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

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Abstract

This study examines how freedom of speech affects firms' cost of equity (COE). Leveraging the adoption of anti-SLAPP (Strategic Lawsuits Against Public Participation) laws—procedures that protect critics from legal intimidation—and an imputation-based difference-in-differences design, we find that these laws lower firms' COE, especially for firms with opaque information environments, weaker governance, or greater public scrutiny. Information asymmetry, firm risk, undervaluation, and stock price crash risk all decline following adoption. Our findings suggest that stronger free speech accelerates the credible dissemination of negative information, thereby facilitating the timely diffusion of bad news, mitigating shocks, and reducing equity mispricing.

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

JEL Classifications: G12, G30, J21, K22

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ABSTRACT

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1 Introduction

Free speech is a cornerstone of democratic societies, yet its implications for financial markets remain largely unexplored. By shaping the ability of employees, journalists, analysts, and other stakeholders to publicly criticize firms, free speech may materially influence firms' information environments and investors' perceptions of risk. Despite its importance, however, free speech has received little attention in the corporate finance and asset pricing literature. This paper asks a simple yet unexplored question: When stronger free speech protections prevent firms from silencing critics, does firms' cost of equity (COE) capital rise or fall?

To answer this question, we leverage the adoption of anti-Strategic Lawsuits Against Public Participation (anti-SLAPP) laws across U.S. states. SLAPPs are lawsuits filed by firms to intimidate individuals or organizations publicly critical of their practices (Canan and Pring, 1988a,b; Pring and Canan, 1996).[1] Anti-SLAPP laws counter such litigation by enabling courts to dismiss cases earlier, enforce stays of discovery, and shift attorneys' fees from defendants to plaintiffs.[2] By limiting firms' ability to deter unfavorable speech through costly litigation, these laws strengthen free speech protections and plausibly alter the information environment surrounding firms.

Nevertheless, the enactment of anti-SLAPP laws is contentious. A growing body of literature shows that these laws deter retaliatory litigation, reducing both the frequency and duration of SLAPPs (Elias, 2015; Ernst, 2015; Bunker and Erickson, 2018; Leader, 2018; Skrzypczak, 2023; Lee, Ng, Yoo, and Zhang, 2026). Advocates argue that these protections foster transparency, protect whistleblowers and journalists, and improve capital market efficiency by increasing the availability of firm-specific information. Consistent with this view,

[1]For example, Sharper Image Corp. sued Consumers Union (the publisher of Consumer Reports) via a SLAPP in 2003 to suppress a negative review claiming its Ionic Breeze air purifier was ineffective, and potentially harmful to consumers. In response, Consumers Union filed a motion to dismiss the case, stating that it "believes the district court will see Sharper Image's lawsuit for what it really is—a meritless case aimed at silencing an honest critic that has held the public's trust for nearly 70 years." For additional SLAPP cases involving U.S. public firms, see Appendix OA in the Online Appendix.

[2]Indeed, Sharper Image's lawsuit against Consumers Union was dismissed in 2004 under California's anti-SLAPP law. The court ruled that Sharper Image failed to prove falsity or invalid testing and awarded Consumers Union $525,000 in legal fees.

workers disclose more unfavorable information in online reviews, media coverage becomes more negative, and journalists devote greater attention to adverse firm events following anti-SLAPP legislation (Chemmanur, Rajaiya, and Sheng, 2020; Böke, Brau, Glaeser, and Jeong, 2025; Chen, Li, and Li, 2025; Jung, Wang, Wei, and Zhang, 2025; Lee et al., 2026).[3] Activist investors similarly emphasize the importance of these protections for a more transparent and efficient marketplace (Poljak, Shapiro, and McCathie, 2018). Conversely, critics warn that anti-SLAPP legislation may facilitate the spread of false or misleading claims, potentially injecting noise into markets and imposing reputational and compliance costs on firms.

Hence, the effect of anti-SLAPP laws and freer speech on firms' COE is theoretically ambiguous. On the one hand, stronger speech protections may reduce uncertainty and improve the information environment by promoting more disclosure, discouraging managerial concealment of bad news, and enhancing investors' ability to differentiate firms with hidden problems from those without, thereby reducing equity risk premia (Diamond and Verrecchia, 1991; Easley and O'Hara, 2004; Lambert, Leuz, and Verrecchia, 2007). On the other hand, an increase in negative, noisy, or misleading information could heighten perceived risks, driving up investors' required returns (Kothari, Shu, and Wysocki, 2009; Dyck, Morse, and Zingales, 2010; Kim and Shi, 2011). Whether greater free speech protections lower or raise firms' COE is therefore ultimately an empirical question.

Since 1992, 40 states and the District of Columbia have enacted anti-SLAPP legislation. While states cite various reasons for passing these laws, such as concerns about free speech, reducing judicial backlogs, and changing public opinion towards such lawsuits, these reasons are largely unrelated to the financing costs of individual firms. To explore potential determinants of anti-SLAPP legislation, we estimate hazard models and find that the timing of the laws is not systematically related to the average COE of firms in the state, state economic conditions, political leanings, demographics, or corporate tax rates. Moreover, across many

[3] We similarly find that after anti-SLAPP legislation, firms experience higher volumes of negative news coverage in RavenPack, receive more negative employee ratings, are less likely to be recommended to other workers, and have lower CEO approval scores on Glassdoor.

observable characteristics, firms in states with these laws are statistically indistinguishable from those in states without them.

Using a difference-in-differences (DiD) framework, we investigate how anti-SLAPP legislation affects a firm's COE. Given recent concerns about potential biases arising from heterogeneous treatment effects in two-way fixed effects (TWFE) staggered DiD settings, we employ an imputation-based approach (e.g., Baker, Larcker, and Wang, 2022; Borusyak, Jaravel, and Spiess, 2024). This method draws on observations from firms in states that either never enacted or have not yet enacted anti-SLAPP legislation to construct counterfactual outcomes for firms in states that adopt such laws. We then derive the average treatment effect on the treated (ATT) by estimating the weighted difference between firms' observed outcomes in states with anti-SLAPP statutes and their imputed counterfactuals, with weights based on treatment status and timing. Our specifications include firm and year fixed effects, and we estimate models with and without a comprehensive set of firm- and state-level controls.

We estimate a firm's *ex ante* COE as the discount rate that equates the present value of its projected cash flows to shareholders with its current stock price. Following the extant literature (e.g., Lee, Ng, and Swaminathan, 2009; Chava and Purnanandam, 2010; Hann, Ogneva, and Ozbas, 2013), we compute a firm's implied COE using its market price, analyst forecasts, and the residual income discounted cash flow model from Gebhardt, Lee, and Swaminathan (2001), as this estimation approach provides a reliable proxy for a firm's expected returns (Pástor, Sinha, and Swaminathan, 2008).

Following the enactment of anti-SLAPP laws, firms experience a statistically significant reduction in their COE. Compared to firms not covered by such legislation, the COE for firms headquartered in states that adopt anti-SLAPP laws declines by 45.5 basis points per annum in specifications with firm and year fixed effects. This effect remains virtually unchanged after controlling for firm- and state-level characteristics. Given a mean COE of 6.6%, the economic effect of the laws on a firm's COE is significant, implying a 6.9% reduction. Moreover, timing tests indicate no discernible differences in COE trends between firms in states that eventually enact anti-SLAPP laws and those that do not in the years leading up to adoption, consistent with satisfying the parallel trends assumption and lending credence to our identification strategy.

Our findings survive a battery of robustness tests. First, the results withstand analyses addressing geographic exposure, state-versus-federal jurisdiction, and forum shopping. Specifically, they hold when (i) restricting the sample to firms with concentrated geographic operations, (ii) excluding consumer-product firms exposed to geographically widespread activism, (iii) removing firms in California and New York—initial adopters of these laws with many public firms and SLAPP cases—and (iv) splitting the sample before and after a 2010 Supreme Court case restricted forum shopping. Second, employing the test proposed by Oster (2019), we find that the relationship between anti-SLAPP legislation and firms' COE is unlikely to be driven by unobserved omitted variables.

Third, we find no effect of anti-SLAPP laws on firms' COE in a placebo simulation that randomly assigns anti-SLAPP adoption dates to states. Fourth, the results are not reliant on a particular implied COE measure: our conclusions hold when applying alternative valuation-based models (Claus and Thomas, 2001; Easton, 2004; Ohlson and Juettner-Nauroth, 2005), or the average of all the models. We also find similar results when replacing analyst earnings forecasts with those generated from cross-sectional earnings prediction models (Li and Mohanram, 2014). Fifth, the findings are consistent across alternative DiD estimators that address potential biases from treatment-effect heterogeneity. Sixth, our results also remain after including industry-year or incorporation state-year fixed effects, which control for time-varying industry or incorporation state heterogeneity. Seventh, re-estimating the models using inverse probability weighting based on the likelihood that a firm is headquartered in a state that adopts an anti-SLAPP law further supports the findings, as this approach mitigates effects of observable differences between treated and untreated firms. Finally, the decline in firms' COE is more pronounced in states with stronger anti-SLAPP laws, as defined by procedural rules and the scope of speech covered. Overall, we find strong evidence that equity financing costs decline when freedom of speech is more strongly protected.

We next examine potential mechanisms through which anti-SLAPP laws could lower firms' COE, focusing on how stronger speech protections might reduce uncertainty by improving the information environment and helping investors assess risk. First, we test whether anti-SLAPP laws lower equity financing costs differently across firms, depending on their *ex ante* information environments, corporate governance, and public scrutiny. Anti-SLAPP laws increase public oversight, which should have a greater impact on firms with opaque information environments and low disclosure quality. Relatedly, the COE should drop more for firms with weak governance because public monitoring can substitute for poor internal controls. Additionally, when media coverage is high, anti-SLAPP laws should further increase scrutiny and the dissemination of information, leading to larger reductions in COE. Our results show that firms with less transparent information environments—such as those lacking management earnings forecasts, exhibiting larger analyst forecast errors, lower accrual quality, or higher R&D intensity—experience greater COE reductions after anti-SLAPP laws are adopted. Similarly, firms with weaker governance or greater public scrutiny, measured by board independence, board busyness, CEO pay-performance sensitivity, and media coverage, also experience larger COE declines after these laws are enacted.

Second, by explicitly protecting public discourse and encouraging open commentary, anti-SLAPP laws can reduce information asymmetries between firms and investors. We find support for this effect via declines in proxies for information asymmetry (e.g., bid-ask spreads, illiquidity, and the probability of informed trading) following the adoption of anti-SLAPP laws. Third, we study whether this legislation affects direct measures of firm risk. These speech protections facilitate an enhanced information environment, which improves investors' ability to assess a firm's activities, and therefore should reduce perceived risk. Consistent with this, we observe decreases in total stock return volatility, implied return volatility, idiosyncratic return volatility, and systematic risk after adoption.

Fourth, we examine whether anti-SLAPP laws mitigate undervaluation arising from investor uncertainty regarding hidden firm risks. Under asymmetric information, investors discount all firms as if they face undisclosed risks, depressing valuations below fundamentals

(Diamond and Verrecchia, 1991). Our results show that, after adoption, the probability that a firm is undervalued relative to fundamentals declines, suggesting that stronger speech protections enable investors to better separate firms with genuine risks and improves valuation accuracy. Fifth, by lowering the risk of legal retaliation for public criticism, free speech protections could reduce uncertainty and allow negative information to reach the market more quickly. After anti-SLAPP legislation, investors could gain more timely access to adverse news that firms may have previously suppressed. To test whether these laws lead to more timely dissemination of information, we examine their effect on several measures of stock price crash risk. Our results reveal significant declines in crash risk after the laws' adoption, suggesting that stronger speech protections facilitate the faster release of negative information. This finding aligns with results in Chen, Li, and Li (2025) and Lee et al. (2026).

Our paper makes three primary contributions. First, we identify free speech protections, or legal measures designed to protect critics from retaliation for speaking out, as a novel legal constraint on information suppression and a determinant of firms' COE. Prior work shows that more informative disclosure environments are associated with lower financing costs (e.g., Diamond and Verrecchia, 1991; Easley and O'Hara, 2004; Lambert, Leuz, and Verrecchia, 2007), but this literature largely focuses on the supply of information, such as voluntary disclosure or mandated reporting. We instead study constraints on the ability of firms to suppress unfavorable information. Specifically, anti-SLAPP laws limit firms' ability to deter public criticism through retaliatory lawsuits, thereby altering the timing, credibility, and completeness of negative information entering capital markets. By identifying free speech protections as a determinant of financing costs, we introduce a new institutional channel linking legal protections to equity risk premia (e.g., La Porta, Lopez-de Silanes, and Shleifer, 2006; Hail and Leuz, 2009; Hope, Hu, and Lu, 2016).

Second, we contribute to the literature on information and asset pricing by showing that greater flows of negative information can reduce, rather than increase, equity investors' required returns. Prior research suggests that anti-SLAPP laws affect the production and dissemination of information, shaping employee reviews (Chemmanur, Rajaiya, and Sheng, 2020; Jung et al., 2025), ESG disclosure and CSR activities (Griffin, Hong, Jung, and Moon, 2024a; Wu, Bruton, and Krause, 2026), whistleblowing (Böke et al., 2025), and the release of negative information (Chen, Li, and Li, 2025; Lee et al., 2026). However, the implications for asset prices are theoretically ambiguous. Greater disclosure, especially of adverse or noisy information, could increase perceived risk and required returns (e.g., Hutton, Marcus, and Tehranian, 2009; Kothari, Shu, and Wysocki, 2009). Thus, a decline in firms' COE is not implied by previous findings. Our evidence that COE falls after anti-SLAPP adoption suggests that stronger free speech protections accelerate the credible and timely dissemination of bad news, reduce firm risk and uncertainty, and improve pricing efficiency.

Finally, we provide evidence that decentralized monitoring affects financing costs. Prior studies show that external actors—such as the media, social movements, and crowdsourced platforms—can discipline firms and influence corporate behavior (e.g., Dyck, Volchkova, and Zingales, 2008; Gurun, Stoffman, and Yonker, 2018; Flammer, Toffel, and Viswanathan, 2021; Green, Huang, Wen, and Zhou, 2019). We show that anti-SLAPP laws and stronger speech protections enhance these decentralized monitoring channels and affect not only firm behavior but also how investors price risk. By limiting firms' ability to delay or suppress adverse information, anti-SLAPP laws reduce information asymmetry and resolve investor uncertainty, ultimately decreasing required returns.

2 Institutional Background and Hypotheses

2.1 Institutional background

SLAPPs are legal actions intended to censor and intimidate critics by imposing burdensome defense costs (Canan and Pring, 1988a,b).[4] Plaintiffs use these lawsuits not to win but to suppress criticism, targeting individuals or organizations—including consumer advocates, journalists, bloggers, and employees—who voice concerns on public issues or corporate prac-

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

tices. By threatening high legal costs, SLAPPs undermine democratic participation and First Amendment rights. Justice Nicholas Colabella of the New York Supreme Court described a SLAPP as just "short of a gun to the head" in its ability to stifle free expression.

Anti-SLAPP legislation protects defendants by giving courts procedural tools—such as expedited hearings, stays of discovery, and recovery of attorneys' fees—to dismiss SLAPPs early. California enacted the first anti-SLAPP law in 1992, becoming a model for later state laws. As of 2026, 40 states and the District of Columbia had enacted similar legislation, though the scope and strength of the laws vary (Greenberg, Keating, and Knowles-Gardner, 2023). For instance, some statutes broadly protect speech on matters of public concern (e.g., California), while others confine coverage to narrower contexts (e.g., Delaware or Nebraska).

States claim to adopt anti-SLAPP laws for three main reasons. First, these laws address free speech concerns, as lawmakers recognize that the threat of litigation deters whistleblowers, journalists, and citizens from reporting on government or corporate actions (Pring and Canan, 1996; Dyck, Morse, and Zingales, 2010; Heese and Pérez-Cavazos, 2021). Second, proponents argue that these statutes reduce judicial backlogs caused by SLAPP cases, ultimately saving time and taxpayer money (Böke et al., 2025).[5] Clear rules also help firms by reducing litigation uncertainty and enabling faster resolution when employees or media speak out. Third, some states cite constituent opinion, especially in high-profile cases, as a driving force. Media coverage of environmental disputes, consumer complaints, and local activism creates political pressure, prompting legislators to pass anti-SLAPP laws to counter intimidation by powerful interests (e.g., Elias, 2015; Schaufele, 2024; Chen, Ke, and Kuang, 2026; Wu, Bruton, and Krause, 2026).

[5] Pring and Canan (1996) find "thousands of SLAPPs have been filed in the last two decades, tens of thousands of Americans have been SLAPPed, and still more have been muted or silenced by the threat." Focusing only on the libel cases, Lee et al. (2026) find an average of approximately 5.93 SLAPP cases per state-year, implying over 300 annually. The "SLAPP Back" database by NYU (https://slappback.org/) has identified 16,094 citations to anti-SLAPP statutes across 10,257 individual cases since 1994, including 500 cases found in 2024 alone. Moreover, these figures likely understate the broader incidence of retaliatory litigation risk for critics of firms, as courts do not uniformly classify cases as SLAPPs, and many disputes never generate a decision citing an anti-SLAPP law. The threat of suit alone may also deter those from speaking out. Thus, the observed case counts and probable undercounting of both filed and deterred claims suggest that the silencing effect of SLAPP-related litigation may be larger than the raw filing numbers imply.

Importantly, prior research confirms that anti-SLAPP laws expand flows of adverse information. For example, Chemmanur, Rajaiya, and Sheng (2020) find that Glassdoor review counts increase and average ratings decrease after adoption. Jung et al. (2025) document more negative employee ratings on Glassdoor, indicating a higher willingness to disclose unfavorable views. Lee et al. (2026) report fewer SLAPP cases and shorter durations, coinciding with broader media coverage of firms facing negative events. Chen, Li, and Li (2025) find a more negative tone in newspapers, Glassdoor reviews, and Seeking Alpha content after enactment. Böke et al. (2025) observe more mentions of discrimination, ethics, fraud, safety, and management, as well as more negative press releases by firms, indicating reduced concealment of bad news. Consistent with these studies, our Table OA1 shows that, following anti-SLAPP legislation, firms experience more negative press releases in RavenPack and employee sentiment on Glassdoor deteriorates (e.g., more pessimistic company outlooks, lower willingness to recommend the firm, and reduced CEO approval). Together, these findings suggest that SLAPP-related litigation risk suppresses disclosure of adverse information, and anti-SLAPP laws counter this effect. Thus, studying firms' COE is a natural setting for exploring how freer speech and less information suppression affects capital markets.

One empirical concern is that a state's anti-SLAPP statute may not perfectly align with the firm's headquarters state due to differences in jurisdiction, venue, and choice-of-law rules. Despite this, using the headquarters state remains the standard for measuring firms' anti-SLAPP exposure (e.g., Chemmanur, Rajaiya, and Sheng, 2020; Dyer, 2021; Griffin et al., 2024a; Böke et al., 2025; Jung et al., 2025). Both economic and institutional reasoning support this approach: economically, the headquarters is the center of managerial decision-making and firm identity, while legally, it provides an important nexus for operations and communications. While this assignment does not capture all jurisdictional nuances for firms operating in multiple states, it offers a consistent and theoretically grounded measure of anti-SLAPP law exposure.

Another concern relates to venue or forum shopping; however, this also does not imply a systematic bias. In many cases, disputes are tied to the firm's headquarters through its managerial and reputational center, and where stakeholders with firm-specific information are located or connected to core operations. In addition, legal precedent (e.g., *Calder* v. *Jones*, 1984) reinforces that jurisdiction can be based on where harm is felt, often pointing to the headquarters state. Moreover, courts and legislatures have historically discouraged forum shopping, limiting the scope for strategic venue selection. To the extent that some firms pursue SLAPPs outside of headquarters states, this would introduce classical measurement error to the analysis, biasing estimates toward zero rather than generating spurious findings.

Some disputes are also handled by federal, and not only state, courts. In federal courts, anti-SLAPP laws can be applied differently across circuits. While this adds institutional complexity, it does not undermine our research design. Indeed, in most cases, jurisdiction and choice of law usually depend on the firm's principal place of business or corporate citizenship. The Supreme Court's decision in *Hertz Corp.* v. *Friend* (2010) reinforces this by adopting a headquarters-based "nerve center" standard (Colonnello and Herpfer, 2021). Further, if differences across federal courts weaken firms' anti-SLAPP exposure, our estimates would be biased toward zero instead of producing spurious effects. As a result, noise from overlapping federal and state legal systems should make it harder—not easier—to link anti-SLAPP laws and firms' COE. Nevertheless, we address each of these three concerns in Section 4.2.

2.2 Hypothesis development

The impact of anti-SLAPP laws on a firm's COE depends on how these statutes affect the information environment and how investors perceive risk. Traditional legal frameworks often discourage employees, journalists, analysts, and community members from making negative comments out of fear of retaliatory lawsuits. In contrast, anti-SLAPP statutes provide procedural safeguards that enable the quick dismissal of meritless lawsuits aimed at silencing critics (Canan and Pring, 1988a,b; Pring and Canan, 1996). This can encourage a freer and more extensive flow of information, potentially altering the volume and quality of publicly available data. Dyck, Morse, and Zingales (2010) and Raleigh (2024) note that stronger legal protections for whistleblowers and commentators increase the likelihood that concealed negative information will be revealed.[6] Meanwhile, a more transparent information environment may reduce equity risk premia as investors gain confidence in the completeness of disclosures (Leuz and Verrecchia, 2000; Easley and O'Hara, 2004).

Information asymmetry is a central determinant of firms' COE (e.g., Diamond and Verrecchia, 1991; Francis, LaFond, Olsson, and Schipper, 2005; Beyer, Cohen, Lys, and Walther, 2010). If investors believe that managers or insiders are concealing material information, they demand higher expected returns as compensation. Legal rules that constrain such concealment can alter equity risk premia. Stronger speech protections may lower the COE by improving information completeness and reducing uncertainty (e.g., Verrecchia, 2001; Lambert, Leuz, and Verrecchia, 2007)—this operates through an "uncertainty resolution" mechanism. Alternatively, stronger speech protections may raise the COE by increasing the flow and salience of negative information, elevating perceived firm risk (e.g., Kothari, Shu, and Wysocki, 2009)—this is the "risk revelation" channel. These two mechanisms imply alternative predictions for the effect of anti-SLAPP laws on firms' COE. We formalize these channels in a simple theoretical model in Appendix OB of the Online Appendix.

Uncertainty Resolution Hypothesis. This perspective holds that anti-SLAPP laws reduce firms' COE by enhancing market transparency and decreasing informational uncertainty. While adverse external scrutiny may increase, the overall effect is a more comprehensive and credible flow of information (e.g., Easley and O'Hara, 2004; Lambert, Leuz, and Verrecchia, 2007). Investors generally penalize opacity by adding a risk premium to expected returns (Healy and Palepu, 2001). Robust speech protections can reduce this premium by allowing a wider range of market participants (e.g., employees, analysts, and journalists) to share insights without fear of retaliation, reducing the uncertainty that drives up the COE.

This increased transparency boosts investor confidence that all news is efficiently reflected in stock prices (e.g., Verrecchia, 2001). It may also deter managers from hoarding bad news, as concealment becomes harder when whistleblowers are legally protected (Kothari, Shu, and

[6] Underscoring the high personal cost of speaking out, Dyck, Morse, and Zingales (2010) report that 82% of whistleblowers are fired, pressured to quit, or reassigned, concluding, "the surprising part is not that most employees do not talk, but that some talk at all."

Wysocki, 2009). The market may thus view such legal safeguards to free speech as making prices more informative and lowering the risk of unexpected shocks (Botosan, Plumlee, and Xie, 2004). As the perceived variance of unreported events shrinks, so does the risk premium.

Additionally, better information enables more accurate assessments of firm fundamentals and reduces pricing errors from asymmetric or incomplete disclosures (e.g., Lambert, Leuz, and Verrecchia, 2007). Thus, anti-SLAPP laws could lower a firm's COE not only by enhancing transparency about potential risks but also by exposing information that might have remained hidden. The net effect is reduced investor uncertainty and, consequently, a lower COE (e.g., Armstrong, Core, Taylor, and Verrecchia, 2011).

Anti-SLAPP laws can also correct mispricing that arises under asymmetric information when investors cannot distinguish between firms with latent negative information and those without (Diamond and Verrecchia, 1991). In the pre-law regime, all firms may be pooled together and priced as if they were concealing bad news. This leads investors to impose unnecessarily high financing costs on "good" firms. A more complete and credible flow of information under anti-SLAPP protections might enable market participants to separate firms that genuinely face risks from those that appear risky only due to opacity. This reduces the likelihood of undervaluation and lowers firms' COE.

Risk Revelation Hypothesis. An alternative view is that anti-SLAPP laws increase the availability and visibility of negative corporate information by shielding critics from legal retaliation. Without such protections, employees, journalists, analysts, activists, and other stakeholders may be deterred from disclosing adverse information due to potential costs and risks of litigation (Dyck, Morse, and Zingales, 2010; Kim and Shi, 2011). Anti-SLAPP laws lower these barriers. This empowers a broader range of actors to share concerns about governance, ethics, environmental practices, and regulatory compliance. As a result, there may be an influx of previously suppressed "bad news," which could change market perceptions of firm risk (Kothari, Shu, and Wysocki, 2009).

According to asset-pricing theory, an increase in perceived risk should yield higher required returns. Information that contradicts management's narrative or reveals hidden vulnerabilities can lead investors to reassess the firm's risk profile. Moreover, not all information revealed under stronger speech protections may be accurate or well-founded. Low-quality, misguided, or deliberately misleading information (e.g., exaggerated online reviews, opportunistic activist claims, or sensationalized media reports) can inject noise into the information environment. This noise increases uncertainty about the firm's fundamentals and complicates investor assessments. Risk premia may rise as markets struggle to distinguish credible signals from misinformation. While anti-SLAPP laws can improve social welfare by encouraging free speech, they may also expose firms to greater scrutiny, reputational volatility, and noisy information flows. Hence, firms subject to more frequent, less filtered criticism could experience increases in COE as investors incorporate a wider range of downside risks and uncertainty (Hutton, Marcus, and Tehranian, 2009; Dyck, Morse, and Zingales, 2010; Kim and Shi, 2011; Green et al., 2019; Chemmanur, Rajaiya, and Sheng, 2020).

Overall, anti-SLAPP laws create an environment for more transparent and robust discussions of corporate and social issues. By reducing the silencing effect of retaliatory lawsuits, these statutes allow stakeholders to share critical information more freely. Whether reduced investor uncertainty or higher perceived firm risk has a stronger effect on a firm's COE is ultimately an empirical question.[7]

3 Data and Empirical Methodology

This section introduces our empirical measure of a firm's implied COE and describes the main sample and identification strategy.

[7]Two alternative channels warrant consideration. First, anti-SLAPP laws could lower firms' COE by reducing expected legal costs and legal uncertainty, independent of information flows. However, these laws deter retaliatory, speech-suppressing lawsuits by firms while preserving legitimate claims against them. By limiting firms' ability to suppress unfavorable information, anti-SLAPP laws could instead increase firms' legal exposure and COE by raising regulatory and enforcement risk. Second, anti-SLAPP laws could affect COE through operating decisions if greater public scrutiny induces safer investment policies. We find no support for this mechanism. In untabulated tests, firms increase the share of R&D relative to capital expenditures following anti-SLAPP adoption, consistent with more risk-intensive investment and with evidence that anti-SLAPP laws increase innovation (Griffin, Hong, Jung, and Ryou, 2024b), not risk avoidance.

3.1 Measuring the implied COE

We estimate a firm's implied COE by identifying 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 the effects of market sentiment (e.g., Stulz, 1999; Hail and Leuz, 2006, 2009). Further, Fama and French (1997) argue that 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. Conversely, approaches to measure the implied COE 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:

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

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 from the Institutional Brokers' Estimate System (IBES).[8]

We construct our primary measure of a firm's equity financing cost using the *ex ante* approach developed by Gebhardt, Lee, and Swaminathan (2001) (GLS), which estimates the implied COE based on the residual income valuation model. Details are in Appendix A. This method relies on a firm's market price, earnings forecasts, and an assumption that its return on equity converges to the industry median over time. Prior research finds that this measure is a reliable proxy for expected returns (Pástor, Sinha, and Swaminathan, 2008), and it has been widely employed across asset pricing and corporate finance contexts (e.g.,

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

Lee, Ng, and Swaminathan, 2009; Chava and Purnanandam, 2010; Chen, Kacperczyk, and Ortiz-Molina, 2011; Chen, Huang, and Wei, 2013; Hann, Ogneva, and Ozbas, 2013; Ortiz-Molina and Phillips, 2014). In Section 4.2, we show that our results are robust to the use of several alternative discount models, including those proposed by Ohlson and Juettner-Nauroth (2005), Claus and Thomas (2001), and Easton (2004), as well as to the use of earnings forecasts derived from statistical models instead of analyst forecasts.

3.2 Sample selection

We use the CRSP/Compustat merged database from 1987 to 2023. It begins in 1987, allowing a pre-treatment window of five years before California enacted the first state-level anti-SLAPP law in 1992, and ends in 2023 due to data availability. We exclude firms in the utility (SIC codes 4900–4999), financial (6000–6999), and quasi-public (above 9900) sectors. Analyst forecast data are from IBES, financial statement data from Compustat, and stock return and price information from CRSP. To determine firms' historical headquarters locations, we use the COMPHIST and CST_HIST files from the CRSP/Compustat Merged database, which offer coverage back to 1994. We backfill this information to cover earlier years as needed. State-level control variables come from various sources: corporate tax rates from the Book of the States, GDP from the U.S. Bureau of Economic Analysis, unemployment rates from the Bureau of Labor Statistics, unemployment benefits from the Department of Labor, and a governor's political affiliation from publicly available online records. Dates of medical marijuana legalization are from Guernsey, Serfling, and Yan (2025).

The final sample includes 51,455 firm-year observations; however, this number varies in the analyses due to the use of the DiD imputation method and model specification. Appendix B provides variable definitions. We winsorize all continuous variables at their 1st and 99th percentiles and express dollar values in 2020 dollars. Table 1 summarizes when and which states have enacted anti-SLAPP statutes.[9] Table 2 reports summary statistics.

[9]Following the approach most common in the literature (e.g., Chemmanur, Rajaiya, and Sheng, 2020; Griffin et al., 2024a; Jung et al., 2025), we focus on the enactment dates of each anti-SLAPP law. This differs slightly from Böke et al. (2025), which focuses only on "strong" anti-SLAPP laws. The discrepancy primarily

3.3 Identification strategy

We leverage the implementation of anti-SLAPP laws across U.S. states, employing a staggered DiD framework. The conventional TWFE regression exploiting staggered legislation in a DiD setting is expressed as:

$$y_{it} = \alpha_i + \beta_t + \tau D_{it} + \gamma X_{it-1} + \varepsilon_{it}. \tag{2}$$

In this specification, y_{it} denotes the implied COE or other outcomes of interest for firm i in year t. The term α_i captures firm fixed effects, and β_t accounts for year fixed effects. The treatment indicator D_{it} equals one if the firm is headquartered in a state that enacts an anti-SLAPP law by year t, and zero otherwise. X_{it-1} includes lagged firm- and state-level control variables. Standard errors are clustered at the state level to address serial correlation over time and cross-sectional dependence among firms within the same state.

Recent methodological developments highlight limitations of the standard TWFE approach when treatment is implemented at different times across units (e.g., Baker, Larcker, and Wang, 2022; Borusyak, Jaravel, and Spiess, 2024). For instance, Goodman-Bacon (2021) demonstrates that the ATT estimated in TWFE models is a weighted average of multiple two-group, two-period DiD comparisons. In staggered settings, this raises concerns because some comparisons involve units already exposed to treatment, which can bias estimates when treatment effects are dynamic. These so-called "forbidden comparisons" may generate misleading inferences, including estimates with the wrong sign. In our data, only 15% of the firm-year observations come from states that never implement anti-SLAPP laws, and, following the decomposition from Goodman-Bacon (2021), roughly 41.5% of the treatment effect estimates are based on such problematic comparisons. This necessitates an alternative estimation strategy to mitigate potential biases arising from the conventional approach.

To address these concerns and obtain valid estimates of the effect of anti-SLAPP laws on firms' COE, we implement the imputation-based estimator introduced by Borusyak, Jaravel,

involves two states (Tennessee and Washington), which account for less than 4% of our sample observations. Nevertheless, our results are robust to following Böke et al. (2025).

and Spiess (2024). Although other approaches have been proposed (e.g., De Chaisemartin and d'Haultfoeuille, 2020; Callaway and Sant'Anna, 2021; Sun and Abraham, 2021; Wooldridge, 2025), we choose the imputation-based method due to its parsimony, transparency, and favorable efficiency properties, including asymptotically conservative standard error estimation.

This method proceeds in three steps. First, we estimate Eq. (2) using observations from untreated states (i.e., where $D_{it} = 0$), yielding estimates of $\hat{\alpha}_i$, $\hat{\beta}_t$, and $\hat{\gamma}$. Second, for treated units, we estimate counterfactual outcomes using $\hat{y}_{it} = \hat{\alpha}_i + \hat{\beta}_t + \hat{\gamma} X_{it-1}$, and compute treatment effects as $\hat{\tau}_{it} = y_{it} - \hat{y}_{it}$. Finally, we aggregate these estimates into the ATT, applying weights that account for variations in treatment timing and exposure.

3.4 Threats to identification

As with any DiD framework, our identification strategy relies on the assumption that firms in states without anti-SLAPP laws serve as appropriate counterfactuals for those in states that enact them. By specifying firm fixed effects in the imputation-based DiD estimator, we account for time-invariant differences between firms located in adopting and non-adopting states. Moreover, the inclusion of year fixed effects further controls for nationwide trends in both anti-SLAPP legislation and changes in firms' COE.

To assess the comparability of firms in states with and without anti-SLAPP legislation, Table 3 presents a covariate balance analysis. Ideally, the enactment of anti-SLAPP laws would be approximately random, implying no systematic differences in firm or state characteristics between the two groups. We evaluate this by testing whether firm- and state-level characteristics differ in the year before an adoption event (t-1). Firm-level variables include market capitalization, book-to-market ratios, leverage, annual stock returns, cash flow, analyst coverage, forecast dispersion, and projected long-term growth rates.[10] At the state level, we consider variables reflecting economic conditions, political affiliation, regulatory environment, and demographics—such as per capita GDP, GDP growth, corporate tax and

[10] Using a firm's implied COE allows us to disentangle discount rate effects from valuation effects by explicitly controlling for factors such as the firm's cash flows, recent stock performance, and growth expectations.

unemployment rates, party affiliation of the state governor, and maximum unemployment benefits. We also include indicators for the presence of medical marijuana laws, the average age of individuals, and the share of the population with a college degree in the state.

Table 3 shows that, in states with and without anti-SLAPP laws, firms are broadly similar across the 17 examined characteristics. The only statistically significant differences are that states that pass anti-SLAPP laws in the following year tend to have higher GDP and a more educated population. To address potential confounding effects, we include these 17 controls in all of our analyses. However, as we will show, our estimates are similar whether or not we include these control variables, indicating that possible differences in these characteristics do not materially affect our findings.[11]

Another key assumption necessary for valid DiD estimation is that the timing of anti-SLAPP law enactment is not systematically related to firm financing costs or to underlying state-level economic or political factors that might independently influence firms' COE. To evaluate this, we estimate Weibull hazard models where the "failure" event corresponds to a state adopting an anti-SLAPP law. The analysis covers the 1987–2023 period, excluding treatment states after their adoption year, and independent variables are lagged by one year. The main explanatory variable is *State GLS*, defined as the average COE across all firms headquartered in the state during a given year. We also include the same state-level controls from the covariate balance analysis.

Table 4 presents the results. The coefficient on *State GLS* is statistically insignificant (t-statistic = -0.64), suggesting that the adoption of anti-SLAPP laws is not systematically associated with firms' implied COE, thereby mitigating concerns about reverse causality. Further, none of the state-level predictors are statistically or economically significant. When we re-estimate the model using the first differences in these state characteristics (column 5), the results remain qualitatively unchanged. Overall, these findings provide support for our identification strategy, showing that anti-SLAPP laws are plausibly exogenous to local firm financing costs and to the adopting state's political and economic conditions. Moreover,

[11] In untabulated results, we also find qualitatively similar results when we further control for incorporation state-level universal demand, corporate opportunity waiver, and anti-takeover laws.

we control for these state-level factors in our fully specified regressions to further safeguard against potential bias from omitted variables.

4 Results

We begin with baseline analyses of the relationship between anti-SLAPP laws and firms' COE, followed by robustness tests. We then examine potential channels for our main finding by exploring heterogeneity in the effects of these laws on firms' COE and analyzing their impact on information asymmetry, firm risk, undervaluation, and stock price crash risk.

4.1 Anti-SLAPP legislation and firms' COE

Table 5 presents how anti-SLAPP laws affect a firm's COE. The results show a negative ATT, suggesting that firms headquartered in states with these laws experience a lower COE than those in states without such protections. In column 1, which includes only firm and year fixed effects, the estimated COE reduction after the enactment of anti-SLAPP legislation is 45.5 basis points (t-statistic = -3.97). Including firm-level controls in column 2 marginally increases the estimated effect to 47.0 basis points (t-statistic = -3.97). When both firm- and state-level covariates are added in column 3, the estimated effect slightly moderates to 45.5 basis points (t-statistic = -5.46). Given a sample mean COE of 6.6%, these results translate into a relative decline of 6.9% to 7.1% in firms' COE, an effect that is both statistically and economically significant. The magnitude of this cost reduction is similar to that recorded in previous studies examining the impact of factors such as cross-listing, unionization, corporate governance, gig economy exposure, regulatory transparency, and medical marijuana legalization (Hail and Leuz, 2009; Chen, Kacperczyk, and Ortiz-Molina, 2011; Chen, Chen, and Wei, 2011; Chino, 2021; Lai, Lin, and Ma, 2024; Guernsey, Serfling, and Yan, 2025).

Figure 1 examines how firms' COE changes around the time states enact anti-SLAPP laws. To support a causal interpretation with the DiD imputation framework, firms in both treated and untreated states should exhibit similar COE trends before the passage of the laws (i.e., the parallel trends assumption holds). We test this by plotting dynamic DiD coefficients for ten years before and after adoption. The estimates are derived from models that do or do not include firm- and state-level covariates. In both cases, there are no significant COE trend differences between the treatment and control groups before the laws pass. After enactment, firms in adopting states show a gradual, statistically significant decline in COE starting in the first year after adoption, with effects persisting for at least ten years. These patterns suggest that equity investors did not anticipate the passage of anti-SLAPP laws and that the free speech protections they provide have a lasting impact. Overall, the results in Figure 1 support our identification strategy and show that anti-SLAPP laws decrease firms' COE.

4.2 Robustness tests

We examine the robustness of our finding that anti-SLAPP legislation reduces firms' COE along several dimensions, with results reported in the Online Appendix.

Dispersed operations. A potential concern is that assigning anti-SLAPP exposure by headquarters state may introduce measurement error because many public firms operate across multiple states. However, as discussed earlier, anti-SLAPP protections depend on where the speech occurs and where the case is filed, often correlating with the headquarters state. Thus, the headquarters state serves as a reasonable, though imperfect, proxy for exposure. To further address this concern, columns 1–6 of Panel A in Table OA2 exclude firms with likely dispersed operations. These are firms in retail, wholesale, and transportation industries; firms with above-median PP&E; and those disclosing operations in more than the median number of states in their 10-K filings, following Garcia and Norli (2012). Panel B uses establishment-level data from Data Axle (formerly Infogroup) and links it to parent firms in our sample. This panel restricts the sample to firms with at least 50%, 75%, 90%, or 100% of their workforce in the headquarters state. While these restrictions reduce sample size, the results remain robust. In fact, the estimated effect appears to increase as more operations concentrate within the headquarters state and measurement improves.

Venue shopping and jurisdiction. We address concerns about venue-shopping and jurisdiction in three ways. First, we re-estimate our models after excluding consumer-product firms to focus on industrial-product firms. These firms usually face less geographically diffuse activism and have fewer incentives to engage in venue shopping (Wu, Bruton, and Krause, 2026). If venue shopping were driving our results, we would expect the effects to weaken in this sample. Instead, columns 1 and 2 of Table OA3 show similar or stronger effects. Second, because California and New York host many firms, SLAPP cases, and were initial adopters of anti-SLAPP laws, columns 3 and 4 exclude these states and report unchanged results. Untabulated tests indicate that dropping any single state does not alter our conclusions. Third, the 2010 Supreme Court decision in *Hertz Corp.* v. *Friend* clarifies that corporate citizenship is determined by the "nerve center," typically the headquarters, which limits forum shopping (Colonnello and Herpfer, 2021). Splitting the sample into pre- and post-2010 periods, columns 5 and 6 show similar effects in both periods. Together, these tests indicate that forum-shopping concerns do not drive our findings and support the headquarters state as a meaningful proxy for exposure to anti-SLAPP protections.

Omitted variable bias. To evaluate sensitivity to omitted variables, we apply the method of Oster (2019). The "delta" statistic shows how strong unobserved factors must be, relative to observed factors, to fully explain the treatment effect. A delta value greater than one is generally considered evidence of robustness. In our analysis, the delta exceeds 1,000, suggesting that unobserved confounders are unlikely to explain our results. Although this value is large, it is plausible. Adding a comprehensive set of firm- and state-level controls increases the imputed R^2 from 16% to 23%, while the coefficient stays almost unchanged at -0.455.

Placebo test. We conduct a placebo randomization test to assess whether our estimates are attributable to chance or to mechanical aspects of staggered adoption. We randomly reassign anti-SLAPP adoption across states, preserving the overall policy structure while breaking the true link between adoption and firms' headquarters states. For each of 1,000 placebo draws, we re-estimate our baseline specification with and without controls, recording the coefficient of the anti-SLAPP indicator. Figure OA1 presents the placebo estimates, centered near zero, with an average COE decline of only 1.2 basis points. In contrast, the actual estimate is a 45.5 basis point decline. Detaching the reform from its true geographic incidence makes the effect disappear. This reinforces our identification strategy and shows that anti-SLAPP laws reduce firms' COE, rather than reflecting spurious correlations related to policy timing.

Alternative COE models. To further test the robustness of our findings, we employ alternative models to estimate firms' implied COE. Panel A of Table OA4 shows that the results hold when using the valuation models of Ohlson and Juettner-Nauroth (2005), Claus and Thomas (2001), and Easton (2004), as well as the average COE derived from all four models.

Alternative EPS forecasts. We also construct alternative COE measures following the approach of Gebhardt, Lee, and Swaminathan (2001), using EPS forecasts generated by statistical models rather than analyst forecasts. Relying solely on analyst forecasts presents two challenges: it limits the analysis to firms with sufficient analyst coverage across multiple time horizons and may expose our estimates to systematic biases within analyst forecasts (e.g., Lim, 2001; Hong and Kubik, 2003). To address these issues, we create EPS forecasts using an earnings-persistence model and a residual-income model, as per Li and Mohanram (2014). For each method, we predict a firm's EPS for the years $t+1$ through $t+5$ using data from the prior ten years, ensuring that all forecasts are out-of-sample. Details of these models are available in Appendix OC of the Online Appendix. Panel B of Table OA4 shows that the reduction in firms' COE following the enactment of anti-SLAPP legislation persists when using model-based rather than analyst forecasts.

Alternative DiD models. We employ the DiD imputation method proposed by Borusyak, Jaravel, and Spiess (2024) to address concerns about heterogeneous treatment effects in TWFE DiD analyses. Table OA5 shows that our results remain robust when using the DiD estimator from Wooldridge (2025) and the stacked DiD estimator from Cengiz, Dube, Lindner, and Zipperer (2019).

Industry-year and incorporation state-year fixed effects. Table OA5 confirms that our findings are robust to potential unobserved, time-varying differences across industries and incorporation states. We add fixed effects for two-digit SIC industries by year, and for incorporation states by year. The latter absorbs shocks to business laws in incorporation states.

Inverse propensity score weighting. Table 3 indicates modest differences in observable characteristics between states that adopt anti-SLAPP laws and those that do not. To assess whether these differences affect our results, Table OA5 presents an inverse probability-weighted analysis. We assign weights based on a firm's predicted likelihood of being in a state that adopts an anti-SLAPP law in the following year, employing the full set of control variables from our baseline specification. This procedure creates a pseudo-population in which treatment is balanced based on observed factors. After weighting, firm- and state-level characteristics no longer exhibit significant differences between groups, and the reduction in firms' COE after adoption remains statistically significant.

Treatment strength heterogeneity. States vary in the strength of their anti-SLAPP procedures and the scope of speech they protect. For instance, some states like California offer broad protections covering all speech on matters of public concern, along with robust procedural tools—such as early dismissal mechanisms, stays on discovery, and mandatory fees. Conversely, states like Delaware and Nebraska have narrower laws that limit anti-SLAPP protections to specific contexts. Table OA6 explores whether the statutes with more expansive procedures and broader speech coverage have a greater impact on firms' COE. Using data from the Institute of Free Speech's 2023 anti-SLAPP report card, we create quartiles based on the strength of a state's law. Following Greenberg, Keating, and Knowles-Gardner (2023), we use three measures of statute strength: (i) an overall score, (ii) the procedural protections for speakers, and (iii) the scope of speech covered. Although the results are not perfectly monotonic, they indicate that firms' COE decrease more as the strength of anti-SLAPP statutes increases.

4.3 Channel analysis

The results are consistent with the *Uncertainty Resolution Hypothesis*, which posits that anti-SLAPP laws reduce firms' COE by enhancing the corporate information environment, enabling investors to better assess firm risk. We examine this channel in five ways. First, if anti-SLAPP laws improve transparency by curbing firms' ability to suppress criticism, their effects should be more pronounced where monitoring frictions are high and external scrutiny is especially valuable. This prediction applies to firms with opaque information environments, where investors face greater difficulty in assessing fundamentals, as well as to firms with weaker internal governance, where public oversight can substitute for internal controls. The impact should also be greater among firms subject to more media scrutiny, which becomes less encumbered after anti-SLAPP law adoption.

Second, speech protections encourage broader and more credible disclosures. When analysts, employees, and other stakeholders can speak without fear of retaliation, important firm-specific information—both positive and negative—enters the market (e.g., Dyck, Morse, and Zingales, 2010). As a result, improved disclosure following the enactment of anti-SLAPP legislation could reduce information asymmetry between firms and investors.

Third, transparency facilitates more accurate risk assessment. Smoother incorporation of news reduces both realized and implied volatility, as investors encounter fewer surprises. Idiosyncratic volatility may decline as firm operations and governance practices become clearer, while systematic risk can fall because improved information lowers covariance risk with the market (e.g., Diamond and Verrecchia, 1991; Hughes, Liu, and Liu, 2007; Lambert, Leuz, and Verrecchia, 2007). Greater information precision also isolates firm-specific returns from broader macroeconomic shocks. Consequently, stronger free speech rights can reduce realized, implied, firm-specific, and systematic volatilities, ultimately lowering firms' COE.

Fourth, anti-SLAPP laws may address systematic undervaluation stemming from opacity. When investors are unable to determine whether firms are concealing adverse information, they may discount all firms as if hidden risks are present, depressing valuations below fundamental values (Diamond and Verrecchia, 1991). Stronger speech protections reduce this pooling effect by enabling negative information to emerge more credibly and timely. This allows investors to better differentiate between firms with genuine problems and those without, thereby reducing mispricing and narrowing the gap between market prices and fundamentals.

Fifth, stock price crash risk refers to the likelihood of sudden, extreme negative returns. A key source of crash risk is the delayed disclosure of bad news, often withheld by managers until forced by legal, regulatory, or external pressure (e.g., Kothari, Shu, and Wysocki, 2009). Anti-SLAPP laws protect individuals who speak out, thereby increasing transparency by constraining firms' ability to suppress negative news. Thus, these laws can reduce crash risk through more timely price adjustments.

4.3.1 Heterogeneity analysis: Effect of *ex ante* information environment

We employ four firm-level measures of information quality to assess whether firms in less transparent information environments experience a greater reduction in their COE after anti-SLAPP legislation. First, whether a firm provides management earnings guidance reflects its willingness to share forward-looking information (e.g., Healy and Palepu, 2001; Beyer et al., 2010; Kim and Shi, 2011). Firms that avoid such disclosures may face greater investor uncertainty and, as a result, stand to benefit more from the transparency these laws provide. Second, analyst forecast errors act as a proxy for information precision (e.g., Lang and Lundholm, 1996), as firms with higher forecast errors face greater informational frictions and are therefore expected to experience larger declines in COE after anti-SLAPP law enactment. Third, accrual quality serves as a measure of financial reporting reliability (e.g., Francis et al., 2005), where lower-quality accruals indicate greater opacity and thus, stronger speech protections could further reduce uncertainty. Finally, a decrease in information asymmetry is expected to have a more pronounced impact on firms with higher R&D intensity, as R&D activities are typically more opaque. If anti-SLAPP laws help to close this information gap, we anticipate a larger COE reduction for firms with higher R&D intensity.

Table 6 shows that the decrease in firms' COE after anti-SLAPP laws are passed is greater for firms with *ex ante* poorer information environments. The cross-sectional variables in columns 1-2, 3-4, 5-6, and 7-8 are, respectively: *NoMngFrcst*, which is an indicator variable equal to one if a firm does not provide any earnings guidance during a year; *FrcstError*, which is the absolute value of actual earnings per share less the mean consensus analyst forecast all scaled by the absolute value of the mean consensus analyst forecast; $Abs(DiscAcc)$, which is the absolute value of discretionary accruals, defined following the model in Francis et al. (2005); and R&D scaled by sales (*R&D*). The results show that the decrease in firms' COE after the laws are enacted is greater for those that do not provide earnings guidance, have greater analyst forecast errors, have higher discretionary accruals, or are more R&D intensive. Moreover, tests comparing the differences in the ATTs between firms with stronger and weaker *ex ante* information environments are all statistically significant at better than the 1% level. In total, these findings support the hypothesis that anti-SLAPP laws enhance transparency by reducing information asymmetry and uncertainty, particularly for firms in less informative environments.

4.3.2 Heterogeneity analysis: Effect of *ex ante* governance and public scrutiny

We next examine whether firms with weaker *ex ante* internal corporate governance practices or greater external public scrutiny experience greater reductions in their COE following the adoption of anti-SLAPP laws. Legal protections for speech can complement internal governance mechanisms by reducing agency costs and investor uncertainty. Firms with poor governance typically face greater opacity and more challenging monitoring, making external scrutiny especially valuable. In such settings, anti-SLAPP laws may serve as a substitute for weak internal governance by enabling stakeholders to voice concerns without fear of retaliation (e.g., Dyck, Morse, and Zingales, 2010; Appel, 2019). We also expect these effects to be stronger for firms subject to greater public scrutiny, as proxied by news coverage. Firms that receive more media attention are more exposed to public criticism and reputational pressure, making them more reliant on the legal environment governing the dissemination of information about the firm. For such firms, limiting the use of retaliatory litigation should have a larger effect on the timeliness and credibility of adverse information reaching investors. Therefore, we expect the COE-reducing effects of anti-SLAPP laws to be more pronounced for firms with lower governance quality and for firms facing greater external public scrutiny.

We measure three different dimensions of governance quality. First, board independence, measured as the percentage of independent directors, reflects the board's ability to monitor management and protect shareholder interests (e.g., Nguyen and Nielsen, 2010). Lower independence may reduce oversight effectiveness, increasing the potential for anti-SLAPP laws to bolster external monitoring. Second, board busyness, measured as the percentage of directors serving on three or more boards simultaneously, captures directors' attention and monitoring capacity (Fich and Shivdasani, 2006). Greater board busyness can limit directors' ability to monitor firm activities, potentially amplifying the value of external speech protections. Third, CEO pay-performance sensitivity (*Delta*) measures how responsive CEO wealth is to changes in stock price, indicating the alignment of managerial incentives with shareholder value (Core and Guay, 2002; Coles, Daniel, and Naveen, 2006). Lower *Delta* values suggest weaker alignment, heightening the need for external monitoring mechanisms. To measure a firm's exposure to public scrutiny, we count the number of news articles covering the firm over the prior three years using data from the RavenPack news database (*Media*).

Table 7 shows that the decrease in firms' COE after the anti-SLAPP legislation is more pronounced for firms with *ex ante* weaker internal governance and greater media coverage. Specifically, the decline in COE is larger for firms with less independent boards of directors, a higher proportion of busy directors, lower CEO pay-performance sensitivity, and more news articles. Tests comparing ATT differences between firms with weaker and stronger *ex ante* governance and public scrutiny are statistically significant at the 1% level, except in column 6, where the difference is significant at the 3.1% level.

4.3.3 Anti-SLAPP legislation and information asymmetry

To examine how anti-SLAPP laws affect information asymmetry between firms and investors, we rely on three well-established market-based measures: bid-ask spreads, the Amihud (2002) illiquidity measure, and the probability of informed trading (PIN) (Easley, Kiefer, O'hara, and Paperman, 1996). Each proxy captures a different dimension of asymmetric information. Bid-ask spreads reflect the cost of trading and often widen when market makers perceive a higher risk of trading against informed investors. The Amihud-illiquidity measure captures the price impact of trades; higher values indicate that even small trades substantially move prices, suggesting limited liquidity and greater information frictions. PIN estimates the likelihood that a trade is driven by private, value-relevant information rather than noise, capturing the market's "suspicion level" that insiders or sophisticated investors are acting on information that is not yet reflected in prices.

Table 8 shows that the adoption of anti-SLAPP laws substantially reduces information asymmetry between firms and investors. Column 1 shows that bid-ask spreads decrease by 0.171 basis points (t-statistic = -4.70) after the laws are enacted, while the ATT moderates to 0.101 basis points (t-statistic = -5.49) in column 2 after adding firm- and state-level controls. Relative to the mean value of *BidAsk*, these estimates imply declines of 7.2% to 12.2%. Column 3 indicates that illiquidity decreases by 0.123 (t-statistic = -2.45) following anti-SLAPP legislation, and including controls in column 4 yields an ATT of 0.084 (t-statistic = -1.91). Compared to the mean value of *Illiq*, these estimates suggest declines of 26.6% to 38.9%. Finally, columns 5 and 6 show that the adoption of anti-SLAPP laws is associated with decreases in PIN of 0.736 basis points without controls and 0.442 basis points with controls (t-statistic of -3.18 and -2.24, respectively). Relative to the mean value of PIN, these estimates indicate declines of 2.5% to 4.2%.

Overall, these findings suggest that anti-SLAPP laws decrease information asymmetry between firms and investors and are consistent with stronger speech protections fostering a more transparent information environment with less investor uncertainty.

4.3.4 Anti-SLAPP legislation and firm risk

To examine whether anti-SLAPP laws also reduce overall firm risk and uncertainty faced by investors, we create measures of total stock return volatility, idiosyncratic volatility, implied volatility, and market beta. Total volatility, defined as the standard deviation of a firm's weekly returns over the year, captures both systematic and firm-specific sources of uncertainty. Implied volatility, derived from at-the-money equity option prices using the Black-

Scholes model, reflects the market's expectations of a firm's future volatility and serves as a forward-looking measure of investor uncertainty. Idiosyncratic volatility, defined as the standard deviation of a firm's weekly idiosyncratic returns over the year, isolates shocks unique to the firm that are unrelated to broader market movements. Finally, beta, which equals the coefficient from a regression of a firm's weekly stock returns on the market portfolio's returns over the year, measures a firm's exposure to systematic risk and investor perceptions of how it comoves with the market.

Table 9 shows that firm risk decreases after anti-SLAPP laws are enacted. The first two columns indicate a decrease in total stock return volatility of 1.2 to 2.0 percentage points (t-statistics of -3.08 and -2.65, respectively) following the adoption of these laws. The second two columns find that implied volatilities decrease by 1.4 to 1.7 percentage points (t-statistics of -2.14 and -2.22, respectively). Similarly, the next two columns show that the passage of anti-SLAPP statutes is associated with decreases in idiosyncratic volatility of 1.0 to 1.8 percentage points (t-statistics of -2.95 and -2.64, respectively). Finally, the last two columns show that market betas fall by 0.043 to 0.049 (t-statistics of -2.35 and -1.93, respectively). Relative to their respective sample means, these effects translate to relative decreases in total return volatility, implied volatility, idiosyncratic volatility, and market beta of 2.7% to 4.5%, 2.8% to 3.4%, 2.5% to 4.5%, and 3.9% to 4.4%, respectively.

In sum, these findings suggest that when firms can use SLAPPs to suppress negative information, uncertainty about the firms' activities and practices is higher. By limiting these legal tactics, anti-SLAPP laws promote greater transparency and enhance the information environment, enabling investors to assess a firm's fundamentals more accurately and resulting in more accurate pricing and lower volatility in stock returns.

4.3.5 Anti-SLAPP legislation and firm undervaluation

To examine whether anti-SLAPP laws reduce mispricing, we calculate undervaluation measures using two established firm-level valuation models. First, following Hoberg and Phillips (2010), we estimate industry-specific present value models to predict market-to-book ratios based on firm fundamentals, including firm age, dividend policy, leverage, size, profitability, and earnings volatility. Second, based on the approach of Rhodes-Kropf, Robinson, and Viswanathan (2005), we estimate industry-specific firm-level market values using book equity, earnings, and leverage. We describe these models in Appendix A. For both models, we create indicator variables that equal one when actual valuations fall below the respective model's prediction.

Table 10 shows that the likelihood that a firm is undervalued decreases after anti-SLAPP laws are enacted. Using the Hoberg and Phillips (2010) model in columns 1 and 2, we find that the probability of undervaluation decreases by 3.0 percentage points without controls and 2.2 percentage points with controls (t-statistics of -3.22 and -2.61, respectively). In our sample, 41.5% of firms are undervalued using this model, implying relative declines in undervaluation of 5.3% to 7.2%. Similarly, when using the Rhodes-Kropf, Robinson, and Viswanathan (2005) model in columns 3 and 4, anti-SLAPP legislation is associated with decreases in the likelihood of undervaluation of 2.4 and 2.1 percentage points (t-statistics of -3.17 and -2.70, respectively). For this model, 23.0% of the firms are considered undervalued, suggesting relative declines in undervaluation of 9.1% to 10.4%.

Overall, these results indicate that anti-SLAPP laws reduce undervaluation by helping investors distinguish between firms with genuine risks. Thus, by increasing valuation accuracy, these laws and the stronger speech protections they provide enhance market efficiency, reducing risk and improving the link between prices and firm fundamentals.

4.3.6 Anti-SLAPP legislation and stock price crash risk

To examine the effect of anti-SLAPP laws on stock price crash risk, we construct four measures of crash risk following the literature (e.g., Chen, Hong, and Stein, 2001; Jin and Myers, 2006; Hutton, Marcus, and Tehranian, 2009; Kim, Li, and Zhang, 2011a,b): (i) $NCSKEW$: negative conditional skewness of firm-specific weekly returns during a year for each firm; (ii) $DUVOL$: down-to-up volatility of firm-specific weekly returns during a year for each firm; (iii) $Crash$: indicator variable for firms experiencing at least one stock price crash week during a year; (iv) *CrashCount*: number of negative minus positive firm-specific crash weeks during a year for each firm. These measures are described in detail in Appendix A.

Table 11 shows that the adoption of anti-SLAPP laws reduces crash risk. Columns 1 and 2 indicate that the ATT for *NCSKEW* is -0.022 without controls and -0.042 with controls (t-statistics of -2.08 and -2.34, respectively). Compared to the standard deviation of *NCSKEW*,[12] the coefficient estimates represent reductions in the negative skewness of stock returns of 3.2% to 6.1% of a standard deviation. Column 3, without controls, indicates that the laws are associated with a -0.012 (t-statistic = -2.15) reduction in *DUVOL*, while column 4, which adds the controls, implies a -0.023 (t-statistic = -2.36) decline. These results suggest that anti-SLAPP laws reduce the down-to-up volatility of stock returns by 2.6% to 5.0% of a standard deviation.

Meanwhile, column 5 shows that, in regressions without firm- and state-level controls, the likelihood that a firm experiences at least one crash event during a year decreases by 2.3 percentage points (t-statistic = -3.40) after anti-SLAPP laws are passed. Adding controls in column 6 yields an ATT of -3.2 percentage points. Given that 18.7% of firms experience a crash event, these estimates imply that anti-SLAPP laws reduce the likelihood of a crash event by 12.3% to 17.1%. When focusing on the frequency of negative versus positive crashes in columns 7 and 8, we find that these laws are associated with decreases in this frequency of 0.033 and 0.046 (t-statistics of -2.81 and -2.83, respectively), implying reductions in crash frequency of 5.7% to 7.9% of a standard deviation.

These results suggest that anti-SLAPP laws reduce the risk of a stock price crash. This is consistent with the view that stronger speech protections encourage greater public participation and scrutiny of corporate activities, thereby reducing the firms' hoarding of bad news and the likelihood of sudden negative revelations.

[12]Because the crash risk measures can take negative values, when computing economic magnitudes, we compare the ATTs to the standard deviation of the respective variable rather than the mean.

4.3.7 Possible reinforcing mechanisms

The reduction in firms' COE following anti-SLAPP legislation likely operates through complementary channels that amplify transparency effects. First, enhanced stakeholder engagement encourages improvements in external governance. Prior research shows that anti-SLAPP protections increase the credibility of employee whistleblowing (Böke et al., 2025) and crowdsourced investment research (Schafhäutle and Veenman, 2024), strengthening monitoring by institutional investors and analysts. Griffin et al. (2024a) similarly demonstrate that speech protections foster ESG disclosures that more accurately reflect a firm's underlying quality. With reduced SLAPP risk, stakeholders can openly critique corporate practices, providing institutional investors with valuable, low-cost signals for refining portfolio allocations (e.g., Appel, 2019). Sell-side analysts, in turn, face greater pressure to incorporate non-traditional data sources into their models (e.g., Jame, Johnston, Markov, and Wolfe, 2016). These dynamics create a self-reinforcing cycle: greater information production enhances market efficiency, which further dampens equity premia linked to uncertainty.

Second, anti-SLAPP laws may interact with regulatory oversight to strengthen internal compliance systems. Lee et al. (2026) find that managers in states with these speech protections are less likely to hoard bad news, as heightened external scrutiny raises the expected costs of concealment. This shift complements SEC whistleblower programs by lowering the threshold for internal reporting of misconduct (Heese and Pérez-Cavazos, 2021). The threat of swift public exposure pushes firms to address operational risks preemptively, a mechanism supported by Chen, Ke, and Kuang (2026) and Wu, Bruton, and Krause (2026) in the context of environmental regulation. These adaptations improve financial reporting quality through two main channels: (i) accelerated loss recognition via more conservative accounting, and (ii) greater litigation risk as external scrutiny substitutes for weak internal governance. Together, these effects lead to clearer earnings signals, which investors price through a lower COE (e.g., Lambert, Leuz, and Verrecchia, 2007). Notably, these channels are not mutually exclusive; rather, they operate concurrently to amplify the overall impact of anti-SLAPP protections on transparency and investor confidence. Further investigation of these mechanisms could be a promising direction for future research.

5 Conclusion

This study examines whether legal protections that allow stakeholders to voice concerns about firms help or hinder corporate financing. To answer this question, we analyze anti-SLAPP legislation across U.S. states. These laws deter lawsuits meant to silence criticism via burdensome legal costs. Unlike disclosure regulations or investor-protection rules, anti-SLAPP laws do not target capital markets. Instead, they stem from concerns about free expression, judicial efficiency, and civic engagement. We leverage their staggered adoption to identify how stronger free speech affects firms' COE.

Our findings show that firms in states enacting anti-SLAPP laws experience a decline in their COE. This reduction remains robust across various COE measures, alternative DiD estimators, and a comprehensive set of firm- and state-level controls. Anti-SLAPP laws lower legal barriers to speaking out, enabling more individuals to share negative information without fear of retaliation. Consequently, negative information is released gradually and credibly, rather than in sporadic, crash-prone events. The impact on COE is more pronounced for firms with opaque information or weaker governance, where public monitoring more effectively substitutes for limited internal oversight. This effect is also stronger for firms with more media attention, where criticism and reputational pressure are higher. Following the adoption of anti-SLAPP legislation, we also observe declines in information asymmetry, firm risk, the likelihood of undervaluation, and stock price crash risk.

One question our study raises is: Why do firms not voluntarily disclose more, given that transparency reduces their COE? A key friction is the tension between firm incentives and market-wide efficiency: managers often face personal costs, such as regulatory scrutiny or reputational harm, when releasing unfavorable or uncertain information, which can outweigh financing benefits. Additionally, investors may view firm-initiated disclosures as biased or incomplete. Anti-SLAPP laws resolve this friction by enabling external parties to share credible information without fear of retaliation. In turn, this external and involuntary transparency improves the information environment and ultimately lowers firms' COE.

Our results contribute to the corporate finance, accounting, and law and finance literatures and provide practical insights for policymakers, managers, and investors. For legislators, our study highlights the economic benefits of anti-SLAPP statutes, extending beyond First Amendment protections to increased capital market efficiency. For firms, the evidence suggests that strong speech protections improve the information environment and reduce financing costs. Finally, for investors, our findings show that legal institutions supporting free speech enhance information and risk assessment.

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

Implied Cost of Equity (Gebhardt, Lee, and Swaminathan, 2001)

Following previous research in this literature, we outline the estimation of a firm's implied COE defined using the *ex ante* approach of Gebhardt, Lee, and Swaminathan (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 the next ith year at time t.
- $POUT$ = Forecasted dividend payout ratio. We use the ratio of the indicated annual dividends from IBES and $FEPS_{t+1}$ 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 within 0 and 1.

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

We employ IBES analysts' predictions as a measure of the market's anticipation of the firm's earnings in the upcoming three years. We measure market expectations for earnings by assuming a linear decrease in the future return on equity (FROE), converging to an equilibrium return on equity (ROE) from the fourth year to the Tth 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*). We categorize all firms into Fama-French 48 industries, encompassing every company, even those with negative ROEs, to calculate the industry ROE. 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., $B_{t+1} = B_t + EPS_{t+1} - DPS_{t+1}$). The future dividend, DPS_{t+i}, is calculated by multiplying EPS_{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 difference of 0.001 dollar.

Crash Risk

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 develop four firm-specific measures of stock price crash risk for each firm-year observation: (i) $NCSKEW$, which represents the negative conditional skewness of firm-specific weekly returns over the year for each firm; (ii) $DUVOL$, the ratio of downside volatility to upside volatility of firm-specific weekly returns during the year for each firm; (iii) $Crash$, a binary variable indicating whether a firm experienced at least one stock price crash week during the year; and (iv) $CrashCount$, which quantifies the total number of negative minus positive firm-specific crash weeks within the year for each firm. Consistent with prior studies (e.g., Hutton, Marcus, and Tehranian, 2009), we calculate firm-specific residual weekly returns using the extended market index model regression applied to year t:

$$\begin{aligned} r_{j,t} = \alpha_j + \beta_{1,j} r_{m,t-2} + \beta_{2,j} r_{ind,t-2} + \beta_{3,j} r_{m,t-1} + \beta_{4,j} r_{ind,t-1} + \beta_{5,j} r_{m,t} + \\ \beta_{6,j} r_{ind,t} + \beta_{7,j} r_{m,t+1} + \beta_{8,j} r_{ind,t+1} + \beta_{9,j} r_{m,t+2} + \beta_{10,j} r_{ind,t+2} + \epsilon_{j,t}, \end{aligned} \tag{A-2}$$

where $r_{j,t}$ represents the weekly return of stock j, $r_{m,t}$ denotes the weekly return of the CRSP value-weighted market index, and $r_{ind,t}$ corresponds to the weekly return of the value-weighted 2-digit SIC industry index. To address non-synchronous trading issues, we enhance the traditional market index model by incorporating lead and lag terms, following the approach of Dimson (1979). This regression decomposes a firm's return into two components: one that aligns with overall market movements and another that reflects firm-specific shocks ($\epsilon_{j,t}$). Consistent with prior studies (e.g., Chen, Hong, and Stein, 2001; Hutton, Marcus, and Tehranian, 2009), the firm-specific weekly return for firm j in week t is calculated as $W_{j,t} = \ln(1 + \epsilon_{j,t})$. Applying the natural logarithm transformation helps mitigate positive skewness in the stock return distribution, thereby enhancing the symmetry of $W_{j,t}$.

A.1 Negative Conditional Skewness

The first metric for assessing firm-specific crash risk is $NCSKEW_{j,t}$, which represents the negative coefficient of skewness for firm-specific weekly returns. In line with Chen, Hong, and Stein (2001), $NCSKEW_{j,t}$ is computed as the negative third central moment of $W_{j,t}$ divided by the cubed standard deviation of $W_{j,t}$:

$$NCSKEW_{j,t} = -\frac{n_{j,t}(n_{j,t}-1)^{1.5} \sum_1^{n_{j,t}} W_{j,t}^3}{(n_{j,t}-1)(n_{j,t}-2) \sum_1^{n_{j,t}} (W_{j,t}^3)^{1.5}}, \tag{A-3}$$

where $n_{j,t}$ represents the count of firm-specific weekly returns available for firm j during year t. To enable comparisons across firm-specific returns with different levels of variance, the raw third central moment is scaled by a normalization factor—the cubed standard deviation in the denominator. The negative sign in Eq. (A-3) ensures that a rise in $NCSKEW_{j,t}$ indicates higher stock price crash risk for firm j in fiscal year t, reflecting a more negatively skewed return distribution.

A.2 Downside-to-Upside Volatility

The second metric for firm-specific crash risk is $DUVOL_{j,t}$, as introduced by Chen, Hong, and Stein (2001), which measures the ratio of downside volatility to upside volatility:

$$DUVOL_{j,t} = ln\left(\frac{(n_{u,j,t}-1)\sum_{1}^{n_{d,j,t}} W_{j,t}^2}{(n_{d,j,t}-1)\sum_{1}^{n_{u,j,t}} W_{j,t}^2}\right), \tag{A-4}$$

where $n_{u,j,t}$ ($n_{d,j,t}$) represents the count of up (down) weeks for firm j's stock during year t. Up weeks are defined as periods when firm-specific weekly returns exceed the annual mean, while down weeks occur when returns fall below the 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 $W_{j,t}$ during down weeks and up weeks. Similar to $NCSKEW_{j,t}$, a higher value of $DUVOL_{j,t}$ indicates greater stock price crash risk for firm j in year t.

A.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, Marcus, and Tehranian (2009), crash weeks for firm j in year t are identified as those weeks when the firm-specific weekly return $W_{j,t}$ falls 3.09 standard deviations below the mean firm-specific weekly returns for the year, which corresponds to the 0.1st percentile of a normal distribution. $Crash_{j,t}$ reflects the probability of a stock price crash for a firm within a given year. Additionally, the fourth measure of firm-specific crash risk, $CrashCount_{j,t}$, quantifies the total number of negative minus positive firm-specific crash weeks for firm j during year t. A positive crash week is when the firm-specific weekly return falls 3.09 standard deviations above the mean firm-specific weekly returns for the year.

Firm Valuation Models

A.4 Hoberg and Phillips (2010) relative valuation model

Based on the framework of Hoberg and Phillips (2010), we estimate an industry-level present value model based on 3-digit SIC industries using firm-level data from year $t-10$ to $t-1$. For each firm i operating in industry j, we regress the market-to-book asset ratio, $\frac{M}{B}_{i,\tau}$, 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:

$$\left(\frac{M}{B}\right)_{i,\tau} = a + b\,FIRMAGE_{i,\tau} + c\,DD_{i,\tau} + d\,LEV_{i,\tau} + e\,Ln(SIZE_{i,\tau}) + f\,VOLP_i + g\,ROA_{i,\tau}, \quad \tau = t-10,\ldots,t-1, \tag{A-5}$$

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

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 actual firm-level natural logarithm of market-to-book asset ratio from its predicted counterpart.

A.5 Rhodes-Kropf, Robinson, and Viswanathan (2005) relative valuation model

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

$$Ln(M)_{i,\tau} = a + bLn(B)_{i,\tau} + cLn(|NI|)_{i,\tau} + d\,NEG_{i,\tau} \times Ln(|NI|)_{i,\tau} + e\,MLEV_{i,\tau}, \quad \tau = t-10,\ldots,t-1, \tag{A-6}$$

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

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

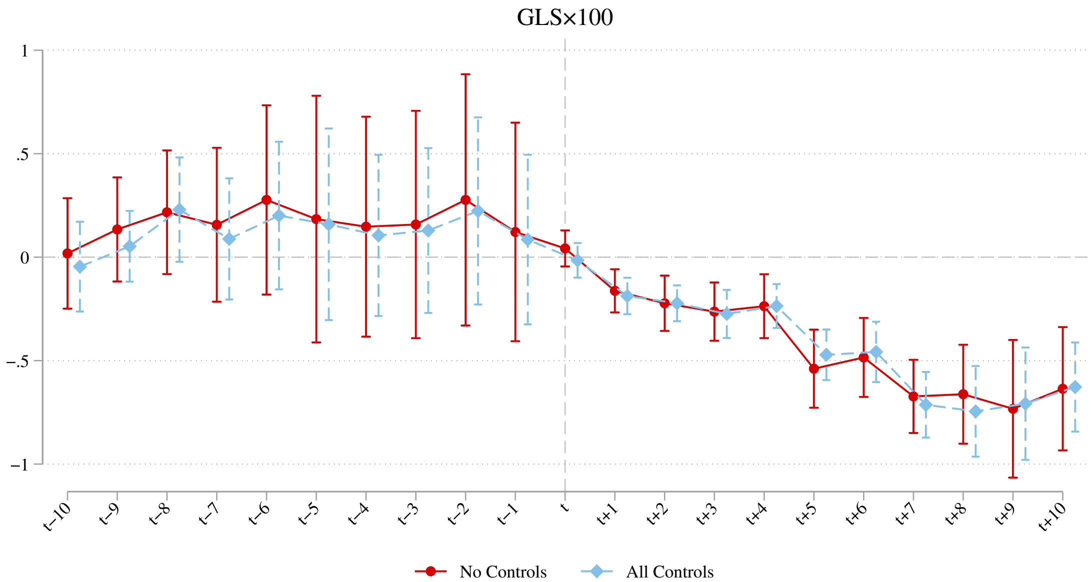
Appendix B. Variable Definitions

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

Variable	Definition
ΔGDP	One year percent change in per capita state-level gross domestic product.
%Busy	Percentage of directors on a board that sit on three or more boards during a year.
%Indep	Percentage of independent directors on a board.
Abs(DiscAcc)	Absolute value of discretionary accruals calculated following Francis et al. (2005).
Age	The average age of a state's population. Data are from the Current Population Survey.
Analysts	Number of analysts providing an annual earnings forecast.
Beta	The beta coefficient from a regression of a firm's weekly stock returns on CRSP value-weighted market portfolio returns over the year (i.e., a firm's exposure to systematic risk).
BidAsk	The average bid-ask spread of a firm's stock over a year.
BLEV	Book value of debt scaled by book value of assets [(*dltt*+*dlc*)/*at*].
BM	Book value of equity divided by market value of equity [*ceq*/(*prcc_f*×*csho*)].
CashFlow	Income before extraordinary items plus depreciation and amortization scaled by book value of assets [(*ib*+*dp*)/*at*].
Crash	An indicator variable equal to one for firms experiencing at least one stock price crash week during a year, and zero otherwise.
CrashCount	The number of negative minus positive firm-specific crash weeks for each firm-year.
Delta	The dollar change in CEO stock and option wealth given a 1% change in stock price. Data are from Lalitha Naveen's website (Coles, Daniel, and Naveen, 2006).
DemGov	An indicator variable that equals one if a state governor is from the Democratic party, and zero otherwise.
DUVOL	The down-to-up volatility of firm-specific weekly returns during each year for each firm.
EDUC	The fraction of a state's population aged 25 or older that have a Bachelor's degree. Data are from the Current Population Survey.
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 all 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, Lee, and Swaminathan (2001).
IdioVol	Standard deviation of weekly idiosyncratic stock returns over a firm's fiscal year and annualized. Idiosyncratic returns are the residuals from a regression of a firm's weekly stock returns on CRSP value-weighted market portfolio returns over the year.
Illiq	The average Amihud (2002) illiquidity measure of a firm's stock over a year, which equals the absolute value of daily returns multiplied by 100,000 and then scaled by trading volume.
ImpVol	Annualized implied volatility of a firm's stock returns, derived by averaging daily implied volatility from 30-day options over a year and requiring at least 21 trading days with valid implied volatility.
LTGRate	Analysts' consensus forecast of a firm's long-term earnings growth rate.
Marijuana	An indicator variable that equals one if a firm is headquartered in a state that has legalized medical marijuana by year t, and zero otherwise.

Media	The number of news articles covering a firm over the prior three years in the RavenPack news database.
MOM	Buy-and-hold stock return over a firm's fiscal year.
MVE	Market value of equity at the end of each fiscal year ($prcc\_f \times csho$) in millions and 2020 dollars.
NCSKEW	The negative conditional skewness of firm-specific weekly returns during each year for each firm.
NoMngFrcst	An indicator variable that equals one if a firm does not provide management earnings forecasts/guidance during year t, and zero otherwise.
PIN	Average probability of informed trading following Easley et al. (1996) over a year.
Post (D in Eq. (2))	An indicator variable that equals one if a firm is headquartered in a state that has passed an anti-SLAPP law by year t, and zero otherwise.
R&D	A firm's research and development expenditures scaled by sales ($xrd/sale$).
TaxRate	State-level highest marginal corporate tax rate.
UnempBen	State-level total unemployment benefits (max benefits times max number of weeks). Data are from the Department of Labor.
UnempRate	State-level fraction of workers that are unemployed.
Volatility	Standard deviation of weekly stock returns over a year and annualized.

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



This figure plots the dynamic effect of anti-SLAPP legislation on a firm's implied COE (*GLS*) over the period 1987 to 2023, estimated using the DiD imputation method. *GLS* is derived from analyst EPS forecasts calculated using the model from Gebhardt, Lee, and Swaminathan (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*), *%ΔGDP*, *TaxRate*, *UnempRate*, *DemGov*, *Ln*(*UnempBen*), *Marijuana*, *Age*, and *EDUC*. All variables are defined in Appendix B. 95% confidence intervals based on standard errors clustered by state are reported.

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, Rajaiya, and Sheng, 2020; Griffin et al., 2024a; Jung et al., 2025) and internet searches.

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	2021
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. Appendix B provides variable definitions.

	Mean	SD	P25	P50	P75
GLS	0.066	0.033	0.044	0.068	0.087
BidAsk	0.014	0.019	0.001	0.008	0.022
Illiq	0.316	2.671	0.001	0.005	0.045
PIN	0.174	0.102	0.109	0.165	0.223
NCSKEW	0.066	0.690	-0.329	0.023	0.406
DUVOL	0.051	0.458	-0.258	0.036	0.340
Crash	0.187	0.390	0.000	0.000	0.000
CrashCount	0.008	0.583	0.000	0.000	0.000
Volatility	0.447	0.237	0.284	0.390	0.544
IdioVol	0.399	0.223	0.245	0.345	0.490
ImpVol	0.498	0.354	0.307	0.410	0.571
Beta	1.103	0.707	0.648	1.047	1.488
UnderValued-HP	0.415	0.493	0.000	0.000	1.000
UnderValued-RRV	0.230	0.421	0.000	0.000	1.000
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 between firms in states that have enacted anti-SLAPP laws and firms in states that have not in the year before adoption. The anti-SLAPP law firms are defined as those in states that will pass anti-SLAPP laws in year t. The non-anti-SLAPP law subsample comprises firms in states that never or have not yet passed anti-SLAPP laws in the year before an adoption event occurs. There are 1,632 observations in the anti-SLAPP law subsample and 21,866 observations in the non-anti-SLAPP law subsample. All variables are defined in Appendix B. t-statistics for tests of the 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, Lee, and Swaminathan (2001) across all firms in a state in a given year. In column 5, all determinants are measured in changes. All variables are defined in Appendix B. t-statistics in parentheses are calculated from standard errors clustered by state. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

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

Table 5: Anti-SLAPP Legislation and COE

This table reports our baseline results from the DiD imputation method examining the effect of anti-SLAPP legislation on a firm's implied COE over the period 1987 to 2023. The dependent variable is a firm's implied COE defined using the model of Gebhardt, Lee, and Swaminathan (2001) (*GLS*). *ATT* measures the average treatment effect of anti-SLAPP legislation on a firm's COE. Lagged firm control variables include *Ln*(*MVE*), *BM*, *BLEV*, *MOM*, *CashFlow*, *Ln*(*Analysts*), *Ln*(*FrcstDisp*), and *LTGRate*. Lagged state control variables include *Ln*(*GDP*), *%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.

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

Table 6: Heterogeneity: Effect of *Ex Ante* Information Environment

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

	Split by NoMngFrcst		Split by FrcstError		Split by Abs(DiscAcc)		Split by R&D	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
ATT - Low	0.043	-0.059	-0.358***	-0.337***	-0.393***	-0.396***	-0.057	-0.141
	(0.28)	(-0.39)	(-2.91)	(-3.59)	(-3.18)	(-3.96)	(-0.46)	(-1.36)
ATT - High	-0.334***	-0.413***	-0.572***	-0.603***	-0.675***	-0.652***	-0.919***	-0.822***
	(-2.81)	(-3.25)	(-5.25)	(-8.15)	(-6.38)	(-8.00)	(-7.55)	(-8.78)
Low = High *p*-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 7: Heterogeneity: Effect of *Ex Ante* Governance and Public Scrutiny

This table reports the heterogeneous effects of *ex ante* governance and public scrutiny on our baseline results from the DiD imputation method over the period 1999 to 2023 in columns 1-4, 1993 to 2023 in columns 5-6, and 2001 to 2023 in columns 7-8. The dependent variable is a firm's implied COE ($GLS \times 100$) defined using the model of Gebhardt, Lee, and Swaminathan (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 board independence (*%Indep*), board busyness (*%Busy*), CEO pay-performance sensitivity (*Delta*), and media coverage (*Media*), respectively. In columns 1-8, *ATT - Low* and *ATT - High* measure the average treatment effect of anti-SLAPP legislation for firms with below and above median values of the respective measure. Lagged firm control variables include $Ln(MVE)$, *BM*, *BLEV*, *MOM*, *CashFlow*, $Ln(Analysts)$, $Ln(FrcstDisp)$, and *LTGRate*. Lagged state control variables include $Ln(GDP)$, $\%\Delta GDP$, *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		Split by Media	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
ATT - Low	-0.444***	-0.455***	-0.157*	-0.187*	-0.246*	-0.295***	-0.095	-0.087
	(-5.19)	(-5.17)	(-1.70)	(-1.96)	(-1.92)	(-2.76)	(-0.75)	(-0.75)
ATT - High	-0.213**	-0.208**	-0.512***	-0.480***	-0.011	-0.069	-0.354***	-0.414***
	(-2.16)	(-1.98)	(-6.48)	(-5.60)	(-0.09)	(-0.48)	(-3.54)	(-3.82)
Low = High p-val	0.002	0.002	<0.001	<0.001	0.005	0.031	0.008	<0.001
Year FE	✓	✓	✓	✓	✓	✓	✓	✓
Firm FE	✓	✓	✓	✓	✓	✓	✓	✓
Controls		✓		✓		✓		✓
N of Obs	17,557	17,557	17,557	17,557	19,342	19,342	20,279	20,279

Table 8: Anti-SLAPP Legislation and Information Asymmetry

This table reports results from the DiD imputation method examining the effect of anti-SLAPP legislation on information asymmetry over the period 1987 to 2023 (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 (*BidAsk*), the Amihud (2002) illiquidity measure of a firm's stock (*Illiq*), and the probability of informed trading of a firm's stock (*PIN*). *ATT* measures the average treatment effect of anti-SLAPP legislation on information asymmetry. Lagged firm control variables include *Ln*(*MVE*), *BM*, *BLEV*, *MOM*, *CashFlow*, *Ln*(*Analysts*), *Ln*(*FrcstDisp*), and *LTGRate*. Lagged state control variables include *Ln*(*GDP*), *%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***	-0.101***	-0.123**	-0.084*	-0.736***	-0.442**
	(-4.70)	(-5.49)	(-2.45)	(-1.91)	(-3.18)	(-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 DiD imputation method examining the effect of anti-SLAPP legislation on firm risk over the period 1987 to 2023 (1996 to 2023 in columns 3-4). The dependent variables in columns 1-2, 3-4, 5-6, and 7-8 are a firm's stock return volatility (*Volatility*), implied stock return volatility (*ImpVol*), idiosyncratic stock return volatility (*IdioVol*), and market return beta (*Beta*), respectively. *ATT* measures the average treatment effect of anti-SLAPP legislation on firm risk. Lagged firm control variables include *Ln*(*MVE*), *BM*, *BLEV*, *MOM*, *CashFlow*, *Ln*(*Analysts*), *Ln*(*FrcstDisp*), and *LTGRate*. Lagged state control variables include *Ln*(*GDP*), %Δ*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.

	Volatility		ImpVol		IdioVol		Beta	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
ATT	-0.020***	-0.012***	-0.017**	-0.014**	-0.018***	-0.010***	-0.049*	-0.043**
	(-2.65)	(-3.08)	(-2.22)	(-2.14)	(-2.64)	(-2.95)	(-1.93)	(-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 DiD imputation method examining the effect of anti-SLAPP legislation on the likelihood that a firm is undervalued over the period 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, Robinson, and Viswanathan (2005) valuation models, respectively. *ATT* measures the average treatment effect of anti-SLAPP legislation on the likelihood a firm is undervalued. Lagged firm control variables include *Ln*(*MVE*), *BM*, *BLEV*, *MOM*, *CashFlow*, *Ln*(*Analysts*), *Ln*(*FrcstDisp*), and *LTGRate*. Lagged state control variables include *Ln*(*GDP*), *%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.

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

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

This table reports results from the DiD imputation method examining the effect of anti-SLAPP legislation on stock price crash risk over the period 1987 to 2023. The dependent variables in columns 1-2, 3-4, 5-6, and 7-8, are $NCSKEW$, $DUVOL$, $Crash$, and $CrashCount$, respectively. ATT measures the average treatment effect of anti-SLAPP legislation on crash risk. Lagged firm control variables include $Ln(MVE)$, *BM*, *BLEV*, *MOM*, *CashFlow*, $Ln(Analysts)$, $Ln(FrcstDisp)$, and *LTGRate*. Lagged state control variables include $Ln(GDP)$, $\%\Delta GDP$, *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.

	NCSKEW		DUVOL		Crash		CrashCount	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
ATT	-0.022**	-0.042**	-0.012**	-0.023**	-0.023***	-0.032***	-0.033***	-0.046***
	(-2.08)	(-2.34)	(-2.15)	(-2.36)	(-3.40)	(-3.36)	(-2.81)	(-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

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

Online Appendix

Appendix OA. Examples of SLAPP Cases

Metabolife International, Inc. v. Wornick
In 1999, Metabolife, a publicly traded dietary supplement company, sued Boston TV station WCVB and reporter Susan Wornick for defamation after a broadcast series criticized the safety of its ephedra-based weight-loss product, linking it to health risks like heart attacks and deaths based on FDA reports and expert interviews. The suit sought to silence the reporting and remove the content. Under California's anti-SLAPP law, the court dismissed the case around 2000, ruling the statements were protected as matters of public interest and that Metabolife couldn't prove falsity or malice. A defendant was awarded over $100,000 in attorney fees. This protection kept the critical health information public, prompting wider scrutiny that contributed to Metabolife's eventual FDA-mandated product recalls in 2003 and enhanced SEC disclosures on risk factors in its filings.

Varian Medical Systems, Inc. v. Delfino
In 1999, Varian sued former employees Michelangelo Delfino and Mary Day for defamation after they posted thousands of derogatory messages about executives online. The defendants filed an anti-SLAPP motion, which the trial court denied. Although the defendants appealed that denial, the trial court proceeded to trial anyway, resulting in a $775,000 verdict for Varian. However, the California Supreme Court eventually voided the verdict, ruling that an appeal 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.

Global Telemedia Intern., Inc. v. Doe 1
Around 2001, Global Telemedia International, Inc. (a publicly traded telecommunications firm) sued anonymous online posters (including Barry King and another user) 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 the CEO of misleading practices). The defendants successfully invoked California's anti-SLAPP law, leading to the suit's dismissal as the court deemed the statements non-actionable opinions in a public forum on a matter of investor interest. This allowed the critical insights to remain public, fostering greater market scrutiny and timely disclosure of potential issues, which aligns with how anti-SLAPP protections reduce concealment risks.

Sharper Image Corporation v. Consumers Union
In 2003, Sharper Image sued Consumers Union (publisher of Consumer Reports) over a negative review claiming its Ionic Breeze air purifier was ineffective and potentially harmful. The suit aimed to suppress the report, but under California's robust anti-SLAPP law (enacted in 1992), the case was dismissed early in 2004. The court ruled that Sharper Image failed to prove falsity or invalid testing, and awarded Consumers Union $525,000 in legal fees. This outcome ensured the critical information reached consumers and investors, highlighting product flaws that Sharper Image had downplayed in marketing. The enforced transparency contributed to broader scrutiny, including later class-action suits over misleading claims,

which pressured the company toward more accurate disclosures in SEC filings and product labeling. Although Sharper Image filed for bankruptcy in 2008 amid other issues (e.g., declining sales), the case illustrates how anti-SLAPP protections prevent information suppression, reducing long-term reputational risks and investor uncertainty—factors tied to lower cost of equity in empirical analyses of similar opaque retail firms.

Troy Group, Inc. v. Tilson
In 2004, Troy Group, a publicly traded printing technology company, sued investor and critic V. Whitney Tilson for defamation after he emailed a statement to his attorney (later made public in litigation) alleging the company's executives engaged in securities fraud by inflating revenues through questionable accounting. Troy aimed to suppress the claims amid ongoing shareholder suits. California's anti-SLAPP law led to a 2005 dismissal, with the court finding Tilson's speech addressed public interest issues (corporate governance in a listed firm) and that Troy failed to show probable success on the merits. Tilson recovered legal fees. The ruling amplified Tilson's allegations, spurring an SEC probe that uncovered revenue recognition issues, resulting in restated financials and executive changes. This enhanced transparency reduced hidden risks for investors, akin to how anti-SLAPP laws lower crash risk by facilitating stakeholder monitoring in opaque firms, potentially cutting Troy's equity costs during a volatile period.

New.net, Inc. v. Lavasoft
Around 2004, New.net, Inc. (a publicly traded internet technology firm specializing in domain name extensions) sued Lavasoft (maker of Ad-aware spyware detection software) for unfair competition and interference, alleging Lavasoft's software falsely flagged New.net's plugins as spyware and warned users to remove them. Lavasoft successfully moved to strike under California's anti-SLAPP law, with the court ruling that the software's alerts constituted protected speech on a public issue (internet security and consumer protection). The dismissal allowed critical software evaluations to persist, enhancing transparency around potentially invasive tech practices and alerting investors to product vulnerabilities or user complaints.

GTX Global Corp v. Left
Around 2005, GTX Global Corporation (a publicly traded company) sued short seller Andrew Left over online reports published on StockLemon.com (later rebranded as Citron Research) that alleged 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. The California Court of Appeal (Second Appellate District) affirmed the dismissal in May 2007, finding that GTX had failed to identify a single piece of evidence that Left's statements were false. By safeguarding Left's critical analysis from suppression, the ruling promoted external monitoring that could deter managerial misconduct and enhance information flow to investors.

Appendix OB. Theory: Anti-SLAPP Laws and Firms' Cost of Equity

We develop a simple asset-pricing framework illustrating how anti-SLAPP laws affect a firm's COE through two opposing channels: (i) risk revelation, where new speech protections expose previously hidden negative information and thereby increase perceived systematic risk, and (ii) uncertainty resolution, where improved information precision lowers the information risk premium demanded by investors.

The model nests established results from the information and COE literatures—particularly Easley and O'Hara (2004), Diamond and Verrecchia (1991), Lambert, Leuz, and Verrecchia (2007), and Hughes, Liu, and Liu (2007)—and connects them to the legal environment governing corporate speech and criticism.

OB.1 Model Setup

We consider a one-period economy with two dates, $t = 0$ (pricing) and $t = 1$ (cash flow realization). The payoff of the firm i is given by:

$$X_i = \theta_i + J_i + \varepsilon_i, \tag{OB-1}$$

where:

- $\theta_i \sim \mathcal{N}(\mu_i, \sigma^2_{\theta i})$ represents the firm's fundamental component of cash flows;
- J_i is a latent "bad-news" component such that $J_i = -d_i < 0$ with probability p_i and $J_i = 0$ otherwise;
- $\varepsilon_i \sim \mathcal{N}(0, \sigma^2_{\varepsilon i})$ is idiosyncratic noise.

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

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

where R_i denotes the gross return on firm i's equity. Under a linear factor model with $m = a - bR_M$, this simplifies to:

$$\mathbb{E}[R_i] - r_f = \beta_i \lambda_M, \tag{OB-3}$$

where $\beta_i = \frac{\mathrm{Cov}(R_i, R_M)}{\mathrm{Var}(R_M)}$ and $\lambda_M = b\,\mathrm{Var}(R_M)/\mathbb{E}[m]$ is the market risk premium.

OB.2 Information Environment and Anti-SLAPP Laws

At date $t = 0$, investors observe a public signal about the firm's fundamentals:

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

where:

- τ_i is the precision of the public signal;
- $\delta_i \in \{0, 1\}$ indicates whether latent bad news is revealed before trading.

Anti-SLAPP laws alter two aspects of the information structure:

1. **Revelation Probability:** The probability of bad-news revelation, $q_i = \Pr(\delta_i = 1)$, increases because whistleblowers, journalists, and analysts are shielded from retaliatory lawsuits (Dyck, Morse, and Zingales, 2010; Kim and Shi, 2011).

2. **Signal Precision:** The quality of the public signal changes from baseline τ_{0i} to:

$$\tau_{1i} = \tau_{0i} + \Delta\tau_i^{\text{qual}} - \Delta\tau_i^{\text{noise}}, \tag{OB-5}$$

   where $\Delta\tau_i^{\text{qual}} > 0$ represents credible new information (e.g., from expert analysis), and $\Delta\tau_i^{\text{noise}} > 0$ captures added misinformation or low-quality content.

Given normal updating, the posterior variance of firm value conditional on observed signals is as follows:

$$V_i \equiv \operatorname{Var}(\theta_i + \delta_i J_i \mid s_i) = \left(\sigma_{\theta i}^{-2} + \tau_i\right)^{-1}. \tag{OB-6}$$

An increase in τ_i (higher precision) reduces V_i, while revelation ($\delta_i = 1$) resolves uncertainty about the presence of hidden losses.

OB.3 Expected Returns and COE

Following Diamond and Verrecchia (1991), Easley and O'Hara (2004), Lambert, Leuz, and Verrecchia (2007) and Hughes, Liu, and Liu (2007), COE depends on both systematic covariance and information risk:

$$\mathbb{E}[R_i] - r_f = \beta_i(\tau_i, q_i)\lambda_M + \pi(\tau_i), \tag{OB-7}$$

where:

- $\beta_i(\tau_i, q_i)$ denotes systematic risk exposure, and
- $\pi(\tau_i)$ is an information risk premium (Easley and O'Hara, 2004).

We parameterize:

$$\beta_i(\tau_i, q_i) = \beta_{0i} - a_i\tau_i + b_i q_i, \quad \pi(\tau_i) = \frac{\kappa_i}{1 + \tau_i}, \tag{OB-8}$$

where:

- $a_i > 0$: increased precision lowers systematic covariance (Lambert, Leuz, and Verrecchia, 2007; Hughes, Liu, and Liu, 2007);
- b_i: captures whether revelation raises ($b_i > 0$) or reduces ($b_i < 0$) systematic exposure;
- $\kappa_i > 0$: captures the magnitude of information risk.

OB.4 Analytical Results

Proposition 1 (Uncertainty Resolution Channel)**.** If anti-SLAPP laws increase net information precision ($\Delta\tau_i > 0$), then posterior variance and COE decrease:

$$\frac{\partial V_i}{\partial \tau_i} < 0, \quad \frac{\partial \text{COE}_i}{\partial \tau_i} < 0.$$

Proof. Differentiating Eq. (OB-7) with respect to τ_i yields:

$$\frac{\partial \text{COE}_i}{\partial \tau_i} = -a_i\lambda_M + \pi'(\tau_i) = -a_i\lambda_M - \frac{\kappa_i}{(1+\tau_i)^2} < 0.$$

Thus, higher information precision lowers estimation risk and the information premium. □

Proposition 2 (Risk Revelation Channel)**.** If anti-SLAPP laws increase the probability that hidden bad news is revealed ($\Delta q_i > 0$) and such news covaries positively with market downturns ($\text{Cov}(J_i, R_M) > 0$), then COE rises:

$$\frac{\partial \text{COE}_i}{\partial q_i} = b_i\lambda_M > 0.$$

Proof. From Eq. (OB-7), differentiating with respect to q_i gives $\partial\text{COE}_i/\partial q_i = (\partial\beta_i/\partial q_i)\lambda_M = b_i\lambda_M$. If revealed bad news loads on systematic risk, $b_i > 0$. □

Proposition 3 (Pooling and Separation Effects)**.** Before anti-SLAPP adoption, investors pool firms with and without latent bad news, demanding a "suspicion premium." If higher q_i allows separation of good and bad types, good-type firms experience a lower COE.
Intuition. As in Diamond and Verrecchia (1991), credible revelation improves liquidity and reduces information asymmetry, lowering financing costs for transparent firms. □

Proposition 4 (Net Effect of Anti-SLAPP Laws)**.** The total differential in COE from anti-SLAPP adoption is:

$$\Delta\text{COE}_i \approx \underbrace{[-a_i\lambda_M + \pi'(\tau_i)]\Delta\tau_i}_{\text{Uncertainty Resolution}} + \underbrace{b_i\lambda_M\Delta q_i}_{\text{Risk Revelation}}. \tag{OB-9}$$

The net effect depends on the relative magnitudes of $\Delta\tau_i$ and Δq_i and the sign of b_i.

OB.5 Interpretation and Empirical Implications

Eq. (OB-9) formalizes the tension between two forces. When credible information dominates noisy information ($\Delta\tau_i > 0$) and revealed risks are largely idiosyncratic, anti-SLAPP laws reduce COE (uncertainty resolution). Conversely, when new information reveals systematic exposures or introduces noise ($\Delta q_i > 0$ and $b_i > 0$), COE increases (risk revelation). Empirically, the net effect depends on whether the transparency gains outweigh the incremental perceived risk.

OB.6 Discussion

This theoretical framework highlights how legal institutions shape information and risk simultaneously. Anti-SLAPP laws can therefore produce heterogeneous effects on financing costs between firms, depending on whether improved transparency ($\Delta\tau_i > 0$) dominates risk revelation ($\Delta q_i > 0$). These predictions align with the dual perspectives in the literature: information as a mechanism for both risk discovery (Kothari, Shu, and Wysocki, 2009; Hutton, Marcus, and Tehranian, 2009) and uncertainty reduction (Easley and O'Hara, 2004; Lambert, Leuz, and Verrecchia, 2007; Hughes, Liu, and Liu, 2007).

OB.7 Symbols and Definitions in the Theoretical Model

Symbols and Definitions

Symbol	Definition
X_i	Firm i's payoff at time $t = 1$
θ_i	Fundamental cash flow component
J_i	Latent bad-news component ($-d_i$ with probability p_i)
ε_i	Idiosyncratic noise term
R_i	Firm i's return
r_f	Risk-free rate
R_M	Market return
λ_M	Market risk premium
m	Stochastic discount factor (SDF)
β_i	Systematic risk exposure
τ_i	Precision of public signal
q_i	Probability that bad news is revealed
δ_i	Indicator for whether bad news is revealed (0/1)
V_i	Posterior variance of firm value given signals
$\pi(\tau_i)$	Information risk premium
a_i, b_i, κ_i	Firm-specific parameters capturing information effects

Appendix OC. Alternative Estimations of the Implied COE and Earnings Forecast Models

Alternative Implied COE Models

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

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

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

where

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

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

OC.2 Claus and Thomas (2001)

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

We employ IBES earnings projections to compute abnormal earnings over the subsequent five years. Projections for earnings in the fourth and fifth years are derived from forecasts for the third year and the long-term earnings growth rate. In instances where the long-term earnings growth rate is unavailable in IBES, an implied earnings growth rate is derived from EPS_{t+2} and EPS_{t+3}. The long-term abnormal earnings growth rate 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 EPS_{t+i} by the payout ratio, $POUT$. We utilize a numerical approximation program to determine R_{CT}, ensuring that the right- and left-hand sides of Eq. (OC-3) are equal within a difference of 0.001 dollar.

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

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

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

Earnings Forecast Models

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

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

OC.4 Earnings Persistence Model

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

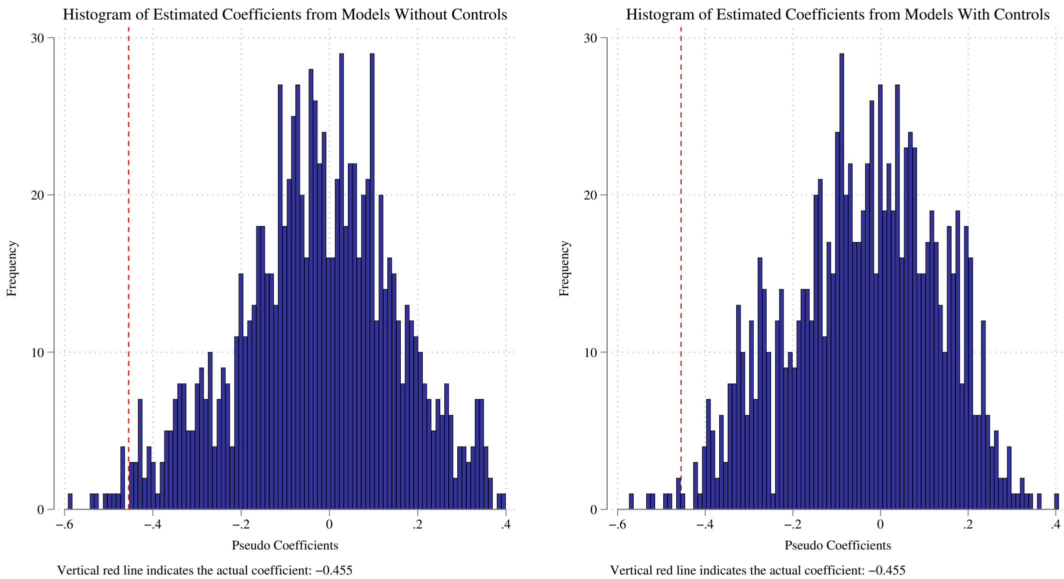
where $E_{i,t}$ represents the per-share income before extraordinary items, excluding special items, for firm i in year t. Additionally, $NegE_{i,t}$ is an indicator variable, taking the value of one if the firm reports negative earnings. The interaction of the two variables is denoted as $NegE \times E_{i,t}$.

OC.5 Residual Income Model

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

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

Figure OA1: Randomize 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*) over the period 1987 to 2023, estimated using the DiD imputation method. *GLS* is derived from analyst EPS forecasts calculated using the model from Gebhardt, Lee, and Swaminathan (2001). We randomly reassign the adoption of anti-SLAPP laws across states, preserving the overall structure of the policy experiment but breaking the actual link between a state's true anti-SLAPP adoption and firms headquartered there. For each placebo draw, we re-estimate our baseline specification without and with controls and record the coefficient on the anti-SLAPP indicator. We repeat this procedure 1,000 times and plot the resulting distribution of placebo estimates. Lagged firm control variables include *Ln*(*MVE*), *BM*, *BLEV*, *MOM*, *CashFlow*, *Ln*(*Analysts*), *Ln*(*FrcstDisp*), and *LTGRate*. Lagged state control variables include *Ln*(*GDP*), *%*Δ*GDP*, *TaxRate*, *UnempRate*, *DemGov*, *Ln*(*UnempBen*), *Marijuana*, *Age*, and *EDUC*. All variables are defined in Appendix B.

Table OA1: Anti-SLAPP Legislation, Negative Press Releases, and Glassdoor Ratings

This table reports results examining the effect of anti-SLAPP legislation on the number of negative press releases and Glassdoor ratings over the period 2000 to 2023 in columns 1-4 and 2008 to 2023 in columns 5-10. In columns 1-4, *NegPr - ESS* and *NegPr - CSS* are the natural logarithm of one plus the number of negative press releases during a firm's fiscal year, where negative sentiment is defined based on RavenPack's Event Sentiment Score and Composite Sentiment Score, respectively. ESS and CSS values below 0 are considered negative, as values of 0 are considered neutral. From RavenPack, we keep press releases with entity and event relevance scores of 100. In columns 5-6, *Business Outlook* takes a value of 3 if the outlook is "Positive", 2 if "Neutral", and 1 if "Negative". In columns 7-8, *Recommend* takes a value of 1 if the rating for recommend business to a friend is "Positive" and 0 if rating is "Negative" (there is no neutral rating for this category). In columns 9-10, *CEO Rating* takes a value of 3 if the rating is "Approve", 2 if "No opinion", and 1 if "Disapprove". ATT measures the average treatment effect of anti-SLAPP legislation on the respective outcome variable in each column. Lagged firm control variables include $Ln(MVE)$, *BM*, $BLEV$, *MOM*, *CashFlow*, $Ln(Analysts)$, $Ln(FrcstDisp)$, and *LTGRate*. Lagged state control variables include $Ln(GDP)$, $\%\Delta GDP$, *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.

	NegPr - ESS		NegPr - CSS		Business Outlook		Recommend		CEO Rating	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
ATT	0.057***	0.042**	0.119***	0.093***	-0.069***	-0.058**	-0.065***	-0.057**	-0.087**	-0.080*
	(3.25)	(2.22)	(5.58)	(4.61)	(-2.80)	(-2.08)	(-3.53)	(-2.48)	(-2.29)	(-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 OA2: Exclude Firms with Dispersed Operations

This table reports results from the DiD imputation method examining the effect of anti-SLAPP legislation on a firm's implied COE over the period 1987 to 2023. The dependent variable is a firm's implied COE ($GLS\times 100$) defined using the model of Gebhardt, Lee, and Swaminathan (2001). *ATT* measures the average treatment effect of anti-SLAPP legislation on a firm's COE. In Panel A, columns 1 and 2 exclude firms in industries with dispersed operations, which include retail, wholesale, and transportation (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 Garcia and Norli (2012). In columns 1-8 of Panel B, we use establishment-level data and only keep firms with at least either 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)*, *%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.

Panel A: Dispersed Operations using Compustat Data

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

Panel B: Dispersed Operations using Establishment-Level Data

	InState>50%		InState>75%		InState>90%		InState=100%	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
ATT	-0.821***	-0.731***	-0.898***	-0.822***	-1.205***	-1.173***	-1.145***	-1.243***
	(-4.20)	(-5.26)	(-4.01)	(-4.55)	(-5.18)	(-6.67)	(-7.35)	(-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 DiD imputation method examining the effect of anti-SLAPP legislation on a firm's implied COE over the period 1987 to 2023. The dependent variable is a firm's implied COE ($GLS\times 100$) defined using the model of Gebhardt, Lee, and Swaminathan (2001). *ATT* measures the average treatment effect of anti-SLAPP legislation on a firm's COE. Columns 1 and 2 drop firms operating in consumer facing industries. Columns 3 and 4 drop firms headquartered in California and New York (i.e., drop CA & NY). In columns 5 and 6, *ATT - Pre2010* and *ATT - Post2010* measure the average treatment effect of anti-SLAPP legislation for firms 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***	-0.695***	-0.338***	-0.377***		
	(-4.41)	(-5.96)	(-2.71)	(-3.24)		
ATT - Pre2010					-0.520***	-0.485***
					(-4.26)	(-6.78)
ATT - Post2010					-0.390***	-0.426***
					(-2.72)	(-3.07)
Low = High p-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 COE Measures

This table reports results from the DiD imputation method examining the effect of anti-SLAPP legislation on the alternative measures of a firm's implied COE over the period 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), Claus and Thomas (2001), Easton (2004), and the mean value (*AvgCOE*) across the previously mentioned three implied COE models plus Gebhardt, Lee, and Swaminathan (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, Lee, and Swaminathan (2001) with EPS forecasts derived from the earnings persistence model (*EP*) and the residual income model (*RIV*), respectively. *ATT* measures the average treatment effect of state-level anti-SLAPP legislation on a firm's COE. Lagged firm control variables include *Ln*(*MVE*), *BM*, *BLEV*, *MOM*, *CashFlow*, *Ln*(*Analysts*), *Ln*(*FrcstDisp*), and *LTGRate*. Lagged state control variables include *Ln*(*GDP*), *%∆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.

Panel A: COE Measured Using Alternative Discount Models

	OJN×100		CT×100		MPEG×100		AvgCOE×100	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
ATT	-0.463***	-0.262***	-0.330***	-0.174**	-0.668***	-0.447***	-0.453***	-0.301***
	(-4.63)	(-6.42)	(-3.11)	(-2.23)	(-5.20)	(-5.78)	(-5.40)	(-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

Panel B: GLS Using Forecasted EPS

	EP×100		RIV×100	
	(1)	(2)	(3)	(4)
ATT	-0.274***	-0.374***	-0.300***	-0.415***
	(-3.26)	(-4.62)	(-3.38)	(-4.75)
Year FE	✓	✓	✓	✓
Firm FE	✓	✓	✓	✓
Controls		✓		✓
N of Obs	93,232	93,232	91,855	91,855

Table OA5: Anti-SLAPP Legislation and COE: Alternative DiD Estimation Methods, Industry-Year FEs, Incorporation-Year FEs, and Propensity Score Weighting

This table reports results examining the effect of anti-SLAPP legislation on a firm's implied COE over the period 1987 to 2023 with alternative DiD estimation methods and propensity score weighting. The dependent variable is a firm's implied COE (*GLS*×*100*) defined using the model of Gebhardt, Lee, and Swaminathan (2001). *ATT* measures the average treatment effect of anti-SLAPP legislation on a firm's COE. Columns 1-2 present results from DiD estimates using the method from Wooldridge (2025), and columns 3-4 present results from stacked DiD estimates (Cengiz et al., 2019). Columns 5-6 control for two-digit SIC industry-year fixed effects. Columns 7-8 control for state of incorporation-year fixed effects. Columns 9-10 use the DiD imputation method but weight the regressions by the inverse of the probability of treatment. We estimate weights using a logit regression as the probability of a firm being 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 or have not yet passed an anti-SLAPP law in the year before a legislation event occurs. These weights are then used for all the firm's 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*), *%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.

	Wooldridge (2025)		Stacked DiD		SIC2×Year FE		Incorp×Year FE		PS Weighted	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
ATT	-0.726***	-0.638***	-0.357***	-0.402***	-0.225***	-0.203***	-0.440***	-0.422***	-0.543***	-0.480***
	(-4.69)	(-5.66)	(-2.74)	(-3.31)	(-3.61)	(-3.93)	(-3.56)	(-4.40)	(-4.67)	(-5.65)
Year FE	✓	✓							✓	✓
Firm FE	✓	✓			✓	✓	✓	✓	✓	✓
Controls		✓		✓		✓		✓		✓
Year × Cohort FE			✓	✓						
Firm × Cohort FE			✓	✓						
SIC2×Year FE					✓	✓				
Incorp×Year FE							✓	✓		
N of Obs	69,960	69,960	308,010	308,010	51,159	51,159	49,061	49,061	50,748	50,748

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

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

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

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