Profession-agnostic standard-setting evolved as a political accommodation that broadened the acceptability of ISSA 5000 while preserving an audit-derived centre of authority. The case therefore shows that transnational private standard-setting authority is sustained not through expertise alone, but through responsiveness to public actors.

Second, the findings recast legitimacy as dynamic rather than fixed. In earlier phases, the IAASB sought to sustain authority through expertise and, increasingly, through cautious agenda management and procedural control. Under regulatory acceleration, however, output legitimacy became more pressing, as timely delivery and regulatory usefulness moved to the centre. Legitimacy, therefore, shifted over time from expertise-driven to process-driven and finally to outcome-driven. The case extends prior work on private standard-setting by showing legitimacy actively mobilised as a strategic tool that can be stretched, slowed, or compressed in response to changing conditions (Pelger & Spieß, 2017; Thies, 2025).

Third, the study extends theories of institutional layering by showing that, in the IAASB case, layering was not only constrained but strategically mobilised (Mahoney & Thelen, 2009; Alon & Dwyer, 2016). The IAASB adapted through extension rather than redesign, repeatedly building on inherited assurance architecture, from ISAE 3000 to ISAE 3410, EER guidance, and ultimately ISSA 5000. IOSCO's support for a non-greenfield approach reinforced this path. Layering functioned not merely as gradual adaptation but as a relevance-preserving strategy through which private authority was maintained and recentred in a fragmented field.

Finally, the case situates SA within a plural, contested, and globally uneven field in which professional authority is unsettled and renegotiated. The IAASB's trajectory from discretion to regulator-conditioned responsiveness shows how audit standard-setting is drawn into the governance of future-orientated and uncertain sustainability disclosures. ISSA 5000 is

therefore better understood not as a purely technical solution but as a mechanism through which a fragmented field is recentred under conditional IAASB authority rather than fully resolved.

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Whistleblowing Risk and Corporate Environmental Performance: Evidence From the False Claims Act

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Abstract

We examine whether and how whistleblowing risk affects corporate environmental performance. Using state-level court rulings associated with the U.S. False Claims Act (FCA) and prior firm-level FCA cases as an identification strategy to capture exogenous variations in whistleblowing risk, we find that firms facing higher whistleblowing risk experience a significant reduction in toxic releases. Further analyses (i) reveal that the impact of whistleblowing risk on environmental performance is more pronounced among firms with more active whistleblowers or stronger corporate governance, and (ii) provide evidence that pollution abatement investments serve as a potential channel driving this effect. Overall, our findings imply that whistleblowing risk complements corporate governance in shaping firms' environmental performance.

Keywords: False Claims Act; Whistleblowing Risk; Toxic Releases; Green Innovations; Whistleblowers

JEL Classification: G32, G38, K20, Q50, Q54, Q5

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“In recent times, it seems that nearly every day has provided us with an opportunity to appreciate the contributions of whistleblowers. Often, they display extraordinary bravery to expose fraud and wrongdoing, and to shine light in some very dark places.”

—Allison Herren Lee, SEC Commissioner, Sept 23, 2020

1. Introduction

Following the notorious accounting frauds of the first few years of this century and the more recent financial crisis in 2008, the U.S. government has implemented various whistleblower provisions, including the False Claims Act (FCA), the Sarbanes-Oxley Act, and the Dodd-Frank Act, to enhance protections for whistleblowers who detect and report fraud as well as incentivising them to help the government to recover losses. From 1987 to 2019, the government recovered \$62.1 billion under the False Claims Act, of which more than \$44.7 billion or 72% came from qui tam cases brought by whistleblowers (DOJ, 2019).

Consistent with the superior performance of these whistleblowing laws, previous literature has documented the deterrence effects of whistleblowing risk as well as the information discovery role of the whistleblowers. Bowen et al. (2010) find that the information exposed by the whistleblowers urges companies to make more financial restatements; Wilde (2017) shows that the Dodd-Frank whistleblowing program deters companies’ tax aggressiveness; this program has also been documented to have deterrence effects on the probability of financial fraud (Berger and Lee, 2022, Wiedman and Zhu, 2023) or even insider trading (Raleigh et al., 2023).

Recently, whistleblowers have started leveraging whistleblowing laws, such as the False Claims Act, to report corporate environmental-related fraud. Some whistleblowers have even

directly filed cases against firms for unlawful pollution¹. These environmentally-focused whistleblowing cases significantly broaden the application of whistleblower laws compared to prior studies, which primarily document their impact on corporate financial decisions due to their original design for detecting financial misconduct. Motivated by these real-world developments, a natural follow-up question emerges: whether and how whistleblowing laws generate externalities that affect environmental outcomes and public health—issues of paramount importance in today's business environment, particularly after the Paris Agreement was signed in 2016. In our paper, we fill this literature gap by studying the unintended impacts on corporate environmental performance of whistleblowing risk by using state-level court rulings associated with the U.S. False Claims Act and prior firm-level FCA cases as an identification strategy to capture exogenous variations in whistleblowing risk.

The tension in our study arises from two competing hypotheses. On the one hand, whistleblowing laws encourage whistleblowers to disclose valuable information, enhancing firms' overall information environment and enabling investors and stakeholders to better monitor corporate environmental performance (Dyck et al., 2010; Wilde, 2017). Previous literature finds that investors and other stakeholders actively acquire information to monitor a firm's environmental performance (Azar et al., 2021; Cohen et al., 2022; Jing et al., 2023). Given that whistleblowing risk can improve a firm's overall information environment through active whistleblower participation (Dyck et al., 2010; Wilde, 2017), we anticipate that investors and other stakeholders are better able to monitor the environmental performance of firms facing

¹ See the following news <https://news.bloomberglaw.com/federal-contracting/biodiesel-fraud-suit-nets-70-million-for-whistleblower-feds> and <https://www.orlandosentinel.com/2023/10/31/gulfstream-racetrack-accused-by-its-former-top-lawyer-of-polluting-water-with-horse-manure/> for details.

higher whistleblowing risk. Although whistleblowers may not directly expose environmental fraud, enhanced transparency in financial disclosures still supports environmental monitoring. This is because key environmental activities—such as environmental expenditures, investments in green innovations, and environmental provisions—are explicitly reflected, audited, and verified within firms' financial statements. Moreover, whistleblowing risk may also discipline managers by deterring short-term opportunistic behaviour and encouraging investments that improve long-term environmental performance (Berger and Lee, 2022; Hsu et al., 2023).

On the other hand, whistleblowing litigation processes can impose significant direct costs (e.g., fines, legal fees, increased insurance premiums) and indirect costs (e.g., heightened financial constraints, higher debt costs, reduced operational efficiency, and negative stock returns) on firms (Arena, 2018; Deng et al., 2014; Bowen et al., 2010). These costs resulting from future whistleblower-initiated litigation will hamper the company's day-to-day business activities, which in turn will make it challenging to satisfy investors' return requirements. As for this circumstance, the firm's response is usually sacrificing the interest of other stakeholders—investing less in emission abatement to secure resources and polluting more (e.g., Liu et al., 2021; Thomas et al., 2022; Xu and Kim, 2022). As whistleblowing risk can shape a firm's environmental performance in two opposite ways, whether and how this risk affects corporate environmental performance is an empirical question.

To provide plausible causal inferences on how whistleblowing risk affects corporate environmental policies, our study leverages the benefits from three state-level court rulings associated with the U.S. False Claims Act and prior firm-level FCA cases as an identification strategy to capture exogenous variations in whistleblowing risk. Exploiting the three shocks

and real FCA cases provides us with several benefits: for the three shocks, (i) these shocks are based on four court rulings from the federal-level Courts of Appeals, thereby suffering less from lobbying or political pressure, (ii) compared to the two federal-level provisions: SOX Act section 806 or the Dodd-Frank Act section 922, which makes it very difficult to identify suitable treated and control groups, decisions by Courts of Appeals that increase the financial incentives for whistleblowers to file FCA lawsuits provide state-level treated and control groups at different points in time and (iii) both the SOX Act and the Dodd-Frank Act contain various corporate governance codes or provisions, therefore it is hard to isolate the effects from one single whistleblowing section; in contrast, these court rulings which rule in favour of the whistleblowers provide a cleaner and more direct impact on whistleblower's incentives. For prior FCA cases, (i) these cases capture firm-level exposure to whistleblowing risk (Bowen et al., 2010) and (ii) study shows that previous litigation experience is important in corporate decision-making (e.g., Deng et al., 2014) and these cases directly capture a firm's previous experiences in FCA.

In our baseline results, we find that firms facing higher whistleblowing risk experience a significant reduction in toxic releases. in the toxic release of about 14%-15%, highlighting the positive externality of these court rulings on the environment. This finding is consistent with our hypothesis that whistleblowing risk encourages whistleblowers to provide useful information and helps investors and other stakeholders to monitor corporate environmental performance more effectively. Also, whistleblowing risk may better discipline managers' shortsighted behaviours and encourage them to invest more in the firm's long-term value—environmental performance.

We then tackle the endogeneity concerns and verify the validity of the parallel trends assumption inherent in our stacked DID approach by examining the dynamic impact of the whistleblowing risk on corporate environmental performance. Our results alleviate the concerns for pre-treatment effects and reverse causality and suggest that our analysis satisfies the parallel trends assumption to use a stacked triple difference model.

After that, we aim to tackle an additional concern driven by systematic differences in the characteristics between the treatment group and the control group. To achieve this, we apply the propensity score matching (PSM, hereafter) and the entropy balancing (EB, hereafter) as two alternative approaches to identify the control sample. Our results suggest that our baseline results are robust to the PSM approach with a more similar control group and the EB approach with a covariates-reweighted sample.

Our study is based on the hypothesis that whistleblowing risk provides stakeholders with useful information, thereby enabling better monitoring of firms' environmental performance; additionally, whistleblowing risk disciplines managers, discouraging short-sighted decisions and promoting investment for long-term value maximization. Central to our argument is the idea that whistleblowing risk improves existing corporate governance. Therefore, we conduct cross-sectional analyses conditional on corporate governance quality based on various proxies from prior studies. The results indicate that the impact of whistleblowing risk on a firm's toxic releases is more pronounced in firms with better existing corporate governance, in line with our prediction that whistleblowing risk complements a firm's corporate governance in shaping a firm's environmental performance.

Whistleblowing risk fundamentally relies on whistleblowers' active engagement within a

country's legal system. Therefore, we examine cross-sectional heterogeneity based on different types of whistleblowers. Dyck et al. (2010) identify short-sellers, financial analysts, and employees as key whistleblowers in capital markets, each with distinct motivations. We find that the effect of whistleblowing risk on corporate toxic releases is more pronounced among firms with greater short-seller coverage, higher analyst coverage, and less rank-and-file stock options. These findings suggest that the presence of more active whistleblowers significantly enhances whistleblowing risk, thereby leading to improved environmental performance.

Finally, we manage to identify the channel through which whistleblowing risk improves firms' environmental performance by focusing on the firm's investment in green patents (Cheng et al., 2025; Jing et al., 2023). We find that firms significantly increase their investment in green patents when facing higher whistleblowing risk, suggesting that green innovation investments may serve as a potential channel driving the decrease in firms' toxic releases.

This study's contribution is twofold. Firstly, this study contributes to the growing body of whistleblowing literature by providing the first evidence of how external whistleblowing risk affects corporate environmental policy. The aforementioned previous research in the field of whistleblowing has concentrated on the effectiveness of whistleblowing in detecting accounting fraud, enhancing the quality of financial reporting or deterring insider trading (Baloria et al., 2017; Berger and Lee, 2022; Bowen et al., 2010; Call et al., 2018; Dyck et al., 2010; ; Lee et al., 2025; Raleigh, 2023; Wiedman and Zhu, 2023; Wilde, 2017). However, these studies cannot explain the real-world cases in which whistleblowers directly leverage whistleblowing laws originally designed for financial fraud to combat corporate environmental misconduct. In contrast, our paper proposes an improved monitoring channel, emphasizing how investors and

other stakeholders can utilize whistleblower-provided information to monitor firms' environmental performance. As the FCA cases utilized in our identification strategies are not directly related to corporate environmental misconduct, our findings further suggest that even if whistleblowers do not explicitly report environmental fraud, enhanced transparency from financial disclosures still facilitates environmental monitoring. This is because crucial environmental activities—such as environmental expenditures, investments in green innovation, and environmental provisions—are explicitly captured, audited, and verified within firms' financial statements. Therefore, our literature fills the gap by illustrating whether and how whistleblowing laws would impose any externalities on the environment and public health, which has become a paramount concern in the recent business world following the signing of the Paris Agreement in 2016. Considering the world trend of transitioning into a net-zero era, establishing a connection between these laws and decisions related to corporate emissions is important and meaningful.

Secondly, this study also contributes to a nascent body of research regarding corporate environmental performance and climate finance. Prior research has explored extensively the factors affecting corporate emissions, including institutional investors (Azar et al., 2021; İlhan et al., 2023), earnings pressure and earnings management (Liu et al., 2021; Thomas et al., 2022), court ruling of limited liability (Akey and Appel, 2021), financial constraints (Xu and Kim, 2022), the role of analysts (Jing et al., 2023) and Greenwashing through divesting (Duchin et al., 2024). Our paper is the first to highlight whistleblowers as an important source of information, enabling the aforementioned stakeholders, such as institutional investors and analysts, to better monitor firms' environmental performance. We therefore extend the views of

previous studies by showing that whistleblowing risk serves as the trigger of information source in helping the capital market monitor a firm's environmental misconduct.

The remainder of this paper is organized as follows. Section 2 provides the literature review and hypothesis formation. Section 3 discusses the institutional background of whistleblower laws and our identification strategy. Section 4 discusses the data and empirical model. Section 5 reports our baseline results, various robustness checks and further analyses. Section 6 concludes the paper.

2. Literature Review and Hypothesis Formation

2.1 Whistleblowing literature

Based on the different whistleblowing channels, previous studies on whistleblowing have distinguished two types of whistleblowing² (e.g., Lee and Xiao, 2018): internal whistleblowing, where whistleblowers blow the whistle within the corporate internal control system—they direct their revelations to top management, the audit committee, or directors; and external whistleblowing, where whistleblowers report to the external regulatory agencies outside the corporate boundary.

One strand of literature focuses on the consequences of external whistleblowing. They use either whistleblower news or whistleblowing laws as identification strategies to study how external whistleblowing affects the capital market. Dyck et al. (2010) implement a systematic

² To distinguish two types of whistleblowing, please refer to Barnett et al. (1993), Dhamija (2014), Dworkin and Baucus (1998), Lee and Xiao (2018), and Smaili and Arroyo (2019) for a detailed review. In our paper, we focus on the exogenous shocks provided by court rulings on the False Claims Act, which is an external whistleblowing channel, since Dyck et al. (2010) indicate that only 19% of whistleblowing cases are raised by employees. Therefore, the internal whistleblowing system loses the chance to receive valuable whistleblowing information from other stakeholders.

analysis of corporate fraud cases and find that the most crucial source in detecting accounting fraud is the employee, and a monetary incentive is a stronger channel for employee whistleblowing compared to the reputation channel. Bowen et al. (2010) provide evidence that whistleblowing allegations are associated with lower short-term abnormal returns, more future restatements and lawsuits, and lower future operating performance and stock returns. Wilde (2017) documents that, compared to control samples, firms subject to whistleblowing experience less financial misreporting and tax aggressiveness. Call et al. (2018) highlight the outcome of financial misrepresentation enforcement actions—more monetary penalties and longer prison sentences—after employee whistleblowing. Raleigh et al. (2023) document that this deterrence effect also plays a vital role in corporate governance and the capital market, preventing insider trading. Finally, Lee et al. (2025) employ the New York FCA and find that state-level FCA helps reduce state-level tax avoidance. In contrast, some research highlights the dark side of whistleblowing on corporate daily operations and disclosure. For instance, Kuang et al. (2021) demonstrate the adverse effects of whistleblowing allegations on audit fees, as whistleblowing risk can increase audit risk. Du et al. (2020) find that both FCA and Dodd-Frank whistleblowing laws increase bank loan spreads. In the same vein, Huang et al. (2023) find that after the promulgation of the Dodd-Frank whistleblowing program, the firms become less transparent—they disclose less information related to their customers to avoid potential whistleblowers using their customers' information to detect their frauds.

Three related studies (Heese and Perez-Cavazos, 2019; Hey et al., 2021; Heese et al., 2021) explore the whistleblowing cases under the False Claims Act. They find that the government chose to sign more fixed-price contracts rather than cost-plus contracts with accused firms; they

also exploit the staggered increase of financial awards for FCA claims in US appeal courts and document that the level-up of financial incentives leads to more lawsuits; they further test the effects of Department of Justice (DOJ) engagement on corporate behaviors and find that the DOJ engagement enhances employee relation, internal control, and board independence. Two recent but related studies document the deterrence effects of the Dodd-Frank whistleblowing provision on corporate financial fraud (Berger and Lee, 2022; Wiedman and Zhu, 2023). Following their studies, Zhou et al. (2023) discover the peer deterrence effect of whistleblowing allegations on corporate aggressive reporting.

Another strand of literature concentrates on the determinants of external whistleblowing. Lee and Fargher (2013) use S&P 200 firms and find that anonymous reporting, organizational support, external directors on the audit committee, and concentrated shareholdings trigger more whistleblowing disclosure. Bereskin et al. (2020) find that prosocial employees and CEOs can increase the likelihood of employee whistleblowing; Heese and Perez-Cavazos (2021) find that the exogenous shock that increases unemployment insurance leads to more employee complaints to OSHA. In contrast, Call et al. (2016) demonstrate that companies strategically grant employees more stock options to prevent them from whistleblowing.

Regarding internal whistleblowing studies, this body of literature is sparse due to the shortage of archival data. Lee and Fargher (2018) use Australian data and find that a high-quality audit committee can increase the likelihood of internal whistleblowing rather than external. Stubben and Welch (2020) utilize a proprietary dataset and document the first evidence of how a firm's internal whistleblowing system assists in decreasing government fines and material lawsuits. Lin et al. (2022) explore the effect of internal whistleblowing in a Chinese

context and document that internal whistleblowing could reduce stock price crash risk.

2.2 Literature on corporate environmental performance

After the signing of the Paris Agreement, we witness a growing trend of literature focusing on climate finance (Hong et al., 2020; Krueger et al., 2020; Stroebel and Wurgler, 2021). Earlier studies have concentrated on CO₂ emissions and document that institutional investors care about the emissions and take the lead in governing corporate environmental performance (Azar et al., 2021; Shive and Forster, 2020). Recent research has focused on more harmful emissions such as NO_x, SO_x emissions, and toxic releases, and it has explored various factors that drive corporate emissions policies.

Consistent with Friedman's (1970) view that "the social responsibility of business is to increase its profits," firms sacrifice the environment when facing external pressures on profitability or financial constraints, thereby generating negative externalities. Liu et al. (2021) find that firms subject to earning pressure have higher sulfur dioxide emissions; firms even strategically increase their ESG ratings to create a reputational slack for future emissions (Thomas et al., 2022). In addition, Xu and Kim (2022) find that relaxing financial constraints reduces toxic emissions from U.S. public companies. This underscores the significant impact financial constraints have on environmental pollution, a costly negative externality that affects society and public health.

The regional cap-and-trade program also seems not to work since firms will transfer production and emissions to states that have not been regulated (Bartram et al., 2022). The situation was made worse when courts ruled to strengthen parent limited liability protections for certain subsidiaries (Akey and Appel, 2021). Some opportunists have even used divestiture

as a tool to greenwash themselves (Duchin et al., 2024). Fortunately, studies do find that financial analysts and socially responsible lenders assist in improving corporate environmental performance (Choy et al., 2024; Jing et al., 2023), and the EPA's enforcement even shows a spillover effect on reducing toxic releases (Dasgupta et al., 2023).

2.3 Hypothesis formation

The tension of our study relies on two competing hypotheses. On the one hand, whistleblowing laws encourage whistleblowers to provide valuable information (Dyck et al., 2010), which improves information disclosure that investors and stakeholders rely on to monitor corporate environmental performance. For example, major institutional shareholders, such as BlackRock, Vanguard, and State Street Global Advisors, directly engage firms with higher CO₂ emissions to influence corporate decisions (Azar et al., 2021). They may also gather climate-related disclosures from the CDP (formerly known as the Carbon Disclosure Project). Similarly, analysts frequently raise environmental-related questions during corporate events, such as earnings conference calls, to obtain relevant insights (Jing et al., 2023). Given that whistleblowing risk can enhance a firm's overall information environment by facilitating whistleblowers' active participation (Dyck et al., 2010; Wilde, 2017), we anticipate that investors and other stakeholders are better equipped to monitor environmental performance in firms subject to higher whistleblowing risk. Although whistleblowers may not explicitly report environmental fraud, improvements in a firm's financial information environment facilitate monitoring environmental performance because critical environmental activities—such as environmental expenditures, investments in green innovations, and environmental provisions—are directly reflected, audited, and verified in firms' financial statements. In practice, many

third-party data providers also base their assessments of corporate environmental performance on precisely such financial disclosures³. Whistleblowing laws also pose a deterrence effect on managers' misconduct (e.g., Berger and Lee, 2022). A key question documented in the corporate finance study is the agency issues of these shortsighted managers (Jensen and Meckling, 1976). As whistleblowing risk can discipline managers and let them focus on the firm's long-term value maximization, we anticipate firms will increase their investment in environmental performance since pollution reduction is better for their long-term firm value maximization (Hsu et al., 2023).

On the other hand, the whistleblowing process is a time- and resource-consuming process⁴. These lawsuits can bring firms direct and indirect costs. Direct costs from a lawsuit include legal fines, attorney fees, and increased insurance premiums. More intuitively, the heightened litigation risk posed by whistleblowing laws can exacerbate financial constraints for companies by raising the cost of debt. Arena (2018) documents that firms facing higher litigation risks experience lower credit ratings and must pay higher yields on loans and bonds. Similarly, Deng et al. (2014) find that defendant firms encounter more financial covenants and are more likely to require collateral. Furthermore, the heightened litigation risk posed by whistleblowing laws can exacerbate financial constraints for companies by raising the cost of debt, which is the

³ For instance, Trucost uses the firm's financial fundamentals to estimate its emission level (Aswani et al., 2024), and MSCI analyzes capital expenditure on green techs and renewable energies in its ESG rating, see <https://www.msci.com/esg-and-climate-methodologies>.

⁴ According to Phillips & Cohen, a leading law firm specializing in whistleblower cases, these cases can be complex and time-consuming. These cases take an average of 3 to 5 years, varying according to the complexity of the fraud, the amount of evidence required, the level of cooperation required, and the extent to which the government investigates and intervenes in the decision.

indirect cost. Other indirect costs include negative future operating performance and stock returns (Bowen et al., 2010). These costs resulting from future whistleblower-initiated litigation will hamper the company's day-to-day business activities, which in turn will make it challenging to satisfy investors' return requirements. As for this circumstance, the firm's response is often sacrificing the interest of other stakeholders—investing less in emission abatement to secure resources and polluting more (e.g., Liu et al., 2021; Thomas et al., 2022; Xu and Kim, 2022).

Based on the previous discussion, we propose the following competing hypotheses:

H1a: Whistleblowing risk is negatively (positively) correlated with toxic releases (environmental performance).

H1b: Whistleblowing risk is positively (negatively) correlated with toxic releases (environmental performance).

3. Institutional Background and Identification Strategy

3.1 Whistleblowing laws and the False Claims Act

Whistleblowing laws have a history of over 700 years, and the most important provision inherent in the whistleblowing laws is called the qui tam provision. At common law, a writ of qui tam is a writ by which an individual who assists in a prosecution may obtain for himself or herself all or part of the damages or financial penalties recovered by the government as a result of the prosecution. The first qui tam law can be traced back to medieval times in England when King Edward II shared one-third of the penalties with the whistleblower who had successfully prosecuted a government official for moonlighting as a wine merchant in 1318. In the modern era, the False Claims Act holds the distinction of becoming the first whistleblowing law in the

US. It was passed by Congress in 1867 as a federal law and was further refined and strengthened in 1986. Since 1987, one year after Congress strengthened the FCA, 18 states have proposed their state-level general False Claims Act to recover governmental losses at the state level (Berger and Lee, 2022).

3.2 Identification strategy and three shocks by appeals courts

The US federal court system consists of three hierarchies: the US district courts, the US courts of appeals, and the Supreme Court of the US. A total of 94 district courts serve the 51 states in the US, since one state might possess more than one district court. The 51 states (94 district courts) have been divided into 13 circuits, each of which has its unique court of appeals, and they are responsible for deciding appeals from their federal jurisdiction against the judgments of district courts. Figure 1 plots the geographic boundaries of the US Courts of Appeals (Circuits).

[Insert Figure 1 Here]

These appeal courts and the Supreme Court are considered the most influential parts of the US federal court system (US Courts, 2023). Since 1994, three decisions have been made by three different appeal courts in three cases under the False Claims Act, which have increased the financial incentives for whistleblowers. These three decisions were made in 1994 (the 9th Circuit), 2002 (the 8th Circuit), and 2007 (the 7th Circuit), affecting 13 district courts in 9 states, 10 district courts in 7 states, and 7 district courts in 3 states, respectively. These state-level staggered decisions create an opportunity for a quasi-natural experiment to examine how financial incentives for whistleblowers moderate their role in affecting corporate environmental performance. We then elaborate on the three shocks we build on the court decisions and explain

why they can increase the financial incentives for whistleblowers.

The first shock contains two court decisions⁵. The first one is *United States ex rel. Killingsworth v. Northrop Corp.* 25 F.3d 715, 722 (9th Cir. 1994). In this case, Max Killingsworth brought a qui tam action against the Northrop Corporation, alleging it defrauded the United States by inflating cost estimates used to support MX missile contract proposals. After a series of consultations, Killingsworth and Northrop attempted to settle the case. However, the government (DOJ) then expressed concern with this agreement, since it thought the whistleblower and the defendants had made a collusive settlement. Therefore, the government informed the court that it did not consent to the settlement agreement. Consequently, the court (California district court) entered its order of dismissal of the action with prejudice, stating that the Attorney General's consent to dismissal in the current circumstances was not required. The government then appealed to the Ninth Circuit (appeals court). Nevertheless, contrary to all other appeals courts, the 9th Circuit still rules in favour of the whistleblower's collusive settlement with the defendant. This decision increased the financial incentives for whistleblowing in two ways (Engstrom, 2013), First, it provides an incentive for whistleblowers and defendants to trade broad releases of liability for larger settlement amounts; and second, it makes it more likely that the DOJ will intervene in cases filed in the Ninth Circuit to monitor collusive settlements. This benefits the whistleblower because DOJ intervention greatly increases the likelihood of a settlement.

⁵ The reason that the first shock contains two court decisions instead of just one is that, after the first decision made by the 9th Circuit which increased the financial incentives for whistleblowers in 1994, it was followed by another decision which decreased the financial incentives for whistleblowers to file lawsuits in this circuit in 2000. Therefore, in our empirical analysis, we end the treatment period for the first shock in 2000 (Dey et al., 2021).

The second decision is *United States v. Mackby*, 261 F.3d 821 (9th Cir. 2001). Here, Peter Mackby, the defendant, appeals to the district court's civil judgment in favour of the United States under the False Claims Act. He argued that the treble damage rewards are unconstitutionally excessive under the Excessive Fines Clause of the Eighth Amendment. This time, the Court of Appeal ruled in favour of the defendant. This decision made it financially less attractive to file lawsuits in the 9th Circuit because the decision raised the possibility that the district courts would assess treble damages as excessive, reducing the overall settlement amount.

The second shock contains one court decision—*United States v. Larry Reed & Sons Partnership*, 280 F.3d 1212 (8th Cir. 2002). In 2001, Larry Reed Sons (the partnership), an agricultural partnership, and its individual partners were found by a jury to have filed false cotton crop insurance claims eight years earlier. The partnership argued that because the plaintiffs alleged that the partnership's damages were only \$49,720, at trial the United States proved only \$56,874 in damages as a result of the partnership's false cotton crop insurance claim (the initial complaint). As a result, the jury awarded the partnership nearly \$94,000 in damages based on insufficient evidence. The United States Court of Appeals for the Eighth Circuit ruled that the damages were not limited to the amount alleged in the initial complaint. Since the whistleblower's share is based on the total recovery (i.e., the sum of damages and penalties), the award increases the financial incentive for whistleblowing by allowing the whistleblower to modify his or her claim upward throughout the investigation and legal proceedings, thereby increasing the total recovery.

Finally, the third shock also contains one court decision—*U.S. ex rel. Tyson v. Amerigroup*

Illinois, Inc., 488 F. Supp. 2d 719 (N.D. Ill 2007). In this case, the jury returned a verdict that the defendant believed was excessive. The defendant then appealed to the Court of Appeals for the Seventh Circuit in 2007. On appeal, the Court ruled in favour of the plaintiff, ruling that the penalty was reasonable. This judgment increased the financial incentive for the whistleblower to file a lawsuit in the Seventh Circuit because it ruled that penalties substantially in excess of damages were permissible, thereby increasing the total amount of the recovery.

Exploiting the three shocks stemming from the appeals courts provides us with three benefits: (i) these three shocks are based on four court rulings from the federal-level Courts of Appeals, thereby suffering less from lobbying or political pressure, (ii) compared to the two federal-level provisions: SOX Act section 806 or the Dodd-Frank Act section 922, which makes it difficult to identify suitable treated and control groups, decisions by Courts of Appeals that increase the financial incentives for whistleblowers to file FCA lawsuits provide state-level treated and control groups at different points in time and (iii) both the SOX Act and the Dodd-Frank Act contain various corporate governance codes or provisions, therefore it is hard to isolate the effects from one single whistleblowing section; in contrast, these court rulings which rule in favour of the whistleblowers provide a cleaner and more direct impact on whistleblower's incentives.

Apart from employing the court rulings, we also follow previous studies which directly use whistleblowing cases as an identification strategy (Bowen et al., 2010; Kuang et al., 2021; Raleigh, 2023). We follow Dey et al. (2021) and collect firm-level FCA cases⁶ to capture the

⁶ Engstrom (2013) files a FOIA request to the Department of Justice for the record of all the FCA lawsuits from 1987-2012. We are grateful to David Freeman Engstrom from Stanford Law School for generously sharing his data on FCA litigations.

firm's own exposure to the False Claims Act. This approach also provides us with two benefits: (i) these cases capture firm-level exposure to whistleblowing risk (Bowen et al., 2010) and (ii) study shows that previous litigation experience is important in corporate decision-making (e.g., Deng et al., 2014) and these cases directly capture firm's previous experiences in FCA. Overall, we use both state-level court rulings associated with the U.S. False Claims Act (FCA) and prior firm-level FCA cases as an identification strategy to capture exogenous variations in whistleblowing risk.

Specifically, in our following stacked regressions, we define the firms located in states that have been affected by a relative shock which increases the financial incentives for whistleblowers as the treated group, and plants located in states that have never been affected by any of the court rulings as our clean control group to build the stacks. We then interact the DID interaction term with the firm's prior FCA experiences and estimate a triple interaction term as our variable of interest for whistleblowing risk.

4. Data and Empirical Model

4.1 Sample selection

Our FCA case data is from a FOIA requirement directly to the U.S. Department of Justice (Engstrom, 2013). The data provided by the US DOJ includes the whistleblowers' names, defendants' names (company names), the time when the whistleblower submitted a case, the attorney firm, and whether the DOJ chose to intervene or not in a case. We search case documents from the Public Access to Court Electronic Records (PACER) website to supplement the details of these cases. We then manually match these cases using the defendants' names to the COMPUSTAT listed firms. The detailed matching processes are provided in the Online

Appendix C. Finally, we manually read and check the nature of these matched cases and find that all these cases are not directly related to the firm's environmental misconduct.

We draw the plant-level toxic release data from the EPA TRI basic files for the period 1991-2010. To obtain firm-level accounting variables as controls, we then match them to the COMPUSTAT North America annual files using the linking table provided by Hsu et al. (2023). We draw institutional ownership data from the Thomson-Reuters Institutional Holdings (13F) database. Earnings conference call transcripts come from Capital IQ. For cross-sectional analysis, we retrieve firm-level analyst coverage data from I/B/E/S, stock option data from EXECUCOMP, and short-sellers holding data from COMPUSTAT. Consistent with previous literature (Hsu et al., 2023), we exclude financial firms (SIC codes 6000-6999). All continuous variables are winsorized at the 1% and 99% percentile.

4.2 Measuring corporate environmental performance

We measure corporate environmental performance using the toxic release data provided by the EPA TRI program, which has been widely used by recent studies (e.g., Akey and Appel, 2021; Jing et al., 2023; Xu and Kim, 2022). This program requires all the toxic release inventories to report their emissions if the plant falls within a TRI reportable industry sector, has 10 or more employees, and crosses a certain threshold in manufactured or processed TRI-listed chemicals. Using TRI data provides us with several advantages. First, TRI data are reliable because 18 U.S.C. § 1101 makes it a criminal offence to falsify information provided to the U.S. Government, including knowingly falsifying records for inspection. Secondly, TRI data provides records of establishments' real emissions levels but not estimated levels. A recent study (Aswani et al., 2024) documents that vendor-estimated emissions (e.g., Trucost data)

differ systematically from company-reported emissions and are highly correlated with financial fundamentals, suggesting that using data on estimated emissions may capture the association between such fundamentals and the variable of interest.

Following previous literature (Hsu et al., 2023), we utilize the value “PROD. WASTE (8.1 THRU 8.7)” to measure corporate environmental performance, that is, the sum of the amounts of all emissions (in pounds) for all chemical categories. Section 8 of the TRI database reports several items, including 8.1 (amount of total releases), 8.2 (energy recovery on-site), 8.3 (energy recovery off-site), 8.4 (recycling on-site), 8.5 (recycling off-site), 8.6 (treatment on-site), 8.7 (treatment off-site), and PROD. WASTE (8.1 THRU 8.7) (the sum of the quantities in items 8.1 through 8.7). We also drop all observations with zero emissions since zero release does not mean a plant emits zero toxic chemicals in the TRI database—TRI inserts zero into a blank field (Akey and Appel, 2021; Thomas et al., 2022; Xu and Kim, 2022).

4.3 Baseline model

Following Cengiz et al. (2019), to avoid comparing an afterwards treated sample to previously treated observations which drive biased estimators caused by staggered treatment times, we create a separate dataset (cohort) for each of the three shocks from appeals courts, each excluding observations from states affected by the other two shocks and including observations that never be treated as “clean controls.” For each cohort, we build a 10-year estimation window $[-5,+5]$ around the respective shock and then stack these shock-specific cohorts together to calculate an average treatment effect across the three shocks (Year 0 is dropped). Therefore, we estimate a staggered difference-in-difference-in-difference (DIDID) baseline specification with two-way fixed effects to test hypotheses 1 and 2:

$$Toxic\ Releases_{i,t} = \beta_1 Courts_i \times Post_t \times Cases_i + \beta_2 Courts_i \times Post_t + \delta Y_{i,t} + \theta Z_{s,t} + \vartheta_{c,i} + \mu_{c,t} + \varepsilon_{i,t} \quad (1)$$

Where i denotes a firm, t denotes a year, s denotes a state, and c denotes a cohort. The dependent variable⁷, *Toxic Releases*, is the log transformation of TRI toxic release, and the variable of interest is $Courts \times Post \times Case$. *Courts* takes the value of 1 if the firm is located in states which have been affected by the court rulings that increase the financial incentives for whistleblowers and zero otherwise. *Post* takes the value of 1 if the observation is in the period after the relevant court rulings and zero otherwise. *Cases* is the number of five-years previous FCA lawsuits before each shock.

Based on the prior studies regarding corporate emissions (e.g., Azar et al. 2021; Duchin et al., 2024; Choy et al., 2024; Shive and Forster 2020), we use the following firm-level control variables ($Y_{i,t}$) for our regressions: firm size (*Size*), Portability (*ROA*), leverage ratio (*Leverage*), tangibility (*Tangibility*), growth opportunities (M/B), Cash-to-liability-ratio (Cash/liabilities), Altman's *Z* score (*AltmanZ*), dividend payout (*Dividend*) and institutional ownership (*Institutional Holding*). We also include state-level control variables ($Z_{s,t}$): state-level GDP growth (*GDP Growth*) and state-level political balance (*Political Balance*) to control for macroeconomic factors. Finally, we control for cohort-firm and cohort-year fixed effects by interacting these firms and years with different cohorts to build more stringent high-dimensional fixed effects as we run stacked regressions. In robustness tests, we also try state-by-year fixed effects, industry-by-year fixed effects or use OLS regressions without stacking our sample and find quantitatively similar results. The details of variable definitions are provided in Online

⁷ We also test *Release Intensity* throughout this paper, defined as the log transformation of firm-level total toxic releases divided by total sales.

Appendix A.

5. Empirical results

5.1 Descriptive statistics

Table 1 presents the summary statistics before building stacks. We report firm-level and state-level variables in different panels. Firm-level variables are from COMPUSTAT annual files, and state-level data are from WRDS. The distribution of our variables is generally comparable with previous literature (e.g., Jing et al., 2023). However, there are discrepancies in several variables stemming from different sample periods.

[Insert Table 1 Here]

5.2 Results for baseline regressions

Table 2 presents the results for our baseline stacked regressions. We report t-statistics based on heteroskedasticity-robust standard errors clustered at the cohort-firm level. In the first column, we regress the corporate environmental performance measure, *Toxic Releases*, on the interaction term of the three shocks from appeals courts, post-court ruling time indicator, and the number of ex-ante FCA cases, $Courts \times Post \times Case$, without any firm-level or state-level control variables but with cohort-firm and cohort-year fixed effects. The result demonstrates that the coefficient on the triple interaction term (DIDID) is significantly negative at 1% level (t-stat = -3.40). We further control for a set of firm-level and state-level controls that may affect corporate toxic releases with the aforementioned fixed effects in columns (2) and (3). The coefficients remain significant and negative (t-stat = -3.57 and -3.61) on the interaction term for the corporate environmental performance measure. These findings indicate that the three shocks from appeals courts, which increase the financial incentives for whistleblowers,

contribute to the reduction of corporate toxic release, which is consistent with *hypothesis 1a* that whistleblowing risk complements corporate governance and helps investors and stakeholders monitor firms' environmental performance. In columns (4), (5), and (6) of Table 2, we replace the dependent variable with *Release Intensity*. The results continue to be significant (t-stat = -4.58 in column (4), -4.08 in column (5), and -4.14 in column (6)), which corroborates our baseline findings.

[Insert Table 2 Here]

Furthermore, in our baseline specifications, the outcome of the three shocks' impact on corporate environmental performance holds both statistical significance and substantial economic relevance. The coefficient for the interaction term, *Courts* × *Post* × *Cases*, implies that while keeping other factors constant, one additional ex-ante FCA case causes a decrease in the toxic releases of about 14.1%-15.7% after the three court decisions, highlighting the positive externality of whistleblowing risk.

5.3 Robustness tests

5.3.1 Parallel trend analysis

Our staggered difference-in-difference-in-difference specification hinges on a key assumption that the treated groups and control groups follow a parallel trend before the three shocks from the US Courts of Appeals (Roth et al., 2022). To tackle the endogeneity concerns and ensure the validity of the parallel trends assumption inherent in our stacked DID approach, we examine the dynamic impact of the court rulings on corporate environmental performance. If concerns regarding reverse causality or pre-treatment effects were real, we would expect to observe ex-ante significant changes in toxic releases. To allay these concerns, we substitute the

time indicator, $Post_t$, in our baseline tests with a series of distinct dummy variables, namely, $Post_{t-5}, Post_{t-4}, Post_{t-3}, Post_{t-2}, Post_{t+1}, Post_{t+2}, Post_{t+3}, Post_{t+4}, Post_{t+5}$, which allows us to assess the impact of the court rulings both before and after their announcement. Specifically, $Post_{t-k}$ takes the value of 1 if the observation is k years before the relevant court rulings and zero otherwise; $Post_{t+k}$ takes the value of 1 if the observation is k years after the relevant court rulings and zero otherwise.

Table 3 presents the results of the dynamic impact. The coefficients on $Courts \times Post_{t-5} \times Cases$, $Courts \times Post_{t-4} \times Cases$, $Courts \times Post_{t-3} \times Cases$, and $Courts \times Post_{t-2} \times Cases$ are not significant across the table, and the coefficients start to be significantly negative on $Courts \times Post_{t+1} \times Cases$. These results alleviate the concerns for pre-treatment effects and reverse causality and suggest that our analysis satisfies the parallel trends assumption to use a stacked DID model.

[Insert Table 3 Here]

5.3.2 Balancing the covariates between treatment and control groups

At this point, we have successfully mitigated some of the endogeneity issues and strengthened the robustness of my DID model approach based on the above sections. In this section, we aim to tackle an additional concern driven by systematic differences in the characteristics between the treatment group and the control group. To achieve this, we apply the propensity score matching (PSM, hereafter) and the entropy balancing (EB, hereafter) as two alternative approaches to identify the control sample. Previous accounting and finance studies (Shipman et al., 2017) have widely used the PSM approach to identify an appropriate control group; whereas the EB approach has gained traction in recent accounting and finance studies (e.g.,

Beck et al., 2022; Dang et al., 2023; McMullin and Schonberger, 2020), it not only preserves all observations in my sample but also mitigates the potential bias of the iterative matching process employed by PSM (Hainmueller, 2012). Specifically, for the PSM approach, we use a logit model to estimate the propensity score based on firm-level covariates and match a treated firm to a control firm using a caliper of 0.05 without replacement; for the EB method, we balance the covariates at their maximum orders (2nd or 3rd order). After finding a more decent group and rebalancing the covariates, we estimate our baseline specification again based on the propensity score matched sample, and the EB reweighed sample in Table 5.

In Table 4 Panel A, the coefficients on the triple interaction term continue to be statistically significant, suggesting that our baseline results are robust to the PSM approach with a more similar control group. In Table 5 Panel B, the coefficients on the triple interaction term are statistically significant at 1% level in all columns, indicating that our baseline results are robust to the EB approach with a covariates-reweighted sample.

[Insert Table 4 Here]

5.3.4 Additional robustness checks

In this section, we summarize a series of robustness tests to corroborate the results of our baseline findings. For brevity, we have included most of these tests in the Online Appendix B. Collectively, our baseline results are robust when (1) adding proxies for internal controls in our baseline specification to control for internal whistleblowing since there has been evidence on how internal whistleblowing affect corporate emissions in the Chinese stock market (Table 5); (2) using a shorter [-3,+3] time window (Table OA2 Panel A); (3) using a sample without the period of the global financial crisis (Table OA2 Panel B); (4) dropping relocated firms which

may cause a spurious identification of our treated and control group (Table OA3 Panel A); (5) implementing a falsification test with pseudo-FCA cases (no results, Table OA3 Panel B); (6) using another specification with cohort-state-by-year fixed effects to rule out any state-level policies or unobservable shocks that may drive our results (Table OA3 Panel C); (7) using another specification with cohort-industry-by-year fixed effects (Table OA4 Panel A); (8) using three-year previous FCA Cases to measure *Cases*⁸ (Table OA4 Panel B); (9) using a original DID without stacking our sample (Table OA9); (10) using a further identification strategy by splitting firms into high versus low polluting firms (Table OA10); (11) we also follow previous studies (e.g., Acharya et al., 2014) and estimate a Cox proportional hazard model that predicts the aforementioned court rulings to alleviate the concerns that some state may expect the courts to rule in favor of whistleblowers, challenging the assumption of our staggered DID model. As illustrated by these columns in Table OA1, none of the independent variables are statistically significant, suggesting that these environmental, whistleblowing, or macroeconomic factors do not contribute to the consequences of these court decisions; (12) controlling for firm's ESG performance and earnings management behaviours (Table OA5); (13) controlling for various proxies of litigation risk (Table OA6); (14) controlling for financial constraints (Xu and Kim, 2022) may drive a firm's environmental performance in Table OA7 and Table OA8.

[Insert Table 5 Here]

5.4. Cross-sectional analysis

5.4.1 Different types of whistleblowers

Our study is based on the hypothesis that whistleblowing risk provides stakeholders with

⁸ In addition to this test, we use five-year previous FCA cases throughout the paper.

useful information, thereby enabling better monitoring of firms' environmental performance. Specifically speaking, whistleblowing risk fundamentally relies on whistleblowers' active engagement within a country's legal system. Therefore, in this section, we examine cross-sectional heterogeneity based on different types of whistleblowers. Dyck et al. (2010) identify employees, regulators, financial analysts, short-sellers and investors as key whistleblowers in capital markets, each with distinct motivations. For instance, employees who are ignored—or even retaliated against—after raising concerns internally may blow the whistle to protect their own interests; regulators detect fraud because it is part of their mandate; analysts uncover misconduct to signal expertise, short-sellers expose wrongdoing to profit from their positions; investors report fraud to safeguard their wealth.

To verify that our results stem from whistleblowing risk driven by active whistleblower participation, we conduct cross-sectional analysis for the aforementioned whistleblowers. Since rank-and-file employees granted with more stock options are less likely to whistleblow on the firm's misreporting (Call et al., 2016), we use the rank-and-file stock options as our conditioner to proxy for employee whistleblowing. We partition firms into two groups based on the five-year ex-ante average median values of the percentage of options granted to the employees, and anticipate that the reduction of toxic releases will be more pronounced for firms granting fewer rank-and-file options. We report our results in Table 6, Panel A. Consistent with our prediction, our DiDiD coefficients in the fewer option groups (columns (1) and (3)) are larger than the coefficients in the groups with more options, which are also larger than our baseline results⁹.

We then follow Lee et al. (2024) and use their industry classifications to proxy for

⁹ We report the Wald tests to assess the statistical significance of the difference in the DiDiD estimates and find that the differences are all significant at 1% level.

regulators' whistleblowing where firms in transportation (Standard Industrial Classification (SIC) 3700–3799), utilities (SIC 4900–4999), healthcare (SIC 5122), and other services (SIC 8000–8999) industries are exposed to higher whistleblowing threats from industry regulators. We then partition our samples into groups based on the firm's respective 4-digit SIC codes and expect that the reduction of toxic releases will be more pronounced when firms are operating in industries with more regulators. We report our results in Table 6, Panel B. Consistent with our expectations, our DiDiD coefficients in the more regulator groups (columns (1) and (3)) are larger than the coefficients in the groups with more options, which are also larger than our baseline results¹⁰.

We then utilize short-seller coverage as proxies for the potential whistleblowing risk originating from short-sellers. Then, we partition firms into two groups based on the five-year ex-ante average median values of short-seller coverages and re-estimate our baseline specifications. We expect that our baseline results will be more pronounced when firms are covered by more short-sellers. We report our results in Table 7, Panel A. Partially consistent with our expectations, for the toxic releases, our DiDiD coefficient in the groups with more short seller coverage (column (1)) is larger than that in groups with fewer short sellers (Column (2)); however, for the release intensity tests (Column (3) and (4)), the coefficients difference are not that large¹¹.

Our next analysis focuses on the role of financial analysts, as they play an important role

¹⁰ The Wald tests to assess the statistical significance of the difference in the DiDiD estimates show that the differences are all significant at 1% level.

¹¹ The Wald test for columns (1) and (2) is statistically insignificant (p-value = 0.370); nevertheless, these DiDiD estimates imply large differences in terms of economic magnitudes ($(0.192-0.126)/0.126 = 52.3$ percent larger). The Wald test for columns (3) and (4) is statistically insignificant (p-value = 0.708).

in emission reduction (Jing et al., 2023). We partition firms into two groups based on the five-year ex-ante average median values of analyst coverage and re-estimate our baseline specifications. We anticipate that our baseline results will be more pronounced when firms are covered by more financial analysts. We report our results in Table 7, Panel B. Consistent with our expectations, our DiDiD coefficients in the groups with more analyst coverage (Columns (1) and (3)) are statistically significant at 1% level, but for the groups with less analyst coverage (Columns (2) and (4)), the coefficients are distinguishable from zero¹².

Our final analysis focuses on the role of institutional investors (Azar et al., 2021). We partition firms into two groups based on the five-year ex-ante average median level of institutional ownership and expect our baseline results to be more pronounced among firms with greater institutional shareholdings. We report our results in Table 8. Consistent with our prediction, our DiDiD coefficients in the groups with more institutional ownership (columns (1) and (3)) are larger than the coefficients in the groups with less institutional ownership, which are also larger than our baseline results¹³.

Collectively, our findings reveal that the effect of whistleblowing risk on corporate toxic releases is significantly more pronounced among firms with fewer rank-and-file stock options, more stringent regulatory oversight, and greater institutional ownership. This pattern suggests that employees, regulators, and institutional investors each serve as meaningful sources of whistleblowing pressure, and that their presence collectively amplifies whistleblowing risk —

¹² The Wald tests for both toxic releases and release intensity tests are statistically insignificant (p-value = 0.283 and 0.196); however, these DiDiD estimates again imply large differences in terms of economic magnitudes.

¹³ We report the Wald tests to assess the statistical significance of the difference in the DiDiD estimates and find that the difference for columns (1) and (2) is significant at 10% level; for columns (3) and (4), the difference is statistically insignificant (p-value = 0.272) but the coefficients imply large differences in terms of economic magnitudes.

ultimately driving improvements in environmental performance. We also find suggestive evidence that financial analysts and short-sellers play a complementary monitoring role, leveraging whistleblowing laws to scrutinize corporate toxic releases.

[Insert Tables 6, 7 and 8 Here]

5.4.2 Corporate governance

Following our hypothesis, we anticipate that the observed improvements in environmental outcomes occur because whistleblowing risk complements existing corporate governance mechanisms by strengthening deterrence effects. To empirically test our anticipation, in this section, we conduct cross-sectional analyses conditional on corporate governance quality. For external corporate governance, we employ product market competition and product similarity data from previous literature (Hoberg and Phillips, 2016). Specifically, we utilize the Herfindahl-Hirschman Index (*HHI*) and product similarity (*TSIMM*) and calculate the five-year ex-ante average number of different market competition proxies to partition our sample into two groups. We anticipate that the reduction of toxic releases will be more pronounced for firms with better corporate governance (higher levels of market competition). Table 9 presents these results. Using *HHI* as a proxy for market competition (Panel A), the DiDiD coefficients are significantly negative at the 1% level for firms operating in more competitive markets (columns (1) and (3)), while those in less competitive markets (columns (2) and (4)) are positive and statistically indistinguishable from zero. Panel B, which employs *TSIMM* as an alternative proxy, yields quantitatively similar results, and all the results are consistent with our prediction¹⁴.

¹⁴ The coefficients of Wald tests that assess the statistical significance of the difference in the DiDiD estimates

For internal corporate governance, we utilize the number of sustainable committees in the board (Jing et al., 2023) as a measure of the existing internal governance mechanism for corporate environmental performance¹⁵. We partition our sample into two groups based on whether firms have established a sustainable committee during the years before whistleblowing shocks. We expect the reduction in toxic releases to be more pronounced among firms with a dedicated sustainability committee on their board. We report our results in Table 10. Consistent with our expectations, our DiDiD coefficients in groups with a sustainable board (columns (1) and (3)) are larger than the coefficients in the groups without a sustainable committee, which are also larger than our baseline results¹⁶. Collectively, these results are in line with our prediction that whistleblowing risk can complement a firm’s internal and external corporate governance in shaping a firm’s environmental performance.

[Insert Tables 9 and 10 Here]

5.5. Additional analysis

Up to this point, we have provided robust evidence that whistleblowing risk significantly reduces toxic releases among treated firms and documented the cross-sectional heterogeneities of corporate governance and different types of whistleblowers. The natural subsequent inquiry is identifying the channel through which whistleblowing risk improves firms' environmental performance. Prior literature emphasizes investments in pollution abatement strategies and

for (1) Panel A’s tests are significant at 10% and 1% level (p-value = 0.056 and 0.009); (2) Panel B’s tests are statistically not significant but imply a huge difference in terms of the economic magnitude.

¹⁵ Building on prior work demonstrating that sustainability committees serve as an effective internal governance mechanism for reducing corporate social irresponsibility and toxic releases (Fu et al., 2020; Jing et al., 2023), we identify such committees by searching for board committees whose names contain the terms “Sustainability,” “Sustainable,” “Social Responsibility,” “Corporate Responsibility,” “Environment,” or “Environmental” from the BoardEx database.

¹⁶ The Wald tests to assess the statistical significance of the difference in the DiDiD estimates show that the differences are all significant at 1% level.

green patents as essential channels for pollution reduction (Akey and Appel, 2021; Cheng et al., 2025; Jing et al., 2023; Xu and Kim, 2022). Specifically, Cheng et al. (2025) demonstrate that green patents targeting pollution control not only enhance environmental outcomes but also improve financial performance, as pollution represents a direct waste of valuable resources. Building on these insights, We hypothesize that firms decrease their toxic releases through increased investment in green patenting activities. Following previous studies (e.g., Jing et al., 2023), we obtain U.S. patent data from Kogan et al. (2017) and classify green patents according to the Cooperative Patent Classification (CPC) system, specifically identifying CPC classes Y02 and Y04S (Cohen et al., 2022).

We replace the dependent variable in our baseline specification with the measures of green patents and report our results for channel tests in Table 11. As there is usually a time gap between the time a firm invests in a patent and the time the firm successfully files a patent with the U.S. government (e.g., He and Tian, 2013), we report the results of green patents at $t+1$ and $t+2$. Table 11 column (1) and (2) shows that firms with prior FCA cases significantly increase their investment in green patents after the court rulings, suggesting that investing in green patents may serve as a potential channel for pollution reduction. To ensure the robustness of our channel tests, we also use a longer time window (e.g., $[t-2, t+2]$) in columns (3) and (4) to measure green patents and find consistent results¹⁷ (Jing et al., 2023).

[Insert Table 11 Here]

6. Conclusion

Using state-level court rulings associated with the U.S. False Claims Act (FCA) and prior

¹⁷ We also try the average number of green patents in the ex-post period and find quantitatively similar results (Bernstein et al., 2015).

firm-level FCA cases as an identification strategy to capture exogenous variations in whistleblowing risk, we find that firms with higher whistleblowing risk reduced their toxic releases by approximately 14%–15%, which is consistent with the notion that an enhanced whistleblowing environment enables shareholders to better monitor managers and encourages a stronger focus on long-term value creation. Further analyses (i) reveal that the impact of whistleblowing risk on environmental performance is more pronounced among firms with stronger corporate governance or more active whistleblowers (ii) provide evidence that pollution abatement investments serve as a potential channel driving this effect and

We contribute to the two strands of literature. Firstly, this study contributes to the growing body of whistleblowing literature by providing the first evidence of how external whistleblowing risk affects corporate environmental policy. The aforementioned previous research in the field of whistleblowing has concentrated on the effectiveness of whistleblowing in detecting accounting fraud, enhancing the quality of financial reporting or deterring insider trading (Baloria et al., 2017; Berger and Lee, 2022; Bowen et al., 2010; Call et al., 2018; Dyck et al., 2010; Lee et al., 2025; Raleigh, 2023; Wiedman and Zhu, 2023; Wilde, 2017). However, these studies cannot explain the real-world cases in which whistleblowers directly leverage whistleblowing laws originally designed for financial fraud to combat corporate environmental misconduct. In contrast, our paper proposes an improved monitoring channel, emphasizing how investors and other stakeholders can utilize whistleblower-provided information to monitor firms' environmental performance. Since the FCA cases utilized in our identification strategy is not directly related to environmental misconduct, our findings further suggest that even if whistleblowers do not explicitly report environmental fraud, enhanced transparency from

financial disclosures still facilitates environmental monitoring. This is because crucial environmental activities—such as environmental expenditures, investments in green innovation, and environmental provisions—are explicitly captured, audited, and verified within firms' financial statements. Therefore, our literature fills the gap by illustrating whether and how whistleblowing laws would impose any externalities on the environment and public health, which has become a paramount concern in the recent business world following the signing of the Paris Agreement in 2016.

Secondly, this study also contributes to a nascent body of research regarding corporate environmental performance and climate finance. Prior research has explored extensively the factors affecting corporate emissions, including institutional investors (Azar et al., 2021; İlhan et al., 2023), earnings pressure and earnings management (Liu et al., 2021; Thomas et al., 2022), court ruling of limited liability (Akey and Appel, 2021), financial Constraints (Xu and Kim, 2022), the role of analysts (Jing et al., 2023) and Greenwashing through divesting (Duchin et al., 2024). Our paper is the first to highlight whistleblowers as an important source of information, enabling the aforementioned stakeholders, such as institutional investors and analysts, to better monitor firms' environmental performance. We therefore extend the views of previous studies by showing that whistleblowing risk serves as the trigger of information source in helping the capital market fight against a firm's environmental misconduct.

Finally, we make policy implications by illustrating the unintended environmental consequences of whistleblowing risk. Therefore, other regions may need to consider the environmental externalities when adopting their False Claims Act or other whistleblower laws. Overall, our findings imply that whistleblowing risk complements corporate governance in

shaping firms' environmental performance.

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Figure 1 The US Court System

This figure plots the geographic boundaries of the US Courts of Appeals (Circuits). There are a total of 13 Circuits in charge of 94 district courts. (Source: US government)

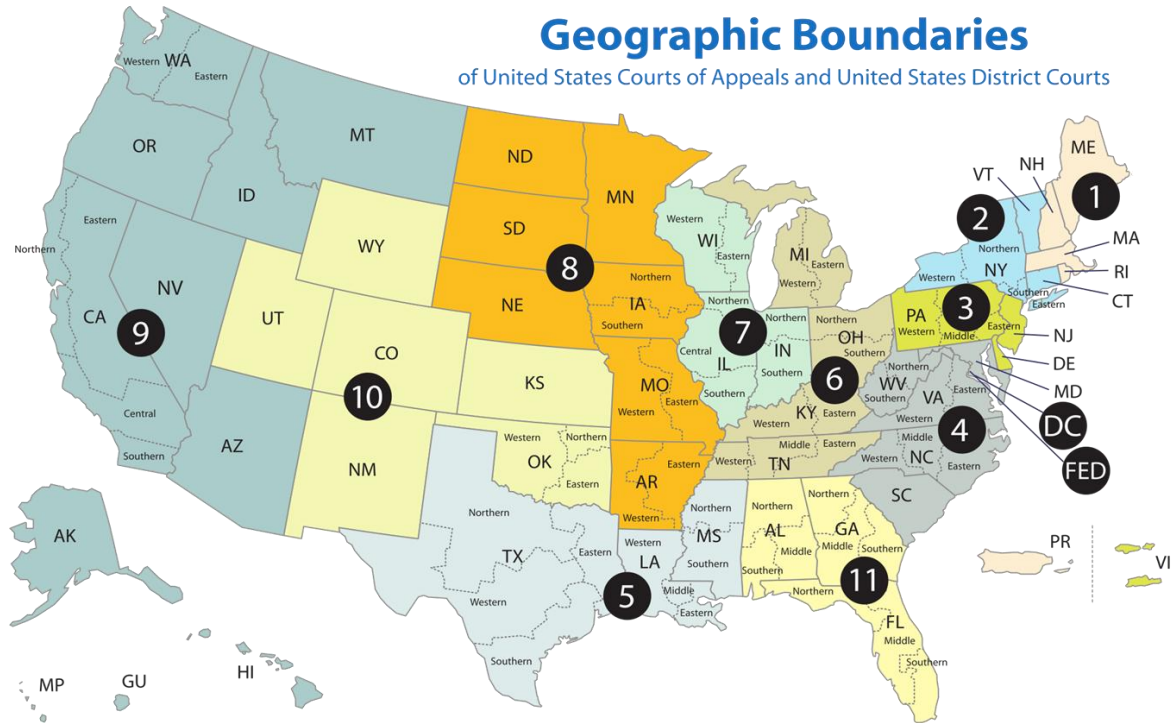


Figure 2 Timeline of Three Shocks and Affected States

This figure plots the timeline of our sample period (1991-2010) and the three shocks stemming from four court decisions. For each shock, we illustrate the lasting period and the states that have been affected by the shock.

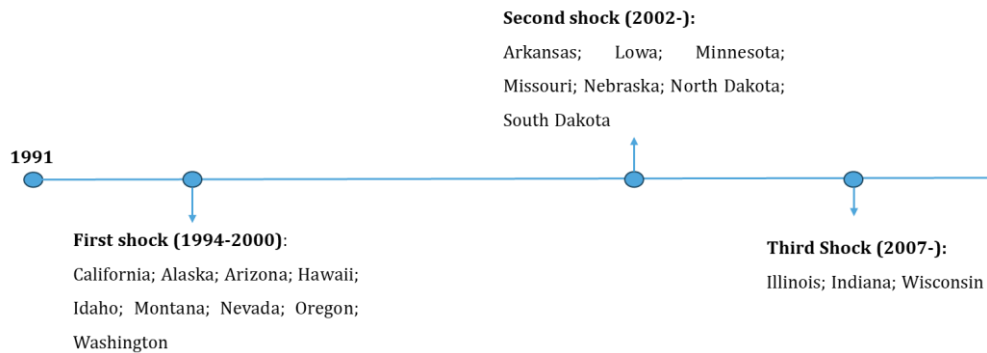


Table 1 Summary Statistics

This table presents the summary statistics before building stacks for further stacked DIDID regressions. We report pollution variables, firm variables, state variables, as well as other dependent variables used in additional analysis in different panels. Pollution data are from the EPA TRI program, firm variables are from COMPUSTAT annual files, and state data are from WRDS. For the sources of the variables used in the additional analysis, please see Appendix A, variable definition.

Variables	N	Mean	SD	p25	Median	p75	p95
Panel A. Pollution Variables							
<i>Toxic Releases</i>	10,633	12.45	3.195	10.593	12.483	14.582	17.48
<i>Release Intensity</i>	10,633	5.542	3.059	3.842	5.829	7.588	9.983
Panel B. Firm Variables							
<i>Cases</i>	10,633	0.138	0.949	0	0	0	1
<i>Institutional Investor</i>	10,633	0.733	0.656	0.019	0.599	1.337	1.855
<i>Tangibility</i>	10,633	0.334	0.173	0.201	0.305	0.437	0.676
<i>I/K</i>	10,633	0.179	0.103	0.109	0.159	0.222	0.386
<i>ROA</i>	10,633	0.093	0.077	0.053	0.091	0.136	0.22
<i>Leverage</i>	10,633	0.269	0.181	0.138	0.254	0.373	0.597
<i>Size</i>	10,633	6.847	1.821	5.557	6.812	8.071	10.016
<i>M/B</i>	10,633	0.523	0.862	0.297	0.49	0.752	1.56
<i>Cash/Liability</i>	10,633	0.173	0.298	0.02	0.061	0.183	0.732
<i>Dividend</i>	10,633	0.59	0.492	0	1	1	1
<i>AltmanZ</i>	10,633	3.647	2.675	2.03	3.063	4.503	8.552
Panel C. State Variables							
<i>Political Balance</i>	10,633	0.049	0.026	0.038	0.049	0.063	0.089
<i>GDP Growth</i>	10,633	0.512	0.178	0.421	0.511	0.586	0.8
Panel D. Additional Analysis Variables							
<i>Green Patents</i> $t+2$	10,633	1.436	5.163	0	0	0	8
<i>Green Patents</i> $[t-1,t+1]$	10,633	7.281	25.373	0	0	2	40
<i>Green Patents</i> $[t-2,t+2]$	10,633	10.723	36.857	0	0	3	60
<i>Green Patents</i> $[t-3,t+3]$	10,633	14.496	49.307	0	0	4	80

Table 2 Baseline Results

This table presents the baseline results of whether previous whistleblowing experience affects corporate environmental performance. Our identification strategy relies on the firm's previous FCA lawsuits with three shocks induced by US appeals courts. We build an estimation window of [-5,+5] for each shock and perform stacked difference-in-difference regressions. The dependent variables, *Toxic Releases* and *Release Intensity*, are the log transformation of firms' toxic releases and release intensity obtained from the EPA TRI datasets, and the independent variable of interest is *Courts×Post×Cases*. *Courts* take the value of 1 if the plant is located in states which have been affected by the court rulings that increase the financial incentives for whistleblowers and zero otherwise. *Post* takes the value of 1 if the observation is in the period after the relevant court rulings and zero otherwise. *Cases* is the number of previous FCA lawsuits before each shock. All control variables are defined in Appendix A. The numbers reported in parentheses are t-statistics based on the standard errors clustered at the firm level. *, **, *** denote statistical significance at the 10%, 5% and 1%, respectively.

Dependent Variable	<i>Toxic Releases</i>			<i>Release Intensity</i>		
	(1)	(2)	(3)	(4)	(5)	(6)
<i>Courts×Post×Cases</i>	-0.157*** (-3.40)	-0.155*** (-3.57)	-0.155*** (-3.61)	-0.153*** (-4.58)	-0.141*** (-4.08)	-0.141*** (-4.14)
<i>Courts×Post</i>	0.030 (0.25)	0.030 (0.24)	0.035 (0.29)	-0.002 (-0.02)	0.019 (0.16)	0.025 (0.21)
<i>Institutional Investor</i>		-0.011 (-0.15)	-0.010 (-0.14)		0.000 (0.00)	0.001 (0.01)
<i>Tangibility</i>		0.303 (0.87)	0.303 (0.87)		-0.087 (-0.23)	-0.087 (-0.23)
<i>I/K</i>		-0.088 (-0.37)	-0.083 (-0.35)		-0.144 (-0.58)	-0.138 (-0.56)
<i>ROA</i>		1.044** (2.19)	1.055** (2.23)		-0.445 (-0.86)	-0.432 (-0.84)
<i>Leverage</i>		-0.394* (-1.68)	-0.396* (-1.69)		-0.321 (-1.30)	-0.324 (-1.31)
<i>Size</i>		0.311*** (4.01)	0.313*** (4.03)		-0.445*** (-5.30)	-0.443*** (-5.27)
<i>M/B</i>		0.038 (1.15)	0.037 (1.15)		0.048 (1.46)	0.048 (1.45)
<i>Cash/ Liability</i>		-0.067 (-0.68)	-0.067 (-0.68)		0.131 (1.26)	0.131 (1.25)
<i>Dividend</i>		0.171* (1.96)	0.169* (1.94)		0.161* (1.84)	0.159* (1.82)
<i>AltmanZ</i>		0.003 (0.18)	0.003 (0.16)		-0.014 (-0.71)	-0.014 (-0.73)
<i>Political Balance</i>			0.180 (0.92)			0.191 (0.94)
<i>GDP Growth</i>			-1.120 (-1.15)			-1.273 (-1.29)
Cohort-Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Cohort-Year FE	Yes	Yes	Yes	Yes	Yes	Yes

Table 2 Baseline Results (Continued)

# of Observations	10,633	10,633	10,633	10,633	10,633	10,633
Adjusted R ²	0.883	0.884	0.884	0.869	0.871	0.871

Table 3 Parallel Trend Analysis

This table presents the baseline results of whether previous whistleblowing experience affects corporate environmental performance. Our identification strategy relies on the firm's previous FCA lawsuits with three shocks induced by US appeals courts. We build an estimation window of $[-5,+5]$ for each shock and perform stacked difference-in-difference regressions. The dependent variables, *Toxic Releases* and *Release Intensity*, are the log transformation of firms' toxic releases and release intensity obtained from the EPA TRI datasets, and the independent variable of interest is $Courts \times Post \times Cases$. *Courts* take the value of 1 if the plant is located in states which have been affected by the court rulings that increase the financial incentives for whistleblowers and zero otherwise. *Post* takes the value of 1 if the observation is in the period after the relevant court rulings and zero otherwise. *Cases* is the number of previous FCA lawsuits before each shock. $Post_{t-k}$ takes the value of 1 if the observation is k years before the relevant court rulings and zero otherwise; $Post_{t+k}$ takes the value of 1 if the observation is k years after the relevant court rulings and zero otherwise. All control variables are defined in Appendix A. The numbers reported in parentheses are t-statistics based on the standard errors clustered at the firm level. *, **, *** denote statistical significance at the 10%, 5% and 1%, respectively.

Dependent Variable	<i>Toxic Releases</i>			<i>Release Intensity</i>		
	(1)	(2)	(3)	(4)	(5)	(6)
<i>Courts</i> \times <i>Post</i> _{t-5} \times <i>Cases</i>	0.058 (1.43)	0.055 (1.31)	0.055 (1.29)	0.057 (1.28)	0.067 (1.36)	0.062 (0.86)
<i>Courts</i> \times <i>Post</i> _{t-4} \times <i>Cases</i>	-0.021 (-0.77)	-0.025 (-0.83)	-0.025 (-0.80)	-0.022 (-0.63)	-0.012 (-0.34)	-0.017 (-0.29)
<i>Courts</i> \times <i>Post</i> _{t-3} \times <i>Cases</i>	-0.024 (-1.19)	-0.024 (-1.33)	-0.024 (-1.29)	-0.011 (-0.66)	-0.004 (-0.35)	-0.009 (-0.24)
<i>Courts</i> \times <i>Post</i> _{t-2} \times <i>Cases</i>	-0.017 (-0.90)	-0.016 (-0.88)	-0.016 (-0.93)	-0.006 (-0.34)	-0.003 (-0.26)	-0.009 (-0.25)
<i>Courts</i> \times <i>Post</i> _{t+1} \times <i>Cases</i>	-0.160*** (-2.63)	-0.152*** (-2.85)	-0.154*** (-2.90)	-0.131*** (-3.53)	-0.127*** (-3.26)	-0.133*** (-3.68)
<i>Courts</i> \times <i>Post</i> _{t+2} \times <i>Cases</i>	-0.153*** (-3.12)	-0.156*** (-3.33)	-0.155*** (-3.28)	-0.153*** (-3.93)	-0.128*** (-4.58)	-0.131*** (-3.06)
<i>Courts</i> \times <i>Post</i> _{t+3} \times <i>Cases</i>	-0.150*** (-5.48)	-0.152*** (-5.65)	-0.153*** (-5.77)	-0.161*** (-8.01)	-0.139*** (-6.74)	-0.144*** (-4.03)
<i>Courts</i> \times <i>Post</i> _{t+4} \times <i>Cases</i>	-0.164*** (-2.86)	-0.164*** (-2.95)	-0.161*** (-3.00)	-0.160** (-2.31)	-0.138* (-1.82)	-0.137* (-1.92)
<i>Courts</i> \times <i>Post</i> _{t+5} \times <i>Cases</i>	-0.185*** (-5.15)	-0.186*** (-4.88)	-0.182*** (-4.73)	-0.175*** (-4.14)	-0.166*** (-3.39)	-0.163*** (-3.34)
<i>Courts</i> \times <i>Post</i>	0.030 (0.25)	0.030 (0.24)	0.035 (0.29)	-0.002 (-0.02)	0.019 (0.16)	0.025 (0.21)
Firm-level Controls	No	Yes	Yes	No	Yes	Yes
State-level Controls	No	No	Yes	No	No	Yes
Cohort-Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Cohort-Year FE	Yes	Yes	Yes	Yes	Yes	Yes
# of Observations	10,633	10,633	10,633	10,633	10,633	10,633
Adjusted R ²	0.883	0.884	0.884	0.869	0.871	0.871

Table 4 Balancing the Covariates

This table presents the robustness test of the baseline results in Table 2 using propensity score matching (Panel A) and Entropy Balancing (Panel B). For the PSM method, we use a logit model to estimate the probability of being a treated observation and match the treated observation to a control observation using a caliber of 0.05 without replacement; for the EB method, we balance the covariates at their maximum orders (2nd or 3rd order). Our identification strategy relies on the firm's previous FCA lawsuits with three shocks induced by US appeals courts. We build an estimation window of [-5,+5] for each shock and perform stacked difference-in-difference regressions. The dependent variables, *Toxic Releases* and *Release Intensity*, are the log transformation of firms' toxic releases and release intensity obtained from the EPA TRI datasets, and the independent variable of interest is *Courts*×*Post*×*Cases*. *Courts* take the value of 1 if the plant is located in states which have been affected by the court rulings that increase the financial incentives for whistleblowers and zero otherwise. *Post* takes the value of 1 if the observation is in the period after the relevant court rulings and zero otherwise. *Cases* is the number of previous FCA lawsuits before each shock. All control variables are defined in Appendix A. The numbers reported in parentheses are t-statistics based on the standard errors clustered at the firm level. *, **, *** denote statistical significance at the 10%, 5% and 1%, respectively.

Panel A. Propensity Score Matching						
Dependent Variable	<i>Toxic Releases</i>			<i>Release Intensity</i>		
	(1)	(2)	(3)	(4)	(5)	(6)
<i>Courts</i> × <i>Post</i> × <i>Cases</i>	-0.182*** (-3.70)	-0.175*** (-3.23)	-0.174*** (-3.22)	-0.182*** (-3.60)	-0.148*** (-2.62)	-0.148** (-2.56)
<i>Courts</i> × <i>Post</i>	-0.213 (-1.45)	-0.196 (-1.35)	-0.185 (-1.27)	-0.230 (-1.63)	-0.208 (-1.46)	-0.196 (-1.37)
Firm-level Controls	No	Yes	Yes	No	Yes	Yes
State-level Controls	No	No	Yes	No	No	Yes
Cohort-Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Cohort-Year FE	Yes	Yes	Yes	Yes	Yes	Yes
# of Observations	5,208	5,208	5,208	5,208	5,208	5,208
Adjusted R ²	0.873	0.875	0.875	0.862	0.864	0.864
Panel B. Entropy Balancing						
Dependent Variable	<i>Toxic Releases</i>			<i>Release Intensity</i>		
	(1)	(2)	(3)	(4)	(5)	(6)
<i>Courts</i> × <i>Post</i> × <i>Cases</i>	-0.221*** (-4.62)	-0.213*** (-4.33)	-0.214*** (-4.33)	-0.223*** (-4.82)	-0.191*** (-3.94)	-0.192*** (-3.88)
<i>Courts</i> × <i>Post</i>	-0.037 (-0.27)	-0.035 (-0.27)	-0.026 (-0.20)	-0.037 (-0.27)	-0.035 (-0.27)	-0.026 (-0.20)
Firm-level Controls	No	Yes	Yes	No	Yes	Yes
State-level Controls	No	No	Yes	No	No	Yes
Cohort-Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Cohort-Year FE	Yes	Yes	Yes	Yes	Yes	Yes
# of Observations	8,549	8,549	8,549	8,549	8,549	8,549
Adjusted R ²	0.877	0.879	0.879	0.865	0.867	0.867

Table 5 Control for Internal Whistleblowing

This table presents the baseline results with additional controls for internal whistleblowing. We follow Doyle et al. (2007), Call et al. (2016) and Heese et al. (2021) and use two internal control weakness measures *ICW_SOX* and *ICW_Doyle*, to proxy for the effectiveness of internal whistleblowing. Our identification strategy relies on the firm's previous FCA lawsuits with three shocks induced by US appeals courts. We build an estimation window of [-5,+5] for each shock and perform stacked difference-in-difference regressions. The dependent variables, *Toxic Releases* and *Release Intensity*, are the log transformation of firms' toxic releases and release intensity obtained from the EPA TRI datasets, and the independent variable of interest is *Courts×Post×Cases*. *Courts* take the value of 1 if the plant is located in states which have been affected by the court rulings that increase the financial incentives for whistleblowers and zero otherwise. *Post* takes the value of 1 if the observation is in the period after the relevant court rulings and zero otherwise. *Cases* is the number of previous FCA lawsuits before each shock. All control variables are defined in Appendix A. The numbers reported in parentheses are t-statistics based on the standard errors clustered at the firm level. *, **, *** denote statistical significance at the 10%, 5% and 1%, respectively.

Panel A. Control internal whistleblowing using the SOX Section 404 measure						
Dependent Variable	<i>Toxic Releases</i>			<i>Release Intensity</i>		
	(1)	(2)	(3)	(4)	(5)	(6)
<i>Courts×Post×Cases</i>	-0.123*** (-2.67)	-0.121*** (-2.97)	-0.121*** (-2.97)	-0.128*** (-3.34)	-0.120*** (-3.14)	-0.120*** (-3.13)
<i>Courts×Post</i>	-0.176 (-0.96)	-0.164 (-0.94)	-0.169 (-0.97)	-0.249 (-1.42)	-0.208 (-1.20)	-0.216 (-1.25)
<i>ICW_SOX</i>	-0.082 (-0.57)	-0.046 (-0.35)	-0.045 (-0.34)	-0.096 (-0.74)	-0.110 (-0.82)	-0.109 (-0.81)
Firm-level Controls	No	Yes	Yes	No	Yes	Yes
State-level Controls	No	No	Yes	No	No	Yes
Cohort-Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Cohort-Year FE	Yes	Yes	Yes	Yes	Yes	Yes
# of Observations	2,933	2,933	2,933	2,933	2,933	2,933
Adjusted R ²	0.916	0.919	0.919	0.909	0.909	0.909
Panel B. Control internal whistleblowing using Doyle et al. (2007) measure						
Dependent Variable	<i>Toxic Releases</i>			<i>Release Intensity</i>		
	(1)	(2)	(3)	(4)	(5)	(6)
<i>Courts×Post×Cases</i>	-0.158*** (-3.36)	-0.155*** (-3.57)	-0.155*** (-3.61)	-0.152*** (-4.58)	-0.141*** (-4.07)	-0.141*** (-4.14)
<i>Courts×Post</i>	0.023 (0.19)	0.027 (0.23)	0.033 (0.27)	-0.005 (-0.05)	0.015 (0.12)	0.021 (0.18)
<i>ICW_Doyle</i>	-0.076*** (-2.96)	0.005 (0.23)	0.005 (0.24)	0.024 (0.99)	-0.028 (-1.15)	-0.028 (-1.14)
Firm-level Controls	No	Yes	Yes	No	Yes	Yes
State-level Controls	No	No	Yes	No	No	Yes
Cohort-Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Cohort-Year FE	Yes	Yes	Yes	Yes	Yes	Yes
# of Observations	10,605	10,605	10,605	10,605	10,605	10,605
Adjusted R ²	0.883	0.884	0.884	0.870	0.872	0.872

Table 6 Role of different types of whistleblowers I

This table presents the results of the role of monitoring. In panel A, we split the observations into two groups based on the five-year ex-ante average number of rank-and-file stock options (*RF_Options*). *RF_Options* is the proportion of stock options granted to rank-and-file employees. In panel B, we split the observations based on a regulated industry classification in Lee et al.'s (2024) study. Regulated industry (*Regulated Industry*) is a dummy variable that equals one if the firm is operating in a regulated industry facing higher threats of whistleblowing. Our identification strategy relies on the firm's previous FCA lawsuits with three shocks induced by US appeals courts. We build an estimation window of [-5,+5] for each shock and perform stacked difference-in-difference regressions. The dependent variables, *Toxic Releases* and *Release Intensity*, are the log transformation of firms' toxic releases and release intensity obtained from the EPA TRI datasets, and the independent variable of interest is *Courts×Post×Cases*. *Courts* take the value of 1 if the plant is located in states which have been affected by the court rulings that increase the financial incentives for whistleblowers and zero otherwise. *Post* takes the value of 1 if the observation is in the period after the relevant court rulings and zero otherwise. *Cases* is the number of previous FCA lawsuits before each shock. All variables are defined in Appendix A. The numbers reported in parentheses are t-statistics based on the standard errors clustered at the firm level. *, **, *** denote statistical significance at the 10%, 5% and 1%, respectively.

Panel A. The role of employees				
Dependent Variable	<i>Toxic Releases</i>		<i>Release Intensity</i>	
	(1)	(2)	(3)	(4)
Variables	Fewer options	More options	Fewer options	More options
<i>Courts×Post×Cases</i>	-0.674*** (-6.82)	-0.122*** (-4.36)	-0.636*** (-5.81)	-0.107*** (-4.08)
<i>Courts×Post</i>	0.023 (0.13)	0.040 (0.22)	0.006 (0.03)	0.032 (0.18)
Firm-level Controls	Yes	Yes	Yes	Yes
State-level Controls	Yes	Yes	Yes	Yes
Cohort-Firm FE	Yes	Yes	Yes	Yes
Cohort-Year FE	Yes	Yes	Yes	Yes
[P-Value Test Differences]	0.000		0.000	
# of Observations	4,762	4,741	4,762	4,741
Adjusted R ²	0.860	0.900	0.837	0.897
Panel B. The role of industry regulators				
Dependent Variable	<i>Toxic Releases</i>		<i>Release Intensity</i>	
	(1)	(2)	(3)	(4)
Variables	More regulators	Fewer regulators	More regulators	Fewer regulators
<i>Courts×Post×Cases</i>	-0.635*** (-5.39)	-0.129*** (-5.01)	-0.559*** (-4.90)	-0.116*** (-6.26)
<i>Courts×Post</i>	0.140 (0.61)	0.033 (0.24)	0.091 (0.40)	0.025 (0.18)
Firm-level Controls	Yes	Yes	Yes	Yes
State-level Controls	Yes	Yes	Yes	Yes
Cohort-Firm FE	Yes	Yes	Yes	Yes
Cohort-Year FE	Yes	Yes	Yes	Yes
[P-Value Test Differences]	0.000		0.000	
# of Observations	1,498	9,135	1,498	9,135
Adjusted R ²	0.885	0.882	0.852	0.872

Table 7 Role of different types of whistleblowers II

This table presents the results of the role of monitoring. In both panels, we split the observations into two groups based on the five-year ex-ante average number of different market whistleblower proxies (*Short-Seller Coverage* and *Analyst Coverage*). *Short-seller Coverage* is the proportion of stocks held in the short position. *Analyst Coverage* is the number of financial analysts following the firm. Our identification strategy relies on the firm's previous FCA lawsuits with three shocks induced by US appeals courts. We build an estimation window of [-5,+5] for each shock and perform stacked difference-in-difference regressions. The dependent variables, *Toxic Releases* and *Release Intensity*, are the log transformation of firms' toxic releases and release intensity obtained from the EPA TRI datasets, and the independent variable of interest is *Courts×Post×Cases*. *Courts* take the value of 1 if the plant is located in states which have been affected by the court rulings that increase the financial incentives for whistleblowers and zero otherwise. *Post* takes the value of 1 if the observation is in the period after the relevant court rulings and zero otherwise. *Cases* is the number of previous FCA lawsuits before each shock. All variables are defined in Appendix A. The numbers reported in parentheses are t-statistics based on the standard errors clustered at the firm level. *, **, *** denote statistical significance at the 10%, 5% and 1%, respectively.

Panel A. The role of short sellers				
Dependent Variable	<i>Toxic Releases</i>		<i>Release Intensity</i>	
	(1)	(2)	(3)	(4)
Variables	More short sellers	Fewer short sellers	More short sellers	Fewer short sellers
<i>Courts×Post×Cases</i>	-0.192*** (-3.86)	-0.126*** (-2.81)	-0.138*** (-2.68)	-0.130*** (-2.92)
<i>Courts×Post</i>	0.098 (0.65)	-0.051 (-0.26)	0.091 (0.62)	-0.051 (-0.26)
Firm-level Controls	Yes	Yes	Yes	Yes
State-level Controls	Yes	Yes	Yes	Yes
Cohort-Firm FE	Yes	Yes	Yes	Yes
Cohort-Year FE	Yes	Yes	Yes	Yes
[P-Value Test Differences]	0.370		0.708	
# of Observations	4,771	4,732	4,771	4,732
Adjusted R ²	0.902	0.845	0.897	0.835
Panel B. The role of financial analysts				
Dependent Variable	<i>Toxic Releases</i>		<i>Release Intensity</i>	
	(1)	(2)	(3)	(4)
Variables	More analysts	Fewer analysts	More analysts	Fewer analysts
<i>Courts×Post×Cases</i>	-0.161*** (-3.78)	-0.003 (-0.01)	-0.149*** (-4.25)	0.085 (0.31)
<i>Courts×Post</i>	0.068 (0.42)	0.029 (0.15)	0.057 (0.36)	0.006 (0.03)
Firm-level Controls	Yes	Yes	Yes	Yes
State-level Controls	Yes	Yes	Yes	Yes
Cohort-Firm FE	Yes	Yes	Yes	Yes
Cohort-Year FE	Yes	Yes	Yes	Yes
[P-Value Test Differences]	0.283		0.196	
# of Observations	4,746	4,757	4,746	4,757
Adjusted R ²	0.893	0.863	0.883	0.847

Table 8 Role of different types of whistleblowers III

This table presents the results of the role of monitoring. In both panels, we split the observations into two groups based on the five-year ex-ante average number of institutional ownership (*Institutional Investor*). *Institutional Investor* is the percentage of shares held by institutional investors. Our identification strategy relies on the firm's previous FCA lawsuits with three shocks induced by US appeals courts. We build an estimation window of [-5,+5] for each shock and perform stacked difference-in-difference regressions. The dependent variables, *Toxic Releases* and *Release Intensity*, are the log transformation of firms' toxic releases and release intensity obtained from the EPA TRI datasets, and the independent variable of interest is *Courts×Post×Cases*. *Courts* take the value of 1 if the plant is located in states which have been affected by the court rulings that increase the financial incentives for whistleblowers and zero otherwise. *Post* takes the value of 1 if the observation is in the period after the relevant court rulings and zero otherwise. *Cases* is the number of previous FCA lawsuits before each shock. All variables are defined in Appendix A. The numbers reported in parentheses are t-statistics based on the standard errors clustered at the firm level. *, **, *** denote statistical significance at the 10%, 5% and 1%, respectively.

Dependent Variable	<i>Toxic Releases</i>		<i>Release Intensity</i>	
	(1)	(2)	(3)	(4)
Variables	More investors	Fewer investors	More investors	Fewer investors
<i>Courts×Post×Cases</i>	-0.238*** (-2.68)	-0.109*** (-4.96)	-0.187** (-2.03)	-0.110*** (-4.95)
<i>Courts×Post</i>	0.013 (0.08)	0.052 (0.29)	-0.013 (-0.08)	0.047 (0.26)
Firm-level Controls	Yes	Yes	Yes	Yes
State-level Controls	Yes	Yes	Yes	Yes
Cohort-Firm FE	Yes	Yes	Yes	Yes
Cohort-Year FE	Yes	Yes	Yes	Yes
[P-Value Test Differences]		0.073		0.272
# of Observations	4,751	4,752	4,751	4,752
Adjusted R ²	0.882	0.880	0.869	0.865

Table 9 Role of corporate governance I

This table presents the results of the role of different types of whistleblowers. In both panels, we split the observations into two groups based on the five-year ex-ante average number of different market competition proxies (*HHI* and *TSIMM*) obtained from Hoberg and Phillip (2016). *HHI* is the HH Index to proxy for market competition. *TSIMM* is a product similarity index to proxy for market competition. Our identification strategy relies on the firm’s previous FCA lawsuits with three shocks induced by US appeals courts. We build an estimation window of [-5,+5] for each shock and perform stacked difference-in-difference regressions. The dependent variables, *Toxic Releases* and *Release Intensity*, are the log transformation of firms’ toxic releases and release intensity obtained from the EPA TRI datasets, and the independent variable of interest is *Courts×Post×Cases*. *Courts* take the value of 1 if the plant is located in states which have been affected by the court rulings that increase the financial incentives for whistleblowers and zero otherwise. *Post* takes the value of 1 if the observation is in the period after the relevant court rulings and zero otherwise. *Cases* is the number of previous FCA lawsuits before each shock. All variables are defined in Appendix A. The numbers reported in parentheses are t-statistics based on the standard errors clustered at the firm level. *, **, *** denote statistical significance at the 10%, 5% and 1%, respectively.

Panel A. The role of market governance (<i>HHI</i>)				
Dependent Variable	<i>Toxic Releases</i>		<i>Release Intensity</i>	
	(1)	(2)	(3)	(4)
Variables	High	Low	High	Low
<i>Courts×Post×Cases</i>	-0.154*** (-3.15)	0.126 (0.61)	-0.145*** (-3.62)	0.243 (1.14)
<i>Courts×Post</i>	0.003 (0.02)	0.051 (0.33)	0.031 (0.15)	0.004 (0.03)
Firm-level Controls	Yes	Yes	Yes	Yes
State-level Controls	Yes	Yes	Yes	Yes
Cohort-Firm FE	Yes	Yes	Yes	Yes
Cohort-Year FE	Yes	Yes	Yes	Yes
[P-Value Test Differences]	0.056		0.009	
# of Observations	4,638	4,629	4,638	4,629
Adjusted R ²	0.892	0.861	0.881	0.849
Panel B. The role of market governance (<i>TSIMM</i>)				
Dependent Variable	<i>Toxic Releases</i>		<i>Release Intensity</i>	
	(1)	(2)	(3)	(4)
Variables	High	Low	High	Low
<i>Courts×Post×Cases</i>	-0.170*** (-3.43)	0.058 (0.20)	-0.159*** (-3.99)	0.156 (0.54)
<i>Courts×Post</i>	0.024 (0.13)	0.063 (0.37)	0.047 (0.25)	0.028 (0.17)
Firm-level Controls	Yes	Yes	Yes	Yes
State-level Controls	Yes	Yes	Yes	Yes
Cohort-Firm FE	Yes	Yes	Yes	Yes
Cohort-Year FE	Yes	Yes	Yes	Yes
[P-Value Test Differences]	0.273		0.175	
# of Observations	4,618	4,649	4,618	4,649
Adjusted R ²	0.900	0.855	0.885	0.846

Table 10 Role of corporate governance II

This table presents the results of the role of employee whistleblowers. We split the observations into two groups based on whether firms have a sustainable committee (*Sustainable Committee*) before the whistleblowing shocks. *Sustainable Committee* is one of the committees related to the environment and corporate social responsibility. Our identification strategy relies on the firm's previous FCA lawsuits with three shocks induced by US appeals courts. We build an estimation window of [-5,+5] for each shock and perform stacked difference-in-difference regressions. The dependent variables, *Toxic Releases* and *Release Intensity*, are the log transformation of firms' toxic releases and release intensity obtained from the EPA TRI datasets, and the independent variable of interest is *Courts*×*Post*×*Cases*. *Courts* take the value of 1 if the plant is located in states which have been affected by the court rulings that increase the financial incentives for whistleblowers and zero otherwise. *Post* takes the value of 1 if the observation is in the period after the relevant court rulings and zero otherwise. *Cases* is the number of previous FCA lawsuits before each shock. All variables are defined in Appendix A. The numbers reported in parentheses are t-statistics based on the standard errors clustered at the firm level. *, **, *** denote statistical significance at the 10%, 5% and 1%, respectively.

The role of internal governance (<i>Sustainable Committee</i>)				
Dependent Variable	<i>Toxic Releases</i>		<i>Release Intensity</i>	
	(1)	(2)	(3)	(4)
Variables	With committee	Without committee	With committee	Without committee
<i>Courts</i> × <i>Post</i> × <i>Cases</i>	-0.754*** (-8.76)	-0.130*** (-4.86)	-0.718*** (-9.10)	-0.115*** (-6.08)
<i>Courts</i> × <i>Post</i>	0.513 (1.58)	-0.012 (-0.09)	0.359 (1.11)	-0.016 (-0.13)
Firm-level Controls	No	Yes	No	Yes
State-level Controls	No	Yes	No	Yes
Cohort-Firm FE	Yes	Yes	Yes	Yes
Cohort-Year FE	Yes	Yes	Yes	Yes
[P-Value Test Differences]	0.000		0.000	
# of Observations	1,055	8,448	1,055	8,448
Adjusted R ²	0.891	0.878	0.895	0.860

Table 11 Additional Test of Green Patents

This table presents the additional tests for baseline results. We test whether firms with previous FCA lawsuits invest in green patents after the shocks. The dependent variable, *Green Patents* is the number of green patents based on the classification of CPC, and the original patent data is obtained from the USPTO. In the first two columns, we measure *Green Patents* at two years and three years after the shock (He and Tian, 2013). In the last two columns, we measure *Green Patents* in time windows two years and three years before and after the shock (Jing et al., 2023). Our identification strategy relies on the firm's previous FCA lawsuits with three shocks induced by US appeals courts. We build an estimation window of [-5,+5] for each shock and perform stacked difference-in-difference regressions. The independent variable of interest is *Courts*×*Post*×*Cases*. *Courts* take the value of 1 if the plant is located in states which have been affected by the court rulings that increase the financial incentives for whistleblowers and zero otherwise. *Post* takes the value of 1 if the observation is in the period after the relevant court rulings and zero otherwise. *Cases* is the number of previous FCA lawsuits before each shock. All variables are defined in Appendix A. All control variables are defined in Appendix A. The numbers reported in parentheses are t-statistics based on the standard errors clustered at the firm level. *, **, *** denote statistical significance at the 10%, 5% and 1%, respectively.

Dependent Variable	<i>Green Patents</i>	<i>Green Patents</i>	<i>Green Patents</i>	<i>Green Patents</i>
Time period	t+1	t+2	[t-2, t+2]	[t-3, t+3]
Variables	(1)	(2)	(3)	(4)
<i>Courts</i> × <i>Post</i> × <i>Cases</i>	0.246*** (7.65)	0.144** (2.18)	1.978*** (4.13)	1.910*** (3.36)
<i>Courts</i> × <i>Post</i>	0.021 (0.11)	0.050 (0.26)	0.783 (0.66)	0.918 (0.59)
Firm-level Controls	Yes	Yes	Yes	Yes
State-level Controls	Yes	Yes	Yes	Yes
Cohort-Firm FE	Yes	Yes	Yes	Yes
Cohort-Year FE	Yes	Yes	Yes	Yes
# of Observations	9,039	8,785	8,288	7,838
Adjusted R ²	0.855	0.867	0.951	0.960

Online Appendix A. Variable Definition

Variables	Definition (Compustat data in parentheses)	Source
Pollution variables		
<i>Toxic Releases</i>	The log transformation of firm-level total toxic releases following Hsu et al. (2023)	<i>EPA TRI</i>
<i>Release Intensity</i>	The log transformation of firm-level total toxic releases divided by total sales (sale)	<i>EPA TRI</i>
<i>State Emissions</i>	The log transformation of state-level total toxic releases	<i>EPA TRI</i>
<i>State Onetime Release</i>	The log transformation of state-level one-time releases	<i>EPA TRI</i>
False Claims Act Variables		
<i>Cases</i>	The number of previous FCA lawsuits before each shock	<i>US DOJ</i>
<i>Cases_pseudo</i>	The number of randomly generated pseudo FCA lawsuits before each shock (same statistical distribution as real lawsuits)	<i>Own calculation</i>
<i>Courts</i>	An indicator takes the value of 1 if the plant is located in states which have been affected by the court rulings that increase the financial incentives for whistleblowers and zero otherwise.	<i>Own calculation</i>
<i>Post</i>	An indicator takes the value of 1 if the observation is in the period after the relevant court rulings and zero otherwise	<i>Own calculation</i>
<i>Post_{t+k}</i>	An indicator takes the value of 1 if the observation is k years after the relevant court rulings and zero otherwise	<i>Own calculation</i>
<i>Post_{t-k}</i>	An indicator takes the value of 1 if the observation is k years before the relevant court rulings and zero otherwise	<i>Own calculation</i>
Firm Characteristics		
<i>Institutional Investor</i>	Percentage of shares held by institutional investors	<i>13F</i>
<i>Tangibility</i>	Tangible assets (ppent) divided by total assets (at)	<i>COMPUSTAT</i>
<i>I/K</i>	Capital expenditures (capx) divided by tangible assets (ppent)	<i>COMPUSTAT</i>
<i>ROA</i>	Return on assets (ni/lag(at))	<i>COMPUSTAT</i>
<i>Leverage</i>	Leverage for the firm ((dlc+dltt)/at)	<i>COMPUSTAT</i>
<i>Size</i>	Log transformation of total assets (at)	<i>COMPUSTAT</i>
<i>M/B</i>	Natural log of market value (csho*prcc_f) divided by book value (ceq)	<i>COMPUSTAT</i>
<i>Cash/Liability</i>	Cash (ch) to liability (lt) ratio	<i>COMPUSTAT</i>
<i>Dividend</i>	Leverage for a firm ((dlc+dltt)/at)	<i>COMPUSTAT</i>
<i>AltmanZ</i>	Altman's Z-score calculated as $3.3*(\text{ebit}/\text{at}) + 0.99*(\text{sale}/\text{at}) + 0.6*(\text{prcc}_f*\text{csho}/\text{lt}) + 1.2*(\text{wcap}/\text{at}) + 1.4*(\text{re}/\text{at})$	<i>COMPUSTAT</i>
<i>Firm Age</i>	The number of years listed on the stock exchange	<i>COMPUSTAT</i>
<i>ICW_SOX</i>	The number of internal control weaknesses disclosed in SOX Section 404	<i>Audit Analytics</i>
<i>ICW_Doyle</i>	The fitted values obtained from the following model, as estimated by Doyle et al. (2007): Internal Control Weakness = $b_0 + b_1*\text{Size} + b_2*\log(\text{Firm Age}) + b_3*\text{Losses} + b_4*\text{Segments} + b_5*\text{Foreign Trans} + b_6*\text{Extreme SG} + b_7*\text{Restructure} + \text{error}$. Size and Firm Age are already defined above, Losses is an indicator variable equal to 1 if earnings before extraordinary items in the two most recent years sum to less than zero, and 0 otherwise, Segments is the log of the number of operating and geographic segments reported by the Compustat Segments database, Foreign Trans is an indicator variable equal to 1 if the firm has nonzero foreign translation, and 0 otherwise, Extreme SG is an indicator variable equal to 1 if year-over-year industry-adjusted sales growth falls into the top quintile, and 0 otherwise, Restructure is the aggregate restructuring charge in the two most recent years, scaled by the firm's market capitalization	<i>Own Calculation</i>
Cross-sectional Analysis & Additional Analysis Variables		

<i>Green Patents</i>	The number of green patents filed in a year	<i>KPSS (2017)</i>
<i>Regulated Industry</i>	A dummy variable equals one if the firm is operating in a regulated industry facing higher threats of whistleblowing.	<i>Lee et al. (2024)</i>
<i>HHI</i>	The HH Index to proxy for market competition	<i>Hoberg and Phillip (2016)</i>
<i>TSIMM</i>	The product similarity to proxy for market competition	<i>Hoberg and Phillip (2016)</i>
<i>Sustainable Committee</i>	The number of committees related to the environment and corporate social responsibility.	<i>BoardEX</i>
<i>Short-seller Coverage</i>	The proportion of stocks held in the short position	<i>COMPUSTAT</i>
<i>Analyst Coverage</i>	The number of financial analysts following the firm	<i>I/B/E/S</i>
<i>RF_Options</i>	The proportion of stock options granted to rank-and-file employees.	<i>EXECUCOMP</i>
<i>Other variables</i>		
<i>GDP per Capita</i>	State-level Growth Domestic production per capita	<i>COMPUSTAT</i>
<i>GDP Growth</i>	State-level GDP Growth	<i>COMPUSTAT</i>
<i>Political Balance</i>	State political balance	<i>COMPUSTAT</i>

Online Appendix B.

Table OA1 Hazard Model of Changes in Whistleblower's Incentives

This table presents the results from a Cox proportional hazard model. Following previous literature (e.g., Acharya et al., 2014), we define the change in whistleblowers' incentives in a state as a "failure event". The observation of a state is excluded from the sample when a state is subject to a relative court ruling which increases the financial incentives for whistleblowers. All independent variables are of interest and are measured as of year t-1. All the variables are defined in Appendix A. The numbers reported in parentheses are t-statistics based on the standard errors clustered at the state level. * , ** , *** denote statistical significance at the 10%, 5% and 1%, respectively.

Variables	(1)	(2)	(3)
<i>State Emissions</i>	-0.224 (-1.19)	-0.246 (-1.30)	-0.305 (-1.46)
<i>State Onetime Release</i>	0.006 (0.08)	0.007 (0.10)	0.033 (0.43)
<i>State False Claims Acts</i>		0.695 (1.18)	0.863 (1.30)
<i>GDP per Capita</i>			-24.146 (-0.70)
<i>GDP Growth</i>			5.214 (0.65)
<i>Political Balance</i>			0.621 (0.79)
# of observations	791	791	791
Year FE	Yes	Yes	Yes

Table OA2 Robustness Checks I (different time windows)

This table presents the robustness test of the baseline results in Table 2 using a shorter time window [-3,+3] (Panel A) and a sample without the period of the global financial crisis (Panel B). Our identification strategy relies on the firm's previous FCA lawsuits with three shocks induced by US appeals courts. We build an estimation window of [-5,+5] for each shock and perform stacked difference-in-difference regressions. The dependent variables, *Toxic Releases* and *Release Intensity*, are the log transformation of firms' toxic releases and release intensity obtained from the EPA TRI datasets, and the independent variable of interest is *Courts×Post×Cases*. *Courts* take the value of 1 if the plant is located in states which have been affected by the court rulings that increase the financial incentives for whistleblowers and zero otherwise. *Post* takes the value of 1 if the observation is in the period after the relevant court rulings and zero otherwise. *Cases* is the number of previous FCA lawsuits before each shock. All control variables are defined in Appendix A. The numbers reported in parentheses are t-statistics based on the standard errors clustered at the firm level. *, **, *** denote statistical significance at the 10%, 5% and 1%, respectively.

Panel A. Shorter Time Window						
Dependent Variable	<i>Toxic Releases</i>			<i>Release Intensity</i>		
	(1)	(2)	(3)	(4)	(5)	(6)
<i>Courts×Post×Cases</i>	-0.174*** (-6.99)	-0.171*** (-6.79)	-0.173*** (-6.82)	-0.164*** (-6.25)	-0.143*** (-4.75)	-0.144*** (-4.82)
<i>Courts×Post</i>	-0.009 (-0.08)	-0.018 (-0.16)	-0.015 (-0.13)	-0.048 (-0.45)	-0.029 (-0.26)	-0.026 (-0.24)
Firm-level Controls	No	Yes	Yes	No	Yes	Yes
State-level Controls	No	No	Yes	No	No	Yes
Cohort-Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Cohort-Year FE	Yes	Yes	Yes	Yes	Yes	Yes
# of Observations	6,960	6,960	6,960	6,960	6,960	6,960
Adjusted R ²	0.908	0.909	0.909	0.898	0.899	0.899
Panel B. Drop the period with the global financial crisis						
Dependent Variable	<i>Toxic Releases</i>			<i>Release Intensity</i>		
	(1)	(2)	(3)	(4)	(5)	(6)
<i>Courts×Post×Cases</i>	-0.216*** (-4.79)	-0.218*** (-4.72)	-0.216*** (-4.78)	-0.183*** (-4.79)	-0.165*** (-3.57)	-0.164*** (-3.61)
<i>Courts×Post</i>	0.128 (0.83)	0.119 (0.77)	0.133 (0.85)	0.114 (0.76)	0.128 (0.83)	0.142 (0.93)
Firm-level Controls	No	Yes	Yes	No	Yes	Yes
State-level Controls	No	No	Yes	No	No	Yes
Cohort-Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Cohort-Year FE	Yes	Yes	Yes	Yes	Yes	Yes
# of Observations	7,700	7,700	7,700	7,700	7,700	7,700
Adjusted R ²	0.868	0.869	0.869	0.851	0.855	0.855

Table OA3 Robustness Checks II

This table presents the robustness test of the baseline results in Table 2 using a sample without relocated firms (Panel A), falsification tests with pseudo FCA Cases (Panel B) and a baseline test with state-by-year FE (Panel C). Variable definitions are reported in Appendix A. The numbers reported in parentheses are t-statistics based on the standard errors clustered at the firm level. *, **, *** denote statistical significance at the 10%, 5% and 1%, respectively.

Panel A. Drop Relocated Firms						
Dependent Variable	<i>Toxic Releases</i>			<i>Release Intensity</i>		
Variables	(1)	(2)	(3)	(4)	(5)	(6)
<i>Courts×Post×Cases</i>	-0.141*** (-3.05)	-0.140*** (-3.21)	-0.141*** (-3.21)	-0.148*** (-3.88)	-0.140*** (-3.42)	-0.141*** (-3.43)
<i>Courts×Post</i>	0.078 (0.47)	0.094 (0.59)	0.096 (0.61)	0.046 (0.29)	0.069 (0.44)	0.071 (0.45)
Firm-level Controls	No	Yes	Yes	No	Yes	Yes
State-level Controls	No	No	Yes	No	No	Yes
Cohort-Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Cohort-Year FE	Yes	Yes	Yes	Yes	Yes	Yes
# of Observations	7548	7548	7548	7548	7548	7548
Adjusted R ²	0.891	0.893	0.893	0.879	0.880	0.880
Panel B. Falsification test with pseudo FCA Cases						
Dependent Variable	<i>Toxic Releases</i>			<i>Release Intensity</i>		
Variables	(1)	(2)	(3)	(4)	(5)	(6)
<i>Courts×Post×Cases_pseudo</i>	0.061 (0.64)	0.051 (0.54)	0.049 (0.52)	0.029 (0.32)	0.030 (0.34)	0.028 (0.32)
<i>Courts×Post</i>	-0.007 (-0.05)	-0.004 (-0.03)	0.002 (0.02)	-0.030 (-0.25)	-0.008 (-0.06)	-0.001 (-0.01)
Firm-level Controls	No	Yes	Yes	No	Yes	Yes
State-level Controls	No	No	Yes	No	No	Yes
Cohort-Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Cohort-Year FE	Yes	Yes	Yes	Yes	Yes	Yes
# of Observations	10,633	10,633	10,633	10,633	10,633	10,633
Adjusted R ²	0.883	0.884	0.884	0.869	0.871	0.871
Panel C. Baseline test with State-by-year FE						
Dependent Variable	<i>Toxic Releases</i>		<i>Release Intensity</i>			
Variables	(1)	(2)	(3)	(4)		
<i>Courts×Post×Case</i>	-0.114** (-2.43)	-0.117*** (-2.60)	-0.115*** (-2.97)	-0.113*** (-2.67)		
Firm-level Controls	No	Yes	No	Yes		
Cohort-Firm FE	Yes	Yes	Yes	Yes		
Cohort-State-by-Year FE	Yes	Yes	Yes	Yes		
# of Observations	9,098	9,098	9,098	9,098		
Adjusted R ²	0.901	0.902	0.889	0.890		

Table OA4 Robustness Checks III

This table presents the robustness test of the baseline results using a sample with industry-by-year FE (Panel A) and three years previous FCA Cases to measure *Cases* (Panel B). Variable definitions are reported in Appendix A. The numbers reported in parentheses are t-statistics based on the standard errors clustered at the firm level. *, **, *** denote statistical significance at the 10%, 5% and 1%, respectively.

Panel A. Baseline test with Industry-by-year FE						
Dependent Variable	<i>Toxic Releases</i>			<i>Release Intensity</i>		
Variables	(1)	(2)	(3)	(4)	(5)	(6)
<i>Courts×Post×Cases</i>	-0.134*** (-3.24)	-0.117*** (-2.93)	-0.119*** (-3.04)	-0.090** (-2.32)	-0.104** (-2.55)	-0.107*** (-2.63)
<i>Courts×Post</i>	-0.020 (-0.15)	-0.011 (-0.09)	-0.002 (-0.01)	-0.047 (-0.35)	-0.033 (-0.25)	-0.021 (-0.16)
Firm-level Controls	No	Yes	Yes	No	Yes	Yes
State-level Controls	No	No	Yes	No	No	Yes
Cohort-Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Cohort-Year FE	Yes	Yes	Yes	Yes	Yes	Yes
# of Observations	10,322	10,322	10,322	10,322	10,322	10,322
Adjusted R ²	0.885	0.887	0.887	0.870	0.872	0.872
Panel B. Baseline test with three years previous FCA Cases						
Dependent Variable	<i>Toxic Releases</i>			<i>Release Intensity</i>		
Variables	(1)	(2)	(3)	(4)	(5)	(6)
<i>Courts×Post×Cases_pseudo</i>	-0.188*** (-7.36)	-0.185*** (-6.92)	-0.185*** (-6.93)	-0.179*** (-6.65)	-0.158*** (-4.76)	-0.157*** (-4.70)
<i>Courts×Post</i>	0.028 (0.23)	0.028 (0.23)	0.033 (0.27)	-0.004 (-0.03)	0.016 (0.13)	0.022 (0.18)
Firm-level Controls	No	Yes	Yes	No	Yes	Yes
State-level Controls	No	No	Yes	No	No	Yes
Cohort-Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Cohort-Year FE	Yes	Yes	Yes	Yes	Yes	Yes
# of Observations	10,633	10,633	10,633	10,633	10,633	10,633
Adjusted R ²	0.883	0.884	0.884	0.869	0.871	0.871

Table OA5 Robustness Checks IV

This table presents the robustness test of the baseline results using a sample with ESG and earnings management controls. Variable definitions are reported in Appendix A. The numbers reported in parentheses are t-statistics based on the standard errors clustered at the firm level. *, **, *** denote statistical significance at the 10%, 5% and 1%, respectively.

Panel A. Baseline test with ESG and earnings management controls						
Dependent Variable	<i>Toxic Releases</i>			<i>Release Intensity</i>		
Variables	(1)	(2)	(3)	(4)	(5)	(6)
<i>Courts×Post×Cases</i>	-0.157*** (-3.29)	-0.155*** (-3.45)	-0.154*** (-3.49)	-0.153*** (-2.32)	-0.139*** (-2.55)	-0.139*** (-2.63)
<i>Courts×Post</i>	0.047 (0.38)	0.044 (0.36)	0.046 (0.38)	0.025 (0.21)	0.040 (0.33)	0.043 (0.36)
<i>ESG_Score</i>	-0.011 (-0.55)	-0.007 (-0.36)	-0.007 (-0.34)	0.003 (0.15)	-0.002 (-0.11)	-0.002 (-0.08)
<i>Financial_Restatement</i>	-0.030 (-0.50)	-0.018 (-0.29)	-0.021 (-0.34)	-0.045 (-0.73)	-0.039 (-0.65)	-0.043 (-0.70)
Firm-level Controls	No	Yes	Yes	No	Yes	Yes
State-level Controls	No	No	Yes	No	No	Yes
Cohort-Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Cohort-Year FE	Yes	Yes	Yes	Yes	Yes	Yes
# of Observations	8,020	8,020	8,020	8,020	8,020	8,020
Adjusted R ²	0.885	0.887	0.887	0.873	0.874	0.875

Table OA6 Robustness Checks V

This table presents the robustness test of the baseline results with various proxies for corporate litigation risk. Variable definitions are reported in Appendix A. The numbers reported in parentheses are t-statistics based on the standard errors clustered at the firm level. *, **, *** denote statistical significance at the 10%, 5% and 1%, respectively.

Panel A. Baseline test with proxies of litigation risk						
Dependent Variable	<i>Toxic Releases</i>			<i>Release Intensity</i>		
Variables	(1)	(2)	(3)	(4)	(5)	(6)
<i>Courts×Post×Cases</i>	-0.152*** (-3.60)	-0.152*** (-3.80)	-0.152*** (-3.90)	-0.152*** (-4.87)	-0.139*** (-4.19)	-0.138*** (-4.29)
<i>Courts×Post</i>	0.043 (0.31)	0.045 (0.33)	0.057 (0.42)	0.023 (0.17)	0.028 (0.21)	0.043 (0.32)
<i>UD_Laws</i>	0.094 (0.93)	0.086 (0.85)	0.084 (0.83)	0.078 (0.75)	0.084 (0.83)	0.082 (0.81)
<i>Ninth_Circuit</i>	-0.241* (-1.76)	-0.265* (-1.88)	-0.246* (-1.72)	-0.274* (-1.83)	-0.255* (-1.80)	-0.234 (-1.63)
<i>Neveda_Firm</i>	-0.188*** (-3.72)	-0.119 (-1.44)	-0.169* (-1.96)	0.059 (1.20)	-0.073 (-0.87)	-0.132 (-1.51)
<i>Litigation_Likelihood</i>	-0.001 (-1.58)	-0.001 (-1.51)	-0.001 (-1.56)	-0.001* (-1.80)	-0.001** (-2.00)	-0.001** (-2.06)
<i>Good_Faith</i>	-0.061 (-0.43)	-0.057 (-0.40)	-0.076 (-0.54)	-0.059 (-0.40)	-0.033 (-0.23)	-0.056 (-0.39)
<i>Implied_Contract</i>	-0.115 (-0.85)	-0.120 (-0.88)	-0.150 (-1.09)	-0.117 (-0.82)	-0.116 (-0.84)	-0.152 (-1.08)
<i>Public_Policy</i>	-0.006 (-0.03)	-0.022 (-0.11)	-0.016 (-0.08)	0.018 (0.09)	0.024 (0.12)	0.032 (0.16)
Firm-level Controls	No	Yes	Yes	No	Yes	Yes
State-level Controls	No	No	Yes	No	No	Yes
Cohort-Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Cohort-Year FE	Yes	Yes	Yes	Yes	Yes	Yes
# of Observations	9,481	9,481	9,481	9,481	9,481	9,481
Adjusted R ²	0.883	0.884	0.884	0.871	0.873	0.873

Table OA7 Robustness Checks VI

This table presents the robustness test of the baseline results with various proxies of financial constraints based on accounting data. Variable definitions are reported in Appendix A. The numbers reported in parentheses are t-statistics based on the standard errors clustered at the firm level. *, **, *** denote statistical significance at the 10%, 5% and 1%, respectively.

Panel A. Baseline test with financial constraints controls						
Dependent Variable	<i>Toxic Releases</i>			<i>Release Intensity</i>		
Variables	(1)	(2)	(3)	(4)	(5)	(6)
<i>Courts×Post×Cases</i>	-0.190*** (-3.02)	-0.153*** (-3.57)	-0.155*** (-3.60)	-0.159*** (-2.84)	-0.148*** (-4.10)	-0.140*** (-4.15)
<i>Courts×Post</i>	0.056 (0.45)	0.035 (0.29)	0.034 (0.28)	0.046 (0.38)	0.024 (0.20)	0.022 (0.18)
<i>KZ_Index</i>	0.000 (0.73)			-0.001 (-1.51)		
<i>SA_Index</i>		-0.144 (-0.48)			0.402 (0.68)	
<i>MM_Index</i>			-0.001* (-1.84)			-0.001 (-0.57)
Firm-level Controls	Yes	Yes	Yes	Yes	Yes	Yes
State-level Controls	Yes	Yes	Yes	Yes	Yes	Yes
Cohort-Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Cohort-Year FE	Yes	Yes	Yes	Yes	Yes	Yes
# of Observations	9,869	10,633	10,561	9,869	10,633	10,561
Adjusted R ²	0.884	0.884	0.884	0.870	0.871	0.871

Table OA8 Robustness Checks VII

This table presents the robustness test of the baseline results with various text-based financial constraints measures. Variable definitions are reported in Appendix A. The numbers reported in parentheses are t-statistics based on the standard errors clustered at the firm level. *, **, *** denote statistical significance at the 10%, 5% and 1%, respectively.

Panel A. Baseline test with financial constraints controls				
Dependent Variable	<i>Toxic Releases</i>		<i>Release Intensity</i>	
Variables	(1)	(2)	(3)	(4)
<i>Courts×Post×Cases</i>	-0.119*** (-6.61)	-0.188*** (-3.06)	-0.121*** (-6.74)	-0.157*** (-2.87)
<i>Courts×Post</i>	0.112 (0.66)	0.067 (0.51)	0.106 (0.63)	0.056 (0.43)
<i>HM_Equity</i>	-0.727 (-1.48)		-0.709 (-1.37)	
<i>HM_Debt</i>	0.201 (0.39)		0.146 (0.28)	
<i>HM_Private</i>	-0.516 (-0.93)		-0.378 (-0.68)	
<i>LW_Equity</i>		-0.144** (-2.45)		-0.137** (-2.29)
<i>LW_Debt</i>		0.010 (0.25)		-0.019 (-0.44)
Firm-level Controls	Yes	Yes	Yes	Yes
State-level Controls	Yes	Yes	Yes	Yes
Cohort-Firm FE	Yes	Yes	Yes	Yes
Cohort-Year FE	Yes	Yes	Yes	Yes
# of Observations	6,456	9,214	6,456	9,214
Adjusted R ²	0.902	0.878	0.889	0.870

Table OA9 Robustness Checks VIII (original DID)

This table presents the robustness test of the baseline results in Table 2 using the original DID (non-stacked sample). Our identification strategy relies on the firm's previous FCA lawsuits with three shocks induced by US appeals courts. We build an estimation window of $[-5,+5]$ for each shock and perform stacked difference-in-difference regressions. The dependent variables, *Toxic Releases* and *Release Intensity*, are the log transformation of firms' toxic releases and release intensity obtained from the EPA TRI datasets, and the independent variable of interest is $Courts \times Post \times Cases$. *Courts* take the value of 1 if the plant is located in states which have been affected by the court rulings that increase the financial incentives for whistleblowers and zero otherwise. *Post* takes the value of 1 if the observation is in the period after the relevant court rulings and zero otherwise. *Cases* is the number of previous FCA lawsuits before each shock. All control variables are defined in Appendix A. The numbers reported in parentheses are t-statistics based on the standard errors clustered at the firm level. *, **, *** denote statistical significance at the 10%, 5% and 1%, respectively.

Dependent Variable	<i>Toxic Releases</i>			<i>Release Intensity</i>		
	(1)	(2)	(3)	(4)	(5)	(6)
Variables						
<i>Courts</i> × <i>Post</i> × <i>Cases</i>	-0.154*** (-3.33)	-0.147*** (-3.59)	-0.147*** (-3.63)	-0.150*** (-4.51)	-0.132*** (-4.03)	-0.132*** (-4.10)
<i>Courts</i> × <i>Post</i>	0.040 (0.33)	0.042 (0.35)	0.045 (0.38)	0.011 (0.09)	0.031 (0.27)	0.036 (0.31)
Firm-level Controls	No	Yes	Yes	No	Yes	Yes
State-level Controls	No	No	Yes	No	No	Yes
Cohort-Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Cohort-Year FE	Yes	Yes	Yes	Yes	Yes	Yes
# of Observations	8,075	8,075	8,075	8,075	8,075	8,075
Adjusted R ²	0.887	0.889	0.889	0.875	0.877	0.877

Table OA10 Further Identification

This table presents the baseline results of whether previous whistleblowing experience affects corporate environmental performance. We split the observations into high/low polluting groups based on their industries. Our identification strategy relies on the firm's previous FCA lawsuits with three shocks induced by US appeals courts. We build an estimation window of [-5,+5] for each shock and perform stacked difference-in-difference regressions. The dependent variables, *Toxic Releases* and *Release Intensity*, are the log transformation of firms' toxic releases and release intensity obtained from the EPA TRI datasets, and the independent variable of interest is *Courts×Post×Cases*. *Courts* take the value of 1 if the plant is located in states which have been affected by the court rulings that increase the financial incentives for whistleblowers and zero otherwise. *Post* takes the value of 1 if the observation is in the period after the relevant court rulings and zero otherwise. *Cases* is the number of previous FCA lawsuits before each shock. All control variables are defined in Appendix A. The numbers reported in parentheses are t-statistics based on the standard errors clustered at the firm level. *, **, *** denote statistical significance at the 10%, 5% and 1%, respectively.

Dependent Variable Variables	<i>Toxic Releases</i>		<i>Release Intensity</i>	
	(1)	(2)	(3)	(4)
	High polluting	Low polluting	High polluting	Low polluting
<i>Courts×Post×Cases</i>	-0.504*** (-5.55)	-0.138*** (-5.48)	-0.443*** (-4.69)	-0.130*** (-5.79)
<i>Courts×Post</i>	0.037 (0.22)	0.084 (0.48)	0.026 (0.16)	0.066 (0.37)
Firm-level Controls	Yes	Yes	Yes	Yes
State-level Controls	Yes	Yes	Yes	Yes
Cohort-Firm FE	Yes	Yes	Yes	Yes
Cohort-Year FE	Yes	Yes	Yes	Yes
[P-Value Test Differences]	<0.01		<0.01	
# of Observations	6,062	4,256	6,062	4,256
Adjusted R ²	0.890	0.868	0.877	0.858

Appendix C. Matching the FCA cases with Compustat Firms.

We manually match over 9,000 FCA cases from 1987-2013 to Compustat listed firms. Our manual matching follows three steps. Firstly, we manually extract each defendant's name (focusing on the companies but not individual persons) from the case caption. Secondly, we manually check each defendant company's name using Google. We look at the history of each company to determine if it has ever changed its name or been acquired. Finally, we use the Compustat North America company code lookup tool from Wharton Research Data Service (WRDS) to search for the company name that we had extracted and checked through the first two steps. When searching for names, we simultaneously remove all punctuation marks and delete corporate designators, such as "Corporation," "Company," "INC," or "LLC," If it yields a result, we will then assign the unique GVKEY identifier to that case; if there is no result we will then search for the old name or the parent company's name of the defendant company in the company code lookup tool. We repeat these three steps for each FCA case.

Then, we will use an example to illustrate how these steps work. In the case "*US ex rel Goodstein, David v Kaplan Inc; Washington Post Co (2007)*", we first extract two defendant's names: Kaplan Inc and Washington Post Co. Secondly, we utilize Google to search for "Kaplan Inc" and find that it is now a wholly-owned subsidiary of Graham Holdings Company. We then search for "Washington Post Co." and find that it changed its name to Graham Holdings Company in 2013. Finally, we use the company code lookup tool in Compustat North America provided by WRDS. We first search for "Kaplan" in the tool, and it yields two results: one is "KAPLAN INDUSTRIES INC" and another is "LAZARE KAPLAN INTERNATIONAL." However, these two companies are not Kaplan Inc., an education company, which we are targeting, since we check their names using Google and find that "KAPLAN INDUSTRIES INC" produces compressed gas cylinders and equipment, and "LAZARE KAPLAN INTERNATIONAL" is a diamond manufacturing company. Thus, our first search yields no effective result. Then we search for "Graham Holdings" in the company code lookup tool, and it only yields one result: GRAHAM HOLDINGS CO, which is the parent company of Kaplan we are looking for. Therefore, we assign the GVKEY—011300 to the case *US ex rel Goodstein, David v Kaplan Inc; Washington Post Co (2007)*.