Employee Non-Disclosure Agreements and Corporate News

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Abstract

This study examines whether weakening employee non-disclosure agreements (NDAs) affects the flow of information to capital markets via the business press. After state laws weakened NDAs related to misconduct, treated firms exhibit a significant increase in corporate news relative to control firms. The increase is driven by non-financial news, particularly articles about legal issues and corporate social responsibility, and is concentrated among firms with large workforces and high media visibility. We find that articles become significantly more negative in tone, and these articles generate stronger market reactions, indicating that employees increasingly share negative information with journalists that is informative to capital markets. Using textual analysis, we document direct evidence of increased interactions between employees and journalists, with more articles citing employees as sources, particularly in news about legal issues. Collectively, our findings illustrate that the business press serves as an important channel through which employee information reaches capital markets, and employment NDAs can impede this channel.

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1 Introduction

When the MeToo movement exposed long-buried stories of employee mistreatment, policymakers turned a critical eye on non-disclosure agreements (NDAs) in employment contracts (Steel, 2019).[1] In particular, attention focused on blanket NDAs, which are ostensibly meant to protect trade secrets but can restrict disclosure of any negative information about a firm, including sexual harassment, discrimination, or other misconduct (Lobel, 2016; Mendick, 2018a).[2] The MeToo movement highlighted that these NDAs could restrict employees from sharing their stories with the media, impeding the work of journalists and the revelation of negative corporate news (Jones and Hamrick, 2019; Mendick, 2018b). Concerned about the potential chilling effect of blanket NDAs, U.S. policymakers began to weaken the enforceability of NDAs at the state and federal levels (Ray, 2022). Advocates for these laws have argued that weakening NDAs will help both employees to disclose and journalists to gather information relevant to the public interest (Jones and Hamrick, 2019). Despite its practical importance, little is known about whether weakening NDAs affects the corporate news environment or whether resulting news is informative for capital markets.

We study whether firms experience an increase in corporate news after state laws weaken the enforceability of NDAs. Rank-and-file employees have private information about corporate performance and culture (Dyck et al., 2010; Huang et al., 2020; Dube and Zhu, 2021). After NDAs are weakened, employees may disclose more private information to journalists if the information was previously restricted by NDAs and if employees perceive a decrease in the net costs of disclosure. The first premise is plausible because estimates of NDA use are high, with approximately 57% of U.S. employees being bound by one (Balasubramanian et al., 2024), and publicly available NDAs often restrict disclosure broadly, covering employees' "general knowledge, skill, and experience" (Hrdy and Seaman, 2023, p. 678). It is also plausible that employees perceive a decrease in the net costs of speaking to journalists because the media and activists spotlighted the NDA debate when these laws were passed.[3] Journalists may have also increased their efforts to gather information from

[1] Throughout the paper, we use "NDA" to collectively refer to non-disclosure agreements, non-disparagement agreements, and confidentiality agreements, unless otherwise stated.

[2] As an example of this broad language, Lobel (2016) notes that an online template for NDAs defines "confidential information" as including "any other information not generally known to the public which, if misused or disclosed, could reasonably be expected to adversely affect Company's business" (p. 876). We provide additional background on blanket NDAs in Section 2.

[3] As an example, while California state legislators debated these bills, the Associated Press published an article stating that a proposed bill "would prohibit employers from requiring nondisclosure agreements related to sexual misconduct

employees, which could have further reduced the net costs of employee disclosure. Based on this reasoning, we hypothesize that after NDAs are weakened, corporate news increases. In particular, we predict that firms see an increase in articles written by the business press rather than by firms themselves (i.e., press releases).

However, there are a couple of reasons why weakening NDAs may not change the corporate news environment. Employees with relevant information may either not have been bound by NDAs or may have already disclosed through other channels. In this case, relevant information could enter the news environment and capital markets regardless of NDA enforceability. For example in 2017, an Amazon employee used Reddit to disclose discrimination, feeling comfortable with Reddit's anonymity rather than "go[ing] to the media... because they would have fired me for NDA agreements all employees sign when they get hired."[4] If similar disclosures were processed by the media and investors before the NDA laws, then we would not expect the corporate news environment to change ex post. Second, employees may have been unaware of the NDA laws or not perceived a change in net disclosure costs. Though firms could no longer legally enforce blanket NDAs, employees could still have perceived a high burden from preemptively hiring a lawyer or expecting informal retaliation. If either one of these reasons dominates, then a firm's information environment may not change after NDAs are weakened.

To study this question, we focus on state laws in California, New Jersey, and Illinois that were enacted between January 1, 2019 and January 1, 2020. These laws declared that employers could no longer enforce blanket NDAs that covered the disclosure of corporate misconduct (Johnson et al., 2019). We use a staggered difference-in-differences (DiD) design to examine whether corporate news increases for treated firms after the laws are passed. Firms are treated if they are headquartered in California, New Jersey, or Illinois, and control firms are headquartered in other states. Our sample period begins in 2018, which gives us one full year of pre-period data before the earliest law is enacted (California), and ends in 2021 to avoid commingling with later NDA legislation.[5] We obtain data from RavenPack to calculate the number of articles written by the business press about a firm in a given quarter, which is our main measure of corporate news.

as a condition of getting or keeping a job" (Associated Press, 2018). This article ran in multiple newspapers, suggesting it was widely read. One version can be accessed here: https://tinyurl.com/AssociatePress

[4]The full Reddit post can be accessed here: https://tinyurl.com/redditamzn

[5]In 2022, a flurry of other states and the federal government passed NDA laws related to sexual harassment (Martin et al., 2024).

In our primary analysis, we find that after NDAs are weakened, treated firms see a statistically significant increase in corporate news compared to control firms. Our empirical design includes fixed effects for a firm's headquarter state, industry-by-quarter, and stock exchange, allowing us to control for time-invariant state characteristics, industry trends, and stock market conditions that could influence a firm's information environment. Furthermore, we find no statistically significant difference in business-press coverage between treated and control firms prior to the NDA laws, suggesting that the change in corporate news is driven by the weakening of NDAs rather than by pre-existing trends.

Though we observe an increase in corporate news, an important feature of our setting is that the NDA laws weaken clauses covering misconduct, but do not apply to trade secrets. We exploit this distinction to tighten our prediction: we anticipate the increase in corporate news to be concentrated in non-financial news. To test this prediction, we classify business-press articles into non-financial or financial news based on RavenPack's topic-labeling system.[6] The increase in articles about treated firms indeed comes from non-financial news. Within non-financial topics, the increase comes from articles related to legal issues, corporate social responsibility, and crime. Notably, we do not observe any change in financial news. These findings are consistent with our hypothesis that after NDAs are weakened, employees increase their disclosure to the media, leading to more non-financial news.

We conduct two cross-sectional analyses on this main result. In the first test, we explore whether the result is driven by firms with more employees. At these firms, internal information is diffused among more people, meaning employees may be more likely to communicate with the media. This diffusion makes it more difficult for the firm to identify the source of a leak and retaliate (Dahl and Knepper, 2021). Any individual employee may have a higher perception of anonymity, lowering the net costs of disclosing to the media. In addition, this information diffusion means that journalists can harvest information from a larger pool of potential sources. To test this possibility, we divide the sample into firms with more or fewer employees. Firms with more employees drive the increase in non-financial news, consistent with the idea that after NDAs are weakened, the net costs of employee disclosure decrease.

In the second cross-sectional test, we explore whether more visible firms experience a stronger

[6]RavenPack labels articles with different topics. We define financial news by topics of earnings, revenues, dividends, costs, and credit. We define non-financial news by the topics of legal, corporate social responsibility, regulatory, crime, and others.

increase in non-financial news. Prior evidence documents a negativity bias in the demand for and the supply of news (e.g., Trussler and Soroka, 2014; Gaa, 2008). This negativity bias implies that journalists are more inclined to research and write negative articles about well-known firms (Gaa, 2008; Rees et al., 2015). We expect that after the NDA laws, journalists may target outreach towards employees of these types of firms. Consistent with our hypothesis, the increase in non-financial news comes exclusively from firms that historically received more media attention.

Next, we examine whether weakening NDAs affects the tone of corporate news. These laws target agreements that could suppress negative workplace information, leading us to predict that treated firms will have more negative corporate news after the legislation. To test this possibility, we classify articles as negative or positive based on their tone, as measured by RavenPack. After the NDA laws, we document that treated firms see an increase in the number of negative non-financial news relative to control firms, while there is no evidence of a change in positive non-financial news. This result supports our prediction that more negative news is produced about firms after employees and journalists can more easily communicate.

Having documented significant changes in the volume and tone of coverage, we next examine whether these articles convey novel and relevant information to capital markets. If employee information was already being transmitted to investors, then we would not expect articles after the NDA laws to be more informative. However, the business press can verify employee information, place it in a broader context, and disseminate it more effectively than other channels such as social media. We use an article-level analysis to study market reactions after articles are published, and we find that investors respond more strongly to articles about treated firms after the NDA laws. This heightened market sensitivity is driven by negative articles, suggesting that when NDAs no longer constrain employee-journalist communication, the resulting coverage contains novel and informative news to capital markets.

In our final set of analyses, we provide more direct evidence on the channel we have in mind. First, we examine whether journalists more frequently cite employees as sources in articles about treated firms. To measure this occurrence, we hand-collect the full text of articles in our sample between 2018 to 2021 and use textual analysis to identify articles where journalists cite conversations with employees or anonymous sources. After NDAs are weakened, treated firms experience a larger increase in employee-sourced articles than control firms, and this increase comes from articles about

legal issues, as labeled by RavenPack. This result is consistent with our hypothesis that weakening NDAs increases employee disclosure to journalists, though we caveat that we cannot observe the full set of interactions between the two groups. Second, because the NDA laws cover clauses about misconduct, we examine whether treated firms experience an increase in misconduct-related news. We identify articles with phrases related to regulatory investigations or lawsuits. After the NDA laws, more articles related to both types of misconduct are published about treated firms than control firms. Together, these analyses suggest that weakening NDAs encourages employees to disclose misconduct-related information to journalists.

By exploring the implications of NDAs for corporate news and capital markets, our study contributes to a rapidly evolving policy discussion. Though policymakers have taken steps to weaken NDA clauses covering misconduct, we have sparse evidence on the consequences of these laws.[7] The nascent literature on NDAs has documented that compared with noncompetes, NDAs are more widespread (Balasubramanian et al., 2024), carry broader restrictions (Hrdy and Seaman, 2023), and influence employees' views of their companies (Sockin et al., 2024). This early evidence focuses on labor markets, and we expand on it by drawing an intuitive link to the media and capital markets. We find that after NDAs are weakened, making it easier for employees and journalists to communicate, journalists write more negative non-financial articles about firms, and investors find these articles novel and useful for decision-making. By shedding light on a potential benefit of weakening NDAs, our results are relevant for policymakers.

More broadly, this paper bridges two streams of accounting literature on the business press and labor. Regarding the former, Miller and Skinner (2015) argue that our understanding of the media's role in financial markets lags behind our knowledge of other intermediaries, and they call for more research on how the media interacts with other market participants. Though others have studied the business press and short-sellers (e.g., Bushman and Pinto, 2022; Ahn et al., 2024) or analysts (e.g., Miller, 2006), we respond to this call by highlighting that employees can also play an important role in the media's production of corporate news. Our findings advance our understanding of how labor disclosure restrictions can influence the activity of the business press and, subsequently, the information environments of firms.

[7] Historically, research on employee restrictions has revolved around noncompete agreements. Prior work has studied the relation between noncompetes and firm disclosure (Aobdia, 2018), investment (Jeffers, 2024), insider trading (Gao et al., 2023), and earnings management (Tang et al., 2021).

In terms of the labor literature, we provide evidence that the media augments and disseminates the private information of employees. Prior work has used social media platforms, such as Glassdoor, to examine whether rank-and-file employees possess information about corporate activity rather than how this information reaches capital markets. For example, studies have found that employee ratings and business predictions are positively associated with future financial performance (Hales et al., 2018; Green et al., 2019; Huang et al., 2020) and CSR disclosure (Dube and Zhu, 2021). However, it appears that capital markets struggle to process employee reviews on social media (Green et al., 2019; Huang et al., 2020).[8] We add to these findings by providing insight into how employee information can be more easily processed by investors via the media channel. Through the work of journalism, the business press can solicit and verify employee information, use it to build a larger story, and disseminate the news broadly to capital markets.

Finally, our paper relates to work on employee whistleblowing because in both settings, employees reveal negative information to an external monitor. Research on whistleblowing has focused on employees' incentives for blowing the whistle to regulators (Bowen et al., 2010; Dyck et al., 2010; Call et al., 2016; Dey et al., 2021; Heese and Pérez-Cavazos, 2021; Leonelli, 2023). We complement this work in two ways. First, we examine the channel of employee communication with the media. Unlike whistleblowing to regulators, where enforcement can take years, using the media is a timely way to escalate concerns about misconduct (Dworkin and Callahan, 1992). Communicating with journalists can also be effective when regulators cannot devote enough resources to an employee's case or when an employee does not believe their experience is severe enough to warrant the regulator's attention. Second, most of the previous studies focus on financial fraud, where whistleblowers are incentivized by large monetary rewards from regulators. In our setting, legislation weakens NDAs with respect to areas of misconduct where regulators lack such large incentive tools. As a result, the media takes on a more significant role in our setting.

[8] For example, Huang et al. (2020) find that "investors inefficiently use Glassdoor predictions" (p. 201) and Green et al. (2019) argue that "the evidence of return predictability... is inconsistent with perfect market efficiency and instead points towards... the roles of costly information processing and investor inattention" (p. 238).

2 Background and hypothesis development

2.1 Institutional background

Employment NDAs in the United States originated in the 1940s and were initially used by technology firms to protect trade secrets (Dean, 2018). In the 1980s, NDAs spread to other areas of corporate America and began to broaden in scope to suppress negative information, both in contracts signed as a matter of course at the start of employment and as parts of legal settlements between employers and employees (Dean, 2018). Though cases arose over the enforceability of NDAs in the ensuing decades, these cases did not spark a wider public policy discussion.[9] Instead, more attention was paid to noncompete agreements, which were viewed as more controversial for explicitly limiting employee mobility (Hrdy and Seaman, 2023).

It took the MeToo movement for NDAs to take center stage in the public discourse. Since then, evidence has begun to emerge on the scope and prevalence of NDAs. Focusing on scope, Hrdy and Seaman (2023) collect employment contracts released through federal trade secrets litigation and describe two consistent patterns. First, NDAs are broad: while 77% of contracts explicitly cover trade secrets, 97% of contracts go further to cover all information deemed "confidential" by the employer (Hrdy and Seaman, 2023).[10] 40% of contracts do not carve out any exceptions to this umbrella, meaning that the contracts prohibit disclosure even if information is publicly known or part of an employee's general skills, knowledge, and experience. Second, NDAs are unlimited in duration or geography: almost all of the contracts last indefinitely and do not have geographic boundaries (Hrdy and Seaman, 2023). Both of these patterns imply that in practice, employers use blanket NDAs rather than NDAs that only cover trade secrets, and we present an example NDA that reflects these patterns in Appendix A. Focusing on NDA prevalence, Balasubramanian et al. (2024) gather evidence from surveys of employees and firms. The authors conclude that 57% of U.S. employees are bound by NDAs, and NDAs are the most commonly reported employee

[9] As Short (1999) describes, one such case was between Brown & Williamson, a tobacco company, and Jeffrey Wigand, a former employee. Wigand signed NDAs with the firm as a condition of employment, but after being fired in 1992, he began a public crusade to reveal the health risks of cigarettes, including filming an interview for CBS' *60 Minutes*. Based on the NDAs, Brown & Williamson successfully sued Wigand to prevent him from "disclosing any information about his experiences" (Short, 1999, p. 1211) at the firm, and the *60 Minutes* interview was quashed (Dean, 2018).

[10] In one example, "Confidential/Proprietary Information shall mean trade secrets, confidential or proprietary information, and **all other knowledge, information, documents, and materials owned, developed, or possessed by Employee**" (Hrdy and Seaman, 2023, p. 734) (emphasis added). This contract comes from: Confidentiality and Non-Competition Agreement §1.1, USG Ins. Servs. v. Bacon, No. 16-CV-1024 (W.D. Pa. July 11, 2016).

restriction. By contrast, noncompetes are reported least commonly, binding an estimated 22% of employees (Balasubramanian et al., 2024). This combined descriptive evidence indicates that NDAs are pervasive and sweeping in scope.

Amidst the backdrop of the MeToo movement, California, New Jersey, and Illinois acted earliest to pass legislation weakening the enforceability of blanket NDAs. In California, two NDA-related bills were signed into law in September 2018 and became effective on January 1, 2019 (McInerney, 2018). Senate Bill (SB) 1300, the broader bill, addresses NDAs signed as a condition of initial or continued employment; it prohibits these employment NDAs from covering any unlawful or potentially unlawful act in the workplace (California State Legislature, 2018a).[11] SB 820 more narrowly prohibits settlement agreement NDAs covering sexual assault, sexual harassment, or sex discrimination (California State Legislature, 2018b).[12] In effect, starting on January 1, 2019, employees could not be required to sign NDAs covering corporate misconduct as a condition of employment or to sign NDAs covering sexual misconduct as part of a settlement agreement. We present an example NDA that reflects the California provisions in Appendix A.

New Jersey was the second state to enact major laws weakening blanket NDAs. Senate Bill (S) 121 was signed into law on March 18, 2019 and became effective immediately (New Jersey Legislature, 2019). Similar to the combined effect of California's two bills, S121 addresses NDAs in employment contracts and in employment-related settlement agreements.[13] However, unlike California's SB 1300, which covers all potential unlawful conduct, New Jersey's S121 is narrower in scope because it only addresses NDAs covering discrimination, retaliation, or harassment.[14]

Lastly, in August 2019, Illinois' Workplace Transparency Act (WTA) was signed into law and became effective on January 1, 2020 (Ogletree Deakins, 2020). Compared to the California and New

[11] The specific text reads that it is unlawful for "an employer to require an employee to sign a **nondisparagement agreement** or **other document** that purports to **deny the employee the right to disclose information about unlawful acts in the workplace**, including, but not limited to, sexual harassment... For purposes of this paragraph, "information about unlawful acts in the workplace" **includes, but is not limited to,** information pertaining to **sexual harassment or any other unlawful or potentially unlawful conduct**" (emphasis added). The bill can be accessed here: https://tinyurl.com/CAbill

[12] Note that SB 820 is not retroactive. While the bill references prior legislation from January 1, 2017, the effective date of SB 820's provisions was January 1, 2019. The bill can be accessed here: https://tinyurl.com/casb820

[13] Note that California abbreviates "Senate Bill" as "SB," and New Jersey abbreviates it as "S."

[14] The specific text reads, "A provision in any **employment contract** or **settlement agreement** which has the purpose or effect of **concealing the details** relating to a claim of **discrimination, retaliation, or harassment** (hereinafter referred to as a "non-disclosure provision") shall be deemed against public policy and **unenforceable** against a **current or former employee**... who is a party to the contract or settlement" (emphasis added). The bill can be accessed here: https://tinyurl.com/NJbill

Jersey bills, the WTA's NDA provisions are narrower in scope for a few reasons. First, NDA provisions apply to employment contracts, but not to settlement agreements (Illinois General Assembly, 2020). Second, the WTA only weakens NDA provisions that cover discrimination, harassment, or retaliation, which is similar to New Jersey's S121 but narrower than California's SB 1300.[15] Third, the WTA only prohibits these employment NDAs when they are "unilateral conditions" of employment – that is, when the NDA is a non-negotiable condition that an employee takes as given. However, the WTA allows NDAs that cover discrimination, harassment, or retaliation if the contract is negotiated bilaterally between the employee and employers, which is prohibited in California and New Jersey (Ogletree Deakins, 2020).

We note two additional points about the landscape of these bills. First, none of these laws affect NDA provisions that explicitly protect trade secrets. For example, S121 notes that the law does not apply to NDAs where "the employee agrees not to disclose proprietary information, which includes only non-public trade secrets, business plan and customer information" (New Jersey Legislature, 2019). Second, while other states passed NDA-related laws following the MeToo movement, the California, New Jersey, and Illinois bills were among the earliest that were relatively broad in scope, going beyond sexual harassment alone.[16] As a result, we focus on these three states to study whether weakening NDAs affects the information environments of firms.[17]

2.2 Hypothesis development

Conceptually, the presence of NDAs can affect employee disclosure to the media through the costs and benefits of employee disclosure and journalist information-gathering. Employees possess private information about corporate performance, workplace conditions, and potential misconduct (e.g., Dyck et al., 2010; Huang et al., 2020; Sockin et al., 2024). When deciding whether to share

[15] The specific text reads, "Any agreement, clause, covenant, or waiver that is a **unilateral condition** of **employment or continued employment** and has the purpose or effect of **preventing** an employee or prospective employee from making **truthful statements or disclosures** about **alleged unlawful employment practices** is against public policy" (emphasis added). Per the bill, "unlawful employment practices" is defined as "any form of **discrimination, harassment, or retaliation**" (Section 1-15). The bill can be accessed here: https://tinyurl.com/ILbill.

[16] For example, while Tennessee and Vermont also passed bills prohibiting employment NDAs in 2018, these only applied to sexual harassment (Johnson et al., 2019). See Johnson et al. (2019) for more detail on other state laws.

[17] We end our sample before 2022 to avoid a cluster of other state laws and the federal Speak Out Act (see Section 3 for more discussion of the design). The Speak Out Act was signed into law in December 2022 and makes unenforceable employment NDAs related to sexual harassment and sexual assault (Lalik et al., 2022). As an example of a later state law, California enacted the Silenced No More Act (SB 331) in January 2022, which prohibits NDAs in settlement agreements related to any harassment or discrimination, broadening SB 820, and prohibits NDAs in severance agreements related to any unlawful conduct (California State Legislature, 2022).

information about the firm that may be considered negative, employees weigh the costs and benefits (Dahl and Knepper, 2021). The benefits could include addressing the concerning issues, while the costs could involve retaliation, such as termination of employment. NDAs can complicate this decision-making process in two ways. First, they decrease disclosure benefits by making misconduct improvement less likely. Second, they simultaneously increase the costs of disclosure by exacerbating the possibility of retaliation and career concerns (e.g., Boone and van Ours, 2006; Dyck et al., 2010; Dahl and Knepper, 2021). On the whole, NDAs increase the net costs of sharing information with the media. Anecdotally, employees express fear of breaching NDAs in comments such as, "I couldn't go to the media either because they would have fired me for NDA agreements all employees sign when they get hired."[18]

Journalists concur that NDAs increase the net cost of employee disclosure, and this wariness raises their net cost of gathering information (Dean, 2018). At the outset, journalists face an ethical struggle over persuading a potential source to break an NDA.[19] In addition, when employees are less forthcoming, it is more difficult for journalists to gather, verify, and contextualize information. Finally, when journalists publish stories where employees breach NDAs, media outlets themselves can face litigation from employers, which consumes time and resources (Short, 1999; Jones and Hamrick, 2019). For these reasons, when NDAs increase employees' net cost of disclosing to journalists, NDAs also "restrict journalists' ability to gather news" (Jones and Hamrick, 2019).

We posit that after state laws weaken NDAs, the net costs of both employee disclosure and journalist information-gathering decrease. In response, we predict that employees and journalists may share information more readily, enriching the corporate news environment.[20] Because journalists verify employee information and use it to craft a broader story about a firm, we further predict that the resulting news will be relevant and novel to capital markets. As an example of this channel, the *Wall Street Journal* published an article in September 2020 about Amazon where the journalists

[18] This comment on Reddit can be accessed at this link: https://tinyurl.com/redditamzn. In another Reddit post, an employee discusses being fired for allegedly breaching an NDA by making a Facebook comment that could have cast the employer in a negative light; the link can be accessed here: https://tinyurl.com/redditndafire.

[19] As the lawyer who represented Gretchen Carlson against Fox News commented, "I think journalists should not take this lightly... If you persuade a lay person to breach a confidentiality agreement, you're putting them in grave financial danger" (Dean, 2018).

[20] This prediction is also consistent with research showing how the business press processes and disseminates qualitative information that market participants find valuable (Bradshaw et al., 2021) and influences firms' overall information environments (Lock, 2024)

explicitly cite Amazon employees and reveal knowledge of internal emails (Mattioli et al., 2020).[21]

However, our prediction is not obvious ex ante. It requires that certain employee information must have been restricted by NDAs before the laws. Despite the estimated prevalence of NDAs, employees may have shared information through channels they perceived to be anonymous and safe from retaliation, such as Glassdoor or Reddit. If these forms of employee information seeped into corporate news before the NDA laws, then we may not observe a change ex post. Furthermore, our prediction depends on employees perceiving a decrease in their net costs of disclosure. The NDA laws were discussed in the media, and employers may have carved out exceptions for misconduct in existing NDAs, which could have raised employee awareness (Tippett, 2018). However, employees awareness may not have increased or employees may have felt that other costs of disclosure, such as hiring legal counsel, remained too high. Thus, it is an empirical question whether weakening NDAs enriches the corporate news environment.

3 Empirical strategy

3.1 Design and data

We examine the change in corporate news after the California, New Jersey, and Illinois bills were enacted on January 1, 2019, March 18, 2019, and January 1, 2020, respectively. We use a staggered difference-in-differences design around these three events, arguing that these bills weaken NDAs for firms headquartered in those states, relative to firms headquartered in other states. Our sample period begins in January 2018, meaning that the pre-period for an event spans from January 2018 until the law is enacted. We end the sample period at the end of 2021 to better isolate changes of the CA, NJ, and IL laws from narrower NDA laws passed in 2022.

We source corporate news from RavenPack News Analytics, a leading provider of media data about firms (e.g., Miller, 2006; Bushman and Pinto, 2022; Holstead et al., 2021). One advantage of RavenPack is its ability to link news to U.S. public companies, classify the news based on its content, and identify the tone of each article. We also use information from the SEC Analytics Suite accessed via WRDS to identify firms' headquarter states, and we use quarterly and annual financial data from Compustat for control variables in our later analyses. To be included in our

[21] Appendix C provides extracts from this article and other anecdotes of our proposed channel.

sample, firms must have accounting data in Compustat, headquarters data, and RavenPack data.

We measure corporate news by the number of articles written by the business press about a firm in a quarter. Following prior studies, we remove news articles composed of only a headline (i.e., news flashes) and articles with only a headline and mainly tables (i.e., tabular material news) (e.g., Bushman and Pinto, 2022; Bushman et al., 2017). To ensure that the news articles are related to the firm, we further restrict the sample to articles with a relevance score of 75 and above.[22] To identify novel news, we follow prior studies and keep only articles with an event novelty score (ENS) of 100. This filter aims to identify articles that are more likely to contain new information about the firm instead of a repeat or discussion of previous news. Finally, as recommended by RavenPack, we also remove news about technical analysis and insider trading.[23] After applying these filters, we aggregate the total number of business-press articles for a firm in each quarter. The final sample contains 2,293 unique firms, totaling 31,567 firm-quarter observations from 2018 to 2021.

3.2 Descriptive statistics

Table 1 Panel A presents the summary statistics for our sample, with observations at the firm-quarter level. The average firm in our sample has 3.9 articles written by the business press in a quarter (*CorpNews*), and the median firm has 2.0 business-press articles. There is significant variation in the number of articles for firms, as evidenced by the standard error of 5.9. The average firm in our sample is slightly unprofitable (mean ROA = -0.003), but the median firm is profitable (median ROA = 0.005). The median firm in our sample reports no R&D (median R&D = 0), but there is substantial variation, as evidenced by the high standard error and mean of R&D. The average firm in our sample is large, where the logarithm of the firm's total assets is 7.689.

[Insert Table 1 around Here]

Table 1 Panel B presents the correlation matrix for our key variables. The correlation between *CorpNews* and *Post_Law* is positive and statistically significant ($\rho = 0.060$, p-value < 0.01). At first glance, this correlation suggests that after the NDA laws, treated firms are the subjects of

[22] As described by RavenPack and Bushman and Pinto (2022), RavenPack assigns a relevance score to indicate how strongly a firm is featured in the underlying news story. The relevance score ranges from 0 (low relevance) to 100 (high relevance), with scores above 75 signifying that the article is relevant for a firm.

[23] For other suggestions on how to work with RavenPack, please see: https://tinyurl.com/wrdsrvpk

more business-press articles than control firms. We examine this association more rigorously in a multiple regression model that accounts for firms' financial characteristics and geography.

4 Results

4.1 NDA laws and corporate news

To test whether weakening NDAs affects corporate news, we estimate the following linear regression model, where i denotes the firm, j denotes the industry, s denotes the state, t denotes the quarter, and k denotes the stock exchange:

$$CorpNews_{it} = \beta_0 + \beta_1 Post\_Law_{it} + \beta Controls_{it} + \delta_s + \tau_{jt} + \gamma_k + \epsilon_{it} \qquad (1)$$

$CorpNews$ is the total number of articles written by the business press about a firm in a given quarter. The main coefficient of interest is β_1 on $Post\_Law$, which is an indicator variable that equals 1 for firms headquartered in California, New Jersey, and Illinois in quarters after each NDA law goes into effect, and 0 otherwise.[24] If more business-press articles are written about treated firms than control firms after each state passes its NDA law, then we would expect β_1 to be positive.

We include a range of fixed effects to control for potential determinants of corporate news. Specifically, we include fixed effects for a firm's headquarter state, industry-by-quarter, and stock exchange. These fixed effects aim to control for time-invariant state conditions, changes in industry outlook, and stock market conditions that may influence corporate news. In addition, we control for time-varying financial characteristics: size, profitability, leverage, research and development expense, whether a firm has missing R&D data, and whether a firm is loss-making. We define all variables in Appendix D. Because our treatment occurs at the state level, we cluster standard errors by state (Abadie et al., 2023).

Table 2 presents the results of estimating Equation (1). For completeness, we first present the results from estimating Equation (1) without the financial controls in Column (1); in Column (2), we present our main specification in full. Across both models, the coefficient on *Post_Law*

[24] For instance, California's laws went into effect on January 1, 2019, so *Post_Law* equals 1 for California firms starting in 2019Q1. *Post_Law* equals 0 for California firms up through 2018Q4 and equals 0 for non-California firms during all quarters. For New Jersey, where S121 went into effect on March 18, 2019, *Post_Law* equals 1 for New Jersey firms starting in 2019Q2, and 0 otherwise. For Illinois, where the WTA went into effect on January 1, 2020, *Post_Law* equals 1 for Illinois firms starting in 2020Q1, and 0 otherwise.

is positive and statistically significant ($\beta = 0.243$, p-value < 0.05 in Column (2)). When we incorporate time-varying financial controls, the coefficient remains similar and the adjusted R^2 doubles, indicating that our result is less vulnerable to correlated omitted variables. The estimated magnitude represents 4.6% of the within-standard deviation in business-press articles, indicating an economically meaningful increase in news coverage.[25] This result suggests that after NDAs are weakened, journalists produce more information about treated firms.[26]

[Insert Table 2 around Here]

One potential concern is that firms headquartered in CA, NJ, and IL may have had different trends in corporate news than control firms, regardless of the NDA laws. Our identifying assumption is that after accounting for fixed effects and control variables, treated and control firms had similar trends leading up to the NDA laws. We examine this parallel trends assumption in Figure 1, where we plot the coefficients from regressing the number of business-press articles on event-time indicators of *Post_Law*. In the four quarters leading up to each state's NDA law, there is not a statistically distinguishable difference between the number of business-press articles for treated and control firms. However, in the first two quarters after the laws are enacted, we observe that treated firms have a statistically larger increase in articles. This figure reassures us that our primary finding is likely driven by the NDA laws rather than by other trends in corporate news.

[Insert Figure 1 around Here]

The laws weaken NDAs related to potential misconduct but not proprietary financial information. This feature of the setting sharpens our prediction, leading us to expect journalists to publish more non-financial articles about treated firms. If we find that the increase is isolated in non-financial news, then we would also be less concerned that our results could be driven by changes in economic conditions, which would be more likely to influence financial news.

We leverage RavenPack's article classification system to separate financial from non-financial news. Financial articles have labels of earnings, revenues, dividends, costs, and credit. For non-

[25] The within-standard deviation for non-financial news is 5.33. 4.6% is calculated as 0.243/5.33.

[26] We also estimate the same model for press releases published by firms and find no significant change. The results are tabulated in the online appendix, Table A.1

financial articles, we group RavenPack's topics into nine categories: corporate social responsibility, legal, regulatory, labor, crime, civil unrest, industry accidents, equity actions, and others.[27]

[Insert Table 3 around here]

In Table 3 Panel A, we estimate Equation (1) where *CorpNews* is *Nonfinancial* or *Financial* news. As presented in Column (1), the coefficient on *Post_Law* is positive and statistically significant for non-financial news ($\beta = 0.275$, p-value < 0.05). The magnitude represents 6.2% of the within-standard deviation in non-financial news, indicating that the change in non-financial news is economically meaningful.[28] In contrast, we find no significant change in financial news coverage.

We further examine which RavenPack topics explain the increase in non-financial news for treated firms by estimating Equation (1) for each topic within non-financial news. Table 3 Panel B summarizes the results. The coefficients of interest are positive and statistically significant for "corporate social responsibility," "legal," and "crime." Across Table 3, the results demonstrate that journalists publish more non-financial articles about treated firms than control firms after NDAs are weakened while there is no discernible difference in financial articles. These results support our hypothesis that the NDA laws encourage the production of non-financial corporate news.

4.2 Cross-sectional analyses

Having established that weakening NDAs increases corporate news, we examine whether our results are amplified within two types of firms where we would expect to observe a stronger association: firms with more employees or firms with higher media visibility.

[Insert Table 4 around here]

In the first test, we explore whether firms with more employees drive the increase in non-financial news. When a firm has more employees, internal information is dispersed among more people. This dispersion has two implications: if an employee leaks information about the firm, then the dispersion makes it more difficult for the firm to identify the employee and potentially retaliate

[27] We aggregate similar topic labels into broader categories because many topics are sparsely populated. Note that "others" includes the topic labels of marketing, partnerships, products-services, war-conflicts, security, indexes, bankruptcy, and acquisitions-mergers.

[28] The within-standard deviation for non-financial news is 4.43. 6.2% is calculated as 0.275/4.43.

(Dahl and Knepper, 2021). If an employee believes they are more likely to remain anonymous, then the NDA laws will further lower their net costs of disclosing to the media. In addition, when information is dispersed among more employees, journalists can gather information from more potential sources. We divide the sample into firms with more or fewer employees around the median and re-estimate Equation (1) on each group. Table 4 Column (1) presents the results. The coefficient on *Post_Law_HighEmployee* is positive and statistically significant ($\beta = 0.558$, p-value < 0.01), while the coefficient on *Post_Law_LowEmployee* is smaller in magnitude and not statistically significant. This result indicates that among treated firms, it is the firms with more employees that drive the increase in non-financial news after the NDA laws.

In the second test, we examine whether more visible firms drive the increase in non-financial news. Prior evidence documents a negativity bias in consumers' demand for and the media's supply of news (e.g., Trussler and Soroka, 2014; Gaa, 2008). Prior work in journalism also suggests that the media writes more articles about firms that are larger and more well-known (Moon and Hyun, 2014). These pieces imply that journalists are more inclined to research and write negative articles about more visible firms (Gaa, 2008; Rees et al., 2015). We test whether more visible firms contribute more to our main results by constructing a measure of visibility based on a firm's pre-period number of media articles. We split the sample based on having above-median or below-median visibility. Table 4 Column (2) presents the results of regressing non-financial news on the interaction of *Post_Law_HighVisibility*. The coefficient of interest is positive and statistically significant ($\beta = 0.303$, p-value < 0.05), while the coefficient on *Post_Law_LowVisibility* is negative, though not statistically distinguishable from zero. Consistent with our hypothesis, the increase in non-financial news comes exclusively from treated firms that historically receive more media attention.

4.3 Article tone

If NDAs restricted employee-journalist interactions in the pre-period, then we would expect post-period news to be more negative in tone. To test this prediction, we classify non-financial articles as positive or negative based on RavenPack's sentiment measures. We estimate Equation (1) with these tone-based measures as the dependent variables.

[Insert Table 5 around here]

Table 5 presents the results. In Column (1), *Pos–Neg* equals the number of positive articles minus the number of negative articles in a firm-quarter, divided by the total number of non-financial articles in a firm-quarter. The coefficient on *Post_Law* is negative and statistically significant (β = -0.010, p-value < 0.10), indicating that after NDAs are weakened, treated firms experience a shift towards negative non-financial news. In Columns (2) and (3), we separately present the results for *% Positive* and *% Negative*, which equal the proportions of positive or negative non-financial news in a firm-quarter. There is no statistically distinguishable change in the proportion of positive articles, but the proportion of negative articles increases markedly ($\beta = 0.010$, p-value < 0.05). These results support the hypothesis that the NDA laws encourage employees to disclose negative corporate news to journalists.

4.4 Article informativeness

A natural question is whether this resulting news is informative to investors.[29] To test for potential changes in article informativeness, we examine market reactions to articles' event sentiment scores (ESS), a measure of tone from RavenPack. We perform this analysis at the article-level, which allows us to control for article characteristics that could influence informativeness, such as the article's topic. We estimate the following equation for article a about firm i in quarter t:

$$CAR[0,\ 1]_a = \beta_0 + \beta_1 Post\_Law_{it} \times ESS_a + \beta_2 Post\_Law_{it} + \beta_3 ESS_a + \beta Controls + \epsilon_a \quad (2)$$

CAR[0, 1] is the cumulative abnormal return in a [0, 1] window around article publication dates. *ESS* ranges from -1 (most negative) to +1 (most positive). The coefficient of β_1 on *Post_Law* $\times$ *ESS* captures the differential market response to article sentiment after the NDA laws. Controls include firm fixed effects, date $\times$ topic fixed effects, and industry $\times$ topic fixed effects. These fixed effects aim to control for time-invariant firm characteristics, trends influencing investor response about a topic (e.g., "labor issues") on a particular date, and trends influencing investor response about a topic published about a particular industry. We cluster standard errors by date.

[Insert Table 6 around here]

[29] It is possible that investors already accessed this information because employees could have shared their experiences on social media anonymously. If so, then we would not expect to see the market react to the increase in negative news articles. However, prior work finds that investors struggle to process employee views on social media (Green et al., 2019; Huang et al., 2020). Because the business press verifies and contextualizes information, related news articles could still be informative to investors.

Table 6 presents our market reaction analysis. In Column (1), the coefficient on *Post_Law* × *ESS* is positive and significant ($\beta = 0.010$, p-value < 0.01), indicating that the association between article tone and stock market reaction strengthens for treated firms after the NDA laws. Column (2) decomposes this result into negative and positive tone. We split *ESS* into *ESS_Negative* and *ESS_Positive*, which are continuous variables that range from zero to one. For *ESS_Negative* (*ESS_Positive*), higher values reflect more negative (positive) sentiment scores. The coefficient on *Post_Law* × *ESS_Negative* is negative and significant ($\beta = -0.025$, p-value < 0.01), while the coefficient on *Post_Law* × *ESS_Positive* is statistically indistinguishable from zero. These results indicate that negative articles become more strongly associated with stock market reactions for treated firms in the post-period. Because investors respond more strongly to negative news than to positive news, the NDA laws appear to improve investors' access to and processing of negative corporate information.

4.5 More direct evidence on the channel

Our mosaic of results suggests that the NDA laws decrease employees' net costs of disclosing to journalists. Ideally, we would like to examine whether direct interactions between employees and journalists increase. Though these interactions are often unobservable, we develop a proxy based on the textual analysis of articles.

To complement the article headlines provided by RavenPack, we hand-collect the full text of business-press articles for firms in our sample from 2018 to 2021. We search these articles for phrases that indicate journalists spoke directly with employees or anonymous sources. When journalists explicitly reference conversations with employees, we can be confident that they have communicated with employees. More commonly, though, articles cite anonymous sources with attributions such as, "people familiar with the matter." When journalists reference these anonymous sources around corporate news, context suggests the source could be an employee, but the source's affiliation is unobservable. We define "employee-sourced articles" as articles that either cite anonymous or employee sources.[30] If the NDA laws encourage employees and journalists to share information, then we would anticipate that employee-sourced articles would increase after the laws.

We test whether treated firms have a larger increase in employee-sourced articles than con-

[30] See Appendix C for the full list of phrases that we search for within articles.

trol firms after the NDA laws are passed. We estimate Equation (1) with the outcome variable of *Employee-sourced Articles*, which is the number of employee-sourced articles about a firm in a given quarter. Similar to our primary analysis, the specification includes fixed effects for a firm's headquarter state, industry-by-quarter, and stock exchange to control for time-invariant state characteristics, industry trends, and time-invariant stock exchange conditions. Table 7 Panel A presents the results without time-varying financial controls in Column (1) and with them in Column (2). In both columns, the coefficient of interest on *Post_Law* is positive and statistically significant ($\beta = 0.015$, p-value < 0.05). This result illustrates that after NDAs are weakened, journalists write more articles about treated firms than control firms that rely on information from employees or anonymous sources.

To examine whether this result varies across article topics, we count the number of employee-sourced articles in a firm-quarter that fall under RavenPack's topics of legal, regulatory, or labor issues. Panel B presents the results where the outcome variable is the number of *Employee-sourced Articles* in the legal, regulatory, or labor topics in a firm-quarter. In Column (1), within the legal topic, the coefficient on *Post_Law* is 0.029 and statistically significant at the 5% level, indicating that employee-sourced articles particularly increase for treated firms when the news is about legal issues. In Columns (2) and (3), the coefficients on *Post_Law* are positive but not statistically significant for regulatory or labor issues. We believe these tests provide more direct evidence that our main result reflects an increase in employee disclosure to journalists, although we reiterate our caveat that most interactions between employees and journalists are unobservable.

[Insert Table 7 around here]

One might be concerned that RavenPack's topic labels for articles are coarse relative to the scope of the NDA laws. While the laws weaken NDAs related to potential misconduct, observing an increase in certain RavenPack topics does not necessarily imply that journalists are writing more articles about misconduct. We seek to assuage this concern by using textual analysis to search for misconduct-related keywords within the full text of articles. We count the number of articles in a firm-quarter that are related to misconduct (*Misconduct*) or unrelated to misconduct (*Non-Misconduct*). Within misconduct-related articles, we classify articles as explicitly related to regulatory issues or other lawsuit issues. For example, *Regulatory* articles have strings such as,

"regulator," "federal," or "department" within 200 words of strings such as, "accuse," "allege," or "investigate," and *Lawsuit* articles have "lawsuit" or "lawsuits" within 200 words of strings such as, "allege," or "whistleblower."[31]

[Insert Table 8 around here]

In Table 8, we regress these counts of *Misconduct*, *Regulatory*, *Lawsuit*, and *Non-Misconduct* articles on *Post_Law*, with the same financial controls and fixed-effect structure as in Equation (1). We find that for *Misconduct* in Column (1), the coefficient on *Post_Law* is 0.012 and statistically significant at the 10% level. Thus, the NDA laws are positively associated with an increase in general misconduct-related articles. Within *Misconduct*, the NDA laws are also positively associated with *Regulatory* articles ($\beta = 0.008$, p-value < 0.10) and *Lawsuit* articles ($\beta = 0.012$, p-value < 0.05). These results suggest that because of the NDA laws, more misconduct-related news was revealed. However, we hesitate to overinterpret this result because our textual analysis is relatively conservative. Overall, though it is difficult to observe interactions between journalists and employees, our findings are consistent with weakened NDAs leading to more employee-sourced articles and more news about misconduct.

5 Robustness tests

We perform a number of additional tests to assess the validity of our main findings. In our primary analyses, our main dependent variable is the number of business-press articles written about a firm, which is a count variable. One challenge of working with right-skewed count data is that traditional OLS regressions are inefficient (Cohn et al., 2022). To mitigate concerns about our main dependent variable, we estimate two alternative specifications: one where we replace *CorpNews* in Equation (1) with an indicator variable, and one where we use a Poisson regression with our original dependent variable of *CorpNews*.

[Insert Table 9 around here]

Table 9 Panel A presents the results of these tests, which are consistent with our primary analysis. In Column (1), we replace *CorpNews* with *I(High News)*, which equals one for firms with above-

[31] See Appendix C for more details on this classification process.

median article counts. The coefficient on *Post_Law* remains positive and statistically significant, indicating a 2 percentage point increase in the probability of high news coverage. In Column (2), we employ a Poisson estimator, which better accommodates the distributional properties of *CorpNews* (Cohn et al., 2022). The coefficient of *Post_Law* remains positive and statistically significant, providing additional support for our main findings. These alternative specifications confirm that the increase in news coverage following the enactment of the NDA laws is not a spurious outcome of our main specification choice.

Our second set of tests addresses recent concerns about two-way fixed effects models with staggered treatment adoption (e.g., Baker et al., 2022; Barrios, 2024). One key concern with these models is the potential for negative weighting when estimating heterogeneous treatment effects. As Baker et al. (2022) highlight, this concern is often present in settings where treatment is staggered over long periods of time. First, we note that the treatments included in our study are close to each other but staggered. Second, we try to mitigate the negative weighting concern by conducting our analyses without including time-varying covariates, and we find similar inferences. To further address these concerns, we estimate stacked regressions (Barrios, 2024). Table 9 Panel B presents the results. In Column (1), we estimate a stacked regression where *I(High News)* is the dependent variable and where we include financial controls. In Column (2), we estimate a stacked regression where *CorpNews* is the dependent variable and where we do not include financial controls. Consistent with our main results, we find a positive and statistically significant association between the NDA laws and corporate news in both columns.

In the online appendix, we present the results of several additional tests to assess the validity of our inferences. In Table A.1, we examine firm-initiated news (i.e., press releases), where we would not expect to observe a change after NDAs are weakened. As expected, the coefficients on *Post_Law* for firm-initiated news are not statistically significant. In Table A.2, we repeat our primary analyses by using entropy-balanced weighting. This analysis allows us to ensure that treatment and control groups have a similar distribution (first and second moment) of observable variables. We match firms based on ROA, size, R&D, and leverage. We continue to find that the business press writes more non-financial articles about treated firms than control firms after the NDA laws.

Second, we examine whether our findings are sensitive to assumptions about error correlation structures. Table A.3 presents results with standard errors clustered by firm or two-way clustered

by state and industry. Firm-level clustering accounts for potential serial correlation in a firm's news coverage over time, while two-way clustering by state and industry allows for correlation both within states (where treatment occurs) and within industries (where news coverage patterns may be similar). The statistical significance of our findings remains unchanged across these alternative approaches, suggesting our inferences are not driven by specific assumptions about error dependence.

Third, we investigate treatment effect heterogeneity across states. Specifically, we divide *Post_Law* into three variables: *Post_CA*, *Post_IL*, and *Post_NJ*, which are indicator variables capturing the period after the enactment of each specific state law. This decomposition reveals whether our main result is driven by a single influential state. In Table A.4, we find positive and statistically significant associations across all three states, reassuring us that no one state explains the results.

In untabulated analyses, we examine the sensitivity of our results to control group composition. We restrict the control group to states that share borders with California, New Jersey, or Illinois. This neighboring-state specification addresses concerns that regional patterns in economic conditions or media coverage might explain our results. Second, we limit the control group to states that consistently voted for the Democratic candidate in presidential elections during our sample period because these states are more similar to our treatment states in terms of the political environment that enabled NDA legislation. Our main findings remain robust to both alternative control groups, suggesting that neither geographic proximity nor state-level political factors explain the observed increase in media coverage.

6 Conclusion

In the wake of the MeToo movement, policymakers began to pass laws making employment NDAs unenforceable with respect to sexual harassment, discrimination, retaliation, and other potential corporate misconduct. This policy shift provides a valuable setting to examine how NDAs influence the flow of information from employees to capital markets via the business press. Using a staggered difference-in-differences design around state-level NDA laws, we document that treated firms experience a significant increase in media coverage post-legislation. Importantly, this increase is concentrated in non-financial news, particularly articles about legal issues and corporate social responsibility. Through textual analysis of hand-collected articles, we find direct evidence that after

NDAs are weakened, articles about treated firms more frequently cite employees as sources and contain more references to potential misconduct.

Two additional findings underscore the economic significance of these legislative changes. When NDAs are weakened, articles become more negative, and this negative news spurs stronger market response. These results suggest that the additional information produced by employees and journalists is both novel and informative to investors.

Our findings contribute to the ongoing policy debate surrounding NDA restrictions. While NDAs serve legitimate purposes in protecting trade secrets, our evidence suggests they can simultaneously impede the transmission of negative news to capital markets. Understanding these economic trade-offs is increasingly relevant for policymakers concerned about the potential chilling effect of blanket NDAs on the production of corporate news.

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Appendix A – Samples of NDAs

Example 1 – Employment Agreement (2015):

Non-Disparagement. During and after any employment with the company, regardless of how, when or why such employment ends, (a) you shall not make, either directly or by or through another person, any oral or written negative, disparaging or adverse statements or representations of or concerning the company or its subsidiaries or affiliates, any of their clients or businesses or any of their current or former officers, directors, employees or shareholders and (b) Company Parties (as defined below) shall not make, either directly or by or through another person, any oral or written negative, disparaging or adverse statements or representations of or concerning you (…)

Source: https://www.sec.gov/Archives/edgar/data/890491/000095015715000744/ex10-1.htm

Example 2* – Employment Agreement (2019)

* Company headquartered in CA (after state law)

Non-Disparagement: (Section 8) The Executive agrees and covenants that he will not at any time make, publish or communicate to any person or entity or in any public forum any defamatory or disparaging remarks, comments, or statements concerning the company or its businesses, or any of its employees, directors, officers, customers, suppliers, investors and other associated third parties.

This Section 8 does not, in any way, restrict or impede the Executive from exercising protected rights to the extent that such rights cannot be waived by agreement or from complying with any applicable law or regulation or a valid order of a court of competent jurisdiction or an authorized government agency, provided that such compliance does not exceed that required by the law, regulation, or order. The Executive shall promptly provide written notice of any such order to the Board.

Source: https://www.sec.gov/Archives/edgar/data/1772177/000119312519212992/d771260dex103.htm

Appendix B – Employee Stories of NDA Use

Example 1

Perkins, who worked for Weinstein as an assistant in the 1990s, signed a nondisclosure agreement in 1998 after her colleague Rowena Chiu alleged that Weinstein had attempted to rape her.

The NDA legally prohibited her from speaking to anyone about the incident — including Chiu (…)

"[I broke my NDA] Because I discovered through Ronan Farrow that there were several rape allegations being levelled against Harvey Weinstein."

"We were not allowed to speak to anybody. No friends, no family, husband, boyfriend, parents."

Source: Link

Example 2

For several years I watched my former employer blatantly breaking the law in various ways, while I was naively wondering how they were going to get away with it. Obviously, I didn't know much about NDAs back then. All my former colleagues I spoke to left with NDAs. (…)

Source: Link

Example 3

As part of my hiring agreement, in working for a health care authority, I was told that I could not talk publicly about the verbal and emotional abuse that I was put through by senior clinicians.

When I went to my supervisor for support, I was told that it was up to me to resolve the situation with this person. (…)

The end result for me was trying to push through the days (over 3 years). Eventually, my mind would not take it and I spent 3 weeks in a local hospital as I had non-epileptic seizures. This was 10 years ago, and I am still not able to work, according to my psychiatrist and neurologist.

There were other abuse situations that I and others in my work place "lived with". (…)

At the age of 57 now, it does not seem possible, as I am still broken.

Source: Link

Appendix C – Discussion of Textual Analysis Tests

C.1 Employee-Sourced Articles

We hand-collect the full text of business-press articles in our sample. To more directly investigate how often employees and journalists share information after the NDA laws, we use textual analysis to search for articles where journalists explicitly cite employees or anonymous sources in articles. We identify these articles by combining two keyword-search methods in Python: (1) a search for exact phrases, and (2) a proximity search of certain strings within 20 words of each other.

In (1), we identify articles that contain the following exact phrases:

current and former employees; former employee; note to employees; memo to employees; fired employees; employees said; informant; anonymous source; unnamed source; people familiar with the matter; according to people familiar; off the record

In (2), we identify articles that contain the following string pairs within 20 words of each other. Because the individual strings can be generic, we make the proximity bandwidth relatively tight.

employee + [complaint; anonymous; said; former; whistleblow; internal]; anonymous + source; several + complaint; internal + complaint

If an article contains (1) or (2), we define an indicator variable equal to 1 for an employee-sourced article at the article level. In our analysis, we define *Employee-sourced Articles* as the total number of employee-sourced articles in a firm-quarter. Below are extracts from examples of these articles in our sample (emphasis added):

"Amazon restricts ad buying by rivals" by Dana Mattioli, Patience Haggin, and Shane Shifflett (*Wall Street Journal*, September 23, 2020):
Amazon.com Inc. is limiting the ability of some competitors to promote their rival smart speakers, video doorbells and other devices on its dominant e-commerce platform, according to Amazon employees and executives at rival companies and advertising firms... The e-commerce giant routinely lets companies buy ads that appear inside search results, including searches for competing products. Indeed, search advertising is a lucrative part of the company's business. But Amazon won't let some of its own large competitors buy sponsored-product ads tied to searches for Amazon's own devices, such as Fire TV, Echo Show and Ring Doorbell, according to some Amazon employees and others familiar with the policy. Roku Inc., which makes devices that stream content to TVs, can't even buy such Amazon ads tied to its own products, some of these people said. In some cases, Amazon has barred competitors from selling certain devices on its site entirely… When the devices team launches a new product, part of its strategy for bringing it to market is to determine which keywords to suppress in advertising, the people said. Employees are told

to mark any discussion of this practice internally at Amazon with "privileged and confidential" in the subject line of emails so that regulators cannot access them, the people said.

"Wells Fargo staffers, fired in scandal, face hiring backlash" by Rachel Louise Ensign (*Wall Street Journal*, September 16, 2019):

It was a few bank accounts opened years ago that got Gerard Camerino fired from Wells Fargo & Co. last October. He didn't even remember some of the customers in question, but the company suspected their accounts were among the millions of potentially fake ones that have dogged the firm for years.

A former private banker in a San Francisco-area Wells Fargo branch, Mr. Camerino denies opening fake accounts. He has since applied for more than two dozen industry jobs and been rejected from them all. He wrote a letter to Wells Fargo retail-bank head Mary Mack asking for help but she sent him to the human-resources department, which told him nothing could be done. After spending most of his career in banking, the 33-year-old isn't sure what to do next. "It's pretty devastating," he said.

Mr. Camerino is one of thousands of low-level branch employees fired by Wells Fargo as part of the bank's effort to get its fake-account problem under control. Those firings began years before a 2016 settlement brought the problems into public view, and they continue quietly today.

Firing employees suspected of dishonesty is standard practice at banks, where success depends on customers trusting a firm enough to leave their money there. But in the case of Wells Fargo, regulators, lawmakers and even the bank's own board have questioned whether the junior staffers were really the ones to blame. Pervasive pressure from managers to meet aggressive sales goals was the root cause of the problem, according to a report from the board.

That has been little consolation for low-level employees caught up in the scandal. Many have found they are now effectively blacklisted from the banking industry.

A bank spokeswoman said Wells Fargo has "made fundamental changes to address the issues that may have contributed to undue sales pressure, while also holding team members at all levels accountable." Executives believe they must fire anyone suspected of opening fake accounts to meet the terms of insurance coverage that protects the bank from fraud-related losses, according to the board report.

But many fired employees say the investigations are stacked against them. The Wall Street Journal spoke to nearly two dozen former retail-bank staffers, including many who were fired…

C.2 Misconduct Articles

We also use textual analysis to identify articles that mention misconduct. For this purpose, we solely use a proximity search of certain strings within 200 words of each other. Because each string is more specific, the proximity bandwidth is wider than in the employee search. To identify misconduct, we search for combinations of the below columns (e.g., we search for both "regulator + allege" and "department + allege"). If an article has one of these combinations, we set an indicator variable equal to 1 for a misconduct-related article. Within this group, if an article has combinations with "regulator," "federal," "department," or "congress," we consider those regulatory-related articles; if an article has "lawsuit" combinations, we consider those lawsuit-related articles.

regulator	review
federal	investigat
department	scrutin
congress	enforcement
lawsuit	probe
	probing
	allege
	alleging
	allegation
	whistleblower
	accus
	wrongdoing
	bribery
	corruption
	violation
	scandal
	lawsuit

Below is an extract from a misconduct-related article in our sample (emphasis added):

"Prosecutors, regulators probe Boeing 737 MAX production issues" by Andy Pasztor and Andrew Tangel (*Wall Street Journal*, April 28, 2020):
Boeing Co. faces criminal and civil scrutiny into years of widespread quality-control lapses on its 737 MAX assembly line, according to people familiar with the details, potentially exposing the plane maker to greater legal liability than previously anticipated by industry and government officials. The inquiries build on a federal grand-jury investigation into hazardously designed flight-control systems, these people said. As part of the expanded probes, Justice Department prosecutors and federal air-safety regulators have been scrutinizing potentially significant safety problems stemming from 737 MAX production missteps, these people said… But simultaneously, the people familiar with the inquiries said, DOJ prosecutors and FAA investigators also have been examining factory problems that raise red flags about the Chicago

plane maker's compliance with mandatory production rules and safeguards. Boeing found debris mistakenly left behind by workers in fuel tanks or other interior spaces of approximately half of the MAX aircraft it inspected starting last November, according to a company spokesman. Another person briefed on the details said most of the undelivered planes have been inspected.

Neither the Justice Department's interest in MAX assembly issues nor the extent of debris discovered inside undelivered MAX planes has been reported before.

Appendix D – Variable Definitions

Post_Law	Indicator variable equal to one for firms headquartered in California, New Jersey, or Illinois during quarters after each state's NDA law is enacted, and zero otherwise.
CorpNews	The total number of articles written by the business press about a firm in a given calendar quarter.
I(High News)	Indicator variable equal to one for firm-quarters with an above-median number of articles written by the business press, and zero otherwise.
CorpNews_Financial	The total number of *CorpNews* categorized by RavenPack in the topics of earnings, revenues, dividends, costs, and credit.
CorpNews_Nonfinancial	The total number of *CorpNews* categorized by RavenPack in the topics of corporate social responsibility, legal, regulatory, labor, crime, civil unrest, industry accidents, equity actions, and others. Others includes the RavenPack topics of marketing, partnerships, products-services, war-conflicts, security, indexes, bankruptcy, and acquisitions-mergers.
ROA	Return on assets, calculated as net income divided by total assets as of the prior fiscal quarter.
Size	Logarithm of one plus the firm's quarterly total assets.
Lev	Leverage is calculated as the ratio of total debt divided by total assets.
I(Loss)	Indicator variable equal to one for firm-quarters with negative ROA, and zero otherwise.
R&D	Total amount invested in R&D. Missing values are set to zero.
I(Missing R&D)	Indicator variable equal to one for firm-quarters with missing values for R&D, and zero otherwise.
Post_Law_HighEmployee	High-employee firms are defined by having an above-median number of employees. *Post_Law_HighEmployee* is an indicator variable equal to one for these firms headquartered in California, New Jersey, or Illinois after the passage of each NDA law, and zero otherwise.
Post_Law_LowEmployee	Low-employee firms are defined by having a below-median number of employees. *Post_Law_LowEmployee* is an indicator variable equal to one for these firms headquartered in California, New Jersey, or Illinois after the passage of each NDA law, and zero otherwise.
Post_Law_HighVisibility	High-visibility firms are defined by having an above-median number of articles in RavenPack in 2018. *Post_Law_HighVisibility* is an indicator variable equal to one for these firms headquartered in California, New Jersey, or Illinois after the passage of each NDA law, and zero otherwise.
Post_Law_LowVisibility	Low-visibility firms are defined by having a below-median number of articles in RavenPack in 2018. *Post_Law_LowVisibility* is an indicator variable equal to one for these firms headquartered in California, New Jersey, or Illinois after the passage of each NDA law, and zero otherwise.
ESS	RavenPack's Event Sentiment Score (ESS) is a number between 0 and 100, where 50 reflects neutral sentiment, above 50 reflects positive sentiment, and below 50 reflects negative sentiment. We recenter ESS to range from -1 to 1, with 0 reflecting neutral sentiment, negative numbers reflecting negative sentiment, and positive numbers reflecting positive sentiment. ESS is provide by RavenPack at the article level.
Pos-Neg	The number of positive non-financial articles minus the number of negative non-financial articles in a firm-quarter, divided by the total number of *Nonfinancial* articles in a firm-quarter. Sentiment is based on *ESS*.
% Positive	The number of positive non-financial articles divided by the total number of *Nonfinancial* articles in a firm-quarter. Sentiment is based on *ESS*.

% Negative	The number of negative non-financial articles divided by the total number of *Nonfinancial* articles in a firm-quarter. Sentiment is based on *ESS*.
Employee-sourced Articles	The number of articles in a firm-quarter written by the business press that reference employee or anonymous sources. Refer to Appendix C for details on how these articles are identified.
Misconduct	The number of articles in a firm-quarter written by the business press that contain keywords related to potential misconduct through regulatory involvement or lawsuits. Refer to Appendix C for details on how these articles are identified.
Regulatory	The number of articles in a firm-quarter written by the business press that contain keywords related to regulatory involvement; a subset of *Misconduct*.
Lawsuit	The number of articles in a firm-quarter written by the business press that contain keywords related to lawsuits outside of regulatory investigations; a subset of *Misconduct*.
Non-Misconduct	The number of articles in a firm-quarter written by the business press that do not contain keywords related to regulatory involvement or lawsuits.

Figure 1: Parallel Trends Analysis of Corporate News

This figure displays the average difference in corporate news of firms in treated states relative to corporate news of firms in control states over time. The figure plots the coefficients of a dynamic difference-in-differences design where we estimate Equation (1), regressing *CorpNews* on indicator variable versions of *Post_Law* that capture the time to treatment. Equation (1) includes time-varying financial controls and fixed effects for a firm's headquarter state, industry-by-quarter, and stock exchange. The x-axis shows the time period relative to the treatment quarter. The y-axis shows the magnitude of the coefficient of our dynamic difference-in-differences estimations. The lines represent 90% confidence intervals. We omit the coefficient of T-1 to use it as a benchmark in the estimations.

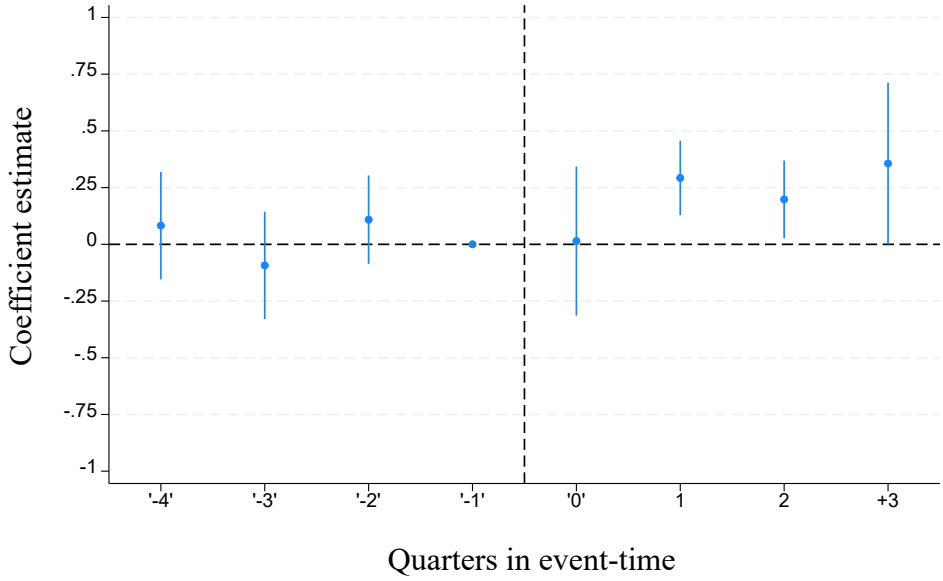

Table 1: Descriptive Statistics

Panel A of Table 1 reports descriptive statistics for key variables. The sample consists of 31,563 firm-quarter observations from 2018 to 2021. All continuous variables are winsorized at the 1% and 99% levels. Panel B of Table 1 presents the Pearson correlation coefficients for all variables. All variables are described in Appendix D.

Panel A: Summary Statistics

	Mean	*Std. Error*	*P25*	*P50*	*P75*
CorpNews	*3.946*	*5.910*	*1.000*	*2.000*	*5.000*
CorpNews_Financial	*1.347*	*1.760*	*0.000*	*1.000*	*2.000*
CorpNews_Nonfinancial	*2.599*	*4.917*	*0.000*	*1.000*	*3.000*
Post_Law	*0.164*	*0.371*	*0.000*	*0.000*	*0.000*
ROA	*-0.003*	*0.056*	*-0.003*	*0.005*	*0.018*
Size	*7.689*	*2.094*	*6.477*	*7.764*	*9.021*
Lev	*0.279*	*0.229*	*0.073*	*0.254*	*0.427*
I(Loss)	*0.289*	*0.453*	*0.000*	*0.000*	*1.000*
R&D	*32.253*	*137.352*	*0.000*	*0.000*	*8.070*
I(Missing R&D)	*0.492*	*0.500*	*0.000*	*0.000*	*1.000*

Panel B: Correlation Matrix

	(1)	*(2)*	*(3)*	*(4)*	*(5)*	*(6)*	*(7)*	*(8)*	*(9)*	*(10)*
CorpNews	*1.000*									
CorpNews_Financial	*0.666****	*1.000*								
CorpNews_Nonfinancial	*0.964****	*0.442****	*1.000*							
Post_Law	*0.060****	*0.025****	*0.063****	*1.000*						
ROA	*0.009*	*0.046****	*-0.006*	*-0.016***	*1.000*					
Size	*0.347****	*0.341****	*0.295****	*-0.051****	*0.356****	*1.000*				
Lev	*0.122****	*0.141****	*0.096****	*-0.031****	*-0.023****	*0.175****	*1.000*			
I(Loss)	*0.007*	*-0.016***	*0.015***	*0.031****	*-0.585****	*-0.329****	*0.077****	*1.000*		
R&D	*0.330****	*0.203****	*0.324****	*0.071****	*0.045****	*0.273****	*0.039****	*-0.001*	*1.000*	
I(Missing R&D)	*-0.056****	*-0.009*	*-0.064****	*-0.131****	*0.115****	*0.221****	*-0.087****	*-0.162****	*-0.228****	*1.000*

Table 2: NDA Laws and Corporate News

This table displays estimates from OLS regressions at the firm-quarter level of *CorpNews* on an indicator variable capturing the enactment of the NDA laws (*Post_Law*), controlling for firm characteristics and fixed effects for a firm's headquarter state, industry-by-quarter, and stock exchange. All variables are defined in Appendix D. Standard errors are clustered by state. ***, **, * indicate significance at the 0.01, 0.05, and 0.10 levels, respectively. T-statistics are shown in parentheses.

Dependent Variable:	*CorpNews*	
	(1)	(2)
Post Law	0.244**	0.243**
	(2.18)	(2.07)
ROA		-11.584***
		(-4.91)
Size		1.146***
		(6.28)
Lev		-0.021
		(-0.05)
I(Loss)		0.590***
		(3.96)
R&D		0.008***
		(4.43)
I(MissingR&D)		-0.225
		(-1.45)
N	31,556	31,556
Adj. R^2	0.164	0.316
State FE	Yes	Yes
Industry x Year-Qtr FE	Yes	Yes
Exchange FE	Yes	Yes

Table 3: NDA Laws and the Type of News

This table displays estimates from OLS regressions at the firm-quarter level of different types of corporate news on an indicator variable capturing the enactment of the NDA laws (*Post_Law*), controlling for firm characteristics and fixed effects for a firm's headquarter state, industry-by-quarter, and stock exchange. In Panel A, we split *CorpNews* into *Nonfinancial* and *Financial* news. *Financial* articles have RavenPack labels of earnings, revenues, dividends, costs, and credit. For *Nonfinancial* articles, we group RavenPack's topics into nine categories: corporate social responsibility, legal, regulatory, labor, crime, civil unrest, industry accidents, equity actions, and others. In Panel B, we divide *Nonfinancial* into the nine separate categories. Control variables are defined in Appendix D. Standard errors are clustered by state. ***, **, * indicate significance at the 0.01, 0.05, and 0.10 levels, respectively. T-statistics are shown in parentheses.

Panel A: *Nonfinancial* and *Financial* news

	CorpNews	
Dependent Variable:	*Nonfinancial*	*Financial*
	(1)	(2)
Post_Law	0.275**	0.004
	(2.22)	(0.10)
ROA	-8.584***	-1.939***
	(-4.68)	(-3.79)
Size	0.795***	0.314***
	(6.16)	(8.52)
Lev	-0.180	0.231**
	(-0.69)	(2.34)
I(Loss)	0.401***	0.192***
	(4.22)	(3.47)
R&D	0.007***	0.001**
	(5.05)	(2.44)
I(MissingR&D)	-0.282**	-0.079
	(-2.33)	(-1.50)
N	31,556	31,556
Adj. R^2	0.298	0.191
State FE	Yes	Yes
Industry x Year-Qtr FE	Yes	Yes
Exchange FE	Yes	Yes

Panel B: Type of *Nonfinancial* news

Type of News (Group)	*Post_Law*	*T-stat*	*N*	*Adj. R^2*
Corporate Social Responsibility	0.001**	2.35	31,556	0.03
Legal	0.022**	2.03	31,556	0.07
Regulatory	0.001	0.18	31,556	0.09
Labor	0.010	1.34	31,556	0.09
Crime	0.001*	1.74	31,556	0.08
Civil Unrest	0.000	1.32	31,556	0.07
Industry Accidents	-0.000	-0.02	31,556	0.05
Equity Actions	0.011	0.77	31,556	0.02
Others	0.228	-0.33	31,556	0.16

Table 4: Cross-sectional Analyses

This table displays estimates from OLS regressions at the firm-quarter level of *Nonfinancial* news on an indicator variable capturing *Post_Law* for (i) firms with higher or lower numbers of employees and (ii) firms with higher or lower media visibility. We control for firm characteristics and fixed effects for a firm's headquarter state, industry-by-quarter, and stock exchange. Control variables are defined in Appendix D. Standard errors are clustered by state. ***, **, * indicate significance at the 0.01, 0.05, and 0.10 levels, respectively. T-statistics are shown in parentheses.

Dependent Variable:	*Nonfinancial*	
	(1)	*(2)*
Post Law HighEmployee	0.558***	
	(3.58)	
Post_Law_LowEmployee	0.079	
	(0.48)	
Post Law HighVisibility		0.303**
		(2.44)
Post_Law_LowVisibility		-0.595
		(-1.31)
ROA	-8.517***	-8.665***
	(-4.57)	(-4.76)
Size	0.785***	0.788***
	(5.90)	(6.07)
Lev	-0.178	-0.192
	(-0.68)	(-0.73)
I(Loss)	0.416***	0.393***
	(4.23)	(4.19)
R&D	0.007***	0.007***
	(4.94)	(5.05)
I(MissingR&D)	-0.282**	-0.265**
	(-2.33)	(-2.18)
N	31,556	31,556
Adj. R^2	0.298	0.298
State FE	Yes	Yes
Industry x Year-Qtr FE	Yes	Yes
Exchange FE	Yes	Yes

Table 5: Positive or Negative Article Tone

This table displays estimates from OLS regressions at the firm-quarter level of a measure of article tone on *Post_Law*. *Pos-Neg* equals the number of positive articles minus negative articles in a firm-quarter, divided by the total number of *Nonfinancial* articles in a firm-quarter. *% Positive (% Negative)* equals the number of positive (negative) tone articles divided by the total *Nonfinancial* articles in a firm-quarter. We control for firm characteristics and fixed effects for a firm's headquarter state, industry-by-quarter, and stock exchange. Control variables are defined in Appendix D. Standard errors are clustered by state. ***, **, * indicate significance at the 0.01, 0.05, and 0.10 levels, respectively. T-statistics are shown in parentheses.

Dependent Variable:	*Nonfinancial Tone*		
	Pos-Neg	*% Positive*	*% Negative*
	(1)	*(2)*	*(3)*
Post_Law	-0.010*	0.000	0.010**
	(-1.77)	(0.09)	(2.56)
ROA	-0.056	-0.187***	-0.131***
	(-0.68)	(-2.84)	(-3.39)
Size	0.005*	0.034***	0.029***
	(1.97)	(20.55)	(13.28)
Lev	0.021*	0.013	-0.008
	(1.72)	(0.94)	(-0.74)
I(Loss)	0.013	0.042***	0.029***
	(1.40)	(5.15)	(4.90)
R&D	0.000***	0.000***	0.000***
	(3.01)	(11.96)	(4.12)
I(MissingR&D)	-0.006	-0.042***	-0.036***
	(-0.73)	(-3.66)	(-5.96)
N	31,556	31,556	31,556
Adj. R^2	0.015	0.102	0.075
State FE	Yes	Yes	Yes
Industry x Year-Qtr FE	Yes	Yes	Yes
Exchange FE	Yes	Yes	Yes

Table 6: Article Informativeness for Investors

This table displays estimates from article-level OLS regressions of market returns *(CAR[0, 1])* on the tone of the article (*ESS)* and an indicator variable capturing the enactment of the NDA laws (*Post_Law).* We include fixed effects for an article's firm, date-by-topic, and industry-by-topic. Control variables are defined in Appendix D. Standard errors are clustered by date. ***, **, * indicate significance at the 0.01, 0.05, and 0.10 levels, respectively. T-statistics are shown in parentheses.

Dependent Variable:	*CAR[0, 1]*	
	(1)	(2)
Post_Law x ESS	0.010***	
	(3.67)	
ESS	0.062***	
	(38.04)	
Post_Law x ESS_Negative		-0.025***
		(-3.73)
Post_Law x ESS_Positive		0.003
		(0.88)
ESS_Negative		-0.080***
		(-24.67)
ESS_Positive		0.051***
		(25.47)
Post_Law	-0.001	0.001
	(-0.63)	(0.38)
N	96,047	96,047
Adj. R^2	0.120	0.129
Firm FE	Yes	Yes
Date x Topic FE	Yes	Yes
Ind x Topic FE	Yes	Yes

Table 7: Direct Evidence on Employee-Sourced Articles

This table displays estimates from OLS regressions that examine whether journalists cite more employee sources after NDA laws. Panel A presents firm-quarter level regressions of *Employee-sourced Articles* on *Post_Law*. *Employee-sourced Articles* is the total number of articles in a firm-quarter that cite employee or anonymous sources. Panel B presents similar regressions split by article topic (*Legal*, *Regulatory*, and *LaborIssues*). We identify employee-sourced articles through textual analysis of the full article content, searching for explicit references to employee sources or anonymous sources (see Appendix C for details). All specifications include firm characteristics and fixed effects for a firm's headquarter state, industry-by-quarter, and stock exchange. Control variables are defined in Appendix D. Standard errors are clustered by state. ***, **, * indicate significance at the 0.01, 0.05, and 0.10 levels, respectively. T-statistics are shown in parentheses.

Panel A: Articles Citing Employee or Anonymous Sources

Dependent Variable:	*Employee-sourced Articles*	
	(1)	*(2)*
Post_Law	0.015**	0.015**
	(2.57)	(2.67)
N	31,556	31,556
Adj. R^2	0.05	0.16
Controls	No	Yes
State FE	Yes	Yes
Industry x Year-Qtr FE	Yes	Yes
Exchange FE	Yes	Yes

Panel B: Topics of Employee-sourced Articles

Dependent Variable:	*Employee-sourced Articles*		
	Legal	*Regulatory*	*LaborIssues*
	(1)	*(2)*	*(3)*
Post_Law	0.029**	0.009	0.017
	(2.19)	(1.45)	(0.60)
N	31,556	31,556	31,556
Adj. R^2	0.07	0.08	0.10
Controls	Yes	Yes	Yes
State FE	Yes	Yes	Yes
Industry x Year-Qtr FE	Yes	Yes	Yes
Exchange FE	Yes	Yes	Yes

Table 8: Misconduct-related Articles

This table displays estimates from OLS regressions examining whether firms experience increased coverage of misconduct-related news after NDA laws. The dependent variable varies across columns: *Misconduct* indicates articles related to any type of corporate misconduct, *Regulatory* indicates articles specifically about regulatory investigations or enforcement, *Lawsuit* indicates articles about legal proceedings, and *Non-Misconduct* captures all other articles. We identify these articles through textual analysis of full articles, using proximity searches of keywords related to misconduct, regulatory action, and litigation (see Appendix C for details). All specifications include firm characteristics and fixed effects for a firm's headquarter state, industry-by-quarter, and stock exchange. Control variables are defined in Appendix D. Standard errors are clustered by state. ***, **, * indicate significance at the 0.01, 0.05, and 0.10 levels, respectively. T-statistics are shown in parentheses.

Dependent Variable:	*CorpNews*			
	Misconduct	*Regulatory*	*Lawsuit*	*Non-Misconduct*
	(1)	*(2)*	*(3)*	*(4)*
Post_Law	0.012*	0.008*	0.012**	0.024
	(1.77)	(1.90)	(2.31)	(0.22)
N	31,556	31,556	31,556	31,556
Adj. R^2	0.14	0.13	0.06	0.20
Controls	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes
Industry x Year-Qtr FE	Yes	Yes	Yes	Yes
Exchange FE	Yes	Yes	Yes	Yes

Table 9: Robustness Analyses

This table displays robustness analyses examining the association between the NDA laws and corporate news using alternative specifications. In Panel A Column (1), the dependent variable is *I(High News)*, an indicator variable equal to 1 if a firm-quarter has an above-median number of business-press articles. In Column (2), the dependent variable is *CorpNews* but the model is a Poisson regression. In Panel B, we estimate stacked regressions. In Column (1), the dependent variable is *I(High News)* and we include financial controls. In Column (2), the dependent variable is *CorpNews* and we do not include financial controls. All specifications include fixed effects for a firm's headquarter state, industry-by-quarter, and stock exchange. Control variables are defined in Appendix D. Standard errors are clustered by state. ***, **, * indicate significance at the 0.01, 0.05, and 0.10 levels, respectively. T-statistics are shown in parentheses.

Panel A: Alternative Specifications

Dependent Variable:	*I(High News)*	*CorpNews* *Poisson Regression*
	(1)	(2)
Post_Law	0.022***	0.047**
	(2.98)	(2.37)
N	31,556	31,563
Adj. R^2	0.201	-
Controls	Yes	Yes
State FE	Yes	Yes
Industry x Year-Qtr FE	Yes	Yes
Exchange FE	Yes	Yes

Panel B: Stacked Regressions

Dependent Variable:	*I(High News)*	*CorpNews*
	(1)	(2)
Post_Law	0.024***	0.261**
	(3.05)	(2.07)
N	79,235	79,235
Adj. R^2	0.214	0.335
Controls	Yes	No
State FE	Yes	Yes
Industry x Year-Qtr FE	Yes	Yes
Exchange FE	Yes	Yes

Online Appendix

Table A.1: Firm-Initiated News

This table repeats Equation (1) using *Firm-Initiated News* as the dependent variable, which equals the total number of firm-initiated press releases in a firm-quarter. *Post_Law* is an indicator variable capturing the enactment of the NDA laws. All specifications include fixed effects for a firm's headquarter state, industry-by-quarter, and stock exchange. All variables are defined in Appendix D. Standard errors are clustered by state. ***, **, * indicate significance at the 0.01, 0.05, and 0.10 levels, respectively. T-statistics are shown in parentheses.

Dependent Variable:	*Firm-Initiated News*	
	(1)	(2)
Post_Law	0.066	0.059
	(1.41)	(1.33)
ROA		-1.367
		(-1.44)
Size		0.011
		(0.36)
Lev		0.553***
		(2.74)
I(Loss)		-0.205***
		(-3.30)
R&D		-0.000
		(-0.08)
I(MissingR&D)		0.065
		(0.70)
N	31,556	31,556
Adj. R^2	0.040	0.045
State FE	Yes	Yes
Industry x Year-Qtr FE	Yes	Yes
Exchange FE	Yes	Yes

Table A.2: Robustness Tests - Entropy-Balanced Weighting

This table regresses *Nonfinancial* corporate news on *Post_Law* using entropy-balanced weighting. *Post_Law* is an indicator variable capturing the enactment of the NDA laws. All specifications include fixed effects for a firm's headquarter state, industry-by-quarter, and stock exchange. All variables are defined in Appendix D. Standard errors are clustered by state. ***, **, * indicate significance at the 0.01, 0.05, and 0.10 levels, respectively. T-statistics are shown in parentheses.

Dependent Variable:	*CorpNews Nonfinancial*	
	(1)	(2)
Post_Law	0.261**	0.274**
	(2.28)	(2.21)
ROA		-8.129***
		(-5.71)
Size		0.676***
		(8.48)
Lev		0.009
		(0.05)
I(Loss)		0.356***
		(3.36)
R&D		0.007***
		(6.89)
I(MissingR&D)		-0.278***
		(-3.37)
N	31,556	31,556
Adj. R^2	0.165	0.319
State FE	Yes	Yes
Industry x Year-Qtr FE	Yes	Yes
Exchange FE	Yes	Yes

Table A.3: Alternative Clustering

This table regresses *Nonfinancial* corporate news on *Post_Law* as in Table 3 Column (2) but alters the clustering of standard errors. In Column (1), we cluster standard errors by firm, and in Column (2), we cluster by state and industry. *Post_Law* is an indicator variable capturing the enactment of the NDA laws. All specifications include fixed effects for a firm's headquarter state, industry-by-quarter, and stock exchange. All variables are defined in Appendix D. ***, **, * indicate significance at the 0.01, 0.05, and 0.10 levels, respectively. T-statistics are shown in parentheses.

Dependent Variable:	*CorpNews Nonfinancial*	
	(1)	(2)
Post_Law	0.275**	0.275**
	(2.44)	(2.19)
N	11,480	11,480
Adj. R^2	0.189	0.328
Controls	Yes	Yes
State FE	Yes	Yes
Industry x Year-Qtr FE	Yes	Yes
Exchange FE	Yes	Yes
Cluster	Firm	State and Industry

Table A.4: Is the Result Driven by one unique State?

This table regresses *Nonfinancial* corporate news on separate indicator variables for the post-period in each treated state (*Post_CA, Post_IL, Post_NJ).* All specifications include fixed effects for a firm's headquarter state, industry-by-quarter, and stock exchange. All variables are defined in Appendix D. Standard errors are clustered by state. ***, **, * indicate significance at the 0.01, 0.05, and 0.10 levels, respectively. T-statistics are shown in parentheses.

Dependent Variable:	*CorpNews Nonfinancial*
	(1)
Post_CA	0.126*
	(1.70)
Post_IL	0.448***
	(7.32)
Post_NJ	0.542***
	(5.84)
N	31,556
Adj. R^2	0.298
Controls	Yes
State FE	Yes
Industry x Year-Qtr FE	Yes
Exchange FE	Yes