

Suppressing Third-Party Information and Firms' Cost of Equity: Evidence from Anti-SLAPP Laws

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Abstract

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Keywords: Anti-SLAPP Laws, Cost of Equity, Third-Party Information, Information Asymmetry, External Verification, Information Suppression

JEL Classification: G12, G30, G32, D82, K22

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October 2, 2026

ABSTRACT

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1 Introduction

Understanding the factors that determine firms' financing costs is central to corporate finance. Existing research shows that firms' information environments affect the returns investors require to supply equity capital (e.g., [Diamond and Verrecchia, 1991](#); [Botosan, Plumlee, and Xie, 2004](#); [Easley and O'Hara, 2004](#); [Lambert, Leuz, and Verrecchia, 2007](#)). Much of this research, however, focuses on information supplied through firms' own financial reporting and disclosure systems. Yet investors also learn about firms from third-party sources, such as employees, customers, journalists, analysts, short sellers, and activists, whose information can complement, challenge, or contradict what managers choose to disclose. Far less is known about how financing costs respond to independently generated third-party information, particularly when firms can suppress its production and dissemination.

Third-party information is economically distinct from managerial disclosure because it can provide external verification beyond what managerial disclosure can establish on its own. This distinction is especially important for adverse information, as managers may have incentives to delay, soften, or withhold information about product-quality problems, workplace conditions, misconduct, or other unfavorable developments. Moreover, managers may have difficulty convincing investors that such problems do not exist. A managerial assertion that a firm faces no employee complaints, compliance failures, or other latent risks may provide limited reassurance given managers' incentives and informational advantages. Information produced independently by third parties can thus be valuable not simply because it increases the amount of information available, but because it can help investors evaluate risks that firm-controlled disclosure alone may not resolve.

Greater access to unfavorable third-party information nevertheless has ambiguous implications for a firm's financing costs. On one hand, newly surfaced information may reveal adverse conditions that investors had not previously incorporated into their assessments of the firm's risk (e.g., [Dyck, Morse, and Zingales, 2010](#); [Fang, Huang, and Karpoff, 2016](#); [Lee, Ng, Yoo, and Zhang, 2026](#)). If this information leads investors to revise upward the priced risk they assign to the firm, required returns

may increase. Third-party information may also be incomplete, conflicting, exaggerated, or hard to verify, thereby increasing disagreement and uncertainty about firm fundamentals (e.g., [Carlin, Longstaff, and Matoba, 2014](#)). On the other hand, independently generated information can make otherwise difficult-to-observe risks easier to evaluate. By improving external verification, third-party information can reduce information asymmetry and uncertainty about adverse conditions, improving investors' understanding of firm fundamentals and lowering required returns (e.g., [Diamond and Verrecchia, 1991](#); [Easley and O'Hara, 2004](#); [Lambert et al., 2007](#)). Thus, greater availability of unfavorable third-party information can raise financing costs through the priced-risk and noise channels or reduce them through improved external verification and lower information asymmetry.

In this paper, we study this tradeoff using the staggered adoption of state 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 (e.g., [Canan and Pring, 1988a,b](#); [Pring and Canan, 1996](#)). Even when the underlying claims ultimately fail, the expected costs of defending a lawsuit can discourage third parties from producing or disseminating unfavorable information. Anti-SLAPP laws weaken this deterrent by providing procedural protections that allow courts to dismiss meritless lawsuits against critics earlier, limit costly discovery, and allow prevailing defendants to recover attorneys' fees.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 suppress third-party information.

Recent research shows that anti-SLAPP laws affect the production and dissemination of unfavorable information about public firms. Stronger protections are associated with more adverse information reaching markets, less managerial concealment of bad news, and greater employee disclosure (e.g., [Chen, Li, and Li, 2025](#); [Böke, Brau, Glaeser, and Jeong, 2026](#); [Lee et al., 2026](#)). But

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. Sharper Image subsequently paid Consumers Union \$525,000 in attorneys' fees and costs. Online Appendix [OA](#) provides additional examples involving public firms.

because greater unfavorable information can raise required returns either by revealing previously unrecognized adverse conditions that increase priced risk or by introducing informational noise, or lower them through improved external verification, this evidence does not imply a clear prediction for firms' financing costs. We fill this gap by examining whether reducing firms' ability to impede adverse third-party information increases or decreases their cost of equity (COE) and, more broadly, whether this ability constitutes a priced information friction in equity markets.

The staggered adoption of anti-SLAPP statutes provides variation across firms and over time in their ability to suppress unfavorable third-party information. Since 1992, 40 states and the District of Columbia have enacted such protections, with substantial variation in adoption timing and statutory strength. States generally adopted these laws in response to concerns about free speech, litigation costs, judicial efficiency, and high-profile attempts to silence critics rather than changes in firms' financing conditions. Consistent with this institutional history, anti-SLAPP adoption is not systematically predicted by the average COE of firms headquartered in the state or by a broad set of state economic, political, and demographic characteristics. Firms in adopting and non-adopting states are also broadly similar along observable dimensions before enactment.

We estimate the effect of anti-SLAPP adoption using the imputation-based difference-in-differences (DiD) estimator of [Borusyak, Jaravel, and Spiess \(2024\)](#). Our primary outcome is firms' *ex ante* COE, measured as the implied discount rate that equates current stock prices with expected future cash flows. We find that firms in adopting states experience a 45.5-basis-point decline in COE relative to firms in states that have not yet adopted or never adopt anti-SLAPP protections, representing approximately 6.9% of the sample mean. Event-study estimates show no evidence of differential trends before adoption, followed by a decline in COE after enactment that persists in subsequent years.

The decline in COE is robust to alternative measures and to tests addressing key identification concerns. Our baseline COE measure follows the residual-income model of [Gebhardt, Lee, and Swaminathan \(2001\)](#) and uses analyst earnings forecasts. Because anti-SLAPP laws could affect

analysts' forecasts and thereby influence the implied COE estimates, we also construct COE measures using model-generated earnings forecasts following [Li and Mohanram \(2014\)](#). We find a similar decline using these measures, reducing concerns that the results reflect changes in analyst forecasts rather than investors' required returns. The findings are also robust to using the implied COE measures of [Claus and Thomas \(2001\)](#), [Easton \(2004\)](#), and [Ohlson and Juettner-Nauroth \(2005\)](#).

We further address concerns about firms' legal exposure. Because the headquarters state may imperfectly capture where firms operate, we show that our results hold when restricting the sample to geographically concentrated firms and when replacing the treatment variable with establishment- and employee-location-weighted measures of anti-SLAPP exposure constructed from Data Axle and Revelio Labs. The results also remain similar when we address state-versus-federal jurisdiction and venue-shopping concerns by excluding firms in consumer-facing industries, excluding firms headquartered in California and New York, and estimating the effects separately before and after the 2010 Supreme Court decision (*Hertz Corp. v. Friend*) that tied a corporation's principal place of business to its headquarters. The findings are robust to alternative DiD estimators, richer fixed effects, controls for concurrent state laws, placebo adoption dates, and other specification checks. Finally, the effect is also larger in states with stronger anti-SLAPP protections, consistent with a dose-response relation between the strength of speaker protections and firms' COE. Together, these tests reduce concerns that COE measurement, geographic assignment of legal exposure, or correlated changes in firms' state-level environments drive the results.

To interpret the decline in COE, we next examine whether anti-SLAPP protections meaningfully alter firms' information environment in our sample. Following adoption, RavenPack identifies more negative news about a firm, while employees on Glassdoor report less favorable business outlooks, lower willingness to recommend their employers, and lower CEO approval. We view these findings primarily as corroborating evidence that stronger anti-SLAPP protections increase the amount of unfavorable information reaching investors, rather than as distinct contributions of the paper. We also examine firms' own 10-K disclosures about the litigation and reputational consequences of outside

criticism. Firms become less likely to report criticism- and speech-related litigation in the “Legal Proceedings” sections of their 10-K filings, while becoming more likely to disclose reputational exposure in their “Risk Factors” sections. Overall, these findings indicate that anti-SLAPP laws have economically meaningful consequences for the flow of unfavorable information and for firms’ exposure to criticism-related litigation and reputational risk.

We then examine why greater third-party information lowers rather than raises firms’ COE. The evidence is most consistent with improved external verification reducing information asymmetry and uncertainty about hidden firm risks. Following anti-SLAPP adoption, bid-ask spreads, illiquidity, and the probability of informed trading decline, consistent with a less asymmetric information environment. Market-based measures of firm risk also decline, including total return volatility, option-implied volatility, idiosyncratic volatility, and market beta. Firms become less likely to be undervalued relative to fundamentals, consistent with investors being better able to evaluate firms when independently generated information supplements firm-controlled disclosure. Finally, stock price crash risk declines, corroborating existing evidence that stronger anti-SLAPP protections facilitate bad-news revelation and reduce the accumulation of concealed adverse information (e.g., [Chen et al., 2025](#); [Lee et al., 2026](#)). Taken together, these findings are consistent with greater third-party information improving external verification, reducing information asymmetry and uncertainty about hidden firm risks, and thereby lowering firms’ COE.

Cross-sectional tests further support the external-verification interpretation. If third-party information is especially valuable when firm-controlled disclosure provides a weaker basis for assessing hidden risks, the decline in COE should be stronger in such settings. Consistent with this prediction, the decline is larger among firms with less transparent information environments, including firms without management earnings forecasts, with larger analyst forecast errors, with higher absolute discretionary accruals, and with greater R&D intensity. The effect is also stronger among firms with weaker internal governance, where independently generated information can provide an additional source of verification beyond internal monitoring. Finally, the decline is larger

among firms more exposed to public scrutiny, including those with greater media coverage, those with a Glassdoor rating, and those with greater criticism-related legal and reputational exposure in their 10-K disclosures. These patterns are consistent with anti-SLAPP protections having larger financing-cost effects when third-party information is more valuable for independently evaluating otherwise difficult-to-observe firm risks.

Our primary contribution is to the literature on information and firms' cost of capital. Prior research shows that disclosure quality, information asymmetry, and accounting information affect expected returns and firms' COE (e.g., [Diamond and Verrecchia, 1991](#); [Botosan et al., 2004](#); [Easley and O'Hara, 2004](#); [Francis, LaFond, Olsson, and Schipper, 2005](#); [Lambert et al., 2007](#); [Dambra, Velikov, and Weber, 2024](#)). This literature has focused predominantly on information supplied through firms' own reporting and disclosure systems. We identify a distinct dimension of firms' information environments: their ability to impede independently generated third-party information that can provide external verification beyond what managerial disclosure can establish. This distinction matters because managers may have difficulty convincing investors that no undisclosed adverse conditions exist, particularly when the relevant information concerns workplace conditions, product quality, misconduct, or other risks that third parties may independently observe. Our findings show that investors price the institutional constraints on this external-verification process. When firms have less ability to impede unfavorable third-party information, their information asymmetry and COE decline, consistent with reduced uncertainty about hidden firm risks. Thus, firms' ability to suppress third-party information constitutes a priced information friction.

We also contribute to research on external monitoring, bad-news revelation, and the economic consequences of anti-SLAPP laws. Prior work shows that employees, journalists, analysts, short sellers, activists, and other third parties can reveal information that managers may prefer to withhold (e.g., [Miller, 2006](#); [Dyck et al., 2010](#); [Fang et al., 2016](#); [Appel, 2019](#); [Da and Huang, 2020](#); [Dyer, Lang, and Oh, 2025](#)). More recent studies show that anti-SLAPP protections affect unfavorable information production, bad-news concealment, employee disclosure, stock price crash risk, and

other corporate outcomes (e.g., [Chemmanur, Rajaiya, and Sheng, 2020](#); [Griffin, Hong, Jung, Moon, and Ryou, 2026](#); [Chen et al., 2025](#); [Jung, Wang, Wei, and Zhang, 2025](#); [Böke et al., 2026](#); [Lee et al., 2026](#); [Wu, Bruton, and Krause, 2026](#)). We extend this literature by showing that changes in third-party information production do not, by themselves, determine the resulting capital-market effects. Greater unfavorable third-party information can raise required returns through the priced-risk and noise channels, but it can also lower required returns by improving external verification and reducing uncertainty about hidden risks. We find that stronger anti-SLAPP protections lower firms' COE, showing that legal protections for third-party information production have meaningful financing consequences even when the additional information reaching investors is unfavorable.

2 Institutional Background and Hypotheses

2.1 Institutional background

SLAPPs are lawsuits used by firms or other powerful actors to deter unfavorable third-party speech by imposing burdensome litigation costs ([Canan and Pring, 1988a,b](#)).² They can target a broad range of 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 third parties from producing or disseminating unfavorable information. SLAPPs can therefore impede the flow of independently generated information about firms to investors and other market participants.

Anti-SLAPP statutes reduce this deterrent effect by providing defendants with procedural tools to challenge such 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

²The Institute for Free Speech estimates the median defense cost for a SLAPP lawsuit at about \$39,000, and even relatively simple cases can exceed \$100,000, with some running into the millions ([Keating, 2022](#)).

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 third-party speakers and thereby weaken firms’ ability to suppress unfavorable information through litigation (Greenberg, Keating, and Knowles-Gardner, 2023).³

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 third-party speakers 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).⁴ 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, Chen, and Sun, 2026a; Chen, Ke, and Kuang, 2026b; Wu et al., 2026). These motivations are important for our empirical design because they suggest that adoption was driven by concerns about speech and litigation rather than changes in firms’ financing conditions. The staggered enactment of anti-SLAPP statutes therefore provides plausibly exogenous variation in firms’ ability to impede unfavorable third-party information.

³In Section 4.2, we exploit cross-state variation in statutory strength. We use adoption rather than strength as our primary treatment because our baseline estimand is the effect of moving from no statutory anti-SLAPP protection to a regime that protects third-party speakers. Statutory strength instead captures treatment intensity that need not represent economically equivalent changes in legal protection. Using strength as the primary treatment would therefore change the estimand and complicate implementation of the imputation-based DiD, which requires defining treatment onset and aggregating effects across units exposed to heterogeneous treatment doses. We instead use adoption to define treatment and examine statutory strength separately as complementary dose-response evidence.

⁴Pring and Canan (1996) describe SLAPPs as widespread and argue that the threat of litigation mutes many speakers. Focusing on libel cases, Lee et al. (2026) estimate 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. 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 third parties from disclosing unfavorable information about their products, workplace conditions, misconduct, operational problems, or other risks. Anti-SLAPP laws limit firms' ability to use litigation to impede such information and therefore increase the likelihood that independently generated third-party information reaches investors. The effect of this additional information on firms' COE, however, is theoretically ambiguous.

One possibility is that limiting firms' ability to suppress unfavorable third-party information lowers the COE by improving external verification. Third-party information can be particularly valuable when managers have incentives to withhold or strategically frame adverse information and when firm-controlled disclosure provides limited assurance about whether unfavorable conditions exist. Managers may bear private costs from revealing adverse information, including reputational damage, litigation exposure, regulatory scrutiny, or career concerns, and some adverse conditions are difficult for managers to establish as absent through firm-controlled disclosure alone. For example, managerial assertions that a firm faces no significant employee complaints, product-quality problems, compliance issues, or other latent concerns may provide limited reassurance given managers' incentives and informational advantages. Information generated independently by third parties can therefore help investors evaluate risks that managerial disclosure alone may not resolve.

Improved external verification can lower financing costs through several related channels. Information asymmetry is an important determinant of firms' COE (e.g., [Diamond and Verrecchia, 1991](#); [Francis et al., 2005](#); [Beyer, Cohen, Lys, and Walther, 2010](#)), and more informative signals can reduce uncertainty and improve investors' assessments of firm fundamentals (e.g., [Leuz and Verrecchia, 2000](#); [Verrecchia, 2001](#); [Easley and O'Hara, 2004](#); [Lambert et al., 2007](#)). When unfavorable third-party information is more difficult for firms to suppress, investors may face less uncertainty about otherwise difficult-to-observe risks, place less weight on the possibility that important adverse information remains concealed, and assess firms' expected cash flows and risk

more precisely. Greater third-party information can also improve price discovery and limit the accumulation of undisclosed bad news (Kothari, Shu, and Wysocki, 2009). Under this view, reducing firms' ability to suppress unfavorable third-party information should lower firms' COE.

A competing possibility is that reducing firms' ability to suppress unfavorable third-party information raises the COE. Stronger protections may allow previously deterred speakers to reveal governance weaknesses, product-quality concerns, workplace problems, regulatory noncompliance, environmental liabilities, or other adverse conditions that investors had not incorporated into their assessments of firm value and risk. Investors may consequently revise expected cash flows downward, priced risk upward, or both, increasing the return they require to hold the firm's equity.

Greater third-party information may also raise financing costs even when it does not reveal objectively greater underlying risk. Information generated by employees, activists, customers, journalists, and other third parties can be incomplete, exaggerated, conflicting, or difficult to verify. If stronger protections increase the quantity of such signals without proportionately increasing their precision, investors may face greater disagreement or uncertainty about firm fundamentals. Thus, additional third-party information can increase required returns either because it reveals previously unrecognized adverse conditions or because it introduces sufficiently noisy information into the market (the priced-risk and noise channels, respectively). Under this view, reducing firms' ability to suppress unfavorable third-party information should raise firms' COE.

The predicted effect of anti-SLAPP protections on the COE is therefore ambiguous. Stronger protections for third-party speakers increase the likelihood that unfavorable information reaches investors, but they do not determine how investors price that information. The COE will decline if the benefits of improved external verification and reduced uncertainty about hidden firm risks outweigh any increase in priced risk or informational noise. Conversely, the COE will rise if newly revealed adverse conditions or noisy signals dominate the benefits of greater external verification.⁵

⁵Although we interpret our findings as arising primarily from changes in third-party information, two alternative explanations merit consideration. First, anti-SLAPP laws could affect the COE by altering expected legal costs rather than information flows. These laws primarily reduce expected litigation costs for protected speech while preserving legitimate legal claims. At the same time, by making unfavorable third-party information more likely to emerge, they may increase firms' exposure to regulatory investigations or enforcement actions. The predicted effect of a legal-cost channel

3 Data and Empirical Methodology

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 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.⁶

We measure a firm’s COE following the *ex ante* approach developed by [Gebhardt et al. \(2001, hereafter GLS\)](#), which estimates the implied COE using the residual income valuation model

is therefore ambiguous. Second, anti-SLAPP protections could affect firms’ COE by changing operating policies if greater third-party 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, 2022](#)), rather than greater managerial risk avoidance.

⁶A benefit of using analyst forecasts made at time t is that if anti-SLAPP laws change a firm’s expected cash flows, analysts can revise their forecasts after adoption, so that forecasts and current prices incorporate information known at time t . This adjustment is most complete at the explicit forecast horizons (years $t + 1$ through $t + 3$ in the [Gebhardt et al. \(2001\)](#) model); beyond them, the model fades return on equity toward the industry median, so persistent changes in long-run profitability may be only partly captured.

(see Appendix A for details). This method uses 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, Sinha, and Swaminathan, 2008](#); [Lee, Ng, and Swaminathan, 2009](#); [Chava and Purnanandam, 2010](#); [Chen, Kacperczyk, and Ortiz-Molina, 2011a](#); [Chen, Huang, and Wei, 2013](#); [Hann, Ogneva, and Ozbas, 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 first broad 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 (*The 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), medical marijuana legalization dates ([Guernsey, Serfling, and Yan, 2026](#)), and demographic characteristics, namely the average age of the state’s population and the fraction of the population aged 25 or older with a bachelor’s degree (Current Population Survey).

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 dollar-denominated variables are in 2020 dollars. Appendix B provides variable definitions. Table 1 reports anti-SLAPP adoption dates,⁷ 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 account for serial correlation and cross-sectional dependence among firms exposed to the same legal environment.⁸

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.

⁷We 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., 2026](#); [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. \(2026\)](#).

⁸Recent work raises concerns about statistical inference when the same policy shocks are repeatedly used as natural experiments across different studies. In particular, [Heath, Ringgenberg, Samadi, and Werner \(2023\)](#) show that conventional inference can overstate statistical significance when researchers reuse a common set of policy changes without accounting for the resulting dependence across estimates. This concern is especially relevant for findings that are only marginally statistically significant. Our main COE results, however, are estimated precisely. Across the baseline specifications in Table 5, the absolute t -statistics on the treatment effect range from 3.97 to 5.46. Thus, although the broader concern about repeated use of state-level policy shocks is important, our central inference does not depend on estimates that lie close to conventional significance thresholds.

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 other estimators address staggered adoption (e.g., [Callaway and Sant’Anna, 2021](#); [Sun and Abraham, 2021](#); [Wooldridge, 2025](#)), the imputation estimator is 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 headquarters is located. This treatment assignment follows prior studies examining anti-SLAPP laws and public firms (e.g., [Griffin et al., 2026](#); [Chen et al., 2025](#); [Jung et al., 2025](#); [Böke et al., 2026](#); [Chen et al., 2026b](#); [Lee et al., 2026](#); [Wu et al., 2026](#)) and is supported by economic and legal considerations. Economically, headquarters serves as the primary center of managerial decision-making, corporate communications, and firm identity. Legally, headquarters frequently provides a principal nexus for alleged reputational injury and litigation involving firm-specific criticism. Although firms operate

across multiple jurisdictions, their headquarters provides 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 can 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 address these issues in detail.

3.4 Threats to identification

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

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 venue shopping.

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; the models also include 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 -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. We then provide evidence that anti-SLAPP laws meaningfully alter firms' information environment. Next, we examine why greater access to third-party information lowers rather than raises firms' COE. Finally, we test whether the decline in COE is stronger where independently generated information should provide greater verification value.

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 implies a decline of 45.5 basis points in column 1, which includes firm and year fixed effects (t -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 -stat = -3.97) and both firm- and state-level controls in column 3 (45.5 basis points; t -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, Lin, and Ma, 2024; Guernsey et al., 2026).

Dynamic treatment effects further support a causal interpretation. Figure 1 plots event-study estimates for the ten years before and after anti-SLAPP adoption, estimated both with and without controls. The 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 several robustness tests to address various identification threats and alternative explanations. Across all analyses, the estimated decline in firms' COE remains robust.

Alternative COE measures. Our findings are also robust to alternative measures of firms' implied COE. Panel A of Table 6 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.¹⁰ The estimated decline is similar across all approaches.

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. We address this concern in two complementary ways. First, we replace the headquarters-based treatment indicator with continuous measures of firm-level anti-SLAPP exposure based on the locations of the firm's establishments and employees. Using Data

¹⁰Model-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 A.

Axle, we construct an employment-weighted measure of the share of a firm’s establishments located in anti-SLAPP law states. Using Revelio Labs, we construct a corresponding employment-weighted measure based on employee locations. We estimate these specifications by OLS with the baseline fixed effects. Table OA1 shows that the estimates remain negative and statistically significant for both exposure measures.

Second, Panel A of Table OA2 addresses 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 OA3 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.¹¹ Third, we estimate the ATT separately for the periods 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 (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

¹¹In untabulated analyses, excluding any individual state does not alter our conclusions.

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.¹² 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. Together, these analyses suggest that the baseline findings are unlikely to reflect omitted variables or mechanical features of staggered policy adoption.

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 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.

¹²The 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 .

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.

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, the choice of estimator, or concurrent state-level reforms.

4.3 Evidence on the information-suppression friction

Our interpretation of the COE results requires that anti-SLAPP laws meaningfully alter firms’ ability to impede unfavorable third-party information. We examine this implication in two ways. First, we revisit two information outcomes examined in prior work. Using the same imputation-based DiD design as in our main analysis, we test whether anti-SLAPP adoption is followed by more negative news about a firm in RavenPack and less favorable employee assessments on Glassdoor. Second, we provide new evidence from firms’ 10-K filings on whether stronger speech protections alter criticism- and speech-related litigation and firms’ disclosed exposure to reputational risk.

4.3.1 Unfavorable information production

Prior research shows that anti-SLAPP protections increase the production and dissemination of unfavorable information about firms: media coverage becomes more negative ([Chen et al., 2025](#)), employees disclose more unfavorable and more credible information in online reviews ([Chemmanur et al., 2020](#); [Jung et al., 2025](#); [Böke et al., 2026](#)), and firms conceal less bad news and issue more

negative press releases (Lee et al., 2026). We revisit this relation for two reasons. First, it provides a useful validation that anti-SLAPP laws alter the production of unfavorable information within the sample used for our COE tests. Second, we estimate these effects using the same imputation-based DiD framework employed throughout the paper, avoiding comparisons between already-treated and newly treated firms that can arise in conventional staggered-treatment specifications.

Table OA7 reports results using two distinct information sources. The RavenPack measure captures negative news about a firm from a broad set of news sources, while Glassdoor captures assessments by current and former employees who may possess information about workplace conditions, managerial quality, internal operations, and firm prospects. Consistent with prior evidence, anti-SLAPP adoption is followed by more negative news in RavenPack and less favorable employee assessments on Glassdoor. We view these results as confirmatory rather than as a distinct substantive contribution. Their role is to establish, using our sample and empirical design, that stronger anti-SLAPP protections are associated with greater production of unfavorable information about firms.

4.3.2 Litigation exposure and reputational risk

An increase in unfavorable information alone does not establish that anti-SLAPP laws materially constrain firms' ability to deter such information through litigation. We therefore provide new evidence on how adoption affects the litigation and reputational environment of public firms. Specifically, we examine firms' disclosures of criticism- and speech-related legal proceedings in Item 3 "Legal Proceedings" and reputational risk in Item 1A "Risk Factors" of their 10-K filings. *Item 3 Criticism* equals one if a firm's Item 3 contains at least one term related to external criticism. *Item 3 Speech* equals one if Item 3 contains at least one term related to legal or contractual restrictions on information flows. *Item 1A Reputation* equals one if the firm's Item 1A contains at least one term related to reputational risk. Appendix B provides variable definitions.

If anti-SLAPP laws reduce firms' ability to deter unfavorable outside speech through litigation, firms should become less entangled in criticism- and speech-related legal proceedings. At the same time, stronger protection for outside speakers may increase firms' exposure to reputational consequences as adverse information becomes more difficult to suppress. We therefore 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. Following adoption, firms report less criticism- and speech-related litigation and disclose greater reputational exposure. Relative to the corresponding sample means, speech-related litigation disclosures decline by approximately 7.2% to 8.2% and reputational-risk disclosures increase by roughly 4.1% to 5.4%; the decline in criticism-related disclosures is economically large relative to its low unconditional mean.¹⁴

Taken together, the evidence indicates that anti-SLAPP laws affect an economically meaningful information-suppression friction facing public firms. Section 4.3.1 confirms, using our sample and imputation-based DiD design, the prior finding that stronger speech protections increase the flow of unfavorable information about firms. The 10-K evidence extends this literature by showing that adoption is also associated with fewer disclosures of criticism- and speech-related litigation and greater disclosed reputational exposure. The increase in disclosed reputational exposure reflects the greater salience of outside criticism in firms' risk-factor disclosures; as Section 4.4 shows, market-based measures of firm risk decline rather than rise. These results support the interpretation that anti-SLAPP laws reduce firms' ability to impede independently generated adverse information rather than merely changing formal legal protections with limited relevance for capital markets.

4.4 How third-party information affects investors' assessments

We next examine why stronger protections for third-party information are associated with a lower COE for firms. There are two competing possibilities. If limiting information suppression improves

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.

investors' ability to assess otherwise difficult-to-observe firm risks, anti-SLAPP adoption should reduce information asymmetry and lower investors' assessments of firm risk. Alternatively, the decline in COE could reflect a verification benefit that merely outweighs a countervailing increase in priced risk or informational noise; in that case, information asymmetry need not fall and investors' assessments of firm risk could rise. We therefore examine changes in market-based measures of information asymmetry and firm risk following adoption, and then two further implications of improved verification: undervaluation relative to fundamentals and stock price crash risk.¹⁵

4.4.1 Anti-SLAPP legislation and information asymmetry

If independently generated information provides investors with external verification beyond firm-controlled disclosure, information asymmetry should decline when such information becomes more difficult to suppress. Outside information can corroborate or challenge managerial disclosures and provide additional evidence about conditions that managers may be unable to credibly certify as absent. As a result, market participants should rely less on privately held information and face lower adverse-selection costs. 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 rather than liquidity trading. Together, these measures capture complementary dimensions of information asymmetry in equity markets.

Table 8 reports results consistent with an improvement in external verification. Bid-ask spreads, illiquidity, and PIN all decline following anti-SLAPP adoption. In the fully specified regressions, bid-ask spreads decline by 0.101 percentage points (t -stat = -5.49), illiquidity declines by 0.084 (t -stat

¹⁵Stronger speech protections may affect the information environment through both realized third-party information production and managers' responses to the threat of greater outside scrutiny. Prior work shows that anti-SLAPP protections increase whistleblowing and other forms of outside information production and reduce managerial bad-news hoarding ([Griffin et al., 2026](#); [Böke et al., 2026](#); [Lee et al., 2026](#)). Both are consistent with the broader mechanism emphasized here: stronger protections reduce firms' ability to impede external verification of adverse information. Importantly, these responses do not determine the sign of the COE effect *ex ante*. The net effect still depends on whether the benefits of improved external verification outweigh any countervailing increase in priced risk or informational noise.

$= -1.91$), and PIN declines by 0.442 percentage points (t -stat $= -2.24$). Across specifications, the declines relative to the corresponding sample means range from 7.2% to 12.2% for bid-ask spreads, 26.6% to 38.9% for illiquidity, and 2.5% to 4.2% for PIN. These findings indicate that stronger protections for third-party speakers are associated with a less asymmetric information environment, consistent with independently generated information providing investors with information that firm-controlled disclosures do not fully substitute for.

4.4.2 Anti-SLAPP legislation and firm risk

If stronger speech protections operate primarily through the priced-risk and noise channels, investors may revise priced risk upward or face greater uncertainty about firm fundamentals. By contrast, if greater third-party information improves investors' ability to assess difficult-to-observe risks, uncertainty surrounding firm fundamentals may decline and investors' assessments of both firm-specific and systematic risk may fall. We examine these possibilities using four complementary measures: total stock return volatility, implied volatility from equity options, idiosyncratic volatility, and market beta. These measures capture realized return variation, investors' expectations of future volatility, firm-specific risk, and exposure to systematic market risk, respectively.

Table 9 shows that all four measures decline 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 $= -2.35$). Across specifications, the estimated declines relative to the corresponding sample means range from 2.7% to 4.5% for total volatility, 2.8% to 3.4% for implied volatility, 2.5% to 4.5% for idiosyncratic volatility, and 3.9% to 4.4% for market beta.

Overall, the results are consistent with greater third-party information improving investors' ability to assess firm fundamentals and reducing the uncertainty and risk premia associated with information that is difficult to verify through firm-controlled disclosure alone. The decline in

market beta merits comment, because information about a single firm might be expected to move firm-specific uncertainty alone. In [Lambert et al. \(2007\)](#), however, better information lowers investors' assessed covariance between a firm's cash flows and those of the market, and, holding the firm's real decisions fixed (the "direct effect" in their terminology), it is this assessed covariance that enters the COE. A lower assessed beta is therefore what their framework predicts when independent information makes fundamentals easier to evaluate. Because our beta is estimated from realized weekly returns, it is only a noisy proxy for investors' assessed beta, but its decline is consistent with this prediction. If anything, the shift toward more risk-intensive investment noted in [Section 2.2](#) works against the declines in volatility and beta and is therefore unlikely to explain them.

4.4.3 Anti-SLAPP legislation and firm undervaluation

If third-party information improves investors' ability to assess otherwise difficult-to-observe firm risks, stronger speech protections should also improve valuation accuracy. When managers have incentives to withhold or strategically frame adverse information, investors may apply a valuation discount because some unfavorable conditions cannot be credibly ruled out using firm-controlled disclosures alone. By making independently generated information more likely to reach the market, anti-SLAPP laws can strengthen external verification and reduce the valuation consequences of this information friction. Under this interpretation, firms should be less likely to trade below values implied by their fundamentals following anti-SLAPP adoption. 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.¹⁶

Table [10](#) reports results consistent with this prediction. Across both valuation models, anti-SLAPP adoption 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

¹⁶Appendix [A](#) describes the valuation models and construction of the undervaluation measures.

[Rhodes-Kropf et al. \(2005\)](#) (t -stat = -2.70). Relative to the corresponding sample means, the estimated declines range from 5.3% to 7.2% using the first model and from 9.1% to 10.4% using the second model. These findings are consistent with stronger protections for third-party information reducing the valuation discount associated with risks that are difficult for firms to credibly self-certify.

4.4.4 Anti-SLAPP legislation and stock price crash risk

Prior research shows that stronger anti-SLAPP protections reduce stock price crash risk by limiting firms' ability to suppress unfavorable information and thereby reducing the accumulation of hidden bad news ([Chen et al., 2025](#); [Lee et al., 2026](#)). We therefore examine crash risk as a confirmatory implication of our setting rather than as a distinct contribution. Following standard approaches, we use four measures based on firm-specific weekly returns: negative conditional skewness (*NCSKEW*), downside-to-upside volatility (*DUVOL*), an indicator for whether the firm experiences at least one crash week during the year (*Crash*), and the number of negative minus positive crash weeks (*CrashCount*). Online Appendix [OB](#) provides variable definitions. Table [OA8](#) confirms the pattern documented in prior work. Anti-SLAPP adoption significantly reduces all four crash-risk measures. Thus, these findings are consistent with stronger protections for third-party speakers allowing adverse information to reach markets more continuously and provide corroborating evidence that anti-SLAPP laws meaningfully affect firms' information environments.

Overall, the evidence in this section is most consistent with stronger protections for third-party speakers improving investors' ability to assess otherwise difficult-to-observe firm risks. Following anti-SLAPP adoption, firms experience lower information asymmetry, lower realized and expected risk, and less undervaluation. We also confirm the previously documented decline in stock price crash risk. These findings provide no evidence that investors' assessment of priced risk rises after adoption, and the decline in information asymmetry suggests that any informational noise is more than offset by improved external verification, providing an explanation for the decline in firms' COE.

4.5 Cross-sectional evidence

We next examine cross-sectional predictions implied by the external-verification interpretation. If third-party information is valuable because it provides external verification beyond firm-controlled disclosure, the decline in COE should be larger when managerial disclosure is a weaker substitute for outside information, when internal monitoring is weaker, and when firms are more exposed to outside information production and public scrutiny.

4.5.1 *Ex ante* information environment

We first examine whether the COE effect is stronger when firm-controlled information provides a weaker substitute for external verification. When firms' information environments are less transparent or managerial disclosures are less informative or credible, investors should place greater value on information generated outside management's control. We examine four proxies for the *ex ante* information environment: management earnings guidance, analyst forecast error, discretionary accruals, and R&D intensity. These measures capture, respectively, the availability of managerial forward-looking information, the precision of analyst expectations, the reliability of financial reporting, and the difficulty of valuing investment opportunities.¹⁷

Table 11 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 firms with weaker and stronger *ex ante* information environments are statistically significant at the 1% level or better across all specifications. These patterns are consistent with third-party information being more valuable when investors cannot rely as heavily on firm-controlled disclosure to assess difficult-to-observe risks.

¹⁷*NoMngFrcst* is an indicator equal to one if the firm does not provide earnings guidance during the year. *FrcstError* 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 expenditures scaled by sales.

4.5.2 *Ex ante* internal governance

We next examine whether the COE effect is stronger for firms with weaker internal governance. If independently generated information can substitute for internal monitoring, stronger protections for outside speakers should be especially valuable when boards and incentive arrangements provide less effective oversight. In such firms, information produced by third parties can provide an additional source of verification that is less dependent on managerial control. 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 12 reports results consistent with this prediction. The decline in COE is 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. These findings are consistent with outside information being particularly valuable when internal mechanisms provide weaker assurance that adverse conditions will be identified and communicated to investors.

4.5.3 *Ex ante* exposure to public scrutiny

Finally, we examine whether the COE effect is stronger when outside stakeholders are more likely to possess, produce, or disseminate value-relevant information. If anti-SLAPP laws operate by reducing firms' ability to impede third-party information, their effects should be larger where greater public scrutiny makes the production of such information more economically relevant. We use three measures of public scrutiny: lagged media coverage from RavenPack, an indicator for whether the firm has a Glassdoor rating, and a 10-K-based measure of criticism-related legal and

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).

reputational exposure. These measures capture exposure to information intermediaries, employee voice, and criticism-related legal and reputational pressure, respectively.¹⁹

Table 13 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 and reputational exposure reflected in their 10-K disclosures. ATT differences are statistically significant across all specifications.

Taken together, the cross-sectional evidence is consistent with the external-verification interpretation of our main findings. The decline in COE is strongest when firm-controlled information is a weaker substitute for independent information, when internal monitoring is weaker, and when outside stakeholders are more likely to produce or disseminate value-relevant information. These patterns are difficult to reconcile with a purely mechanical effect of anti-SLAPP adoption and instead link the financing-cost response to the economic value of independently generated information. They are also difficult to attribute to the legal-cost channel noted in Section 2.2, which does not tie the COE response to the value of outside information.

5 Conclusion

Investors learn about firms not only from what managers choose to disclose, but also from information produced by employees, customers, journalists, analysts, activists, and other outsiders. Yet firms may be able to impede the production and dissemination of this third-party information, particularly when it is unfavorable. We examine whether this ability constitutes a priced information friction by exploiting the adoption of state anti-SLAPP laws, which reduce the expected litigation costs faced by outside speakers and thereby weaken firms' ability to deter unfavorable information.

We find that anti-SLAPP adoption lowers firms' COE and alters the information environment of public firms. Following adoption, firms report less criticism- and speech-related litigation and

¹⁹The 10-K-based measure, *CriticismExposure*, is the sum of three indicator variables—*Item 3 Criticism*, *Item 3 Speech*, and *Item 1A Reputation*. 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.

disclose greater reputational exposure in their 10-K filings. Consistent with prior work, we also find that stronger anti-SLAPP protections are followed by more unfavorable information about firms. We then examine why greater access to unfavorable third-party information lowers rather than raises financing costs. Following adoption, information asymmetry declines, market-based measures of firm risk fall, firms are less likely to be undervalued, and, as documented in prior work, stock price crash risk declines. The reduction in COE is also strongest where independently generated information should provide greater verification value: when firms' existing information environments are less transparent, internal governance is weaker, and exposure to public scrutiny is greater. Taken together, these findings are consistent with stronger protections for third-party information improving investors' ability to evaluate otherwise difficult-to-observe firm risks.

Our findings identify firms' ability to suppress third-party information as an economically important dimension of the corporate information environment. Prior research has focused primarily on the quantity and quality of information firms themselves provide. We show that investors also price the institutional constraints governing information that managers do not control and may prefer not to reveal. More broadly, the information infrastructure of capital markets depends not only on firms' disclosure choices and obligations, but also on whether outsiders can credibly produce and disseminate value-relevant information about firms.

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Appendix A. Estimation of Implied Cost of Equity, Alternative COE Models, Earnings Forecast Models, and Firm Valuation Models

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, $FROE_{t+i}$ converges 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 current book value of equity plus the discounted value of expected future residual income to the current stock price.

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

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

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 that the future return on equity ($FROE$) converges linearly 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.

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 above.

A.1 Ohlson and Juettner-Nauroth (2005) as implemented by Gode and Mohanram (2003)

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

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

$$A = 0.5 \left(g_{lt} + \frac{DPS_{t+1}}{P_t^*} \right), \quad (A-3)$$

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 $FEPS_{t+2} > 0$.

A.2 Claus and Thomas (2001)

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

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. (A-4) are equal within a tolerance of \$0.001.

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

$$P_t^* = \frac{FEPS_{t+1}}{R_{MPEG}} + \frac{FEPS_{t+1} \times (g_{st} - R_{MPEG} \times (1 - POUT))}{R_{MPEG}^2}. \quad (A-5)$$

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. (A-5) within a tolerance of \$0.001.

Earnings Forecast Models

Instead of relying only on analyst forecasts as inputs for the Gebhardt et al. (2001) model, 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.

A.4 Earnings persistence model

$$E_{i,t+\tau} = \beta_0 + \beta_1 NegE_{i,t} + \beta_2 E_{i,t} + \beta_3 NegE_{i,t} \times E_{i,t} + \varepsilon_{i,t+\tau}, \quad (A-6)$$

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 $NegE_{i,t} \times E_{i,t}$.

A.5 Residual income valuation model

$$E_{i,t+\tau} = \beta_0 + \beta_1 NegE_{i,t} + \beta_2 E_{i,t} + \beta_3 NegE_{i,t} \times E_{i,t} + \beta_4 B_{i,t} + \beta_5 TACC_{i,t} + \varepsilon_{i,t+\tau}, \quad (A-7)$$

where $E_{i,t}$, $NegE_{i,t}$, and $NegE_{i,t} \times E_{i,t}$ retain the same definitions as outlined in Eq. (A-6). 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.

Firm Valuation Models

A.6 Hoberg and Phillips (2010) relative valuation model

Following the framework of Hoberg and Phillips (2010), we estimate an industry-level present value model for three-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:

$$\begin{aligned} \left(\frac{M}{B}\right)_{i,k} &= a + b FIRMAGE_{i,k} + c DD_{i,k} + d LEV_{i,k} \\ &+ e \ln(SIZE_{i,k}) + f VOLP_i + g ROA_{i,k}, \quad k = t - 10, \dots, t - 1, \end{aligned} \quad (A-8)$$

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 ($dltr$) 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.7 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 three-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:

$$\begin{aligned} \ln(M)_{i,k} = & a + b \ln(B)_{i,k} + c \ln(|NI|)_{i,k} + d \text{NEG}_{i,k} \times \ln(|NI|)_{i,k} \\ & + e \text{MLEV}_{i,k}, \quad k = t - 10, \dots, t - 1, \end{aligned} \quad (\text{A-9})$$

where M is market value of equity (*prcc_fxcsho*), 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_fxcsho*+*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 the 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.

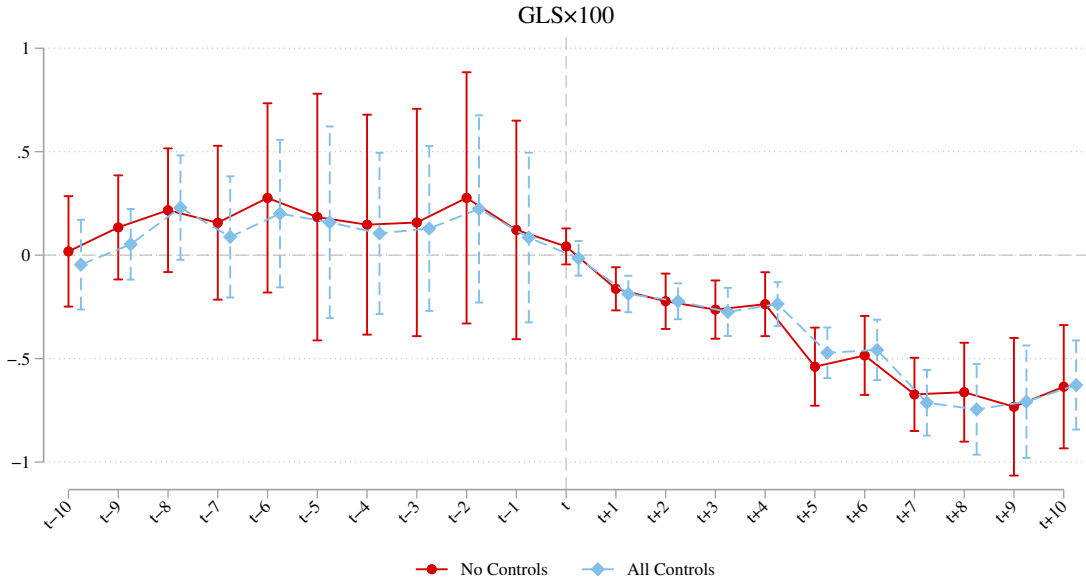
Appendix B. Variable Definitions

This table provides variable definitions. Variables not included here are defined in the corresponding table captions. Compustat variable names are italicized.

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.
AvgCOE	A firm's implied COE derived from analyst EPS forecasts calculated by taking the mean value across four implied COE models (Gebhardt et al., 2001 ; Ohlson and Juettner-Nauroth, 2005 ; Claus and Thomas, 2001 ; Easton, 2004).
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 $[ceq/(prcc_fx\text{csho})]$.
CashFlow	Income before extraordinary items plus depreciation and amortization scaled by book value of assets $[(ib+dp)/at]$.
CT	A firm's implied COE based on Claus and Thomas (2001) .
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.
EDUC	The fraction of a state's population aged 25 or older with a bachelor's degree. Data are from the Current Population Survey.
EP	A firm's implied COE estimated using the method of Gebhardt et al. (2001) with EPS forecasts derived from the earnings persistence model (Li and Mohanram, 2014).
FrcstDisp	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.
FrcstError	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	Annualized standard deviation of weekly idiosyncratic stock returns over a firm's fiscal year. 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 reputational 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.
MPEG	A firm's implied COE based on the modified PEG ratio model by Easton (2004) .
MVE	Market value of equity at the end of each fiscal year (<i>prcc_fxcscho</i>) in millions of 2020 dollars.
NoMngFrcst	An indicator variable that equals one if a firm does not provide management earnings forecasts/guidance during year t , and zero otherwise.
OJN	A firm's implied COE based on Ohlson and Juettner-Nauroth (2005) .
PIN	Average probability of informed trading over a year, following Easley et al. (1996) .
Post ($D_{i,t}$ 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 (<i>xrd/sale</i>).
RIV	A firm's implied COE estimated using the method of Gebhardt et al. (2001) with EPS forecasts derived from a residual income model (Li and Mohanram, 2014).
TaxRate	State-level highest marginal corporate tax rate (in percent).
UnderValued-HP	An indicator variable that equals one if a firm's actual market-to-book asset ratio is below the value predicted by the Hoberg and Phillips (2010) relative valuation model (see Appendix A), and zero otherwise.
UnderValued-RRV	An indicator variable that equals one if a firm's actual market value of equity is below the value predicted by the Rhodes-Kropf et al. (2005) relative valuation model (see Appendix A), and zero otherwise.
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 unemployment rate (in percent).
Volatility	Annualized standard deviation of weekly stock returns over a year.

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., 2026](#); [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
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 passed 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***
%ΔGDP	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 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.

	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)
%ΔGDP	-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)
<i>X</i> 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* is the average treatment effect on the treated, that is, the average effect of anti-SLAPP legislation on the COE of treated firms. 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 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* is the average treatment effect on the treated, that is, the average effect of state-level anti-SLAPP legislation on the COE of treated firms. 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.

<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 7: Anti-SLAPP Legislation, Litigation, and Reputational Risk

This table reports results from the imputation-based DiD method examining the effect of anti-SLAPP legislation on how firms describe criticism- and speech-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 risk. *ATT* is the average treatment effect on the treated, that is, the average effect of anti-SLAPP legislation on each outcome for treated firms. 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.

	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*×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*×100), respectively. *ATT* is the average treatment effect on the treated, that is, the average effect of anti-SLAPP legislation on information asymmetry for treated firms. 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.

	BidAsk×100		Illiq		PIN×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* is the average treatment effect on the treated, that is, the average effect of anti-SLAPP legislation on the risk of treated firms. 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* is the average treatment effect on the treated, that is, the average effect of anti-SLAPP legislation on the likelihood that a treated 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)*, *%ΔGDP*, *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: 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* are the average treatment effects on the treated, that is, the average effects of anti-SLAPP legislation on treated firms with and without management earnings guidance. In columns 3–8, *ATT - Low* and *ATT - High* are the average treatment effects on the treated 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 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 12: 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 (*Delta*), respectively. In columns 1–6, *ATT - Low* and *ATT - High* are the average treatment effects on the treated, that is, the average effects of anti-SLAPP legislation on treated 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 13: Heterogeneity: Effect of *Ex Ante* Public 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 (*CriticismExposure*), respectively. *CriticismExposure* 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* are the average treatment effects on the treated, that is, the average effects of anti-SLAPP legislation on treated firms without and with a Glassdoor rating. In columns 1–2 and 5–6, *ATT - Low* and *ATT - High* are the average treatment effects on the treated 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 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.

	Split by Media		Split by Glassdoor		Split by CriticismExposure	
	(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

SUPPRESSING THIRD-PARTY INFORMATION AND FIRMS' COST OF EQUITY: EVIDENCE FROM ANTI-SLAPP LAWS

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, 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 and two of its executives. 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 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 Corp. 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 firm 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 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 is the kind of independently generated information this paper studies.

Appendix OB. Crash Risk Measures

In line with prior research on crash risk (Chen, Hong, and Stein, 2001; Jin and Myers, 2006; Hutton, Marcus, and Tehranian, 2009; Kim, Li, and Zhang, 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 natural logarithm of 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 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 :

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

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 two-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 reflects market and industry 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 $W_{j,w}$.

OB.1 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 $W_{j,w}$:

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

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. (OB-2) 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.

OB.2 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 log ratio of downside volatility to upside volatility:

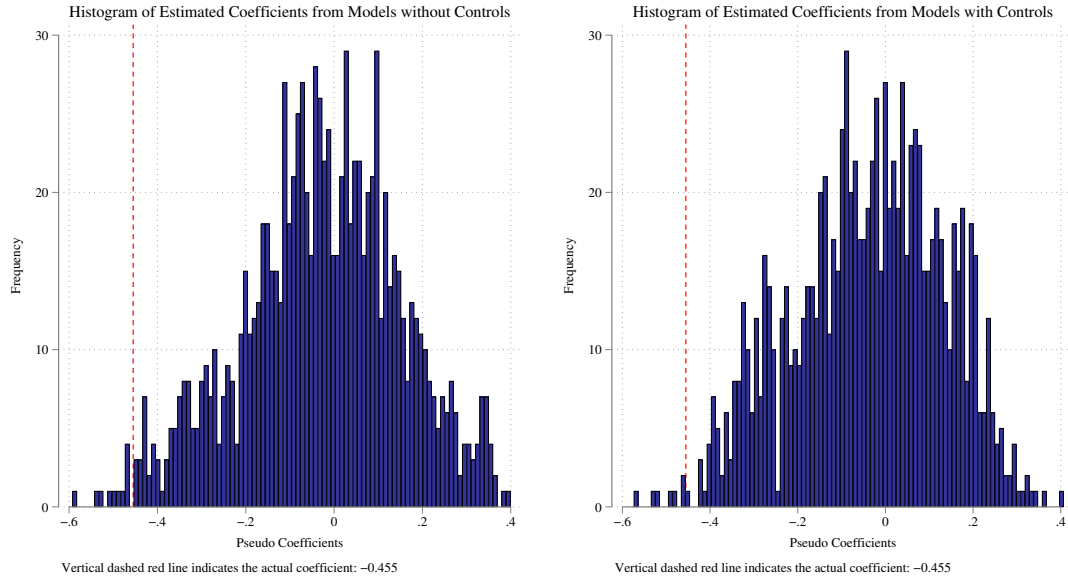
$$DUVOL_{j,t} = \ln \left(\frac{\left((n_{u,j,t} - 1) \sum_{w=1}^{n_{u,j,t}} W_{j,w}^2 \right)^{0.5}}{\left((n_{d,j,t} - 1) \sum_{w=1}^{n_{d,j,t}} W_{j,w}^2 \right)^{0.5}} \right), \quad (\text{OB-3})$$

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. As with $NCSKEW_{j,t}$, a higher value of $DUVOL_{j,t}$ indicates greater stock price crash risk for firm j in year t .

OB.3 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 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.

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 estimated ATT. 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: Weighted Anti-SLAPP Exposure

This table reports OLS regression results over the period 1987–2023 in columns 1 and 2 (2000–2023 in columns 3 and 4) examining the effect of anti-SLAPP legislation on a firm’s implied COE. The dependent variable is a firm’s implied COE defined using the model of Gebhardt et al. (2001) (*GLS*), multiplied by 100 (i.e., $GLS \times 100$). *Anti-SLAPP* is an indicator equal to one if an establishment or an individual is located in a state that has passed an anti-SLAPP law by year t , and zero otherwise. *Anti-SLAPP - DA* is an employment-weighted average of *Anti-SLAPP* across all of the firm’s establishments in a year. Data are from Data Axle. *Anti-SLAPP - Revelio* is an employment-weighted average of *Anti-SLAPP* across all of the firm’s employees in a year. Data are from Revelio Labs. 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 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.

	GLS×100			
	(1)	(2)	(3)	(4)
Anti-SLAPP - DA	-0.411** (-2.16)	-0.380** (-2.11)		
Anti-SLAPP - Revelio			-1.099*** (-2.68)	-1.047*** (-3.06)
Year FE	✓	✓	✓	✓
Firm FE	✓	✓	✓	✓
Controls		✓		✓
N of Obs	47,637	47,637	33,854	33,854

Table OA2: 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* is the average treatment effect on the treated, that is, the average effect of anti-SLAPP legislation on the COE of treated firms. 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 ≥ 50%		InState ≥ 75%		InState ≥ 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 OA3: 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* is the average treatment effect on the treated, that is, the average effect of anti-SLAPP legislation on the COE of treated firms. 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* are the average treatment effects on the treated 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 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* is the average treatment effect on the treated, that is, the average effect of anti-SLAPP legislation on the COE of treated firms. 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 the [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 × Cohort FE			✓	✓
Firm × 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* is the average treatment effect on the treated, that is, the average effect of anti-SLAPP legislation on the COE of treated firms. 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, the 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. $ATT - Q1$ to $ATT - Q4$ are the average treatment effects on the treated, that is, the average effects of anti-SLAPP legislation on the COE of treated firms in each quartile. 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, and the $ATT - Q3$ row in these columns reports the estimate for the combined group. 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.

	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

Table OA7: Anti-SLAPP Legislation, Negative News, and Glassdoor Ratings

This table reports results from the imputation-based DiD method examining the effect of anti-SLAPP legislation on negative news about a firm 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, *NegNews–ESS* and *NegNews–CSS* are the natural logarithm of one plus the number of negative news articles about a firm during the firm’s fiscal year, where negative sentiment is defined based on RavenPack’s Event Sentiment Score (ESS) and Composite Sentiment Score (CSS), respectively. ESS and CSS values below 0 are considered negative, while values of 0 are considered neutral. From RavenPack, we keep articles 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* is the average treatment effect on the treated, that is, the average effect of anti-SLAPP legislation on each outcome for treated firms. 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 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.

	NegNews–ESS		NegNews–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 OA8: 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* is the average treatment effect on the treated, that is, the average effect of anti-SLAPP legislation on the crash risk of treated firms. 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*. The crash-risk measures are defined in Online Appendix [OB](#). 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.

	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

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