

When Speaking Freely Pays: Anti-SLAPP Laws and Firms' Cost of Equity

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ABSTRACT

We examine whether investors price firms' ability to suppress unfavorable information generated by outsiders. We exploit the staggered adoption of state anti-SLAPP (Strategic Lawsuits Against Public Participation) laws, which limit firms' use of litigation to deter outside criticism. Using an imputation-based difference-in-differences design, we find that anti-SLAPP adoption lowers firms' cost of equity (COE). We further show that anti-SLAPP laws meaningfully alter firms' information environments by increasing unfavorable outside information, reducing disclosures of litigation related to criticism and speech, and increasing reputational-risk disclosures. Consistent with an uncertainty-resolution mechanism, anti-SLAPP adoption reduces information asymmetry, firm risk, undervaluation, and stock price crash risk, indicating that investors face less uncertainty about hidden firm risks. The decline in COE is concentrated among firms with poorer information environments, weaker internal governance, and greater exposure to public scrutiny, where outside information is especially valuable for reducing informational uncertainty. Our findings identify firms' ability to suppress outside criticism as a distinct, priced information friction and suggest that legal institutions governing third-party speech play an important role in the information infrastructure of capital markets.

Keywords: Freedom of Speech, Anti-SLAPP Laws, Cost of Equity, Financing Costs, Information Transparency

JEL Classifications: G12, G30, J21, K22

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1 Introduction

A firm's information environment is shaped not only by what its managers choose to disclose, but also by whether outsiders can credibly reveal information managers would prefer to keep hidden. Employees, customers, journalists, analysts, short sellers, activists, and other stakeholders often possess value-relevant information about product quality, workplace conditions, misconduct, and operational and regulatory risks. Because this information is decentralized and generated by parties whose incentives may differ from managers', it can complement, challenge, or contradict firm-provided disclosures. Yet outsiders' willingness to produce and disseminate such information depends on the expected cost of doing so. When firms can deter outside criticism, unfavorable information may be delayed, softened, or suppressed altogether. Moreover, because investors rely on information from both inside and outside the firm to assess cash-flow prospects and risk, firms' ability to suppress unfavorable information generated by outsiders can ultimately affect the return investors require to hold their equity.

While the importance of information in capital markets has long been recognized, the literature has focused predominantly on information provided by firms through financial reporting, voluntary disclosure, management forecasts, and other channels under managerial control. Accordingly, existing evidence that information affects firms' cost of equity (COE) is derived largely from studies of firm-provided information ([Diamond and Verrecchia, 1991](#); [Easley and O'Hara, 2004](#); [Lambert, Leuz, and Verrecchia, 2007](#)). Much less is known about whether investors value limits on firms' ability to suppress unfavorable information generated by outsiders. This omission is important because outside information is often uniquely positioned to reveal risks that managers have incentives not to disclose and, unlike firm-provided disclosure, is largely beyond management's control. We therefore examine whether such limits affect firms' COE.

The effect of information suppression on firms' COE is theoretically ambiguous. On the one hand, limiting firms' ability to suppress outsider-supplied unfavorable information may lower equity financing costs. When adverse information is harder to suppress, bad news reaches the market more quickly, reducing information asymmetry, limiting bad-news hoarding and crash risk, and improving investors' ability to distinguish firms with hidden risks from those without

them. Investors may therefore require a lower return to hold the firm’s equity (Kothari, Shu, and Wysocki, 2009; Dyck, Morse, and Zingales, 2010; Kim and Shi, 2011). On the other hand, reducing firms’ ability to suppress unfavorable information can raise the COE. Newly surfaced criticism may reveal previously hidden reputational, operational, litigation, or regulatory risks, or introduce noisy and difficult-to-verify signals that increase uncertainty about firm fundamentals. Whether investors view reductions in outside-information suppression as uncertainty-resolving or risk-revealing is therefore an empirical question.

To identify variation in firms’ ability to suppress unfavorable information, we exploit the staggered adoption of state-level anti-SLAPP (Strategic Lawsuits Against Public Participation) laws. SLAPPs are lawsuits filed to intimidate or silence individuals and organizations that publicly criticize firms or other powerful actors (Canan and Pring, 1988a,b; Pring and Canan, 1996). Anti-SLAPP laws reduce the expected cost of producing and disseminating unfavorable information by allowing courts to dismiss meritless lawsuits against critics earlier, implement stays of discovery, and shift attorneys’ fees from defendants to plaintiffs.¹ For our purposes, the key feature of these laws is not their specific legal doctrine, but the economic friction they alter: they reduce firms’ ability to use the threat of litigation to deter outside criticism. Thus, their staggered adoption provides plausibly exogenous variation in firms’ ability to suppress information.

Since 1992, 40 states and the District of Columbia have enacted anti-SLAPP statutes, with adoption timing varying across states. Moreover, states typically adopt these laws in response to concerns about free speech, judicial efficiency, and public pressure following high-profile attempts to silence critics, rather than in response to changes in the financing costs of firms in the state. Consistent with this interpretation, hazard models show that adoption timing is not systematically related to the average COE of firms in the state, state economic conditions, political leanings, demographics, or corporate tax rates. Firms in adopting and non-adopting states are also similar in a broad set of observable characteristics prior to enactment.

¹A prominent example involving a public firm is Sharper Image Corp.’s lawsuit against Consumers Union, the publisher of *Consumer Reports*, following the magazine’s negative review of Sharper Image’s Ionic Breeze air purifier. Consumers Union moved to dismiss the case under California’s anti-SLAPP statute, and the court ultimately dismissed the lawsuit after finding that Sharper Image failed to establish falsity or flaws in the testing. The court also awarded Consumers Union \$525,000 in legal fees. Additional examples involving public firms are provided in Appendix OA of the Online Appendix.

We estimate the effect of anti-SLAPP adoption using a staggered difference-in-differences (DiD) design. Because two-way fixed effects estimators can be biased in settings with staggered adoption, we use the imputation-based DiD estimator of [Borusyak, Jaravel, and Spiess \(2024\)](#). The estimator constructs counterfactual outcomes for treated firms using observations from firms in states that have not yet adopted, or never adopt, anti-SLAPP laws. Our specifications include firm and year fixed effects and are estimated with and without firm- and state-level controls. We measure firms' *ex ante* COE as the implied discount rate that equates current stock prices with the present value of expected future cash flows. We estimate the implied COE using analyst forecasts and the residual income model of [Gebhardt, Lee, and Swaminathan \(2001\)](#), which has been widely used as a proxy for expected returns (e.g., [Pástor, Sinha, and Swaminathan, 2008](#); [Lee, Ng, and Swaminathan, 2009](#); [Hann, Ogneva, and Ozbas, 2013](#)). Our main finding is that firms' COE declines after anti-SLAPP adoption. Relative to firms in states without anti-SLAPP protection, firms in adopting states experience a 45.5 basis point decline in their COE. The estimate is economically meaningful relative to the sample mean COE of 6.6% and remains virtually unchanged after including firm- and state-level controls. Moreover, dynamic tests show no differential pre-trends in the years before adoption, supporting the identifying assumption that treated and control firms would have followed similar COE trends absent the laws.

The results are robust to a broad set of alternative explanations and specifications. They remain similar when we address concerns about geographic exposure, state-versus-federal jurisdiction, and forum shopping by restricting the sample to geographically concentrated firms, excluding consumer-product firms, excluding firms in California and New York, and estimating the effects separately before and after a 2010 Supreme Court decision that limited forum shopping. The findings are also robust to alternative implied COE measures, alternative DiD estimators, industry-year and incorporation-state-by-year fixed effects, inverse probability weighting, the test for selection on unobservables proposed by [Oster \(2019\)](#), placebo adoption dates, and controls for other state laws that could affect firms' financing costs. Finally, we show that the COE-reducing effect is greater in states with stronger anti-SLAPP protections, a result that is difficult to attribute to broad trends in state-level political or legal factors and is instead directly attributable to the laws.

Although these tests support our main finding, interpreting the decline in COE as arising from reduced information suppression requires evidence that anti-SLAPP laws alter the information and litigation environments of public firms. We therefore examine whether adoption is followed by more unfavorable outside information and by changes in firms’ criticism-related litigation and reputational-risk disclosures. We find that anti-SLAPP laws meaningfully affect both dimensions. Following adoption, firms experience more negative press releases in RavenPack and receive less favorable employee assessments on Glassdoor. In particular, employees report less favorable business outlooks, are less likely to recommend their employers, and express lower approval of CEOs. Firms also disclose less criticism- and speech-related litigation in the “Legal Proceedings” sections of their 10-K filings while disclosing greater reputational exposure in their “Risk Factors.”²

We next examine whether improvements in the information environment and reductions in downside information risk are channels through which limits to information suppression lower firms’ COE. Anti-SLAPP adoption is followed by reductions in bid-ask spreads, illiquidity, and the probability of informed trading, consistent with lower information asymmetry among investors. We also find declines in total return volatility, implied return volatility, idiosyncratic volatility, and systematic risk, suggesting that investors perceive firms as less risky after enactment. Firms are also less likely to be undervalued relative to fundamentals following anti-SLAPP adoption, consistent with investors better distinguishing firms with hidden risks from those without (Diamond and Verrecchia, 1991). Finally, firms in anti-SLAPP states experience declines in stock price crash risk, indicating that reducing firms’ ability to deter public criticism allows adverse information to reach the market more quickly and limits bad-news hoarding (Chen, Li, and Li, 2025; Lee et al., 2026). Overall, our findings suggest that investors price limits on firms’ ability to suppress information generated by outsiders as a means of resolving uncertainty about hidden risks.

Cross-sectional tests further support this interpretation. The decline in COE is greater among firms with less transparent information environments, including firms without management earnings forecasts, firms with larger analyst forecast errors, firms with higher absolute discretionary accruals, and firms with higher R&D intensity. Investors in these firms face greater uncertainty

²These findings align with work showing that anti-SLAPP laws deter retaliatory litigation and affect unfavorable information flows (Elias, 2015; Ernst, 2015; Bunker and Erickson, 2018; Leader, 2019; Skrzypczak, 2023; Böke, Brau, Glaeser, and Jeong, 2026; Lee, Ng, Yoo, and Zhang, 2026).

about hidden risks and should benefit more when unfavorable outside information is harder to suppress. The COE-reducing effect is also stronger among firms with weaker internal governance, measured using board independence, board busyness, and CEO pay-performance sensitivity, consistent with outside monitoring being more valuable when internal monitoring is limited. Finally, the decline is larger for firms more exposed to external scrutiny, including those with greater media coverage, those rated on Glassdoor, and those with greater litigation- or reputation-related exposure in their 10-K disclosures (i.e., firms with outside stakeholders that are more likely to possess, produce, or disseminate value-relevant information).

This paper makes three main contributions. First, we contribute to the literature on information risk and the cost of capital. Prior research shows that disclosure quality, information asymmetry, and accounting information affect expected returns and firms' COE (Diamond and Verrecchia, 1991; Botosan, Plumlee, and Xie, 2004; Easley and O'Hara, 2004; Francis, LaFond, Olsson, and Schipper, 2005; Lambert et al., 2007; Dambra, Velikov, and Weber, 2024). However, this literature largely focuses on information supplied by firms through voluntary disclosure, mandatory reporting, or accounting systems. We extend these findings by showing that firms' ability to suppress external criticism is a priced information friction. This distinction is important because outside information is often adverse, decentralized, difficult for managers to control, and produced by parties whose incentives differ from those of the firm. Our results suggest that investors price not only the quantity and quality of firm-provided information but also firms' ability to deter unfavorable information generated by outsiders.

Second, we contribute to research on external monitoring, bad-news revelation, and anti-SLAPP laws. A large literature shows that employees, journalists, analysts, short sellers, activists, and other outside monitors can reveal information that managers may prefer to withhold (Miller, 2006; Dyck et al., 2010; Fang, Huang, and Karpoff, 2016; Appel, 2019; Da and Huang, 2020; Dyer, Lang, and Oh, 2025). Related work shows that anti-SLAPP laws deter retaliatory litigation and affect unfavorable information flows and related firm outcomes, including whistleblowing, ESG disclosure, CSR activity, and negative news about firms (Chemmanur, Rajaiya, and Sheng, 2020; Griffin, Hong, Jung, Moon, and Ryou, 2024a; Chen et al., 2025; Jung, Wang, Wei, and Zhang, 2025; Böke et al., 2026; Lee et al., 2026; Wu, Bruton, and Krause, 2026). However, evidence that

anti-SLAPP laws change information production does not reveal how investors price the resulting change in firms’ information environments. More outside criticism could resolve uncertainty about hidden problems and lower required returns, or it could reveal new risks, introduce noise, and raise required returns. We therefore build on these prior studies to answer the unresolved asset-pricing question: whether and how reductions in outside-information suppression arising from anti-SLAPP adoption are priced in capital markets.

Finally, our paper contributes to research on how legal institutions shape corporate financing outcomes. Prior work shows that legal rules, investor protection, and regulatory institutions affect disclosure, governance, and the cost of capital ([La Porta, Lopez-de-Silanes, and Shleifer, 2006](#); [Hail and Leuz, 2009](#); [Hope, Hu, and Lu, 2016](#); [Lai, Lin, and Ma, 2024](#)). We show that legal institutions governing outsiders’ ability to speak freely about firms also affect equity financing costs. This evidence highlights a distinct channel through which legal rules can affect capital markets: not by directly changing firms’ disclosure obligations or governance structures, but by altering the expected cost for outsiders of producing and disseminating unfavorable information. These results also inform policy discussions. Anti-SLAPP protections continue to expand across U.S. states despite ongoing legal debate. Our evidence suggests that protecting critics from retaliatory litigation yields measurable capital market benefits by lowering firms’ COE.

2 Institutional Background and Hypotheses

2.1 Institutional background

SLAPPs are lawsuits that firms or other powerful actors use to deter unfavorable outside criticism by imposing burdensome litigation costs ([Canan and Pring, 1988a,b](#)).³ They can target a broad range of outside speakers, including consumer advocates, journalists, bloggers, employees, activists, and other stakeholders who criticize firms, products, executives, or business practices. Because defending a lawsuit can be costly even when the underlying claims are meritless or unlikely to succeed, the threat of litigation can discourage outsiders from producing or disseminating

³The Institute for Free Speech estimates that the median defense cost for a SLAPP lawsuit is about \$39,000, and that even relatively simple cases can exceed \$100,000, with some cases running into the millions.

unfavorable information. In this way, SLAPPs can serve as a mechanism through which firms suppress information generated outside of the firm.

Anti-SLAPP statutes reduce this deterrent effect by providing defendants with procedural tools to challenge these lawsuits early in the litigation process. These protections commonly include expedited hearings, stays of discovery, and fee-shifting provisions that allow prevailing defendants to recover attorneys’ fees. California enacted the first broad anti-SLAPP statute in 1992, and, as of 2026, 40 states and the District of Columbia have adopted similar legislation. Although these statutes vary in scope and strength across states (e.g., some broadly protect speech on all matters of public concern, whereas others apply to narrower categories of speech or provide weaker procedural protections), their common objective is to reduce the expected litigation costs faced by outside critics and thereby weaken firms’ ability to discourage unfavorable information through litigation ([Greenberg, Keating, and Knowles-Gardner, 2023](#)).⁴

States have adopted anti-SLAPP laws primarily in response to concerns about free speech, litigation costs, and judicial efficiency. Legislators frequently cite the need to protect whistleblowers, journalists, citizens, and other outside monitors from retaliatory lawsuits that could discourage reporting on government or corporate conduct ([Pring and Canan, 1996](#); [Dyck et al., 2010](#); [Heese and Pérez-Cavazos, 2021](#)). Policymakers have also argued that early dismissal procedures conserve judicial resources by resolving meritless claims before costly discovery ([Böke et al., 2026](#)).⁵ In other instances, high-profile disputes involving environmental claims, consumer complaints, local activism, or criticism of powerful interests generated public pressure for statutory protection (e.g., [Elias, 2015](#); [Schaufele, 2024](#); [Chen, Ke, and Kuang, 2026](#); [Wu et al., 2026](#)). These motivations are important for our empirical design because they suggest that anti-SLAPP statutes were enacted in response to concerns about speech and litigation rather than changes in firms’ COE. This institutional setting therefore provides plausibly exogenous variation in firms’ ability to suppress unfavorable information.

⁴In robustness tests, we exploit cross-state variation in statutory strength.

⁵[Pring and Canan \(1996\)](#) describe SLAPPs as widespread and argue that many speakers are muted by the threat of litigation. Focusing on libel cases, [Lee et al. \(2026\)](#) estimate that there are approximately 5.93 SLAPP cases per state-year. The NYU “SLAPP Back” database identifies 16,094 citations to anti-SLAPP statutes across 10,257 individual cases since 1994, including 500 cases in 2024 alone. Moreover, these counts likely understate the broader deterrent effect because courts do not uniformly classify cases as SLAPPs and many threats or settlements do not generate judicial opinions.

2.2 Hypothesis development

Firms may seek to deter employees, customers, journalists, activists, and other stakeholders from disclosing unfavorable information about their products, workplace conditions, misconduct, operational problems, or other risks. When investors recognize that outside criticism may be suppressed, publicly available information may provide an incomplete view of firms' fundamentals. Anti-SLAPP laws limit firms' ability to use litigation to deter such criticism and therefore provide a setting to examine how the availability of unfavorable outside information affects firms' COE. The predicted effect is theoretically ambiguous because a greater flow of unfavorable information can either resolve uncertainty about hidden risks or reveal that firms are riskier than investors previously believed.

Under the first possibility, reducing firms' ability to suppress unfavorable information lowers the COE by reducing uncertainty about hidden firm risks. Information asymmetry is a central determinant of firms' COE (e.g., [Diamond and Verrecchia, 1991](#); [Francis et al., 2005](#); [Beyer, Cohen, Lys, and Walther, 2010](#)), and disclosure theory suggests that more complete and credible information can lower expected returns by reducing estimation risk and improving investors' ability to assess fundamentals (e.g., [Leuz and Verrecchia, 2000](#); [Verrecchia, 2001](#); [Easley and O'Hara, 2004](#); [Lambert et al., 2007](#)). When unfavorable information is more likely to reach the market, investors can more accurately distinguish firms with substantial hidden risks from firms whose underlying fundamentals are stronger. A richer flow of outside information may therefore reduce information asymmetry, constrain managerial bad-news hoarding, improve price discovery, and lower uncertainty surrounding firms' future cash flows and risk ([Diamond and Verrecchia, 1991](#); [Kothari et al., 2009](#); [Kim and Shi, 2011](#)). This reasoning yields our first prediction:

Uncertainty Resolution Hypothesis. Reducing firms' ability to suppress unfavorable information lowers firms' COE by reducing investors' uncertainty about hidden firm risks.

Alternatively, reducing firms' ability to suppress unfavorable information may raise the COE. If outside critics were previously deterred from speaking, anti-SLAPP protections may expose governance weaknesses, product-quality concerns, workplace problems, regulatory noncompliance, environmental liabilities, or other adverse conditions that investors had not fully incorporated

into their assessments of firm risk. Investors may then revise expected cash flows downward or perceived risk upward and demand a higher expected return. A greater flow of outside criticism may also increase the COE even when it does not reveal objectively greater underlying risk. Employee reviews, activist claims, media reports, and other public criticism may contain incomplete, exaggerated, conflicting, or difficult-to-verify signals. Such information can increase disagreement or estimation uncertainty about firm fundamentals. Thus, both the revelation of previously hidden adverse fundamentals and an increase in noisy unfavorable signals can increase the return investors require to hold firms' equity. This reasoning yields our second prediction:

Risk and Noise Hypothesis. Reducing firms' ability to suppress unfavorable information increases firms' COE by revealing previously hidden adverse fundamentals or increasing uncertainty generated by noisy unfavorable information.

The sign of the effect therefore depends on whether the reduction in uncertainty produced by a more complete information environment outweighs any upward revision in perceived firm risk or increase in informational noise. We use the staggered adoption of anti-SLAPP laws to distinguish between these competing predictions. Appendix [OB](#) of the Online Appendix presents a simple model that formalizes this tradeoff.⁶

3 Data and Empirical Methodology

This section describes our implied COE measure, sample, and identification strategy.

3.1 Measuring the implied COE

We estimate a firm's implied COE as the discount rate that equates the present value of its projected cash flows to stockholders with its current share price. We focus on the implied COE

⁶Although we interpret our findings as arising primarily from changes in firms' information environments, two alternative explanations merit consideration. First, restrictions on firms' ability to suppress unfavorable information could affect the COE by altering expected legal costs rather than information flows. Anti-SLAPP laws primarily deter retaliatory, speech-suppressing lawsuits while preserving legitimate legal claims. At the same time, by making unfavorable information more likely to emerge, these laws may increase firms' exposure to regulatory investigations or enforcement actions. The predicted effect of a legal-cost channel is therefore ambiguous. Second, anti-SLAPP protections could affect firms' COE by changing operating policies if greater outside scrutiny induces managers to adopt less risky investment strategies. We find little evidence supporting this mechanism. In untabulated analyses, firms increase the share of R&D relative to capital expenditures following anti-SLAPP adoption, consistent with more risk-intensive investment and with prior evidence that anti-SLAPP laws promote innovation ([Griffin, Hong, Jung, and Ryou, 2024b](#)), rather than greater managerial risk avoidance.

rather than realized returns because observed returns include variation beyond a firm’s financing costs. For instance, observed returns can also reflect fluctuations in growth opportunities, changes in anticipated expansion rates, shifts in investors’ risk appetite, and effects of market sentiment (e.g., [Stulz, 1999](#); [Hail and Leuz, 2006, 2009](#)). Further, expected returns inferred from *ex post* returns and asset pricing models may be imprecise due to uncertainty in factor premia and noise in estimates of factor loadings ([Fama and French, 1997](#)). By contrast, implied COE approaches separate a firm’s equity financing cost from its valuation, allowing researchers to explicitly control for cash flows, growth, and stock price momentum (e.g., [Hail and Leuz, 2006, 2009](#)).

Formally, a firm’s current stock price P_t can be expressed as:

$$P_t = \sum_{i=1}^{\infty} \frac{E_t(FCFE_{t+i})}{(1+R)^i}, \quad (1)$$

where $FCFE_{t+i}$ is free cash flow to equity at time $t+i$, and R is the implied COE. We estimate a firm’s implied COE using analysts’ earnings forecasts.⁷

Our primary measure of a firm’s COE follows the *ex ante* approach developed by [Gebhardt et al. \(2001\)](#), hereafter GLS), which estimates the implied COE using the residual income valuation model. Details are in [Appendix A](#). This method relies on a firm’s market price, earnings forecasts, and an assumption that its return on equity converges to the industry median over time. Prior research shows that the GLS measure provides a reliable proxy for expected returns and has been widely used in the asset pricing and corporate finance literature (e.g., [Pástor et al., 2008](#); [Lee et al., 2009](#); [Chava and Purnanandam, 2010](#); [Chen, Kacperczyk, and Ortiz-Molina, 2011a](#); [Chen, Huang, and Wei, 2013](#); [Hann et al., 2013](#); [Ortiz-Molina and Phillips, 2014](#)). As a robustness check, [Section 4.2](#) reports results using several alternative implied COE models as well as using earnings forecasts from statistical models rather than analyst forecasts.

3.2 Sample selection

Our sample is constructed from the CRSP/Compustat Merged database over 1987–2023. The sample begins in 1987, providing a five-year pre-treatment window before California enacted the

⁷A benefit of using analyst forecasts of multiple horizons made at time t is that if anti-SLAPP laws change a firm’s expected cash flows, analysts will update their forecasts at all horizons post-adoption. Thus, forecasts and current prices will reflect all information known at time t .

first state-level anti-SLAPP law in 1992, and ends in 2023 based on data availability. We exclude firms in the utility (SIC 4900–4999), financial (SIC 6000–6999), and quasi-public (SIC above 9900) sectors. Analyst earnings forecasts are obtained from IBES, accounting data from Compustat, and stock price and return data from CRSP. We identify firms’ historical headquarters locations using the COMPHIST and CST_HIST files from the CRSP/Compustat Merged database, which begin in 1994, and backfill headquarters information for earlier years when necessary. State-level controls include corporate tax rates (Book of the States), GDP (U.S. Bureau of Economic Analysis), unemployment rates (Bureau of Labor Statistics), unemployment benefits (Department of Labor), governor political affiliation (publicly available online records), and medical marijuana legalization dates (Guernsey, Serfling, and Yan, 2026).

The final sample contains 51,455 firm-year observations, although the number of observations varies across analyses depending on the imputation-based DiD specification and data availability. Continuous variables are winsorized at the 1st and 99th percentiles, and all dollar-denominated variables are expressed in 2020 dollars. Appendix B provides variable definitions. Table 1 reports the timing of state anti-SLAPP adoptions,⁸ and Table 2 presents summary statistics.

3.3 Identification strategy

Our empirical strategy exploits the staggered adoption of anti-SLAPP laws across U.S. states using a DiD framework. A conventional two-way fixed effects (TWFE) specification is:

$$y_{i,t} = \alpha_i + \beta_t + \tau D_{i,t} + \gamma X_{i,t-1} + \varepsilon_{i,t}, \quad (2)$$

where $y_{i,t}$ denotes the implied COE or another outcome for firm i in year t . Firm fixed effects (α_i) absorb time-invariant differences across firms, while year fixed effects (β_t) capture aggregate shocks common to all firms. The treatment indicator $D_{i,t}$ equals one if firm i is headquartered in a state that has enacted an anti-SLAPP law by year t , and zero otherwise. The vector $X_{i,t-1}$ contains lagged firm- and state-level controls. Standard errors are clustered at the state level to

⁸We define anti-SLAPP adoption using statutory enactment dates, consistent with the prevailing approach in the literature (e.g., Chemmanur et al., 2020; Griffin et al., 2024a; Jung et al., 2025). One exception is Böke et al. (2026), which classifies only “strong” anti-SLAPP statutes as treatment. This distinction affects only Tennessee and Washington, which together account for fewer than 4% of our firm-year observations. Our conclusions are unchanged when we instead adopt the classification of Böke et al. (2026).

account for serial correlation and cross-sectional dependence among firms exposed to the same legal environment.

Recent econometric research shows that conventional TWFE estimators can be biased when treatment is adopted at different times across units, and treatment effects evolve over time (e.g., [Baker, Larcker, and Wang, 2022](#); [Borusyak et al., 2024](#)). [Goodman-Bacon \(2021\)](#) demonstrates that the TWFE estimator combines many two-group, two-period DiD comparisons into a single coefficient. In staggered settings, some of these comparisons use already-treated units as controls for newly treated units. Because treatment may already have affected those observations, these comparisons need not recover valid counterfactual outcomes and can bias estimated treatment effects. This issue is particularly relevant in our setting. Only 15% of firm-year observations come from states that never adopt anti-SLAPP laws, and the Goodman-Bacon decomposition indicates that approximately 41.5% of the weight in the TWFE estimator comes from comparisons involving already-treated observations. We therefore employ an estimator specifically designed for staggered treatment timing.

We primarily use the imputation-based DiD estimator of [Borusyak et al. \(2024\)](#). Although several recently proposed estimators address staggered adoption (e.g., [Callaway and Sant’Anna, 2021](#); [Sun and Abraham, 2021](#); [Wooldridge, 2025](#)), the imputation estimator is particularly well suited to our setting because it is transparent, parsimonious, and efficient under standard assumptions. The estimator first uses untreated observations—including never-treated firms and pre-adoption observations from eventually treated firms—to estimate untreated potential outcomes using the model from Eq. (2). These estimates ($\hat{\alpha}_i$, $\hat{\beta}_t$, and $\hat{\gamma}$) are then used to impute counterfactual outcomes that treated firms would have experienced absent anti-SLAPP adoption ($\hat{y}_{i,t} = \hat{\alpha}_i + \hat{\beta}_t + \hat{\gamma}X_{i,t-1}$). Treatment effects are calculated as the difference between observed and imputed outcomes ($\hat{\tau}_{i,t} = y_{i,t} - \hat{y}_{i,t}$) and aggregated to obtain the average treatment effect on the treated (ATT). Because already-treated observations are never used as controls for newly treated observations, the estimator avoids the “forbidden-comparison” and “negative-weight” problems that can arise under conventional TWFE estimation.

We measure firm exposure to anti-SLAPP laws using the state where the firm’s corporate headquarters is located. This treatment assignment follows prior studies examining anti-SLAPP

laws and public firms (e.g., [Chemmanur et al., 2020](#); [Griffin et al., 2024a](#); [Chen et al., 2025](#); [Jung et al., 2025](#); [Böke et al., 2026](#); [Chen et al., 2026](#); [Lee et al., 2026](#); [Wu et al., 2026](#)) and is supported by economic and legal considerations. Economically, headquarters serve as the primary center of managerial decision-making, corporate communications, and firm identity. Legally, headquarters frequently provide a principal nexus for alleged reputational injury and litigation involving firm-specific criticism. Although firms operate across multiple jurisdictions, their headquarters provide a consistent and theoretically grounded measure of the legal environment most relevant to managers’ decisions about how to respond to outside criticism.

Two additional institutional considerations merit discussion. First, plaintiffs may attempt to file lawsuits outside the headquarters state (i.e., venue shop). However, disputes involving firm-specific criticism are often closely connected to the firm’s managerial and reputational center, and courts have long recognized jurisdiction where alleged reputational harm is experienced.⁹ To the extent that firms successfully pursue claims outside their headquarters state, our treatment assignment becomes less precise, introducing measurement error that should attenuate estimated treatment effects toward zero.

Second, federal courts differ in the extent to which they apply state anti-SLAPP procedures. This institutional complexity does not undermine our research design. Jurisdiction and choice of law frequently depend on a firm’s principal place of business or corporate citizenship, and the Supreme Court’s decision in *Hertz Corp. v. Friend* (2010) reinforced the central role of a firm’s headquarters by adopting the “nerve center” test for determining its principal place of business ([Colonnello and Herpfer, 2021](#)). More importantly, if differences in federal court treatment weaken the link between state anti-SLAPP statutes and firms’ actual litigation exposure, they likewise reduce the precision of our treatment assignment and bias estimates toward zero rather than generating spurious treatment effects. In Section 4.2, we aim to address these issues in detail.

3.4 Threats to identification

Our identification strategy assumes that, absent anti-SLAPP adoption, firms headquartered in adopting and non-adopting states would have followed similar trends in their COE. The

⁹For example, *Calder v. Jones* (1984) recognized personal jurisdiction based on the location where reputational injury occurred. More generally, courts and legislatures have also historically limited strategic forum shopping.

primary concern is whether anti-SLAPP adoption is systematically related to time-varying firm- or state-level characteristics that also affect firms' COE. We assess this possibility in two ways.

First, we examine covariate balance in the year preceding adoption. Table 3 compares a broad set of firm-level characteristics—including market capitalization, book-to-market ratios, leverage, stock returns, cash flow, analyst coverage, forecast dispersion, and projected long-term growth—as well as state-level economic, political, regulatory, and demographic characteristics. These state-level variables include GDP per capita, GDP growth, corporate tax rates, unemployment rates, governor political affiliation, maximum unemployment benefits, medical marijuana laws, average age, and the share of the population with a college degree. Firms headquartered in adopting and non-adopting states are broadly similar across the 17 characteristics examined. The only statistically significant differences are higher GDP per capita and a more educated population in adopting states in the year before enactment. We therefore include the full set of firm- and state-level characteristics as controls in our baseline specifications. Importantly, the estimated treatment effects (shown in the next section) are nearly identical with and without these controls, indicating that observable differences in these characteristics do not explain our findings.

Second, we examine whether anti-SLAPP adoption is systematically predicted by firms' financing costs or observable state characteristics. We estimate Weibull hazard models in which the failure event is a state's adoption of an anti-SLAPP law. This analysis covers 1987–2023 and excludes states after their adoption years. The key explanatory variable is *State GLS*, defined as the average implied COE among firms headquartered in the state, together with the same state-level controls used in the covariate balance analysis. All explanatory variables are lagged one year. The evidence provides little indication that anti-SLAPP adoption is systematically related to firms' COE or observable state conditions. As reported in Table 4, the coefficient on *State GLS* is economically small and statistically insignificant ($t\text{-stat} = -0.64$), indicating that local financing costs do not predict subsequent anti-SLAPP adoption. None of the remaining state-level variables consistently predicts adoption, and the results are similar when state characteristics are specified in first differences. Overall, these findings reduce concerns that anti-SLAPP adoption reflects evolving state characteristics that also affect firms' COE. As additional evidence, the event-study analyses presented in the next section reveal no indication of differential pre-treatment trends.

4 Results

We first examine whether anti-SLAPP protections lower firms' COE and then assess the robustness of this finding to a broad set of alternative specifications and identification concerns. We then investigate whether anti-SLAPP laws meaningfully constrain firms' ability to suppress unfavorable information. Finally, we examine mechanisms and cross-sectional predictions implied by the *Uncertainty Resolution Hypothesis*.

4.1 Baseline effect on firms' COE

Table 5 presents the baseline estimates and shows that reducing firms' ability to suppress unfavorable information is associated with economically meaningful declines in firms' COE. Across all specifications, firms headquartered in states that have adopted anti-SLAPP laws experience significantly lower equity financing costs than firms headquartered in non-adopting or not-yet-adopting states. The estimated ATT is 45.5 basis points in column 1, which includes firm and year fixed effects ($t\text{-stat} = -3.97$). Relative to the sample mean COE of 6.6%, this corresponds to a decline of approximately 6.9%. The estimated effect remains stable after adding firm-level controls in column 2 (47.0 basis points; $t\text{-stat} = -3.97$) and both firm- and state-level controls in column 3 (45.5 basis points; $t\text{-stat} = -5.46$), indicating that these observable firm and state characteristics do not explain the results. Economically, the estimated reduction is comparable to documented changes in firms' COE associated with cross-listing, unionization, corporate governance, gig-economy exposure, regulatory transparency, and medical marijuana legalization (Hail and Leuz, 2009; Chen et al., 2011a; Chen, Chen, and Wei, 2011b; Chino, 2021; Lai et al., 2024; Guernsey et al., 2026).

Dynamic treatment effects further support a causal interpretation of the baseline estimates. Figure 1 plots event-study estimates for the ten years before and after anti-SLAPP adoption, estimated both with and without firm- and state-level controls. The estimated coefficients prior to adoption are small and statistically insignificant, providing no evidence that treated firms followed differential pre-treatment trends. This finding supports the identifying assumption that treated and control firms would have experienced similar trends in the absence of anti-SLAPP

adoption. Following enactment, firms in adopting states experience a gradual decline in the COE that becomes statistically significant in the first post-adoption year and persists throughout the subsequent decade. The absence of pre-treatment effects also suggests that investors did not systematically anticipate the laws by lowering required returns before enactment.

4.2 Robustness

We conduct a broad set of robustness tests addressing various identification threats. Across all analyses tabulated in the Online Appendix, the estimated decline in firms’ COE remains robust.

Geographic exposure and headquarters assignment. A natural concern is that headquarters location may imperfectly capture firms’ exposure to anti-SLAPP laws, particularly for firms with geographically dispersed operations. Panel A of Table OA1 aims to address this concern by excluding firms with likely dispersed operations, including firms in retail, wholesale, and transportation and communication industries; firms with above-median property, plant, and equipment (PP&E); and firms disclosing operations in more than the median number of states (García and Norli, 2012). Panel B further links establishment-level Data Axle data to our sample and restricts the analysis to firms with at least 50%, 75%, 90%, or 100% of their workforce located in the headquarters state. Across these increasingly restrictive samples, the estimated decline in firms’ COE remains statistically significant and is generally larger than the baseline estimate. These findings indicate that the baseline results are not driven by firms for which headquarters location is a weak proxy for legal exposure.

Venue shopping and jurisdiction. Firms may try to avoid headquarters-state anti-SLAPP protections through venue choice or jurisdictional rules. Table OA2 addresses this possibility in three ways. First, we exclude firms operating in consumer-facing industries, where geographically diffuse activism may create stronger incentives for venue shopping (Wu et al., 2026). Second, we exclude California and New York, which host a disproportionate share of firms and SLAPP litigation and were among the earliest adopters of anti-SLAPP statutes.¹⁰ Third, we estimate the model separately before and after *Hertz Corp. v. Friend* (2010), which clarified that a corporation’s principal place of business is determined by its “nerve center,” typically its headquarters

¹⁰In untabulated analyses, excluding any individual state does not alter our conclusions.

(Colonnello and Herpfer, 2021). The estimated effects remain similar across all specifications, suggesting that venue shopping and jurisdictional concerns do not account for the findings.

Omitted variables and placebo timing. We next consider whether the baseline findings reflect omitted variables or spurious treatment timing rather than anti-SLAPP adoption itself. First, we apply the selection-on-unobservables approach of Oster (2019). The implied delta exceeds 1,000, far above the conventional threshold of one, indicating that unobserved factors would need to be substantially more important than observed factors to explain away the estimated effect.¹¹ Second, we conduct a placebo randomization test in Figure OA1 by randomly reassigning anti-SLAPP adoption across states while preserving the overall policy structure. Across 1,000 placebo draws, the estimated effects are centered near zero, with an average decline in firms' COE of only 1.2 basis points, compared with the actual estimate of 45.5 basis points. Together, these analyses suggest that the baseline findings are unlikely to reflect omitted variables or mechanical features of staggered policy adoption.

Alternative COE measures. Our findings are also robust to alternative measures of firms' implied COE. Panel A of Table OA3 reports results using the valuation models of Ohlson and Juettner-Nauroth (2005), Claus and Thomas (2001), and Easton (2004), as well as the average implied COE across these models and the baseline GLS measure. Panel B further estimates GLS-style implied COE measures using model-generated rather than analyst-generated earnings forecasts.¹² The estimated decline is similar across all approaches.

Alternative DiD estimators and fixed effects. We next examine whether the findings depend on the choice of estimator or fixed-effects structure. Table OA4 shows that the estimated treatment effect is qualitatively similar when using the DiD estimator of Wooldridge (2025) and the stacked DiD estimator of Cengiz, Dube, Lindner, and Zipperer (2019), both of which address concerns arising from staggered treatment timing. Table OA5 further shows that the results are robust to the inclusion of two-digit SIC industry-by-year fixed effects and incorporation-state-by-year fixed

¹¹The large delta reflects the fact that adding firm- and state-level controls increases the imputed R^2 from 16% to 23%, while the estimated treatment effect remains virtually unchanged at -0.455 .

¹²Model-generated forecasts address concerns that analyst-based implied COE measures require analyst coverage and may reflect systematic analyst forecast biases (e.g., Lim, 2001; Hong and Kubik, 2003). Following Li and Mohanram (2014), we generate out-of-sample earnings forecasts for years $t+1$ through $t+5$ using earnings-persistence and residual-income models estimated over the preceding ten years. Details are provided in Appendix OC of the Online Appendix.

effects, which absorb time-varying industry shocks and changes in the legal environment of firms' incorporation states.

Confounding state laws and treatment selection. Another concern is that anti-SLAPP adoption coincides with broader state-level policy reforms that affect firms' financing costs. Table OA5 shows that the COE effect remains robust after controlling for information, employment, innovation, health, and social-policy laws.¹³ We also estimate inverse probability-weighted specifications using the predicted likelihood that a firm is headquartered in a state adopting an anti-SLAPP law in the following year. After weighting, observable firm- and state-level characteristics are balanced across treatment and control groups, and the estimated decline in firms' COE remains robust.

Treatment strength. Finally, we examine whether stronger anti-SLAPP statutes produce larger effects. States differ in both procedural protections afforded to defendants and the scope of protected speech. Using the Institute for Free Speech's 2023 anti-SLAPP report card, we classify states into quartiles based on three measures of statutory strength from Greenberg et al. (2023): the overall score, procedural protections, and the breadth of covered speech. Table OA6 shows that firms' COE declines more in states with stronger anti-SLAPP laws. This dose-response pattern provides additional support for a causal interpretation and is difficult to reconcile with alternative explanations based on broad state-level political, legal, or media trends.

Taken together, these analyses provide consistent evidence that the baseline decline in firms' COE is not driven by treatment assignment choices, measurement choices, omitted variables, alternative estimators, or concurrent state-level reforms.

4.3 Evidence on the information-suppression friction

The results indicate that limiting firms' ability to suppress unfavorable information lowers firms' COE. This interpretation, however, requires evidence that anti-SLAPP laws actually alter the information-suppression friction facing public firms. We therefore examine two complementary predictions. First, anti-SLAPP adoption should increase the amount of unfavorable information

¹³These controls include the FOIA (Freedom of Information Act) law index, data breach notification laws, intellectual property rights protection, pre-employment background-check restrictions, ban-the-box laws, salary history bans, paid sick leave laws, paid family and medical leave laws, investor tax credits, opioid prescription-limit laws, smoking bans, and LGBTQ nondiscrimination laws. Our results are also robust to controlling for incorporation-state universal demand laws, corporate opportunity waiver laws, and anti-takeover laws.

generated by outsiders about public firms. Second, if the laws meaningfully constrain firms’ ability to deter public criticism through litigation, they should alter firms’ criticism-related legal proceedings and reputational-risk disclosures. Together, these analyses establish the informational-first stage supporting the view that investors price firms’ ability to suppress unfavorable information.

4.3.1 Unfavorable outside information

Anti-SLAPP laws reduce the expected litigation costs faced by employees, journalists, consumers, activists, and other outside speakers. If litigation threats previously deterred unfavorable speech, anti-SLAPP adoption should increase the amount of unfavorable information that becomes publicly observable. We examine this prediction using two distinct information channels: firm-related press releases from RavenPack and employee assessments from Glassdoor. RavenPack captures unfavorable information disseminated through public communications, whereas Glassdoor captures assessments from current and former employees with potentially private information about workplace conditions, managerial quality, internal operations, and firm prospects.

We first construct two measures of negative press releases using RavenPack. We retain releases with entity and event relevance scores of 100, thereby restricting the sample to releases directly related to the firm and the identified event. We classify sentiment using RavenPack’s Event Sentiment Score (ESS) and Composite Sentiment Score (CSS). A release is classified as negative when the relevant sentiment score is below zero, with zero indicating neutral sentiment. $NegPr-ESS$ and $NegPr-CSS$ equal the natural logarithm of one plus the number of negative press releases associated with the firm during its fiscal year under the ESS and CSS classifications, respectively.

We also examine three employee assessments from Glassdoor. *Business Outlook* equals three when the reviewer reports a positive outlook, two for a neutral outlook, and one for a negative outlook. *Recommend* equals one when the reviewer would recommend the firm to a friend and zero otherwise. *CEO Rating* equals three for approval, two for no opinion, and one for disapproval. Lower values therefore indicate less favorable employee assessments.

The results in Table 6 consistently indicate that anti-SLAPP adoption increases unfavorable outside information across both data sources. Firms receive significantly more negative press releases under both the $NegPr-ESS$ and $NegPr-CSS$ sentiment classifications. The estimated

effects imply increases ranging from approximately 4.2% to 11.9%. Employee assessments likewise become systematically less favorable following adoption. *Business Outlook* declines by 2.6% to 3.1% relative to the sample mean, the likelihood that employees recommend the firm falls by 9.8% to 11.2%, and CEO approval ratings decline by 3.5% to 3.8%. The consistency of these findings across public communications and employee-generated information is particularly important because the two sources reflect different speakers, information sets, and communication channels.

4.3.2 Litigation exposure and reputational risk

The evidence above shows that anti-SLAPP laws increase unfavorable outside information. A remaining question is whether the laws are sufficiently relevant to the litigation environment of public firms to affect capital-market outcomes. Even if the laws generally encourage outside speech, litigation involving public criticism could be too uncommon or economically unimportant among listed firms to influence investors' required returns. We therefore examine firms' disclosure of criticism- and speech-related legal proceedings in Item 3 of their 10-K filings and reputational risk in Item 1A. Whereas the RavenPack and Glassdoor analyses capture changes in unfavorable outside information, these 10-K disclosures capture changes in firms' litigation exposure and the reputational consequences of outside criticism.

We construct three text-based indicators from the Item 3 (*Legal Proceedings*) and Item 1A (*Risk Factors*) sections of annual reports. *Item 3 Criticism* equals one if a firm's Item 3 section contains at least one term related to external criticism. *Item 3 Speech* equals one if the Item 3 section contains at least one term related to legal or contractual tools that restrict information flows. *Item 1A Reputation* equals one if the firm's Item 1A section contains at least one term related to reputational risk. Appendix B provides detailed variable definitions. If anti-SLAPP laws reduce firms' ability to deter outside criticism through litigation, firms should become less entangled in criticism- and speech-related legal proceedings while placing greater emphasis on reputational risk. Accordingly, we expect declines in *Item 3 Criticism* and *Item 3 Speech* together with an increase in *Item 1A Reputation*.

Table 7 reports results consistent with these predictions. Firms become significantly less likely to disclose legal proceedings involving external criticism or public speech following anti-SLAPP

adoption, while becoming significantly more likely to disclose reputational risk. The magnitudes are economically meaningful, implying declines of approximately 7.2% to 8.2% in criticism- and speech-related litigation disclosures and increases of roughly 4.1% to 5.4% in reputational-risk disclosures relative to the corresponding sample means.¹⁴

Taken together, Sections 4.3.1 and 4.3.2 establish the informational-first stage underlying our interpretation of the baseline results. Anti-SLAPP adoption is followed by more unfavorable information from outside sources and by systematic changes in firms’ criticism-related litigation and reputational-risk disclosures. Collectively, these findings indicate that anti-SLAPP laws alter an economically meaningful information-suppression friction for public firms rather than merely representing formal legal changes with limited relevance for capital markets.

4.4 Evidence supporting the *Uncertainty Resolution Hypothesis*

We next examine whether the decline in firms’ COE after anti-SLAPP adoption stems from stronger speech protections reducing investors’ uncertainty about hidden firm risks, as predicted by the *Uncertainty Resolution Hypothesis*. If so, uncertainty about hidden firm risks should decline. Investors should therefore face less information asymmetry, perceive firms as less risky, more accurately distinguish firms with hidden risks from those without, and be less exposed to stock-price crashes arising from the release of accumulated bad news.

4.4.1 Anti-SLAPP legislation and information asymmetry

A first implication of the *Uncertainty Resolution Hypothesis* is that information asymmetry between firms and investors should decline following anti-SLAPP adoption. We examine this prediction using three market-based measures: bid-ask spreads, the illiquidity measure of Amihud (2002), and the probability of informed trading (PIN) of Easley, Kiefer, O’Hara, and Paperman (1996). Bid-ask spreads capture adverse selection costs faced by market makers; illiquidity measures the price impact of trades; and PIN estimates the likelihood that trades reflect private information

¹⁴Item 3 requires disclosure of *material* legal proceedings and does not identify whether the firm is the plaintiff or defendant. We therefore interpret the two Item 3 measures as capturing firms’ overall entanglement in criticism- and speech-related litigation, rather than direct counts of firm-initiated SLAPPs.

rather than liquidity trading. Together, these measures capture complementary dimensions of information asymmetry in equity markets.

Table 8 reports results consistent with this prediction. Bid-ask spreads, illiquidity, and PIN all decline significantly following anti-SLAPP adoption. In the fully specified regressions, bid-ask spreads decline by 0.101 (t -stat = -5.49), illiquidity declines by 0.084 (t -stat = -1.91), and PIN declines by 0.442 (t -stat = -2.24). Relative to their respective sample means, these estimates correspond to economically meaningful reductions. Across specifications, bid-ask spreads decline by 7.2% to 12.2%, illiquidity declines by 26.6% to 38.9%, and PIN declines by 2.5% to 4.2%.

4.4.2 Anti-SLAPP legislation and firm risk

A second implication of the *Uncertainty Resolution Hypothesis* is that investors should perceive firms as less risky. As unfavorable outside information becomes more likely to reach investors, uncertainty surrounding firms' underlying fundamentals should decline, lowering both realized and expected measures of firm risk. We examine this prediction using four complementary measures: total stock return volatility, implied volatility from equity options, idiosyncratic volatility, and market beta. These measures capture overall return uncertainty, investors' expectations of future volatility, firm-specific risk, and systematic market risk, respectively.

Table 9 reports results consistent with this prediction. Total return volatility, implied volatility, idiosyncratic volatility, and market beta all decline significantly following anti-SLAPP adoption. In the fully specified regressions, total volatility declines by 1.2 percentage points (t -stat = -3.08), implied volatility declines by 1.4 percentage points (t -stat = -2.14), idiosyncratic volatility declines by 1.0 percentage point (t -stat = -2.95), and market beta declines by 0.043 (t -stat = -2.35). Relative to their respective sample means, these estimates correspond to economically meaningful reductions. Across specifications, total volatility declines by 2.7% to 4.5%, implied volatility declines by 2.8% to 3.4%, idiosyncratic volatility declines by 2.5% to 4.5%, and market beta declines by 3.9% to 4.4%.

4.4.3 Anti-SLAPP legislation and firm undervaluation

Lower uncertainty about hidden firm risks should also improve valuation accuracy. When investors cannot distinguish firms concealing unfavorable information from otherwise similar firms without hidden risks, they may discount both groups (Diamond and Verrecchia, 1991). By making adverse outside information more likely to reach the market, anti-SLAPP laws should reduce this pooling problem, allowing investors to better distinguish firms with hidden risks from firms without such risks and thereby improving valuation accuracy. We measure undervaluation using the industry-specific market-to-book model of Hoberg and Phillips (2010) and the market-value model of Rhodes-Kropf, Robinson, and Viswanathan (2005). For each model, we define an indicator equal to one when the firm’s observed valuation falls below its model-implied valuation.¹⁵

Table 10 reports results consistent with this prediction. Across both valuation models, anti-SLAPP adoption significantly reduces the probability that a firm is classified as undervalued. In the fully specified regressions, the probability of undervaluation declines by 2.2 percentage points using the model of Hoberg and Phillips (2010) (t -stat = -2.61) and by 2.1 percentage points using the model of Rhodes-Kropf et al. (2005) (t -stat = -2.70). Relative to the corresponding sample means, these estimates imply economically meaningful reductions. Across specifications, the estimated declines range from 5.3% to 7.2% using the model of Hoberg and Phillips (2010) and from 9.1% to 10.4% using the model of Rhodes-Kropf et al. (2005).

4.4.4 Anti-SLAPP legislation and stock price crash risk

Finally, if stronger speech protections reduce the accumulation of hidden bad news, they should also reduce stock price crash risk. Crash risk arises when unfavorable information remains hidden until it accumulates and is released abruptly (e.g., Chen, Hong, and Stein, 2001; Jin and Myers, 2006; Hutton, Marcus, and Tehranian, 2009; Kim, Li, and Zhang, 2011a,b). If anti-SLAPP laws make outside criticism harder to suppress, unfavorable information should be reflected in stock prices more gradually, reducing the likelihood of extreme negative returns and investors’ exposure to hidden firm risks. We examine four standard crash-risk measures based on firm-specific weekly returns: negative conditional skewness ($NCSKEW$), downside-to-upside volatility ($DUVOL$), an

¹⁵Appendix A describes the valuation models and construction of the undervaluation measures.

indicator for whether the firm experiences at least one crash week during the year (*Crash*), and the number of crash weeks minus the number of jump (positive) weeks (*CrashCount*).¹⁶

Table 11 reports results consistent with this prediction. Anti-SLAPP adoption significantly reduces all four crash-risk measures. In the fully specified regressions, *NCSKEW* declines by 0.042 ($t\text{-stat} = -2.34$), *DUVOL* declines by 0.023 ($t\text{-stat} = -2.36$), the probability of experiencing at least one crash week declines by 3.2 percentage points ($t\text{-stat} = -3.36$), and *CrashCount* declines by 0.046 ($t\text{-stat} = -2.83$). These estimates are economically meaningful. Across specifications, *NCSKEW* declines by 3.2% to 6.1% of a standard deviation, *DUVOL* declines by 2.6% to 5.0% of a standard deviation, the probability of experiencing a crash week declines by 12.3% to 17.1% relative to the sample mean, and *CrashCount* declines by 5.7% to 7.9% of a standard deviation.

Overall, the evidence in Section 4.4 consistently supports the *Uncertainty Resolution Hypothesis*. Following anti-SLAPP adoption, firms experience lower information asymmetry, lower realized and expected risk, less undervaluation, and lower stock price crash risk. These findings suggest that stronger protections for outside speakers reduce investors' uncertainty about hidden firm risks, providing a coherent explanation for the observed decline in firms' COE.

4.5 Cross-sectional evidence

While the previous results support the *Uncertainty Resolution Hypothesis* by showing that anti-SLAPP adoption reduces information asymmetry, firm risk, undervaluation, and stock price crash risk, we next examine several cross-sectional predictions implied by this hypothesis.

4.5.1 *Ex ante* information environment

We first examine whether the COE effect is stronger for firms with poorer *ex ante* information environments. If anti-SLAPP laws lower financing costs by reducing uncertainty about hidden firm risks, the effect should be largest when investors face greater difficulty assessing firm fundamentals. We examine four proxies for information quality: management earnings guidance, analyst forecast error, discretionary accruals, and R&D intensity. These measures capture, respectively, firms'

¹⁶Appendices A and B provide variable construction and definitions, respectively. Because *NCSKEW*, *DUVOL*, and *CrashCount* can take negative values, we scale their economic magnitudes by their standard deviations rather than their means.

willingness to provide forward-looking information, the precision of analyst expectations, the reliability of financial reporting, and the opacity of investment opportunities.¹⁷

Table 12 reports results consistent with this prediction. The decline in COE following anti-SLAPP adoption is significantly larger among firms that do not provide management earnings guidance, have larger analyst forecast errors, report higher absolute discretionary accruals, and are more R&D intensive. Differences in the ATTs between high- and low-opacity firms are statistically significant at the 1% level or better across all specifications.

4.5.2 *Ex ante* internal governance

We next examine whether the COE effect is stronger for firms with weaker internal governance. If outside criticism serves as an external monitoring mechanism, stronger protections for outside speakers should be especially valuable when internal monitoring is weaker and outside monitoring is likely to play a greater role. We consider three governance measures: board independence, board busyness, and CEO pay-performance sensitivity. Lower board independence, greater board busyness, and lower CEO pay-performance sensitivity indicate weaker monitoring or weaker alignment between managers and shareholders.¹⁸

Table 13 reports results consistent with this prediction. The decline in COE is significantly larger for firms with less independent boards, a higher proportion of busy directors, and lower CEO pay-performance sensitivity. Differences in the ATTs between weak- and strong-governance firms are statistically significant across all specifications.

4.5.3 Exposure to public scrutiny

Finally, we examine whether the COE effect is stronger for firms facing greater public scrutiny. If anti-SLAPP laws matter by protecting the production and dissemination of unfavorable outside information, their effects should be largest when outside criticism is more likely to arise and reach

¹⁷*NoMngFrst* is an indicator equal to one if the firm does not provide earnings guidance during the year. *FrstError* is the absolute value of actual EPS minus the mean consensus analyst forecast, scaled by the absolute value of the mean consensus forecast. *Abs(DiscAcc)* is the absolute value of discretionary accruals, measured following Francis et al. (2005). *R&D* is R&D scaled by sales.

¹⁸Board independence is the percentage of independent directors. Board busyness is the percentage of directors serving on three or more boards. CEO pay-performance sensitivity, or *Delta*, measures the sensitivity of CEO wealth to changes in stock price. These measures follow prior work on board monitoring and managerial incentives (Core and Guay, 2002; Coles, Daniel, and Naveen, 2006; Fich and Shivdasani, 2006; Nguyen and Nielsen, 2010).

investors. We use three indicators of public scrutiny: lagged media coverage from RavenPack, whether the firm has a Glassdoor rating, and a 10-K-based measure of criticism related to legal and reputational exposure. These measures capture exposure to information intermediaries, employee voice, and criticism-related legal and reputational pressure, respectively.¹⁹

Table 14 reports results consistent with this prediction. The decline in COE is significantly larger among firms receiving greater media attention, firms rated on Glassdoor, and firms with greater criticism-related legal or reputational exposure reflected in their 10-K disclosures. ATT differences are statistically significant across all specifications, at the 1% level for the media and Glassdoor partitions and at better than the 5% level for the 10-K-based partition.

Taken together, the cross-sectional results in Section 4.5 indicate that anti-SLAPP laws matter most when investors face greater uncertainty about firm fundamentals, where outside information is especially valuable for resolving that uncertainty, and when firms are more exposed to outside information production and public criticism. The findings therefore provide additional support consistent with the *Uncertainty Resolution Hypothesis*.

4.6 Complementary sources of uncertainty resolution

The preceding evidence is most consistent with anti-SLAPP laws lowering firms' COE by reducing investors' uncertainty about hidden firm risks. That uncertainty, however, may decline through more than one complementary channel. First, stronger protections for outside speakers may increase the production and dissemination of firm-specific information. Recent work shows that anti-SLAPP laws strengthen employee whistleblowing (Böke et al., 2026), facilitate crowd-sourced investment research (Schafhäutle and Veenman, 2024), and improve the informativeness of ESG disclosure (Griffin et al., 2024a). By lowering the expected legal costs of public criticism, anti-SLAPP laws may encourage employees, investors, analysts, journalists, and other stakeholders to disclose information that would otherwise remain hidden. A broader and more credible flow of

¹⁹The 10-K-based measure, *Litigation*, is the sum of three indicator variables—*Item 3 Criticism*, *Item 3 Speech*, and *Item 1A Reputation*—each of which equals one if the firm's Item 3 (Legal Proceedings) or Item 1A (Risk Factors) section contains the corresponding criticism-, speech-, or reputation-related language, and zero otherwise. The sum therefore ranges from zero to three, with higher values indicating that external criticism, public speech, and reputational pressure are more salient for the firm before anti-SLAPP adoption.

outside information can help investors better assess firm fundamentals, distinguish firms with hidden risks from those without, and reduce the uncertainty premium reflected in firms' COE.

Second, stronger speech protections may alter managerial behavior before unfavorable information reaches the market. As outside stakeholders become more able to expose misconduct, operational problems, or concealed bad news, managers face higher expected costs of suppression. Consistent with this view, [Lee et al. \(2026\)](#) find that managers become less likely to hoard bad news following anti-SLAPP adoption. Related evidence also suggests that public scrutiny strengthens whistleblowing, regulatory oversight, and corporate activism ([Heese and Pérez-Cavazos, 2021](#); [Chen et al., 2026](#); [Wu et al., 2026](#)). Greater exposure to outside scrutiny may therefore encourage managers to address problems earlier, strengthen internal reporting and compliance, and disclose adverse developments more promptly. These responses reduce the accumulation of hidden bad news and further diminish investors' uncertainty about firms' underlying fundamentals.

These channels are complementary rather than competing. Anti-SLAPP laws may simultaneously increase the amount of outside information available to investors and reduce managers' incentives to conceal unfavorable information. Although our analyses do not separately identify the relative importance of these channels, our findings are consistent with the broader interpretation that stronger protections for outside speakers improve investors' ability to evaluate hidden firm risks, thereby reducing the risk premium investors require to hold firms' equity.

5 Conclusion

While investors price information supplied by firms, it is not known whether and how investors price firms' ability to suppress unfavorable information generated by outsiders. In this paper, we address this gap by exploiting the staggered adoption of state anti-SLAPP laws, which protect outside speakers from litigation intended to silence public criticism. Unlike disclosure mandates or investor-protection rules, anti-SLAPP statutes do not directly regulate capital markets. Instead, they alter firms' ability to suppress unfavorable information by reducing the expected cost of producing and disseminating adverse information about firms.

We find that the adoption of anti-SLAPP laws reduces firms' COE by approximately 45.5 basis points. The evidence is robust to a broad set of alternative specifications, identification

strategies, and placebo tests. We also show that the laws meaningfully constrain public firms' ability to suppress unfavorable outside information: after adoption, firms disclose less criticism- and speech-related litigation in Item 3 of their 10-K filings, and report greater reputational risk in Item 1A. Further, the evidence supports an uncertainty resolution interpretation. Following anti-SLAPP adoption, firms experience lower information asymmetry, realized and implied risk, undervaluation, and stock price crash risk, while the decline in the COE is strongest among firms with weaker information environments, weaker internal governance, and greater exposure to public scrutiny—settings in which outside information should be especially valuable. Taken together, these findings suggest that reducing firms' ability to suppress unfavorable information lowers financing costs by reducing investors' uncertainty about hidden risks in firms.

A natural question is why firms do not voluntarily provide the information that appears to reduce their financing costs. Our evidence points to an important distinction between firm-supplied disclosure and information generated by outsiders. Managers may incur private costs for releasing unfavorable information, including reputational damage, exposure to litigation, regulatory scrutiny, or career concerns. Investors may also discount firm-initiated disclosures when managers have incentives to withhold or strategically frame bad news. By contrast, outside speakers often possess different information and incentives than managers do. Anti-SLAPP laws relax this friction by protecting those speakers from retaliatory litigation, allowing information that managers would prefer to keep hidden to reach the market. The resulting transparency is therefore partly external and involuntary, helping explain why legal protections for public criticism can affect firms' COE.

Our findings contribute to research on information risk, external monitoring, and legal institutions. More broadly, they suggest that investors value institutional environments that limit firms' ability to suppress unfavorable information and facilitate the production and dissemination of value-relevant information by outsiders. Ultimately, capital markets are shaped not only by what firms choose to disclose, but also by whether outsiders can credibly reveal information that managers would prefer to keep hidden.

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Appendix A. Estimation of Implied Cost of Equity, Firm Valuation Models, and Crash Risk

Implied Cost of Equity ([Gebhardt et al., 2001](#))

Following previous research in this literature, we outline the estimation of a firm’s implied COE using the *ex ante* approach of [Gebhardt et al. \(2001\)](#). We begin by defining the variables used in these models.

- P_t^* = Market price of a firm’s common stock at time t . We use the price in June following the latest fiscal year-end to compute P_t^* .
- B_t = Book value of equity from the most recent financial statements at time t .
- $FEPS_{t+i}$ = Median forecasted earnings per share (EPS) from IBES or derived EPS forecasts for year $t+i$.
- $POUT$ = Forecasted dividend payout ratio. We use the ratio of the indicated annual dividends from IBES to $FEPS_{t+1}$ to measure the forecasted payout ratio. If $FEPS_{t+1}$ is negative, we assume a return on assets of 6% to calculate earnings. $POUT$ is winsorized to be between 0 and 1.
- $FROE_{t+i}$ = Forecasted return on equity for year $t+i$. For the first three years, $FROE_{t+i} = FEPS_{t+i}/B_{t+i-1}$; beyond the third year, $FROE_{t+i}$ declines linearly to an industry-specific equilibrium ROE, as described below.
- R_{GLS} = The implied cost of equity, defined as the internal rate of return that equates the discounted value of expected future residual income to the current stock price.

The [Gebhardt et al. \(2001\)](#) model solves for R_{GLS} in

$$P_t^* = B_t + \sum_{i=1}^{T-1} \left[\frac{(FROE_{t+i} - R_{GLS}) \times B_{t+i-1}}{(1 + R_{GLS})^i} \right] + \left[\frac{(FROE_{t+T} - R_{GLS}) \times B_{t+T-1}}{(1 + R_{GLS})^{T-1} R_{GLS}} \right]. \quad (\text{A-1})$$

We employ IBES analysts’ predictions as a measure of the market’s anticipation of the firm’s earnings in the upcoming three years. We measure market expectations for earnings by assuming a linear decrease in the future return on equity ($FROE$), converging to an equilibrium return on equity (ROE) from the fourth year to the T th year. This equilibrium ROE is determined using a historical, 10-year, industry-specific median ROE. The computation of ROE involves scaling the income available for common shareholders (Compustat data item IBC) by the lagged total book value of equity (Compustat data item CEQ). To calculate the industry ROE, we categorize all firms—including those with negative ROEs—into the Fama-French 48 industries. In instances where the industry ROE falls below the risk-free rate, we set the industry ROE equal to the risk-free rate. The future book value of equity is estimated by assuming the clean surplus relation (i.e., $B_{t+1} = B_t + FEPS_{t+1} - DPS_{t+1}$). The future dividend, DPS_{t+i} , is calculated by multiplying $FEPS_{t+i}$ by $POUT$. We assume that $T = 12$. We use a numerical approximation program to solve for the R_{GLS} that equates the right- and left-hand sides of Eq. (A-1) within a tolerance of \$0.001.

Firm Valuation Models

A.1 Hoberg and Phillips (2010) relative valuation model

Following the framework of Hoberg and Phillips (2010), we estimate an industry-level present value model for 3-digit SIC industries using firm-level data from year $t - 10$ to $t - 1$. For each firm i operating in industry j , we regress the market-to-book asset ratio, $(M/B)_{i,k}$, on a set of explanatory variables: firm age ($FIRMAGE$), a dividend dummy (DD), financial leverage (LEV), firm size ($SIZE$), return on assets (ROA), and the volatility of profitability ($VOLP$). Because $VOLP$ does not vary across time within a firm, the estimation is performed on an unbalanced panel using firm random effects models. The econometric specification is:

$$\begin{aligned} \left(\frac{M}{B}\right)_{i,k} = & a + b FIRMAGE_{i,k} + c DD_{i,k} + d LEV_{i,k} \\ & + e \ln(SIZE_{i,k}) + f VOLP_i + g ROA_{i,k}, \quad k = t - 10, \dots, t - 1, \end{aligned} \quad (A-2)$$

where M/B is market value of assets scaled by book value of assets $[(prcc_f \times csho + at - ceq)/at]$. $FIRMAGE$ is one minus the reciprocal of one plus the number of years since the firm first appeared on CRSP. DD is an indicator for whether common dividends (dvc) are positive. LEV is the ratio of long-term debt ($dltt$) to total assets (at). $SIZE$ is book assets (at) in 2020 dollars. ROA is measured as operating income before depreciation and amortization ($oibdp$) over lagged book assets (at). $VOLP$ is the standard deviation of the residuals from industry-level regressions of firm ROA on lagged ROA .

Using the estimated coefficients from the rolling 10-year industry regressions, we generate predicted values of the market-to-book asset ratio for year t . Specifically, for each year, we apply the parameter estimates from the prior 10-year window ($t - 10$ to $t - 1$) to the contemporaneous firm characteristics to obtain predicted market-to-book asset ratio values. Relative valuation is defined as the deviation of the log of the actual M/B ratio from the log of its predicted value.

A.2 Rhodes-Kropf et al. (2005) relative valuation model

Following the framework of Rhodes-Kropf et al. (2005), we estimate an industry-level valuation model for 3-digit SIC industries using firm-level data from year $t - 10$ to $t - 1$. For each firm i operating in industry j , we estimate the following model:

$$\begin{aligned} \ln(M)_{i,k} = & a + b \ln(B)_{i,k} + c \ln(|NI|)_{i,k} + d NEG_{i,k} \times \ln(|NI|)_{i,k} \\ & + e MLEV_{i,k}, \quad k = t - 10, \dots, t - 1, \end{aligned} \quad (A-3)$$

where M is market value of equity ($prcc_f \times csho$), B is book value of equity (seq , $ceq + pstk$, or $at - lt$, in that order) plus $txditc$ minus $pstkrv$, $pstkl$, or $pstk$ (in that order). $|NI|$ is the absolute value of net income (ni). NEG is an indicator variable equal to one if net income is negative. $MLEV$ is the book value of debt ($dltt + dlc$) scaled by market value of assets ($prcc_f \times csho + at - ceq$). All non-ratio variables are converted to 2020 dollars.

Using the estimated coefficients from the rolling 10-year industry regressions, we generate predicted values of the market value of equity for year t . Specifically, for each year, we apply the parameter estimates from the prior 10-year window ($t - 10$ to $t - 1$) to the contemporaneous firm characteristics to obtain predicted market value of equity. Relative valuation is defined as the deviation of the log of the actual market value of equity from the log of its predicted value.

Crash Risk

In line with prior research on crash risk (Chen et al., 2001; Jin and Myers, 2006; Hutton et al., 2009; Kim et al., 2011a,b), we construct four firm-specific measures of stock price crash risk for each firm-year observation: (i) *NCSKEW*, which represents the negative conditional skewness of firm-specific weekly returns over the year for each firm; (ii) *DUVOL*, the ratio of downside volatility to upside volatility of firm-specific weekly returns during the year for each firm; (iii) *Crash*, a binary variable indicating whether a firm experienced at least one stock price crash week during the year; and (iv) *CrashCount*, which quantifies the total number of negative minus positive firm-specific crash weeks within the year for each firm. Consistent with prior studies (e.g., Hutton et al., 2009), we calculate firm-specific residual weekly returns from the extended market index model, estimated separately for each firm-year, where w indexes the weeks of year t :

$$r_{j,w} = \alpha_j + \beta_{1,j}r_{m,w-2} + \beta_{2,j}r_{ind,w-2} + \beta_{3,j}r_{m,w-1} + \beta_{4,j}r_{ind,w-1} + \beta_{5,j}r_{m,w} + \beta_{6,j}r_{ind,w} + \beta_{7,j}r_{m,w+1} + \beta_{8,j}r_{ind,w+1} + \beta_{9,j}r_{m,w+2} + \beta_{10,j}r_{ind,w+2} + \epsilon_{j,w}, \quad (\text{A-4})$$

where $r_{j,w}$ represents the weekly return of stock j in week w , $r_{m,w}$ denotes the weekly return of the CRSP value-weighted market index, and $r_{ind,w}$ corresponds to the weekly return of the value-weighted 2-digit SIC industry index. To address non-synchronous trading issues, we enhance the traditional market index model by incorporating lead and lag terms, following the approach of Dimson (1979). This regression decomposes a firm's return into two components: one that aligns with overall market movements and another that reflects firm-specific shocks ($\epsilon_{j,w}$). Consistent with prior studies (e.g., Chen et al., 2001; Hutton et al., 2009), the firm-specific weekly return for firm j in week w is calculated as $W_{j,w} = \ln(1 + \epsilon_{j,w})$. Applying the natural logarithm transformation helps mitigate positive skewness in the stock return distribution, thereby enhancing the symmetry of $W_{j,w}$.

A.3 Negative conditional skewness

The first measure for assessing firm-specific crash risk is *NCSKEW* $_{j,t}$, which represents the negative conditional skewness of firm-specific weekly returns. In line with Chen et al. (2001), *NCSKEW* $_{j,t}$ is computed as the negative third central moment of $W_{j,w}$ divided by the cubed standard deviation of $W_{j,w}$:

$$NCSKEW_{j,t} = -\frac{n_{j,t}(n_{j,t} - 1)^{1.5} \sum_{w=1}^{n_{j,t}} W_{j,w}^3}{(n_{j,t} - 1)(n_{j,t} - 2) \left(\sum_{w=1}^{n_{j,t}} W_{j,w}^2 \right)^{1.5}}, \quad (\text{A-5})$$

where $n_{j,t}$ represents the count of firm-specific weekly returns available for firm j during year t . To enable comparisons across firm-specific returns with different levels of variance, the raw third central moment is scaled by a normalization factor—the cubed standard deviation in the denominator. The negative sign in Eq. (A-5) ensures that a rise in *NCSKEW* $_{j,t}$ indicates higher stock price crash risk for firm j in fiscal year t , reflecting a more negatively skewed return distribution.

A.4 Downside-to-upside volatility

The second measure for firm-specific crash risk is $DUVOL_{j,t}$, as introduced by [Chen et al. \(2001\)](#), which measures the ratio of downside volatility to upside volatility:

$$DUVOL_{j,t} = Ln \left(\frac{\left((n_{u,j,t} - 1) \sum_{w=1}^{n_{d,j,t}} W_{j,w}^2 \right)^{0.5}}{\left((n_{d,j,t} - 1) \sum_{w=1}^{n_{u,j,t}} W_{j,w}^2 \right)^{0.5}} \right), \quad (\text{A-6})$$

where $n_{u,j,t}$ ($n_{d,j,t}$) represents the count of up (down) weeks for firm j 's stock during year t , and the sums in the numerator and denominator run over the down weeks and up weeks, respectively. Up weeks are defined as weeks in which the firm-specific weekly return exceeds the annual mean, while down weeks are weeks in which it falls below the annual mean. For each firm j in fiscal year t , $DUVOL_{j,t}$ is calculated as the natural logarithm of the ratio between the standard deviations of $W_{j,w}$ during down weeks and up weeks. Similar to $NCSKEW_{j,t}$, a higher value of $DUVOL_{j,t}$ indicates greater stock price crash risk for firm j in year t .

A.5 Crash dummy and counts

The third measure of firm-specific crash risk, $Crash_{j,t}$, is a binary variable that equals one if a firm experiences one or more firm-specific crash weeks during year t , and zero otherwise. Consistent with [Hutton et al. \(2009\)](#), crash weeks for firm j in year t are identified as those weeks in which the firm-specific weekly return $W_{j,w}$ falls 3.09 standard deviations below the mean firm-specific weekly return for the year, corresponding to a 0.1% frequency under a normal distribution. $Crash_{j,t}$ reflects the probability of a stock price crash for a firm within a given year. Additionally, the fourth measure of firm-specific crash risk, $CrashCount_{j,t}$, quantifies the total number of negative minus positive firm-specific crash weeks for firm j during year t . A positive crash week is a week in which the firm-specific weekly return rises 3.09 standard deviations above the mean firm-specific weekly return for the year.

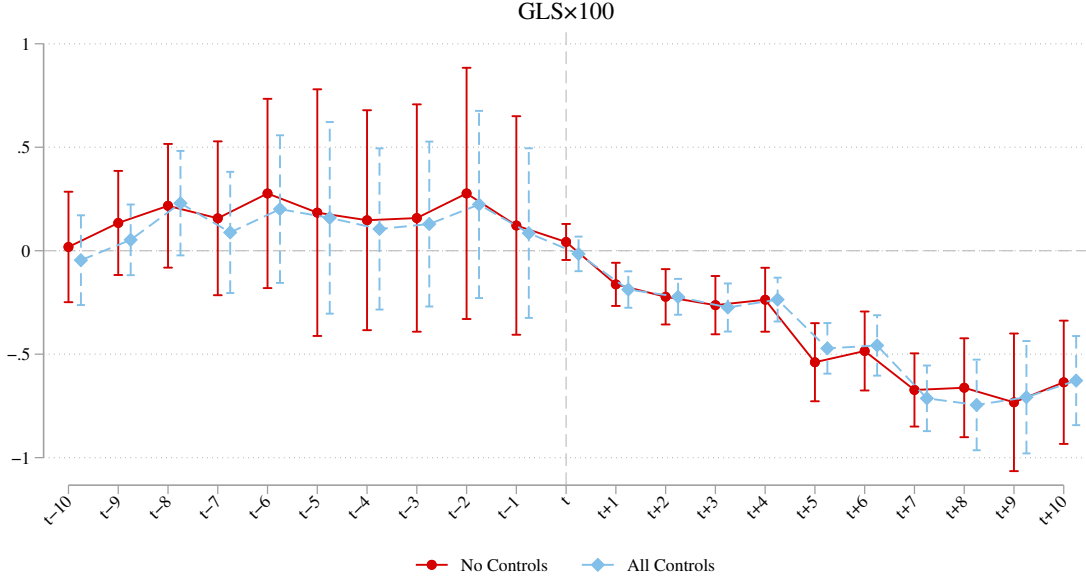
Appendix B. Variable Definitions

This table provides variable definitions. Variables not included here are defined in the corresponding table captions. Compustat and CRSP variable names are italicized as per standard convention.

Variable	Definition
%ΔGDP	One-year percent change in per capita state-level gross domestic product.
%Busy	Percentage of directors on a board that sit on three or more boards during a year.
%Indep	Percentage of independent directors on a board.
Abs(DiscAcc)	Absolute value of discretionary accruals calculated following Francis et al. (2005) .
Age	The average age of a state's population. Data are from the Current Population Survey.
Analysts	Number of analysts providing an annual earnings forecast.
Beta	The beta coefficient from a regression of a firm's weekly stock returns on CRSP value-weighted market portfolio returns over the year (i.e., a firm's exposure to systematic risk).
BidAsk	The average bid-ask spread of a firm's stock over a year.
BLEV	Book value of debt scaled by book value of assets $[(dltt+dlc)/at]$.
BM	Book value of equity divided by market value of equity $[ceq/(prcc_f \times csho)]$.
CashFlow	Income before extraordinary items plus depreciation and amortization scaled by book value of assets $[(ib+dp)/at]$.
Crash	An indicator variable equal to one for firms experiencing at least one stock price crash week during a year, and zero otherwise.
CrashCount	The number of negative minus positive firm-specific crash weeks for each firm-year.
Delta	The dollar change in CEO stock and option wealth given a 1% change in stock price. Data are from Lalitha Naveen's website (Coles et al., 2006).
DemGov	An indicator variable that equals one if a state governor is from the Democratic Party, and zero otherwise.
DUVOL	The downside-to-upside volatility of firm-specific weekly returns during each year for each firm.
EDUC	The fraction of a state's population aged 25 or older with a bachelor's degree. Data are from the Current Population Survey.
FrstDisp	Standard deviation of analysts' annual earnings forecasts scaled by the firm's stock price. Missing values are set to zero, and we add one before taking the natural logarithm of the variable.
FrstError	Absolute value of actual earnings per share less the mean consensus analyst forecast, scaled by the absolute value of the mean consensus analyst forecast.
GDP	Per capita state-level gross domestic product in 2020 dollars.
GLS	A firm's implied COE based on the model from Gebhardt et al. (2001) .
IdioVol	Standard deviation of weekly idiosyncratic stock returns over a firm's fiscal year and annualized. Idiosyncratic returns are the residuals from a regression of a firm's weekly stock returns on CRSP value-weighted market portfolio returns over the year.
Illiq	The average Amihud (2002) illiquidity measure of a firm's stock over a year, which equals the absolute value of daily returns multiplied by 100,000 and then scaled by trading volume.
ImpVol	Annualized implied volatility of a firm's stock returns, derived by averaging daily implied volatility from 30-day options over a year and requiring at least 21 trading days with valid implied volatility.
Item 1A Reputation	An indicator equal to one if a firm's Item 1A (Risk Factors) section contains at least one term related to reputation risk (e.g., <i>reputation</i> , <i>reputational harm</i> , <i>brand damage</i> , <i>public perception</i> , <i>customer/consumer trust</i> , <i>loss of goodwill or trust</i>), and zero otherwise. Item 1A disclosure is required for fiscal years ending on or after December 1, 2005. Data are from 10-K filings on SEC EDGAR.

Item 3 Criticism	An indicator equal to one if a firm's Item 3 (Legal Proceedings) section contains at least one term related to external criticism (e.g., <i>public criticism</i> , <i>negative publicity</i> , <i>media scrutiny</i> , <i>customer/consumer complaints</i> , <i>activist campaigns</i> , <i>boycotts</i> , <i>protests</i> , <i>watchdogs</i> , or <i>advocacy groups</i>), and zero otherwise. Data are from 10-K filings on SEC EDGAR.
Item 3 Speech	An indicator equal to one if a firm's Item 3 (Legal Proceedings) section contains at least one term related to legal or contractual tools that restrict information flows (e.g., <i>online review</i> , <i>consumer review</i> , <i>customer review</i> , <i>comment</i> , <i>posting</i> , <i>publication</i> , <i>published</i> , or <i>social media</i>), and zero otherwise. Data are from 10-K filings on SEC EDGAR.
LTGRate	Analysts' consensus forecast of a firm's long-term earnings growth rate.
Marijuana	An indicator variable that equals one if a firm is headquartered in a state that has legalized medical marijuana by year t , and zero otherwise.
Media	The number of news articles covering a firm over the prior three years in the RavenPack news database.
MOM	Buy-and-hold stock return over a firm's fiscal year.
MVE	Market value of equity at the end of each fiscal year ($prcc_f \times csho$) in millions of 2020 dollars.
NCSKEW	The negative conditional skewness of firm-specific weekly returns during each year for each firm.
NoMngFrst	An indicator variable that equals one if a firm does not provide management earnings forecasts/guidance during year t , and zero otherwise.
PIN	Average probability of informed trading following Easley et al. (1996) over a year.
Post (D in Eq. (2))	An indicator variable that equals one if a firm is headquartered in a state that has enacted an anti-SLAPP law by year t , and zero otherwise.
R&D	A firm's research and development expenditures scaled by sales ($xrd/sale$).
TaxRate	State-level highest marginal corporate tax rate.
UnempBen	State-level total unemployment benefits (maximum weekly benefit amount times maximum number of weeks). Data are from the Department of Labor.
UnempRate	State-level fraction of workers that are unemployed.
Volatility	Standard deviation of weekly stock returns over a year and annualized.

Figure 1: Effect of Anti-SLAPP Legislation on COE Over Time



This figure plots the dynamic effect of anti-SLAPP legislation on a firm's implied COE ($GLS \times 100$) over the period from 1987 to 2023, estimated using the imputation-based DiD method. GLS is the implied COE calculated from analyst EPS forecasts using the model of Gebhardt et al. (2001). Lagged firm control variables include $\ln(MVE)$, BM , $BLEV$, MOM , $CashFlow$, $\ln(Analysts)$, $\ln(FrcstDisp)$, and $LTGRate$. Lagged state control variables include $\ln(GDP)$, $\% \Delta GDP$, $TaxRate$, $UnempRate$, $DemGov$, $\ln(UnempBen)$, $Marijuana$, Age , and $EDUC$. All variables are defined in Appendix B. Bars show 95% confidence intervals based on standard errors clustered by state.

Table 1: Anti-SLAPP Legislation Dates

This table lists when and which states enacted anti-SLAPP laws. Dates are from the literature (e.g., [Chemmanur et al., 2020](#); [Griffin et al., 2024a](#); [Jung et al., 2025](#)) and internet searches. States adopting after 2023 fall outside our 1987–2023 sample period and are treated as never-treated.

State	Abbreviation	Anti-SLAPP Year
Arizona	AZ	2006
Arkansas	AR	2005
California	CA	1992
Colorado	CO	2019
Connecticut	CT	2017
Delaware	DE	1992
District of Columbia	DC	2010
Florida	FL	2000
Georgia	GA	1996
Hawaii	HI	2002
Idaho	ID	2025
Illinois	IL	2007
Indiana	IN	1998
Iowa	IA	2025
Kansas	KS	2016
Kentucky	KY	2022
Louisiana	LA	1999
Maine	ME	1995
Maryland	MD	2004
Massachusetts	MA	1994
Michigan	MI	2025
Minnesota	MN	2024
Missouri	MO	2004
Montana	MT	2025
Nebraska	NE	1994
Nevada	NV	1993
New Jersey	NJ	2023
New Mexico	NM	2001
New York	NY	1992
Ohio	OH	2025
Oklahoma	OK	2014
Oregon	OR	2001
Pennsylvania	PA	2000
Rhode Island	RI	1995
South Dakota	SD	2026
Tennessee	TN	1997
Texas	TX	2011
Utah	UT	2001
Vermont	VT	2005
Virginia	VA	2017
Washington	WA	2010

Table 2: Summary Statistics

This table reports summary statistics for the variables used in our main analyses over the full sample period from 1987 to 2023. There are 51,455 firm-year observations. All continuous variables are winsorized at their 1st and 99th percentiles, and dollar values are expressed in 2020 dollars. All variables are defined in Appendix B.

	Mean	SD	P25	P50	P75
GLS	0.066	0.033	0.044	0.068	0.087
BidAsk	0.014	0.019	0.001	0.008	0.022
Illiq	0.316	2.671	0.001	0.005	0.045
PIN	0.174	0.102	0.109	0.165	0.223
NCSKEW	0.066	0.690	-0.329	0.023	0.406
DUVOL	0.051	0.458	-0.258	0.036	0.340
Crash	0.187	0.390	0.000	0.000	0.000
CrashCount	0.008	0.583	0.000	0.000	0.000
Volatility	0.447	0.237	0.284	0.390	0.544
IdioVol	0.399	0.223	0.245	0.345	0.490
ImpVol	0.498	0.354	0.307	0.410	0.571
Beta	1.103	0.707	0.648	1.047	1.488
UnderValued-HP	0.415	0.493	0.000	0.000	1.000
UnderValued-RRV	0.230	0.421	0.000	0.000	0.000
Post	0.332	0.471	0.000	0.000	1.000
Ln(MVE)	7.148	1.769	5.854	7.054	8.342
BM	0.494	0.427	0.246	0.415	0.650
BLEV	0.217	0.206	0.041	0.166	0.334
MOM	0.178	0.573	-0.154	0.095	0.378
CashFlow	0.073	0.151	0.053	0.093	0.136
Ln(Analysts)	2.095	0.732	1.609	2.079	2.639
Ln(FrcstDisp)	0.029	0.158	0.001	0.002	0.008
LTGRate	0.163	0.091	0.104	0.150	0.200
Ln(GDP)	11.01	0.198	10.88	10.99	11.14
%ΔGDP	0.015	0.027	0.000	0.015	0.031
TaxRate	6.860	2.780	5.500	7.300	9.000
UnempRate	5.728	1.758	4.500	5.400	6.700
DemGov	0.429	0.495	0.000	0.000	1.000
Ln(UnempBen)	9.566	0.306	9.402	9.569	9.763
Marijuana	0.187	0.390	0.000	0.000	0.000
Age	35.87	2.208	34.19	35.73	37.32
EDUC	0.277	0.070	0.228	0.265	0.319

Table 3: Covariate Balance

This table reports results from tests examining covariate balance, measured in the year before adoption, between firms in states that are about to enact anti-SLAPP laws and firms in states that have not. The anti-SLAPP law firms are defined as those in states that will pass anti-SLAPP laws in year t . The non-anti-SLAPP law subsample comprises firms in states that never or have not yet passed anti-SLAPP laws in the year before an adoption event occurs. There are 1,632 observations in the anti-SLAPP law subsample and 21,866 observations in the non-anti-SLAPP law subsample. All variables are defined in Appendix B. t -statistics for tests of differences in means are calculated from standard errors clustered by state. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

	Anti-SLAPP Law Mean	Non-Anti-SLAPP Law Mean	Difference	t -statistic
Ln(MVE)	6.785	6.794	-0.009	-0.06
BM	0.532	0.500	0.032	1.27
BLEV	0.220	0.216	0.004	0.31
MOM	0.129	0.196	-0.067	-1.00
CashFlow	0.061	0.072	-0.012	-1.36
Ln(Analysts)	2.011	1.972	0.039	1.31
Ln(FrcstDisp)	0.033	0.026	0.007	1.64
LTGRate	0.174	0.174	0.000	-0.05
Ln(GDP)	11.012	10.955	0.056	2.73***
% Δ GDP	0.003	0.017	-0.015	-1.29
TaxRate	7.252	6.734	0.518	1.07
UnempRate	5.636	5.477	0.158	0.48
DemGov	0.529	0.408	0.121	0.95
Ln(UnempBen)	9.565	9.569	-0.004	-0.07
Marijuana	0.154	0.090	0.064	1.22
Age	35.680	35.353	0.327	0.60
EDUC	0.279	0.259	0.020	2.24**

Table 4: Timing of Anti-SLAPP Legislation: Duration Model

This table reports results from Weibull hazard models where a failure event is defined as the year when a state passes an anti-SLAPP law. The sample period is from 1987 to 2023, and states are dropped from the sample after they adopt an anti-SLAPP law. Lagged control variables are measured in year $t - 1$. *State GLS* is the average implied COE defined using the model of Gebhardt et al. (2001) across all firms in a state in a given year. In column 5, all explanatory variables are measured in changes. All variables are defined in Appendix B. t -statistics in parentheses are calculated from standard errors clustered by state. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

	Anti-SLAPP				
	(1)	(2)	(3)	(4)	(5)
Ln(GDP)	0.410 (1.01)	0.326 (0.72)	-0.001 (-0.00)	0.295 (0.37)	1.661 (0.30)
% Δ GDP	-2.805 (-0.69)	-2.555 (-0.60)	-1.892 (-0.46)	-2.989 (-0.65)	0.282 (0.06)
TaxRate	0.030 (0.48)	0.024 (0.40)	0.029 (0.46)	0.023 (0.37)	0.008 (0.03)
UnempRate	-0.020 (-0.21)	-0.018 (-0.19)	-0.013 (-0.13)	-0.012 (-0.12)	0.205 (1.56)
DemGov	0.250 (0.66)	0.238 (0.62)	0.297 (0.77)	0.284 (0.72)	-0.297 (-0.50)
Ln(UnempBen)		0.329 (0.43)	0.369 (0.46)	0.319 (0.41)	3.017 (1.38)
Marijuana		-0.104 (-0.21)	-0.125 (-0.23)	-0.088 (-0.15)	0.445 (0.43)
Age			-0.150 (-1.56)	-0.150 (-1.60)	0.298 (1.42)
EDUC			1.497 (0.35)	-0.097 (-0.02)	-4.543 (-0.39)
State GLS				-7.058 (-0.64)	-12.335 (-0.99)
X Vars in Changes					✓
N of Obs	1,218	1,218	1,218	1,182	1,174

Table 5: Anti-SLAPP Legislation and COE

This table reports our baseline results from the imputation-based DiD method examining the effect of anti-SLAPP legislation on a firm's implied COE over the period from 1987 to 2023. The dependent variable is a firm's implied COE defined using the model of [Gebhardt et al. \(2001\)](#) (*GLS*), multiplied by 100 (i.e., $GLS \times 100$). *ATT* measures the average treatment effect of anti-SLAPP legislation on a firm's COE. Lagged firm control variables include $Ln(MVE)$, *BM*, *BLEV*, *MOM*, *CashFlow*, $Ln(Analysts)$, $Ln(FrcstDisp)$, and *LTGRate*. Lagged state control variables include $Ln(GDP)$, $\% \Delta GDP$, *TaxRate*, *UnempRate*, *DemGov*, $Ln(UnempBen)$, *Marijuana*, *Age*, and *EDUC*. All variables are defined in Appendix B. *t*-statistics in parentheses are calculated from standard errors clustered by state. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

	GLS $\times$ 100		
	(1)	(2)	(3)
ATT	-0.455*** (-3.97)	-0.470*** (-3.97)	-0.455*** (-5.46)
Year FE	✓	✓	✓
Firm FE	✓	✓	✓
Firm Controls		✓	✓
State Controls			✓
N of Obs	51,455	51,455	51,455

Table 6: Anti-SLAPP Legislation, Negative Press Releases, and Glassdoor Ratings

This table reports results from the imputation-based DiD method examining the effect of anti-SLAPP legislation on the number of negative press releases and Glassdoor ratings over the period from 2000 to 2023 in columns 1–4 and from 2008 to 2023 in columns 5–10. In columns 1–4, *NegPr-ESS* and *NegPr-CSS* are the natural logarithm of one plus the number of negative press releases during a firm’s fiscal year, where negative sentiment is defined based on RavenPack’s Event Sentiment Score and Composite Sentiment Score, respectively. ESS and CSS values below 0 are considered negative, while values of 0 are considered neutral. From RavenPack, we keep press releases with entity and event relevance scores of 100. In columns 5–6, *Business Outlook* takes a value of 3 if the outlook is “Positive,” 2 if “Neutral,” and 1 if “Negative.” In columns 7–8, *Recommend* takes a value of 1 if the rating for recommending the business to a friend is “Positive” and 0 if the rating is “Negative” (there is no neutral rating for this category). In columns 9–10, *CEO Rating* takes a value of 3 if the rating is “Approve”, 2 if “No opinion”, and 1 if “Disapprove”. *ATT* measures the average treatment effect of anti-SLAPP legislation on the respective outcome variable in each column. Lagged firm control variables include *Ln(MVE)*, *BM*, *BLEV*, *MOM*, *CashFlow*, *Ln(Analysts)*, *Ln(FrcstDisp)*, and *LTGRate*. Lagged state control variables include *Ln(GDP)*, *%ΔGDP*, *TaxRate*, *UnempRate*, *DemGov*, *Ln(UnempBen)*, *Marijuana*, *Age*, and *EDUC*. All variables are defined in Appendix B. *t*-statistics in parentheses are calculated from standard errors clustered by state. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

	NegPr-ESS		NegPr-CSS		Business Outlook		Recommend		CEO Rating	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
ATT	0.057*** (3.25)	0.042** (2.22)	0.119*** (5.58)	0.093*** (4.61)	-0.069*** (-2.80)	-0.058** (-2.08)	-0.065*** (-3.53)	-0.057** (-2.48)	-0.087** (-2.29)	-0.080* (-1.69)
Year FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Firm FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Controls		✓		✓		✓		✓		✓
N of Obs	11,919	11,919	11,919	11,919	1,580	1,580	2,351	2,351	2,326	2,326

Table 7: Anti-SLAPP Legislation, Litigation, and Reputation Risk

This table reports results from the imputation-based DiD method examining the effect of anti-SLAPP legislation on how firms describe criticism-related legal proceedings and reputational risk in their 10-K filings over the period from 1993 to 2023 in columns 1–4 and from 2005 to 2023 in columns 5–6. The dependent variable in columns 1–2, *Item 3 Criticism*, is an indicator equal to one if a firm’s Item 3 (Legal Proceedings) section contains at least one term related to external criticism. The dependent variable in columns 3–4, *Item 3 Speech*, is an indicator equal to one if the Item 3 section contains at least one term related to legal or contractual tools that restrict information flows. The dependent variable in columns 5–6, *Item 1A Reputation*, is an indicator equal to one if a firm’s Item 1A (Risk Factors) section contains at least one term related to reputational harm. *ATT* measures the average treatment effect of anti-SLAPP legislation on each outcome. Lagged firm control variables include $\text{Ln}(MVE)$, BM , $BLEV$, MOM , $CashFlow$, $\text{Ln}(Analysts)$, $\text{Ln}(FrcstDisp)$, and $LTGRate$. Lagged state control variables include $\text{Ln}(GDP)$, $\%\Delta GDP$, $TaxRate$, $UnempRate$, $DemGov$, $\text{Ln}(UnempBen)$, *Marijuana*, *Age*, and *EDUC*. All variables are defined in Appendix B. *t*-statistics in parentheses are calculated from standard errors clustered by state. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

	Item 3 Criticism		Item 3 Speech		Item 1A Reputation	
	(1)	(2)	(3)	(4)	(5)	(6)
ATT	-0.011*** (-3.02)	-0.010*** (-2.67)	-0.037** (-2.26)	-0.042** (-2.29)	0.030** (2.49)	0.039*** (3.45)
Year FE	✓	✓	✓	✓	✓	✓
Firm FE	✓	✓	✓	✓	✓	✓
Controls		✓		✓		✓
N of Obs	27,644	27,644	27,645	27,645	12,879	12,879

Table 8: Anti-SLAPP Legislation and Information Asymmetry

This table reports results from the imputation-based DiD method examining the effect of anti-SLAPP legislation on information asymmetry over the period from 1987 to 2023 (from 1992 to 2023 in columns 5–6). The dependent variables in columns 1–2, 3–4, and 5–6 are the bid-ask spread of a firm’s stock multiplied by 100 ($BidAsk \times 100$), the [Amihud \(2002\)](#) illiquidity measure of a firm’s stock ($Illiq$), and the probability of informed trading of a firm’s stock multiplied by 100 ($PIN \times 100$), respectively. ATT measures the average treatment effect of anti-SLAPP legislation on information asymmetry. Lagged firm control variables include $Ln(MVE)$, BM , $BLEV$, MOM , $CashFlow$, $Ln(Analysts)$, $Ln(FrcstDisp)$, and $LTGRate$. Lagged state control variables include $Ln(GDP)$, $\% \Delta GDP$, $TaxRate$, $UnempRate$, $DemGov$, $Ln(UnempBen)$, $Marijuana$, Age , and $EDUC$. All variables are defined in [Appendix B](#). t -statistics in parentheses are calculated from standard errors clustered by state. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

	BidAsk $\times 100$		Illiq		PIN $\times 100$	
	(1)	(2)	(3)	(4)	(5)	(6)
ATT	-0.171*** (-4.70)	-0.101*** (-5.49)	-0.123** (-2.45)	-0.084* (-1.91)	-0.736*** (-3.18)	-0.442** (-2.24)
Year FE	✓	✓	✓	✓	✓	✓
Firm FE	✓	✓	✓	✓	✓	✓
Controls		✓		✓		✓
N of Obs	51,453	51,453	51,453	51,453	36,383	36,383

Table 9: Anti-SLAPP Legislation and Firm Risk

This table reports results from the imputation-based DiD method examining the effect of anti-SLAPP legislation on firm risk over the period from 1987 to 2023 (from 1996 to 2023 in columns 3–4). The dependent variables in columns 1–2, 3–4, 5–6, and 7–8 are a firm’s stock return volatility (*Volatility*), implied stock return volatility (*ImpVol*), idiosyncratic stock return volatility (*IdioVol*), and market return beta (*Beta*), respectively. *ATT* measures the average treatment effect of anti-SLAPP legislation on firm risk. Lagged firm control variables include *Ln(MVE)*, *BM*, *BLEV*, *MOM*, *CashFlow*, *Ln(Analysts)*, *Ln(FrcstDisp)*, and *LTGRate*. Lagged state control variables include *Ln(GDP)*, *%ΔGDP*, *TaxRate*, *UnempRate*, *DemGov*, *Ln(UnempBen)*, *Marijuana*, *Age*, and *EDUC*. All variables are defined in Appendix B. *t*-statistics in parentheses are calculated from standard errors clustered by state. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

	Volatility		ImpVol		IdioVol		Beta	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
ATT	-0.020*** (-2.65)	-0.012*** (-3.08)	-0.017** (-2.22)	-0.014** (-2.14)	-0.018*** (-2.64)	-0.010*** (-2.95)	-0.049* (-1.93)	-0.043** (-2.35)
Year FE	✓	✓	✓	✓	✓	✓	✓	✓
Firm FE	✓	✓	✓	✓	✓	✓	✓	✓
Controls		✓		✓		✓		✓
N of Obs	51,453	51,453	24,125	24,125	51,453	51,453	51,453	51,453

Table 10: Anti-SLAPP Legislation and Firm Undervaluation

This table reports results from the imputation-based DiD method examining the effect of anti-SLAPP legislation on the likelihood that a firm is undervalued over the period from 1987 to 2023. The dependent variables in columns 1–2 and 3–4 are indicator variables equal to one if a firm is undervalued based on the [Hoberg and Phillips \(2010\)](#) and [Rhodes-Kropf et al. \(2005\)](#) valuation models, respectively. *ATT* measures the average treatment effect of anti-SLAPP legislation on the likelihood a firm is undervalued. Lagged firm control variables include $\ln(MVE)$, BM , $BLEV$, MOM , $CashFlow$, $\ln(Analysts)$, $\ln(FrcstDisp)$, and $LTGRate$. Lagged state control variables include $\ln(GDP)$, $\% \Delta GDP$, $TaxRate$, $UnempRate$, $DemGov$, $\ln(UnempBen)$, $Marijuana$, Age , and $EDUC$. All other variables are defined in Appendix B. *t*-statistics in parentheses are calculated from standard errors clustered by state. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

	UnderValued–HP		UnderValued–RRV	
	(1)	(2)	(3)	(4)
ATT	-0.030*** (-3.22)	-0.022*** (-2.61)	-0.024*** (-3.17)	-0.021*** (-2.70)
Year FE	✓	✓	✓	✓
Firm FE	✓	✓	✓	✓
Controls		✓		✓
N of Obs	51,325	51,325	49,266	49,266

Table 11: Anti-SLAPP Legislation and Stock Price Crash Risk

This table reports results from the imputation-based DiD method examining the effect of anti-SLAPP legislation on stock price crash risk over the period from 1987 to 2023. The dependent variables in columns 1–2, 3–4, 5–6, and 7–8 are *NCSKEW*, *DUVOL*, *Crash*, and *CrashCount*, respectively. *ATT* measures the average treatment effect of anti-SLAPP legislation on crash risk. Lagged firm control variables include *Ln(MVE)*, *BM*, *BLEV*, *MOM*, *CashFlow*, *Ln(Analysts)*, *Ln(FrcstDisp)*, and *LTGRate*. Lagged state control variables include *Ln(GDP)*, *%ΔGDP*, *TaxRate*, *UnempRate*, *DemGov*, *Ln(UnempBen)*, *Marijuana*, *Age*, and *EDUC*. All variables are defined in Appendix B. *t*-statistics in parentheses are calculated from standard errors clustered by state. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

	NCSKEW		DUVOL		Crash		CrashCount	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
ATT	-0.022** (-2.08)	-0.042** (-2.34)	-0.012** (-2.15)	-0.023** (-2.36)	-0.023*** (-3.40)	-0.032*** (-3.36)	-0.033*** (-2.81)	-0.046*** (-2.83)
Year FE	✓	✓	✓	✓	✓	✓	✓	✓
Firm FE	✓	✓	✓	✓	✓	✓	✓	✓
Controls		✓		✓		✓		✓
N of Obs	51,445	51,445	51,444	51,444	51,455	51,455	51,455	51,455

Table 12: Heterogeneity: Effect of *Ex Ante* Information Environment

This table reports the heterogeneous effects of the *ex ante* information environment on our baseline results from the imputation-based DiD method over the period from 1987 to 2023 (from 1997 to 2023 in columns 1–2). The dependent variable is a firm’s implied COE ($GLS \times 100$) defined using the model of Gebhardt et al. (2001). In columns 1–2, 3–4, 5–6, and 7–8, treatment effects are estimated for firms with low and high values of lagged management forecast nonprovision (*NoMngFrcst*), analyst forecast errors (*FrcstError*), absolute value of discretionary accruals (*Abs(DiscAcc)*), and R&D intensity (*R&D*), respectively. In columns 1–2, *ATT - Low* and *ATT - High* measure the average treatment effect of anti-SLAPP legislation for firms with and without management earnings guidance. In columns 3–8, *ATT - Low* and *ATT - High* measure the average treatment effect of anti-SLAPP legislation for firms with below and above median values of the respective measure. Lagged firm control variables include *Ln(MVE)*, *BM*, *BLEV*, *MOM*, *CashFlow*, *Ln(Analysts)*, *Ln(FrcstDisp)*, and *LTGRate*. Lagged state control variables include *Ln(GDP)*, *%ΔGDP*, *TaxRate*, *UnempRate*, *DemGov*, *Ln(UnempBen)*, *Marijuana*, *Age*, and *EDUC*. All variables are defined in Appendix B. *t*-statistics in parentheses are calculated from standard errors clustered by state. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

	Split by NoMngFrcst		Split by FrcstError		Split by Abs(DiscAcc)		Split by R&D	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
ATT - Low	0.043 (0.28)	-0.059 (-0.39)	-0.358*** (-2.91)	-0.337*** (-3.59)	-0.393*** (-3.18)	-0.396*** (-3.96)	-0.057 (-0.46)	-0.141 (-1.36)
ATT - High	-0.334*** (-2.81)	-0.413*** (-3.25)	-0.572*** (-5.25)	-0.603*** (-8.15)	-0.675*** (-6.38)	-0.652*** (-8.00)	-0.919*** (-7.55)	-0.822*** (-8.78)
Low = High <i>p</i> -val	<0.001	<0.001	<0.001	<0.001	<0.001	<0.001	<0.001	<0.001
Year FE	✓	✓	✓	✓	✓	✓	✓	✓
Firm FE	✓	✓	✓	✓	✓	✓	✓	✓
Controls		✓		✓		✓		✓
N of Obs	28,022	28,022	49,906	49,906	44,969	44,969	51,455	51,455

Table 13: Heterogeneity: Effect of *Ex Ante* Governance

This table reports the heterogeneous effects of *ex ante* governance on our baseline results from the imputation-based DiD method over the period from 1999 to 2023 in columns 1–4, and from 1993 to 2023 in columns 5–6. The dependent variable is a firm’s implied COE ($GLS \times 100$) defined using the model of Gebhardt et al. (2001). In columns 1–2, 3–4, and 5–6, treatment effects are estimated for firms with low and high values of lagged board independence ($\%Indep$), board busyness ($\%Busy$), and CEO pay-performance sensitivity (Δ), respectively. In columns 1–6, *ATT - Low* and *ATT - High* measure the average treatment effect of anti-SLAPP legislation for firms with below and above median values of the respective measure. Lagged firm control variables include $Ln(MVE)$, BM , $BLEV$, MOM , $CashFlow$, $Ln(Analysts)$, $Ln(FrcstDisp)$, and $LTGRate$. Lagged state control variables include $Ln(GDP)$, $\%\Delta GDP$, $TaxRate$, $UnempRate$, $DemGov$, $Ln(UnempBen)$, $Marijuana$, Age , and $EDUC$. All variables are defined in Appendix B. *t*-statistics in parentheses are calculated from standard errors clustered by state. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

	Split by %Indep		Split by %Busy		Split by Delta	
	(1)	(2)	(3)	(4)	(5)	(6)
ATT - Low	-0.444*** (-5.19)	-0.455*** (-5.17)	-0.157* (-1.70)	-0.187* (-1.96)	-0.246* (-1.92)	-0.295*** (-2.76)
ATT - High	-0.213** (-2.16)	-0.208** (-1.98)	-0.512*** (-6.48)	-0.480*** (-5.60)	-0.011 (-0.09)	-0.069 (-0.48)
Low = High <i>p</i> -val	0.002	0.002	<0.001	<0.001	0.005	0.031
Year FE	✓	✓	✓	✓	✓	✓
Firm FE	✓	✓	✓	✓	✓	✓
Controls		✓		✓		✓
N of Obs	17,557	17,557	17,557	17,557	19,342	19,342

Table 14: Heterogeneity: Effect of *Ex Ante* Scrutiny

This table reports the heterogeneous effects of *ex ante* public scrutiny on our baseline results from the imputation-based DiD method over the period from 2001 to 2023 in columns 1–2, from 2009 to 2023 in columns 3–4, and from 2006 to 2023 in columns 5–6. The dependent variable is a firm’s implied COE ($GLS \times 100$) defined using the model of Gebhardt et al. (2001). In columns 1–2, 3–4, and 5–6, treatment effects are estimated for firms with low and high values of lagged media coverage (*Media*), an indicator for whether the firm has a Glassdoor rating (*Glassdoor*), and lagged criticism-related legal and reputational exposure in the 10-K (*Litigation*), respectively. *Litigation* is the sum of three indicator variables—*Item 3 Criticism*, *Item 3 Speech*, and *Item 1A Reputation*—each of which equals one if the firm’s 10-K contains the corresponding disclosure, and zero otherwise, so that the sum ranges from zero to three. In columns 3–4, *ATT - Low* and *ATT - High* measure the average treatment effect for firms without and with a Glassdoor rating. In columns 1–2 and 5–6, *ATT - Low* and *ATT - High* measure the average treatment effect of anti-SLAPP legislation for firms with below and above median values of the respective measure. Lagged firm control variables include $Ln(MVE)$, BM , $BLEV$, MOM , $CashFlow$, $Ln(Analysts)$, $Ln(FrcstDisp)$, and $LTGRate$. Lagged state control variables include $Ln(GDP)$, $\% \Delta GDP$, $TaxRate$, $UnempRate$, $DemGov$, $Ln(UnempBen)$, *Marijuana*, *Age*, and *EDUC*. All variables are defined in Appendix B. *t*-statistics in parentheses are calculated from standard errors clustered by state. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

	Split by Media		Split by Glassdoor		Split by Litigation	
	(1)	(2)	(3)	(4)	(5)	(6)
ATT - Low	-0.095 (-0.75)	-0.087 (-0.75)	-0.526*** (-3.18)	-0.412*** (-2.85)	-0.365** (-2.19)	-0.239* (-1.74)
ATT - High	-0.354*** (-3.54)	-0.414*** (-3.82)	-0.894*** (-4.49)	-0.690*** (-3.93)	-0.532*** (-2.60)	-0.402** (-2.50)
Low = High <i>p</i> -val	0.008	<0.001	<0.001	<0.001	0.011	0.009
Year FE	✓	✓	✓	✓	✓	✓
Firm FE	✓	✓	✓	✓	✓	✓
Controls		✓		✓		✓
N of Obs	20,279	20,279	9,893	9,893	11,611	11,611

WHEN SPEAKING FREELY PAYS: ANTI-SLAPP LAWS AND FIRMS' COST OF EQUITY

ONLINE APPENDIX

Appendix OA. Examples of SLAPP Cases

This appendix describes illustrative SLAPP suits in which publicly traded firms sued outside critics—employees, journalists, message-board investors, consumer advocates, and short sellers—and the critics prevailed under state anti-SLAPP statutes. Listing venues and tickers reflect each firm’s status at the time of the underlying speech.

Varian Medical Systems, Inc. v. Delfino

In 1999, Varian (NYSE: VAR) sued former employees Michelangelo Delfino and Mary Day for defamation after they posted thousands of derogatory messages about executives online. The defendants filed an anti-SLAPP motion, which the trial court denied. Although the defendants appealed that denial, the trial court proceeded to trial anyway, resulting in a \$775,000 verdict for Varian. However, the California Supreme Court voided the verdict, ruling that an appeal of the denial of an anti-SLAPP motion triggers an automatic stay of trial proceedings. The case settled years later, but it remains a pivotal precedent for procedural rights under California’s anti-SLAPP law.

ComputerXpress, Inc. v. Jackson

In 2000, ComputerXpress, a publicly traded computer-products company quoted on the OTC Bulletin Board and later the pink sheets, sued a group of outside critics after they posted messages on the Raging Bull message board and their own website describing the company as a stock “scam” run by “pump and dump” promoters, and after two of them filed a complaint against the company with the SEC. The company claimed the postings had cost it \$10 million in lost investment; its own annual report, filed with the SEC while the suit was pending, stated explicitly that “The lawsuit has been served and the Company’s management hopes that it will have a chilling effect on the defendants’ actions.” The California Court of Appeal held that the message-board postings addressed a matter of public interest—the affairs of a publicly traded company—and that the SEC complaint was absolutely privileged, ordering all claims arising from the postings and the complaint stricken under the anti-SLAPP statute and holding the defendants entitled to attorneys’ fees on those claims. The decision became a leading authority extending anti-SLAPP protection to investor speech about public companies.

Global Telemedia International, Inc. v. Doe 1

In 2000, Global Telemedia International, Inc. (a telecommunications firm publicly traded on the OTC Bulletin Board) sued anonymous online posters for allegedly defamatory comments on the Raging Bull investor message board criticizing the company’s financial health, management, and stock performance (e.g., calling it a “sinking ship” and accusing management of misleading practices). The defendants successfully invoked California’s anti-SLAPP law in federal court, leading to the suit’s dismissal in 2001 as the court deemed the statements non-actionable opinions in a public forum on a matter of interest to the investing public, and both defendants were later awarded their attorneys’ fees. This allowed the critical commentary to remain public, fostering greater market scrutiny and timely disclosure of potential issues, which aligns with how anti-SLAPP protections reduce concealment risks.

Ampex Corporation v. Cargle

In 2001, a former employee of a subsidiary of Ampex Corporation (AMEX: AXC), laid off the previous year, posted messages under the name “examper” on the Yahoo! Finance message board

for Ampex, describing the subsidiary as a mismanaged and “sleazy” operation. Ampex and its chairman sued the then-anonymous poster for libel, unmasked him through a subpoena, and then voluntarily dismissed the suit. The California Court of Appeal held that the Yahoo! Finance board is a public forum and that the postings concerned an issue of public interest precisely because Ampex was a publicly traded, widely followed company that promoted itself through press releases; it further held that the plaintiffs could not show a probability of prevailing and that the critic, as the prevailing anti-SLAPP defendant, was entitled to mandatory attorneys’ fees. The case underscores that employee speech about listed firms on investor forums receives anti-SLAPP protection even when the employer withdraws the suit after unmasking the speaker.

Troy Group, Inc. v. Tilson

In 2004, Troy Group, a printing technology company then traded on the Nasdaq National Market, sued investor and critic Whitney Tilson for defamation after he emailed fellow investors, a Wall Street Journal reporter, and counsel in his fund’s pending litigation against the company, asking—in reference to the controlling family’s going-private buyout of the firm—“Are these guys the biggest crooks on the planet or what?” Troy aimed to suppress the criticism amid the pending transaction and shareholder litigation. A federal court applying California’s anti-SLAPP law dismissed the suit in 2005, finding that Tilson’s statements addressed an issue of public interest—the governance of a corporation with millions of publicly held shares—and that Troy failed to show a probability of success on the merits, entitling Tilson to recover his attorneys’ fees. By protecting an investor’s critical communications, the ruling illustrates how anti-SLAPP laws safeguard the stakeholder monitoring on which outside investors in opaque firms rely.

GTX Global Corp. v. Left

Around 2005, GTX Global Corporation, whose shares were quoted on the Pink Sheets, sued short seller Andrew Left over online reports published on StockLemon.com (later rebranded as Citron Research), alleging the reports contained falsehoods about GTX’s operations, valuation, and management integrity. Left filed an anti-SLAPP motion under California’s statute (Cal. Code Civ. Proc. § 425.16), which the trial court granted, dismissing the entire complaint and awarding fees. The California Court of Appeal affirmed the dismissal in May 2007, concluding that GTX had not identified evidence that Left’s statements were false. By safeguarding Left’s critical analysis from suppression, the ruling promoted the kind of external monitoring that can deter managerial misconduct and enhance information flow to investors.

Medifast, Inc. v. Minkow

In 2010, Medifast (NYSE: MED) and one of its executives sued a group of outside critics—including fraud investigator Barry Minkow, a journalist, forensic accountant Tracy Coenen, and a pyramid-scheme researcher—for defamation, market manipulation, and unfair competition after they published analyses questioning whether the company’s multi-level marketing program constituted a pyramid scheme. In 2011, a federal court applying California’s anti-SLAPP statute struck the entire complaint as to Coenen and the Minkow defendants, and in 2014 the Ninth Circuit affirmed Coenen’s anti-SLAPP victory in full; Coenen reportedly recovered approximately \$200,000 in attorneys’ fees. The episode shows anti-SLAPP protection operating for precisely the outside monitors—forensic accountants and short-oriented investigators—whose research production the baseline analysis links to firms’ information environments.

Appendix OB. Theory: anti-SLAPP laws and firms' COE

We develop a simple asset-pricing framework to illustrate how anti-SLAPP laws can affect firms' COE by changing investors' beliefs about unfavorable outside information. The model formalizes the central tradeoff in the paper. On the one hand, stronger speech protections can reduce investors' uncertainty about hidden risks by making it harder for firms to deter outside stakeholders from revealing adverse information. This is the *uncertainty resolution* channel. On the other hand, stronger speech protections can increase the amount or salience of negative, noisy, or difficult-to-verify information, causing investors to revise upward their assessments of reputational, operational, litigation, regulatory, or systematic risk. This is the *risk and noise* channel.

The model nests standard insights from the information and cost-of-capital literature, including Diamond and Verrecchia (1991), Easley and O'Hara (2004), Lambert et al. (2007), and Hughes, Liu, and Liu (2007), and applies them to the legal environment governing public criticism of firms.

OB.1 Model setup

Consider a one-period economy with two dates, $t = 0$ and $t = 1$. At $t = 0$, investors price firm i 's equity. At $t = 1$, firm cash flows are realized. The payoff of firm i is:

$$X_i = \theta_i + J_i + \varepsilon_i, \quad (\text{OB-1})$$

where:

- $\theta_i \sim \mathcal{N}(\mu_i, \sigma_{\theta_i}^2)$ is the firm's fundamental cash-flow component, with mean μ_i and prior variance $\sigma_{\theta_i}^2$;
- J_i is a latent adverse-information component, where $J_i = -d_i < 0$ with probability p_i and $J_i = 0$ otherwise;
- $\varepsilon_i \sim \mathcal{N}(0, \sigma_{\varepsilon_i}^2)$ is idiosyncratic noise with variance $\sigma_{\varepsilon_i}^2$.

The latent component J_i captures unfavorable information that outsiders may possess but managers may prefer to suppress. This information can concern product quality, workplace conditions, misconduct, operational problems, litigation exposure, regulatory risk, or other firm-specific risks. Before anti-SLAPP adoption, firms can more credibly threaten critics with costly litigation, making J_i less likely to reach investors before prices are set.

Investors price assets using a stochastic discount factor m , so that:

$$\mathbb{E}[R_i] - r_f = -\frac{\text{Cov}(m, R_i)}{\mathbb{E}[m]}, \quad (\text{OB-2})$$

where R_i is the return on firm i 's equity and r_f is the risk-free rate. Under a linear factor structure, $m = a - bR_M$, where a and $b > 0$ are constants of the pricing kernel and R_M is the market return, the standard risk-premium component is:

$$\mathbb{E}[R_i] - r_f = \beta_i \lambda_M, \quad (\text{OB-3})$$

where $\beta_i = \frac{\text{Cov}(R_i, R_M)}{\text{Var}(R_M)}$ and λ_M is the market risk premium.

OB.2 Information environment and anti-SLAPP laws

At $t = 0$, investors observe a public signal:

$$s_i = \theta_i + \delta_i J_i + \eta_i, \quad \eta_i \sim \mathcal{N}(0, \tau_i^{-1}), \quad (\text{OB-4})$$

where η_i is the noise in the public signal, τ_i is the precision of the public signal, and $\delta_i \in \{0, 1\}$ indicates whether the adverse component J_i is revealed before pricing. Let

$$q_i = \Pr(\delta_i = 1) \quad (\text{OB-5})$$

denote the probability that unfavorable outside information reaches investors before prices are set.

Anti-SLAPP laws affect the information environment through two related forces. First, they increase the probability that unfavorable outside information reaches investors by lowering the expected legal cost of public criticism. Letting q_{0i} and q_{1i} denote the revelation probabilities before and after adoption, this force implies:

$$\Delta q_i \equiv q_{1i} - q_{0i} > 0. \quad (\text{OB-6})$$

Second, they change the precision of the public signal. Stronger speech protections can increase precision by allowing employees, customers, journalists, analysts, activists, and other outside stakeholders to reveal credible information. But they can also introduce noise if some outside information is incomplete, exaggerated, or difficult to verify. We write post-adoption precision as:

$$\tau_{1i} = \tau_{0i} + \Delta \tau_i^{\text{cred}} - \Delta \tau_i^{\text{noise}}, \quad (\text{OB-7})$$

where τ_{0i} is the pre-adoption precision of the public signal, $\Delta \tau_i^{\text{cred}} > 0$ captures credible information produced by outside stakeholders, and $\Delta \tau_i^{\text{noise}} > 0$ captures noisy or low-quality information. Net precision increases when:

$$\Delta \tau_i \equiv \Delta \tau_i^{\text{cred}} - \Delta \tau_i^{\text{noise}} > 0. \quad (\text{OB-8})$$

When $\delta_i = 1$, investors observe the revealed component J_i itself and can subtract it from the public signal, so in either case the effective signal about the firm's fundamentals is $\theta_i + \eta_i$. Given normal updating, the posterior variance of the fundamental component is therefore decreasing in signal precision:

$$V_i \equiv \text{Var}(\theta_i \mid s_i) = \left(\sigma_{\theta_i}^{-2} + \tau_i \right)^{-1}. \quad (\text{OB-9})$$

Thus, higher net signal precision lowers investors' uncertainty about firm fundamentals. In addition, a higher probability of revelation, q_i , reduces uncertainty about whether the firm is concealing latent adverse information.

OB.3 Expected returns and COE

We assume that firm i 's COE contains three components—a systematic-risk premium, an information-risk premium, and a hidden-risk or pooling premium:

$$\text{COE}_i - r_f = \beta_i(\tau_i, q_i) \lambda_M + \pi_i(\tau_i) + \Omega_i(q_i). \quad (\text{OB-10})$$

The first term captures priced covariance risk. The second term, $\pi_i(\tau_i)$, captures the information-risk premium investors demand when signals about firm value are imprecise. The third term, $\Omega_i(q_i)$, captures a hidden-risk or pooling premium: when investors cannot determine whether a firm is concealing adverse information, they may require compensation for the possibility of latent bad news.

We parameterize these terms as:

$$\beta_i(\tau_i, q_i) = \beta_{0i} - a_i\tau_i + b_iq_i, \quad (\text{OB-11})$$

where β_{0i} is firm i 's baseline systematic-risk exposure absent information effects.

$$\pi_i(\tau_i) = \frac{\kappa_i}{1 + \tau_i}, \quad (\text{OB-12})$$

and

$$\Omega_i(q_i) = \omega_i(1 - q_i), \quad (\text{OB-13})$$

where $a_i > 0$, $\kappa_i > 0$, and $\omega_i > 0$. The parameter a_i captures the idea that more precise information can reduce covariance risk by improving investors' assessment of firm fundamentals. The parameter b_i captures the risk component of the risk and noise channel. If revealed adverse information exposes risks that covary with market downturns or raises investors' assessment of systematic risk, then $b_i > 0$. If revelation primarily resolves uncertainty about idiosyncratic hidden risks, then b_i may be small or negative. The parameter κ_i scales the information-risk premium investors demand when the public signal about firm value is imprecise. The parameter ω_i captures the premium investors demand when firms are pooled together because hidden adverse information may be suppressed.

OB.4 Analytical results

Proposition 1 (Uncertainty Resolution through Signal Precision). If anti-SLAPP laws increase net signal precision, $\Delta\tau_i > 0$, then posterior variance and the information-risk component of COE decline:

$$\frac{\partial V_i}{\partial \tau_i} < 0, \quad \frac{\partial \pi_i(\tau_i)}{\partial \tau_i} < 0.$$

Proof. Differentiating posterior variance gives:

$$\frac{\partial V_i}{\partial \tau_i} = -\left(\sigma_{\theta i}^{-2} + \tau_i\right)^{-2} < 0.$$

Differentiating the information-risk premium gives:

$$\frac{\partial \pi_i(\tau_i)}{\partial \tau_i} = -\frac{\kappa_i}{(1 + \tau_i)^2} < 0.$$

Thus, higher net signal precision lowers both posterior uncertainty and the information-risk premium. $\square$

Proposition 2 (Uncertainty Resolution through Separation). If anti-SLAPP laws increase the probability that unfavorable outside information reaches investors, $\Delta q_i > 0$, then the hidden-risk

or pooling premium declines:

$$\frac{\partial \Omega_i(q_i)}{\partial q_i} < 0.$$

Proof. Since $\Omega_i(q_i) = \omega_i(1 - q_i)$,

$$\frac{\partial \Omega_i(q_i)}{\partial q_i} = -\omega_i < 0.$$

Thus, increasing the probability that outside information reaches investors reduces the premium investors demand for the possibility that adverse information is being suppressed. $\square$

Proposition 3 (Risk and Noise Channel). If revealed adverse information increases investors' assessment of priced risk, then anti-SLAPP laws can increase COE through the systematic-risk component:

$$\frac{\partial}{\partial q_i} [\beta_i(\tau_i, q_i) \lambda_M] = b_i \lambda_M.$$

When $b_i > 0$, greater revelation raises the systematic-risk component of expected returns. In addition, if outside criticism introduces sufficient noise that net signal precision falls ($\Delta \tau_i < 0$), the information-risk premium $\pi_i(\tau_i)$ rises.

Proof. Differentiating the systematic-risk component with respect to q_i yields:

$$\frac{\partial}{\partial q_i} [\beta_i(\tau_i, q_i) \lambda_M] = \frac{\partial \beta_i(\tau_i, q_i)}{\partial q_i} \lambda_M = b_i \lambda_M,$$

which is positive whenever $b_i > 0$. For the noise effect, Proposition 1 establishes $\partial \pi_i(\tau_i) / \partial \tau_i < 0$, so $\Delta \tau_i < 0$ implies an increase in $\pi_i(\tau_i)$. $\square$

Proposition 4 (Net Effect of Anti-SLAPP Laws). The change in firm i 's COE following anti-SLAPP adoption is approximately:

$$\Delta \text{COE}_i \approx \underbrace{\left[-a_i \lambda_M - \frac{\kappa_i}{(1 + \tau_{0i})^2} \right] \Delta \tau_i}_{\text{Information precision}} + \underbrace{[b_i \lambda_M - \omega_i] \Delta q_i}_{\text{Revelation and separation}}, \quad (\text{OB-14})$$

where the information-precision term is evaluated at the pre-adoption precision τ_{0i} . The sign of ΔCOE_i depends on whether uncertainty resolution and separation effects dominate risk and noise effects.

OB.5 Interpretation and empirical implications

Equation (OB-14) formalizes the paper's two competing hypotheses. Under the *Uncertainty Resolution Hypothesis*, anti-SLAPP laws lower COE because they increase net signal precision and reduce the hidden-risk premium investors demand when firms can deter unfavorable outside information. In the model, this corresponds to $\Delta \tau_i > 0$ and/or $\omega_i \Delta q_i$ being large relative to $b_i \lambda_M \Delta q_i$. This case predicts lower information asymmetry, lower volatility, lower undervaluation, and lower crash risk after adoption.

Under the *Risk and Noise Hypothesis*, anti-SLAPP laws raise COE because outside criticism reveals additional risks or introduces noisy adverse signals. In the model, this corresponds to b_i

being positive and sufficiently large, or to noisy information reducing net signal precision so that $\Delta\tau_i < 0$. This case predicts higher perceived risk and higher required returns.

The net effect is therefore an empirical question. If investors primarily value the reduction in information suppression and hidden-risk uncertainty, COE should decline after anti-SLAPP adoption. If investors instead view the newly protected outside criticism as revealing greater firm risk or increasing noise, COE should rise.

OB.6 Cross-sectional predictions

The model also generates cross-sectional predictions that map to our empirical tests. The COE-reducing effect should be stronger when the hidden-risk premium parameter ω_i or the information-risk parameter κ_i is larger. This prediction applies to firms with weaker information environments, where investors face greater uncertainty about hidden risks. It also applies to firms with weaker governance, where internal monitoring is less effective and latent adverse information is more likely to remain suppressed, corresponding to a larger ω_i . Finally, the effect should be stronger for firms with greater public scrutiny, because outside stakeholders are more likely to possess, produce, or disseminate value-relevant information when speech protections reduce the cost of public criticism; in the model, this corresponds to a larger increase in the revelation probability, Δq_i .

OB.7 Discussion

This framework highlights that anti-SLAPP laws can affect financing costs even though they do not directly regulate disclosure or capital markets. By changing the expected cost of producing unfavorable outside information, these laws alter both the probability that hidden bad news reaches investors and the precision of the public signal. The model therefore rationalizes why stronger speech protections can either lower or raise COE. The empirical results in the paper support the uncertainty resolution case: after anti-SLAPP adoption, firms' COE falls, information asymmetry declines, firm risk and crash risk decrease, and undervaluation becomes less likely.

OB.8 Main symbols and definitions in the theoretical model

Symbols and Definitions	
Symbol	Definition
X_i	Firm i 's payoff at time $t = 1$
θ_i	Fundamental cash-flow component
μ_i	Mean of the fundamental cash-flow component
$\sigma_{\theta_i}^2$	Prior variance of the fundamental cash-flow component
J_i	Latent adverse-information component
d_i	Magnitude of latent adverse information
p_i	Probability that latent adverse information exists
ε_i	Idiosyncratic noise term
$\sigma_{\varepsilon_i}^2$	Variance of the idiosyncratic noise term
R_i	Firm i 's equity return
r_f	Risk-free rate
R_M	Market return
λ_M	Market risk premium
m	Stochastic discount factor
a, b	Constants of the pricing kernel ($m = a - bR_M$)
β_i	Systematic-risk exposure
β_{0i}	Baseline systematic-risk exposure absent information effects
s_i	Public signal observed by investors
η_i	Noise in the public signal
τ_i	Precision of the public signal
τ_{0i}, τ_{1i}	Pre- and post-adoption precision of the public signal
$\Delta\tau_i^{\text{cred}}$	Precision gain from credible outside information
$\Delta\tau_i^{\text{noise}}$	Precision loss from noisy or low-quality information
$\Delta\tau_i$	Net change in signal precision ($\Delta\tau_i^{\text{cred}} - \Delta\tau_i^{\text{noise}}$)
q_i	Probability that unfavorable outside information is revealed before pricing
q_{0i}, q_{1i}	Pre- and post-adoption revelation probabilities
Δq_i	Change in the revelation probability ($q_{1i} - q_{0i}$)
δ_i	Indicator for whether latent adverse information is revealed
V_i	Posterior variance of firm fundamentals given the public signal
$\pi_i(\tau_i)$	Information-risk premium
$\Omega_i(q_i)$	Hidden-risk or pooling premium
a_i	Sensitivity of systematic risk to signal precision
b_i	Sensitivity of systematic risk to bad-news revelation
κ_i	Magnitude of the information-risk premium
ω_i	Magnitude of the hidden-risk or pooling premium

Appendix OC. Alternative estimations of the implied COE and earnings forecast models

Alternative implied COE models

Following previous research in this literature, we outline the alternative methodologies commonly used to estimate a firm's implied COE (e.g., [Chen et al., 2011b](#); [Li and Mohanram, 2014](#); [Brushwood, Dhaliwal, Fairhurst, and Serfling, 2016](#); [Dhaliwal, Judd, Serfling, and Shaikh, 2016](#)) using the variables defined in Appendix A.

OC.1 [Ohlson and Juettner-Nauroth \(2005\)](#) as implemented by [Gode and Mohanram \(2003\)](#)

$$R_{OJN} = A + \sqrt{A^2 + \frac{FEPS_{t+1}}{P_t^*}(g_{st} - g_{lt})}, \quad (\text{OC-1})$$

where R_{OJN} denotes the implied COE estimate from this model and

$$A = 0.5 \left(g_{lt} + \frac{DPS_{t+1}}{P_t^*} \right), \quad (\text{OC-2})$$

and where g_{st} denotes the short-term earnings growth rate, computed as the mean of the earnings growth rate implied by $FEPS_{t+1}$ and $FEPS_{t+2}$, and g_{lt} denotes the long-term growth rate in expected earnings. We calculate g_{lt} by subtracting 3% from the contemporaneous risk-free rate (i.e., the yield on a 10-year Treasury bond). The application of this model necessitates the conditions $FEPS_{t+1} > 0$ and $FEPS_{t+2} > 0$.

OC.2 [Claus and Thomas \(2001\)](#)

$$P_t^* = B_t + \sum_{i=1}^5 \left[\frac{FEPS_{t+i} - R_{CT} \times B_{t+i-1}}{(1 + R_{CT})^i} \right] + \left[\frac{(FEPS_{t+5} - R_{CT} \times B_{t+4}) \times (1 + g_{lt})}{(R_{CT} - g_{lt}) \times (1 + R_{CT})^5} \right]. \quad (\text{OC-3})$$

We employ IBES earnings projections to compute abnormal earnings over the subsequent five years. Projections for earnings in the fourth and fifth years are derived from forecasts for the third year and the long-term earnings growth rate. In instances where the long-term earnings growth rate is unavailable in IBES, an implied earnings growth rate is derived from $FEPS_{t+2}$ and $FEPS_{t+3}$. The long-term abnormal earnings growth rate, g_{lt} , is calculated by subtracting 3% from the contemporaneous risk-free rate (i.e., the yield on a 10-year Treasury bond). The estimation of the future book value of equity follows the assumption of the clean surplus relation. The subsequent dividend, DPS_{t+i} , is computed by multiplying $FEPS_{t+i}$ by the payout ratio, $POUT$. We utilize a numerical approximation program to determine R_{CT} , ensuring that the right- and left-hand sides of Eq. (OC-3) are equal within a tolerance of \$0.001.

OC.3 The modified PEG ratio model by Easton (2004)

$$P_t^* = \frac{FEPS_{t+1}}{R_{MPEG}} + \frac{FEPS_{t+1} \times (g_{st} - R_{MPEG} \times (1 - POUT))}{R_{MPEG}^2}. \quad (OC-4)$$

The model imposes the conditions $FEPS_{t+2} > FEPS_{t+1} > 0$. The value of R_{MPEG} is determined numerically to balance both sides of Eq. (OC-4) within a tolerance of \$0.001.

Earnings forecast models

Instead of relying only on analyst forecasts as inputs for the aforementioned implied COE models, we also employ model-based approaches following the methodologies proposed by Li and Mohanram (2014).

For each of the two models, we generate predicted earnings per share for years $t + 1$ to $t + 5$. This involves estimating the models for each year between 1987 and 2023, and utilizing all available data from the preceding ten years. The independent variables in year t are multiplied by their corresponding coefficient estimates. This approach ensures that the earnings forecasts remain strictly out of sample. For instance, when forecasting earnings for year $t + 1$ (e.g., the year 2001, with year t being 2000), we use data from the years 1990 to 1999. After obtaining the coefficient estimates, we multiply them by the values from the year 2000 to obtain forecasted earnings for the year 2001. Similarly, to forecast year $t + 2$ (year 2002), we estimate the regressions using data from the years 1989 to 1998 and multiply the estimated coefficients by the year 2000 values.

OC.4 Earnings persistence model

$$E_{i,t+\tau} = \beta_0 + \beta_1 NegE_{i,t} + \beta_2 E_{i,t} + \beta_3 NegE_{i,t} \times E_{i,t} + \varepsilon_{i,t+\tau}, \quad (OC-5)$$

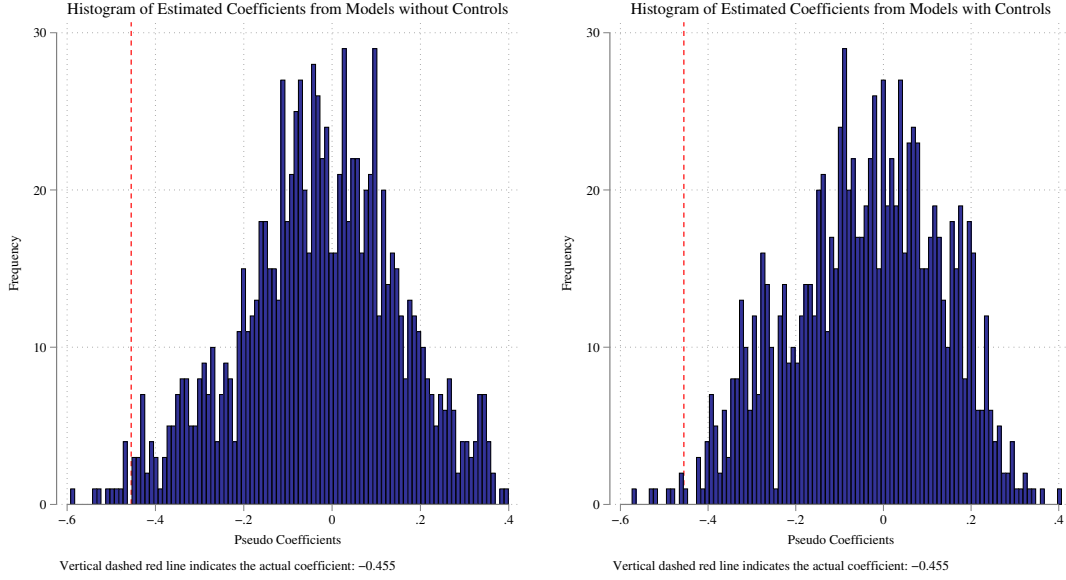
where τ indexes the forecast horizon ($\tau = 1, \dots, 5$), and $E_{i,t}$ represents the per-share income before extraordinary items, excluding special items, for firm i in year t . Additionally, $NegE_{i,t}$ is an indicator variable, taking the value of one if the firm reports negative earnings and zero otherwise. The interaction of the two variables is denoted as $NegE_{i,t} \times E_{i,t}$.

OC.5 Residual income valuation model

$$E_{i,t+\tau} = \beta_0 + \beta_1 NegE_{i,t} + \beta_2 E_{i,t} + \beta_3 NegE_{i,t} \times E_{i,t} + \beta_4 B_{i,t} + \beta_5 TACC_{i,t} + \varepsilon_{i,t+\tau}, \quad (OC-6)$$

where $E_{i,t}$, $NegE_{i,t}$, and $NegE_{i,t} \times E_{i,t}$ retain the same definitions as outlined in Eq. (OC-5). Additionally, $B_{i,t}$ denotes the common shareholder equity per share, while $TACC_{i,t}$ represents total accruals. Total accruals are defined as the aggregate of changes in working capital, changes in non-current operating accruals, and changes in net financial assets, all divided by the number of shares outstanding.

Figure OA1: Randomized Anti-SLAPP Adoption Years



This figure plots the estimated coefficients from placebo tests examining the effect of anti-SLAPP legislation on a firm's implied COE ($GLS \times 100$) over the period from 1987 to 2023, estimated using the imputation-based DiD method. GLS is the implied COE calculated from analyst EPS forecasts using the model of [Gebhardt et al. \(2001\)](#). We randomly reassign the adoption of anti-SLAPP laws across states, preserving the overall structure of the policy experiment but breaking the actual link between a state's true anti-SLAPP adoption and firms headquartered there. For each placebo draw, we re-estimate our baseline specification without and with controls and record the coefficient on the anti-SLAPP indicator. We repeat this procedure 1,000 times and plot the resulting distribution of placebo estimates. Lagged firm control variables include $Ln(MVE)$, BM , $BLEV$, MOM , $CashFlow$, $Ln(Analysts)$, $Ln(FrcstDisp)$, and $LTGRate$. Lagged state control variables include $Ln(GDP)$, $\% \Delta GDP$, $TaxRate$, $UnempRate$, $DemGov$, $Ln(UnempBen)$, $Marijuana$, Age , and $EDUC$. All variables are defined in [Appendix B](#).

Table OA1: Excluding Firms with Dispersed Operations

This table reports results from the imputation-based DiD method examining the effect of anti-SLAPP legislation on a firm's implied COE over the period from 1987 to 2023. The dependent variable is a firm's implied COE ($GLS \times 100$) defined using the model of [Gebhardt et al. \(2001\)](#). *ATT* measures the average treatment effect of anti-SLAPP legislation on a firm's COE. In Panel A, columns 1 and 2 exclude firms in industries with dispersed operations, which include retail, wholesale, and transportation and communication industries (two-digit SIC codes of 52-59, 50-51, and 40-48, respectively). Columns 3 and 4 exclude firms with values of PP&E above the median. Columns 5 and 6 exclude firms with operations in an above-median number of states following [García and Norli \(2012\)](#). In columns 1–8 of Panel B, we use establishment-level data and only keep firms with at least 50%, 75%, 90%, or 100% of their employees located in the headquarters state. Lagged firm control variables include $Ln(MVE)$, BM , $BLEV$, MOM , $CashFlow$, $Ln(Analysts)$, $Ln(FrcstDisp)$, and $LTGRate$. Lagged state control variables include $Ln(GDP)$, $\% \Delta GDP$, $TaxRate$, $UnempRate$, $DemGov$, $Ln(UnempBen)$, $Marijuana$, Age , and $EDUC$. All variables are defined in Appendix B. *t*-statistics in parentheses are calculated from standard errors clustered by state. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

<i>Panel A: Dispersed Operations using Compustat Data</i>						
	Non-Disp Industries		Smaller Firms		#States < Median	
	(1)	(2)	(3)	(4)	(5)	(6)
ATT	-0.656*** (-4.66)	-0.640*** (-6.10)	-0.478*** (-4.49)	-0.435*** (-3.90)	-0.481*** (-3.90)	-0.386*** (-4.80)
Year FE	✓	✓	✓	✓	✓	✓
Firm FE	✓	✓	✓	✓	✓	✓
Controls		✓		✓		✓
N of Obs	39,848	39,848	11,772	11,772	19,165	19,165

<i>Panel B: Dispersed Operations using Establishment-Level Data</i>								
	InState $\geq$ 50%		InState $\geq$ 75%		InState $\geq$ 90%		InState = 100%	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
ATT	-0.821*** (-4.20)	-0.731*** (-5.26)	-0.898*** (-4.01)	-0.822*** (-4.55)	-1.205*** (-5.18)	-1.173*** (-6.67)	-1.145*** (-7.35)	-1.243*** (-7.06)
Year FE	✓	✓	✓	✓	✓	✓	✓	✓
Firm FE	✓	✓	✓	✓	✓	✓	✓	✓
Controls		✓		✓		✓		✓
N of Obs	15,617	15,617	9,171	9,171	5,859	5,859	3,194	3,194

Table OA2: Venue Shopping and Jurisdiction Concerns

This table reports results from the imputation-based DiD method examining the effect of anti-SLAPP legislation on a firm's implied COE over the period from 1987 to 2023. The dependent variable is a firm's implied COE ($GLS \times 100$) defined using the model of [Gebhardt et al. \(2001\)](#). *ATT* measures the average treatment effect of anti-SLAPP legislation on a firm's COE. Columns 1 and 2 drop firms operating in consumer-facing industries. Columns 3 and 4 drop firms headquartered in California and New York (i.e., drop CA & NY). In columns 5 and 6, *ATT - Pre2010* and *ATT - Post2010* measure the average treatment effect of anti-SLAPP legislation in the periods before and after 2010. Lagged firm control variables include $Ln(MVE)$, BM , $BLEV$, MOM , $CashFlow$, $Ln(Analysts)$, $Ln(FrcstDisp)$, and $LTGRate$. Lagged state control variables include $Ln(GDP)$, $\% \Delta GDP$, $TaxRate$, $UnempRate$, $DemGov$, $Ln(UnempBen)$, $Marijuana$, Age , and $EDUC$. All variables are defined in [Appendix B](#). *t*-statistics in parentheses are calculated from standard errors clustered by state. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

	Drop Consumer		Drop CA & NY		Split by 2010	
	(1)	(2)	(3)	(4)	(5)	(6)
ATT	-0.677*** (-4.41)	-0.695*** (-5.96)	-0.338*** (-2.71)	-0.377*** (-3.24)		
ATT - Pre2010					-0.520*** (-4.26)	-0.485*** (-6.78)
ATT - Post2010					-0.390*** (-2.72)	-0.426*** (-3.07)
Pre2010 = Post2010 <i>p</i> -val					0.246	0.674
Year FE	✓	✓	✓	✓	✓	✓
Firm FE	✓	✓	✓	✓	✓	✓
Controls		✓		✓		✓
N of Obs	26,033	26,033	44,607	44,607	51,455	51,455

Table OA3: Anti-SLAPP Legislation and COE: Alternative COE Measures

This table reports results from the imputation-based DiD method examining the effect of anti-SLAPP legislation on the alternative measures ($\times 100$) of a firm's implied COE over the period from 1987 to 2023. The dependent variables in columns 1–2, 3–4, 5–6, and 7–8 of Panel A are a firm's implied COE derived from analyst EPS forecasts using the models of [Ohlson and Juettner-Nauroth \(2005\)](#) ($OJN \times 100$), [Claus and Thomas \(2001\)](#) ($CT \times 100$), and [Easton \(2004\)](#) ($MPEG \times 100$), and the mean value ($AvgCOE \times 100$) across the previously mentioned three implied COE models plus [Gebhardt et al. \(2001\)](#), respectively. The dependent variables in columns 1–2 and 3–4 of Panel B are a firm's implied COE estimated using the method of [Gebhardt et al. \(2001\)](#) with EPS forecasts derived from the earnings persistence model ($EP \times 100$) and the residual income valuation model ($RIV \times 100$), respectively. *ATT* measures the average treatment effect of state-level anti-SLAPP legislation on a firm's COE. Lagged firm control variables include $Ln(MVE)$, BM , $BLEV$, MOM , $CashFlow$, $Ln(Analysts)$, $Ln(FrcstDisp)$, and $LTGRate$. Lagged state control variables include $Ln(GDP)$, $\% \Delta GDP$, $TaxRate$, $UnempRate$, $DemGov$, $Ln(UnempBen)$, *Marijuana*, *Age*, and *EDUC*. All variables are defined in Appendix B. *t*-statistics in parentheses are calculated from standard errors clustered by state. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

<i>Panel A: COE Measured using Alternative Discount Models</i>								
	OJN $\times 100$		CT $\times 100$		MPEG $\times 100$		AvgCOE $\times 100$	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
ATT	-0.463*** (-4.63)	-0.262*** (-6.42)	-0.330*** (-3.11)	-0.174** (-2.23)	-0.668*** (-5.20)	-0.447*** (-5.78)	-0.453*** (-5.40)	-0.301*** (-5.86)
Year FE	✓	✓	✓	✓	✓	✓	✓	✓
Firm FE	✓	✓	✓	✓	✓	✓	✓	✓
Controls		✓		✓		✓		✓
N of Obs	44,499	44,499	50,928	50,928	49,796	49,796	43,616	43,616

<i>Panel B: GLS using Forecasted EPS</i>				
	EP $\times 100$		RIV $\times 100$	
	(1)	(2)	(3)	(4)
ATT	-0.274*** (-3.26)	-0.374*** (-4.62)	-0.300*** (-3.38)	-0.415*** (-4.75)
Year FE	✓	✓	✓	✓
Firm FE	✓	✓	✓	✓
Controls		✓		✓
N of Obs	93,232	93,232	91,855	91,855

Table OA4: Anti-SLAPP Legislation and COE: Alternative DiD Methods

This table reports results examining the effect of anti-SLAPP legislation on a firm's implied COE over the period from 1987 to 2023 with alternative DiD estimation methods. The dependent variable is a firm's implied COE ($GLS \times 100$) defined using the model of [Gebhardt et al. \(2001\)](#). *ATT* measures the average treatment effect of anti-SLAPP legislation on a firm's COE. Columns 1–2 present results from DiD estimates using the method from [Wooldridge \(2025\)](#), and columns 3–4 present results from stacked DiD estimates ([Cengiz et al., 2019](#)). The number of observations from [Wooldridge \(2025\)](#) regressions is larger than that in Table 5 because the imputation-based DiD estimator ([Borusyak et al., 2024](#)) excludes treated observations for which no counterfactual can be imputed, whereas the [Wooldridge \(2025\)](#) estimator retains all such observations. Lagged firm control variables include $Ln(MVE)$, BM , $BLEV$, MOM , $CashFlow$, $Ln(Analysts)$, $Ln(FrcstDisp)$, and $LTGRate$. Lagged state control variables include $Ln(GDP)$, $\% \Delta GDP$, $TaxRate$, $UnempRate$, $DemGov$, $Ln(UnempBen)$, $Marijuana$, Age , and $EDUC$. All variables are defined in Appendix B. *t*-statistics in parentheses are calculated from standard errors clustered by state. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

	Wooldridge (2025)		Stacked DiD	
	(1)	(2)	(3)	(4)
ATT	-0.726*** (-4.69)	-0.638*** (-5.66)	-0.357*** (-2.74)	-0.402*** (-3.31)
Year FE	✓	✓		
Firm FE	✓	✓		
Controls		✓		✓
Year $\times$ Cohort FE			✓	✓
Firm $\times$ Cohort FE			✓	✓
N of Obs	69,960	69,960	308,010	308,010

Table OA5: Anti-SLAPP Legislation and COE: Industry-Year FEs, Incorporation-Year FEs, Additional Law Controls, and Propensity Score Weighting

This table reports results from the imputation-based DiD method examining the effect of anti-SLAPP legislation on a firm's implied COE over the period from 1987 to 2023. The dependent variable is a firm's implied COE ($GLS \times 100$) defined using the model of [Gebhardt et al. \(2001\)](#). *ATT* measures the average treatment effect of anti-SLAPP legislation on a firm's COE. Columns 1–2 control for two-digit SIC industry-year fixed effects. Columns 3–4 control for incorporation-state-by-year fixed effects. Columns 5–6 further include lagged controls for related state-level laws and policies: investor tax credits, data breach notification laws, pre-employment background-check restrictions, intellectual property rights protection, FOIA law index, salary history bans, paid sick leave laws, paid family and medical leave laws, opioid prescription-limit laws, ban-the-box laws, LGBTQ nondiscrimination laws, and smoking bans. Except for the FOIA law index, these additional law controls are measured as indicator variables. Columns 7–8 use the imputation-based DiD method but weight the regressions by the inverse of the probability of treatment. We estimate the weights from a logit regression of the probability that a firm is in a state that will pass an anti-SLAPP law in the following year, using the same set of firm- and state-level controls as in our main regressions. For this regression, treated firms are those in states that will pass an anti-SLAPP law in year t and control firms are those in states that never passed or have not yet passed an anti-SLAPP law in the year before a legislation event occurs. Lagged firm control variables include $Ln(MVE)$, BM , $BLEV$, MOM , $CashFlow$, $Ln(Analysts)$, $Ln(FrcstDisp)$, and $LTGRate$. Lagged state control variables include $Ln(GDP)$, $\% \Delta GDP$, $TaxRate$, $UnempRate$, $DemGov$, $Ln(UnempBen)$, *Marijuana*, *Age*, and *EDUC*. All variables are defined in Appendix B. t -statistics in parentheses are calculated from standard errors clustered by state. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

	SIC2×Year FE		Incorp×Year FE		Additional Laws		PS Weighted	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
ATT	-0.225*** (-3.61)	-0.203*** (-3.93)	-0.440*** (-3.56)	-0.422*** (-4.40)	-0.283*** (-3.65)	-0.342*** (-5.49)	-0.543*** (-4.67)	-0.480*** (-5.65)
SIC2×Year FE	✓	✓						
Incorp×Year FE			✓	✓				
Firm FE	✓	✓	✓	✓	✓	✓	✓	✓
Year FE					✓	✓	✓	✓
Controls		✓		✓		✓		✓
Additional Laws					✓	✓		
N of Obs	51,159	51,159	49,061	49,061	51,455	51,455	50,748	50,748

Table OA6: Heterogeneity: Effect of Strength of Anti-SLAPP Statutes

This table reports results from the imputation-based DiD method examining the effect of anti-SLAPP legislation strength on a firm's implied COE over the period from 1987 to 2023. The dependent variable is a firm's implied COE ($GLS \times 100$) defined using the model of Gebhardt et al. (2001). Using information from the Institute for Free Speech's 2023 anti-SLAPP report card, we create quartiles ($ATT - Q1$ to $ATT - Q4$) based on the strength of a state's anti-SLAPP statutes. The overall score (*Overall Score*, columns 1–2) has two components: one-third is based on the procedural protections for speakers in each state's law (*Procedure Score*, columns 3–4) and two-thirds on the scope of speech that the statute covers (*Covered Score*, columns 5–6). In columns 5–6, 49% of firms have a *Covered Score* of 100, so the third and fourth quartiles are grouped together. Lagged firm control variables include $Ln(MVE)$, BM , $BLEV$, MOM , $CashFlow$, $Ln(Analysts)$, $Ln(FrcstDisp)$, and $LTGRate$. Lagged state control variables include $Ln(GDP)$, $\% \Delta GDP$, $TaxRate$, $UnempRate$, $DemGov$, $Ln(UnempBen)$, *Marijuana*, *Age*, and *EDUC*. All variables are defined in Appendix B. *t*-statistics in parentheses are calculated from standard errors clustered by state. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

	Split by Overall Score		Split by Procedure Score		Split by Covered Score	
	(1)	(2)	(3)	(4)	(5)	(6)
ATT - Q1	-0.288* (-1.85)	-0.324** (-2.51)	-0.237 (-1.51)	-0.303** (-2.31)	-0.288* (-1.85)	-0.324** (-2.51)
ATT - Q2	-0.228* (-1.86)	-0.286** (-2.19)	-0.616*** (-6.53)	-0.498*** (-6.53)	-0.340*** (-2.96)	-0.423*** (-3.26)
ATT - Q3	-0.677*** (-6.17)	-0.620*** (-7.92)	-0.673*** (-6.15)	-0.616*** (-7.53)	-0.644*** (-5.85)	-0.576*** (-7.42)
ATT - Q4	-1.184*** (-7.51)	-1.245*** (-6.41)	-0.487*** (-4.58)	-0.580*** (-4.69)		
Year FE	✓	✓	✓	✓	✓	✓
Firm FE	✓	✓	✓	✓	✓	✓
Controls		✓		✓		✓
N of Obs	51,455	51,455	51,455	51,455	51,455	51,455

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